The effects of school resources on participation in post-compulsory education:

Danish quasi-experimental evidence, and evidence that controls for family, school and neighbourhood effects*

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Abstract:

This paper is concerned with the relationship between class size and the student outcome - participation in post-compulsory schooling. Research on this topic has been problematic partly because omitted unobservables, like parents' incomes and education levels, are likely to be correlated with class size. Two potential ways to resolve this problem are to exploit experimental or instrumental variation. In both cases, the methods require that variation in both class size and the outcome should not be contaminated by other unobservable factors that affect the outcome – like family background. An alternative approach is to take advantage of variation in class size between siblings which allows unobservable effects to be differenced out. The distinctive aspect of this study is that we contrast results which attempts to exploit natural "quasi-experimental" variation in class size, arising from an administrative rule that determines size, with results that control for family, neighbourhood and school effects by exploiting differences between siblings within households. Our aim is to examine the robustness of estimates of the effects of class size in the context of length of education.

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1. Introduction

Academic and policy interest in improving schools comes from recognising the importance of human capital formation for individuals and society. This is based on theoretical models, and empirical evidence, that relates income, productivity and economic growth to the quantity of schooling - the most common proxy for the stock of human capital.

Class size is often a focus for both policy action and research interest because it is easy to measure and, apart from the opportunity cost of students' time, it is the most important cost of education. In Denmark, 80% of compulsory schooling expenditure goes to pay teachers' wages, and this factor alone explains 60% of the variance in expenditure between schools. Similar expenditure shares are accounted for by teachers pay in the US and the UK (Hanushek 2002).

There are many models of the effects of class size on learning outcomes, from economics and other disciplines. For example, Lazear (2001) postulates that children in smaller classes can learn more, because of the lower probability of interruption to teaching, if the probability of a student interrupting teaching is independent across students. Since an interruption in class requires that teaching be temporarily suspended this imposes a negative externality on everyone else in the class which is larger the larger is the class size. Of course there are other benefits to teaching in small classes too, but this model captures an important feature of class size and gives rise to a specific functional form for the educational production function.

One important implication of the Lazear (2001) model is that optimal class size is larger if students are well behaved, and/or if schools can assign weaker and/or more disruptive children to smaller classes, then local public education authorities should facilitate smaller class sizes in schools with a higher proportion of disruptive and/or weaker children. If such resource allocation occurs, but it is not able to entirely offset existing achievement differentials, then empirically this should give rise to a spurious association between smaller classes and lower student achievement. It is exactly this raw correlation which has been found in datasets from around the world (Hanushek, 2003). This motivates the need for sources of exogenous variation in class size in order to uncover the size of whatever causal mechanism is at work.

On important weakness in the Lazear model is that there is no role for other inputs into the educational production function. In particular, parental provided inputs – which might be their own time, or time purchased from others, or capital inputs like a place to study at home. We would expect that such parental inputs would compensate for variation in school provided inputs. Thus, the existing literature may well estimate the effect of school resources net of the compensation that parental resources make. But parental resources are finite and parents may have a preference for ensuring that their children each have equal resources, so we might expect changes in school inputs for one child to result in a switch in home inputs away from siblings.

This paper is about the effect of school resources on length of completed education: in particular the effects of the Danish rules that determine class size and students per teacher hour. As such this paper is placed within the class size and student outcome literature. Like Browning and Heinesen (2004) (henceforth BH) for Denmark, and earlier work by Angrist and Lavy (1999) for Israel, our analysis applies a regression discontinuity design based on administrative rules for compulsory schooling. The variation in actual class size is driven by the interaction between random variation in cohort size and administrative rules that place a cap on class size. However, our analysis also pays attention to the home environment.

The presumption in the approach is that parents do not (or cannot) exploit the administrative rules because they do not know how large the cohort is. Whether this is true is arguable. Parents may be able to form a reasonable forecast, from pre-enrolment school meetings, of the likely number of classes in the cohort several months before enrolment. Parents who place a high value on education quality may be more likely to avail themselves of the (relatively inexpensive) private schooling option, or the option of delaying entry for a year, when faced with a cohort is of a size likely to generate large class sizes. On the other hand, there is some risk associated with such an action, and there may be a free-rider issue that mitigates towards parents taking no action.

Thus, here we contrast results based on the administrative rules with analysis which eliminates unobservable family preferences by sibling differencing. We control for family effects by exploiting our ability to take sibling differences for the population of students attending 8th grade during the 1980's. We restrict ourselves to siblings who attended the same school to control for school effects. And since schools are strictly associated with catchment areas (although this has been relaxed since 1993) this effectively controls for neighbourhood effects.

Our headline sibling difference result is that a 5% reduction in class size in 8th grade gives rise to 0.015 more years of education and a 5% reduction in the students per teacher hour ratio in 8th grade gives 0.022 more years on average. In contrast our results based simply on levels typically show that class size has an insignificantly positive effect on education length.

The remainder of the paper is organised as follows. The next section reviews the literature and places our contribution within that. A data description is followed by estimation results, interpretation and discussion. Finally we conclude with an agenda for more research.

2. Literature

As befits the importance of the issue, the relevant literature is extensive. Recent reviews of the literature can be found in Hanushek (2003) and Krueger (2003). Much of the literature consists of correlations between outcomes such as test scores, education length, or educational attainment and class size and related inputs using observational data and is therefore vulnerable to the criticism that the correlations are contaminated by unobservable heterogeneity. Here we highlight those contributions that are particularly relevant to our own research which focuses on the estimation of the causal effect of class size.

Hanushek (2003), based on a meta-analysis of many studies where each chosen *estimate* gets equal weight and the estimated standard error of each estimate is ignored, argues that input-based schooling policies have failed. Krueger (2003) on the other hand, conducts a meta-analysis based on the same set of studies, but gives each *paper* equal weight, and finds that reducing class size does improve educational outcomes.

The one and only truly experimental study is Krueger (1999) which analyses the Tennessee Student/Teacher Achievement Ratio (STAR) experiment which was conducted in the late 1980's. This involved random assignment of 11k students during grades 1-4 into classes of either about 15 students or about 22. Students attending smaller classes obtained significantly higher test scores 10 years after having left 4th grade. Criticisms of such experimental work include Hawthorne effects, cream skimming administrative placement, and charges of parental influence in student allocations. Nonetheless, a substantive contribution of Krueger (2003) is to make cost-benefit calculations of class size reductions based on his earlier Tennessee STAR estimates. Krueger uses estimates of the effects of test scores on subsequent earnings together with his estimates of class size on test scores to show, assuming a 4% discount rate, that these effects imply that the economic benefits of such a class size reduction are worth roughly twice what it costs.

Angrist and Lavy (1999) use the Maimonides' rule that limits the maximum class size in Israeli schools to be 40. The implied discontinuity in the relationship between grade enrolment and class size is used to provide exogenous identifying variation. In this regression discontinuity design, the administrative rule-based class size is used as an instrument for observed class size. Reductions in class size are found to increase end of grade test scores for 4th and 5th graders but not for 3rd graders.

Hoxby (2000) looks at Connecticut elementary schools and exploits cross county variation in the birth rate and the cross county variation in rules that determine the minimum and maximum class size. No class size effects are found.

Case and Deaton (1999) analyse class size during apartheid in South Africa. Black parents were unable to choose their children's school and school resource allocation was arguably exogenous. On the basis of aggregated data at the district level, reductions in class size in the range 80-50 students were found to have positive educational outcomes.

Woessmann and West (2002) use the Third International Maths and Science Study (TIMSS) to examine the relation between class size and test scores for two classes in two consecutive grades in schools. They address within-school between-class and between-school sorting using respectively average class size within the grade as an instrument together with school fixed effects. Sizable beneficial effects of smaller class sizes are found only for Greece and Iceland, where teacher salaries are relatively low. Recent work by BH follows Angrist and Lavy (1999) in using the Danish version of Miamonides' rule for maximum class size applied to Danish 8th grade students. Administrative rules that determine the ratios of students per teacher hour are also used. They find large, but imprecise, effects of reducing these resource measures on increasing length of completed education. Their results imply, for example, that a 5% reduction in class size and students per teacher hour ratio during 8th grade causes a 0.07 and 0.14 increase in the length of completed education.

By looking at students with the same mother, same father and attending the same school, our paper extends BH since we can then control for family, school and neighbourhood effects that may, despite exploiting the rule-based resource allocations, contaminate their estimates. Indeed, BH find that including observable family variables makes a large difference to the estimated effect of class size, even though they presume that the class size variation is exogenous. In our preferred specification we find small but very precisely determined positive effects of school resources on length of education: A 5% reduction in class size and students per teacher hour ratio during 8th grade causes a 0.015 and 0.022 years increase in length of completed education 10 years later respectively. Although our results are statistically significant they are less than one-quarter of the size of the effects imprecisely estimated by BH. The greater precision of our estimates is due to our larger sample size, the whole population compared to BH's 10%, and our ability to control for more variation in the data which might otherwise compromise the experimental nature of the institutional setup that we are both exploiting. Controlling for all that is fixed (both observable and unobservable) about the school, family and neighbourhood distinguishes the effect of different (locally random) realisations of the rules from the confounding effects of allocations of resources and students between schools, families and neighbourhoods. For BH, a stochastic implementation of the rule, or a fuzzy design, reduces the explanatory power of their instrument (the class size predicted by the rule) but should not bias their estimated class size coefficient of interest. For us, applying the rule directly induces measurement error, which should bias the estimated coefficients of interest towards zero - at least if it were classical measurement error. For our differenced or within-family model, measurement error is much greater than in the levels and we attempt to address this problem in our analysis.

Most studies of class size examine the effect on test scores taken at then end of a grade. While immediate cognitive achievement changes are useful short run outcome measures, their persistence has been called into question. Educational attainment, or length of completed education, is the outcome we consider here. It is a long run outcome, which is strongly correlated with later earnings, and other adult outcomes.

3. Danish Education System

3.1 Financing public school expenditure.

Attendance at primary and lower secondary school (grades 1-9, corresponding roughly to ages 7-15) is compulsory in Denmark. Education is a requirement from 1 August in the year that the child turns seven years old until 31 July in the year which regular instruction has been received for 9 years. During the period 1981-1990 analysed in this paper, 89% of children attended public (i.e. state funded) schools. These 1826 (in 1990) schools are run by 275 municipalities, and are attended by an average of 309 students. Municipalities have a mean population of 36,094 residents, but this ranges from 2,512 to 466,723 (Copenhagen), and the number of schools per municipality ranges from 1 to 76 accordingly. Public school expenditure is financed through municipal income tax, together with a complex between-municipality redistribution scheme, which subsidises expenditures in low income municipalities. Average total expenditure per student per year was DKK 31,360 in 1990 (corresponding to €4,248 in 2005 prices), having risen steadily from DKK 18,447 in 1981 (€3,713 in 2005 prices). The total number of students fell consistently throughout the period, from 728,900 in 1981 to 559,600 in 1990 due to smaller birth cohorts. The net effect was a reduction in expenditure on public schools between 1981 and 1990 from €2.629 billion to €2.365 billion (2005 prices).[1]

There is a large variance in public school expenditure between municipalities (coefficient of variation of 0.13). Changes in expenditure can largely be attributed to reductions in agreed teacher working hours and increased seniority. Between-municipality variation in teacher salary weighting, proportions of school children of different ages, and students whose mother tongue is not Danish, explains some of the variation, but much of the variance cannot be explained by observable municipality characteristics (Graversen and Heinessen, 1999).

3.2 Allocating students to schools and subsequent schooling choices.

During the analysis period, the allocation of public school places was on the basis of catchment area of place of residence at the beginning of the calendar year of first grade start. Parents are required to sign their children up to a school latest the start of the year in which the child turns seven years old. Should a child move home to a different catchment area, that public school is obliged to offer a place from the beginning of the month following the move. 11% of children attended a private school and these are heavily subsidised (on average 85% of expenditures are provided by the municipality)[2]. Private schools are mostly found in urban areas and are disproportionately attended by the children of highly educated parents. While average educational attainment is higher for students having attended private school, this is no longer the case after allowing for selection into private schooling on the basis of observable characteristics (Rangvid, 2002). If it is the case that children attending private schools respond differently to class size then this may lead to bias in a class size coefficient estimated only on public school children. Private schools have a lower mean class size than public schools, and if parents are behaving rationally they ought to place children who respond better to class size in smaller private school classes. This ought to bias, towards zero, class size coefficients estimated on a sample where such students are selected out.

Students can leave lower secondary school after grades 7 (or 8) in order to attend a "continuation school", usually a private boarding school, and 1% (8%) take up this opportunity. In addition to the nine compulsory grades there is a voluntary 10th grade attended by 50% of those leaving 9th grade. On completing 9th or 10th grade respectively 95 and 90% of students take the public school final examinations..

Having completed lower secondary education, 7% never return to the educational system, 33% go to upper secondary school and 59% do vocational training. These transitions are most often immediate after a summer recess and the courses last two or three years, with completion rates of 88% for lower secondary school and 86% for vocational training. Upon completion, subsequent transitions to higher education occur on average after 18 and 13 months respectively. This study gap is explained by short term employment, travel and admission criteria limiting places. Destinations from upper secondary are 26% vocational education, 62% higher education, 11% no further education. Times to completion average 2.4 years for vocational training and 3.6 years for higher education, with completion rates of 73% and 60% respectively. Education Ministry estimates of the average expected total time to completion of

[1] See Danish Ministry of Education and Research (1993) for further details.

education for those commencing first grade in 1981, 1990 and 2000 was 13.1, 14.0 and 15.1 years respectively. In summary, post-compulsory education is in two broad phases with a study gap of more than a year on average between the two. Completion rates are lower for higher and longer courses. There is a large variance in times to completion, explained by different routes, gaps and course lengths. Of those entering 8th grade in 1990 15% were enrolled at an educational institution in 2001, although less than 5% of the cohort were enrolled in 2003.

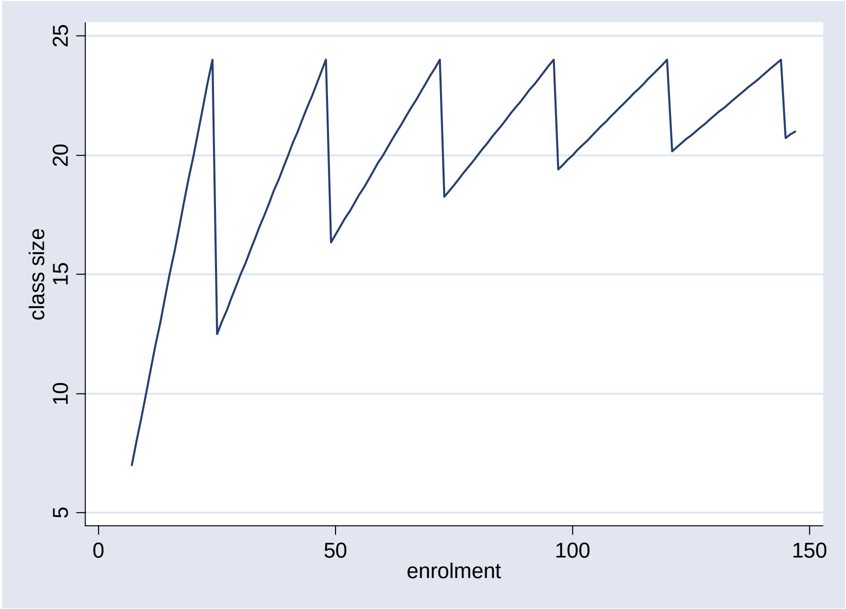
3.2 Class size and students per teacher hour rules

The student per teacher ratio averaged 11.9 in 1981 and fell gradually to 10.1 in 1990. Mean class size remained at 18.2 throughout.

Primary and lower secondary public schools are comprehensive, whereby students are allocated to a class on entry, and most lessons will be taught to the same class group throughout all grades. A national curriculum stipulates the number of hours required teaching in each of 15 subjects at each grade level. Two hours of optional subjects are introduced first at 8th and 9th grades. Danish education law stipulates a maximum class size of 28 students for primary and lower secondary schools. Municipalities are free to implement their own class size rules subject to this restriction. In practice, BH and Heninsen and Rangvid (2003) show that an additional class is typically added at multiples of 24 students, making the effective class size maximum 24 students. This is to avoid the situation where new student enrolment at later grades would force a class to be divided in accordance with national law. The result is the discontinuous relationship between class size and school year group enrolment shown in Figure 1. Formally, the number of classes that a given school-grade-year needs to be split into, NCLASS = (INT (ENROL-1) / 24) + 1, where ENROL is the number of students enrolled in the given school-grade-year. Average class size for the school-grade-year is then CSIZE = ENROL / NCLASS. For example, enrolments 1-24, 25-48, 49-72 correspond to 1, 2, 3 classes respectively. Enrolments 24, 25, 48, and 49 correspond to average class sizes of 24, 12.5, 24, and 16.3 respectively. This is similar to Angrist and Lavy (1999) use of Maimonides' rule whereby Israeli public school class sizes cannot exceed 40 students.

[2] Danish Ministry of Education (2002).

Figure 1 Class size and 8th grade enrolment



Over and above the requirements for specific teaching hours dedicated to different curriculum subjects, the Danish Ministry of Education recommends a number of teacher hours per student per week, but does not impose these recommendations. Municipalities are free to interpret the guidelines for teacher hours, and their implementation varies between municipalities and within municipality over time. However, it is municipalities rather than schools that finance the incurred teacher and class expenditures associated with the class size rule and teacher hour recommendation. While municipalities themselves may trade-off, for example, books for teachers, this is not a substitution that is being made at the school level. Following BH and Heninsen and Rangvid (2003) we use the administrative rule for teacher hours which was in force in Copenhagen, the largest municipality, during the school year beginning 1991. The rule is a step function of enrolment per school-grade-year, ENROL, and the number of classes, NCLASS as indicated in Figure 2.

There is obviously a discontinuous relationship between number of students per teacher hour and enrolment - given in Figure 3. The maximum class size rule is behind the larger of the discontinuities, but compensatory allocation means that variation either side of the discontinuity is not as large as for the class size rule. Also, peaks in the students per teacher hour rule trend up with enrolment, whereas peaks in the class size rule do not. However, it is clear that, in expectation, schools with large enrolments have larger class sizes and more students per teacher hour.

Figure 2 Teacher hours per student rule: by number of classes 1-4

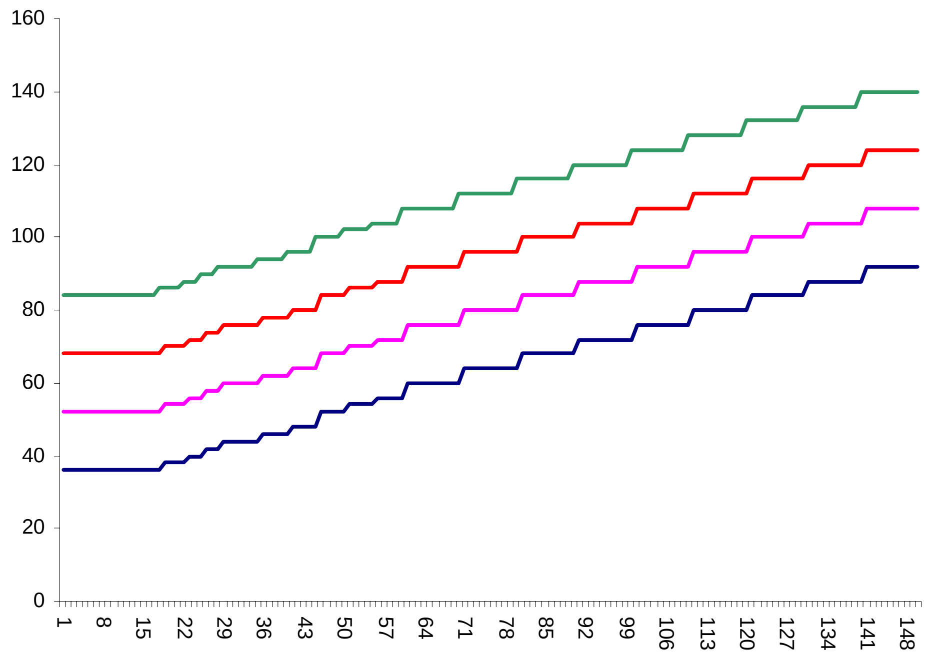
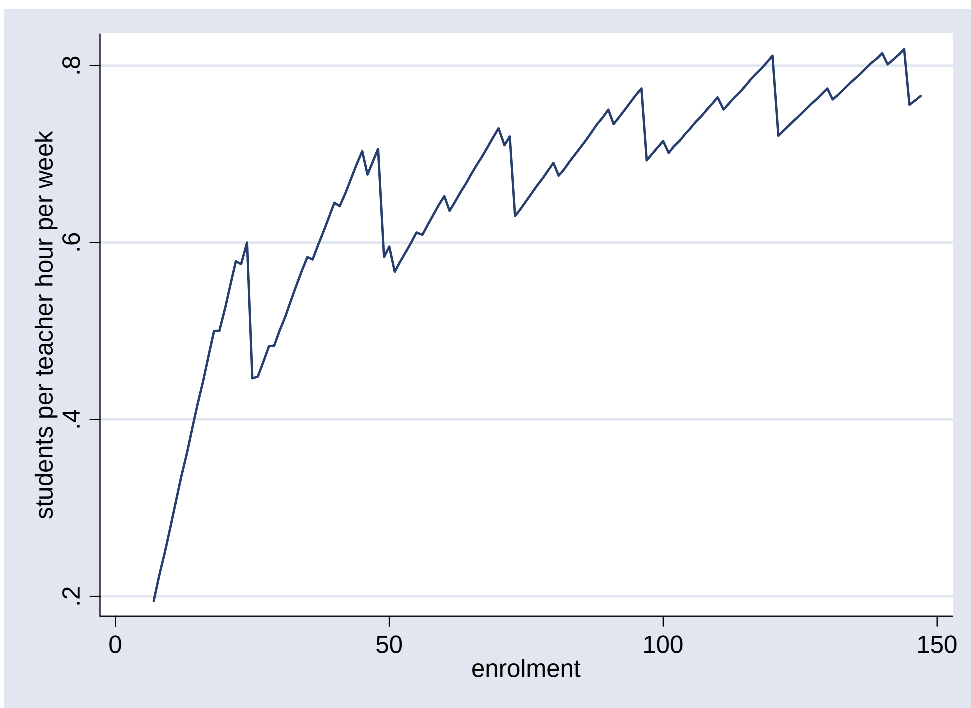


Figure 3 Students per teacher hour and 8th grade enrolment



4. Data Description

The dataset we use is based upon a very small number of variables from two administrative databases containing individual information for *all* residents of Denmark: the Central Person Register and the Integrated Student Register. The Central Person Register is a national administrative database that contains social security numbers that enable links between all children and their legal mother and legal father. Moreover, this enables us identify siblings. Our objective is to choose a sampling frame that controls for as many unobservable fixed school and family effects as possible, and allows us to estimate a relatively clean class size and students per teacher hour effect. Our motivation is to control for school (and, hence, neighbourhood), mother and father fixed effects at the same time by estimating class size difference effects within the group: same mother, same father and same school. Non-informative observations are dropped: (1) singletons (2) sibling groups where each goes to a different school at 8th grade (3) half-siblings (4) multiple-births. Finally dropping the 0.1% of remaining households with more than 6 siblings leaves an estimation sample which is described in Tables 1 and 2.

The student register links unique student social security numbers to school identifiers for 8th grade (children aged around 14) and above on 1 October each year, 2 months after the start of the school year. We are able to use this match and so calculate school enrolments in each grade-year consistently from 1981 until 1990.

It is important to note that, unlike BH, the data available to us, although much larger, does not contain actual class size and teacher hours. However, we do observe enrolment and can apply the administrative rules to compute the class size and teacher hours that should affect each child. BH use this information to create instrumental variables for actual class size and teacher hours. We use this information directly as explanatory variables, following Van der Klaauw (2003).

The outcome of interest, and dependent variable throughout, is number of years schooling completed after beginning 8th grade. This is a long-run outcome measure which is not subject to the criticisms faced by immediate test score measures that they are not persistent. It is also simple to compute, and not subject to value judgements on the part of the researcher regarding the number of years a particular education is "worth" in comparison to other educations. However, although more years in education is positively correlated with obtaining higher qualifications, it is not unambiguously a good outcome. Late completers in typically short educations are counted equally as those with average completion times in educations that typically take longer.[3]

Table 1 describes the dataset used in analysis. There are 141,186 households containing 299,283 children (note that one child households have been dropped) – 77% of them are in 2-sibling, 20% in 3-sibling, 2.5% in 4-sibling, 0.3% in 5-sibling, and 0.08% in 6-sibling households. The distribution of our outcome (education length), and of the explanatory variables that are of primary interest (class size and students/teacher hour/week) is tabulated according to values of other explanatory variables used in the analysis. It is clear that neither class size not students per teacher hour are constant across groups which reflects the strong concentration of large households in rural areas where class sizes tend to be smaller. There are correspondingly large differences in education length across sibling sizes.

Table 2 describes the sibling differenced data. There is a marked tendency for the differences between siblings to get larger in larger households, and inter-sibling differences in both class size and students/hour tend to be larger in larger households.

Figure 4 shows the distribution of 8th grade school enrolment - that is, the number of schools which have that number in the 8th grade cohort. Figures 5 and 6 show the distributions of class sizes and pupil/teacher hours. Comparing the enrolment distribution with Figures 1 and 3 shows that there are large discontinuities where the distribution of school year group sizes is quite dense. Thus, as can be seen, in Figures 5 and 6, there are many small schools but few schools which exceed an enrolment of 100 in 8th grade.

Figure 7 shows the operation of the administrative rule applied to our data. There are clear class size discontinuities. However, there is a clear variance in class sizes that is systematic. Where we get close to the discontinuity the variance rises.

[3] In future work we intend to link qualifications obtained to “normal” completion times (Education Ministry 2002) in order to measure effects on normalized education length and timely completion.

Table 1 Summary Statistics for Levels

	frequency	%	education length Mean	education length *st.dev.*	students/hour mean	students/hour *st.dev.*	class size mean	class size *st.dev.*
# Siblings								
2	251050	83.9	7.18	*2.43*	0.653	*0.074*	20.17	*2.49*
3	43578	14.6	6.86	*2.52*	0.652	*0.076*	20.14	*2.53*
4	4136	1.4	6.34	*2.66*	0.645	*0.079*	19.98	*2.56*
5	435	0.15	5.51	*2.69*	0.642	*0.084*	19.89	*2.62*
6	84	0.03	5.82	*2.63*	0.605	*0.105*	18.88	*2.94*
Female	147839		7.21	*2.37*	0.653	*0.074*	20.16	*2.49*
Male	151444		7.03	*2.53*	0.653	*0.075*	20.16	*2.50*
Subsequent children	158097		7.08	*2.39*	0.650	*0.075*	20.09	*2.53*
First child	141186		7.17	*2.52*	0.656	*0.074*	20.24	*2.46*

Table 2 Summary Statistics for Sibling Differences: Differences from Family Mean

	frequency	education length mean	education length *st.dev.*	students/hour mean	students/hour *st.dev.*	class size mean	class size *st.dev.*
# Siblings							
2	251050	1.084	*0.866*	0.025	*0.021*	1.167	*0.937*
3	43578	1.282	*0.988*	0.030	*0.024*	1.383	*1.085*
4	4136	1.387	*1.074*	0.033	*0.026*	1.492	*1.154*
5	435	1.497	*1.183*	0.033	*0.028*	1.526	*1.212*
6	84	1.635	*1.286*	0.033	*0.030*	1.515	*1.054*
Female	147839	1.102	*0.876*	0.026	*0.021*	1.201	*0.963*
Male	151444	1.133	*0.906*	0.026	*0.021*	1.206	*0.971*
Subsequent children	158097	1.126	*0.897*	0.026	*0.021*	1.215	*0.976*
First child	141186	1.108	*0.885*	0.026	*0.021*	1.191	*0.958*

Figure 4 Distribution of 8th grade enrolment size

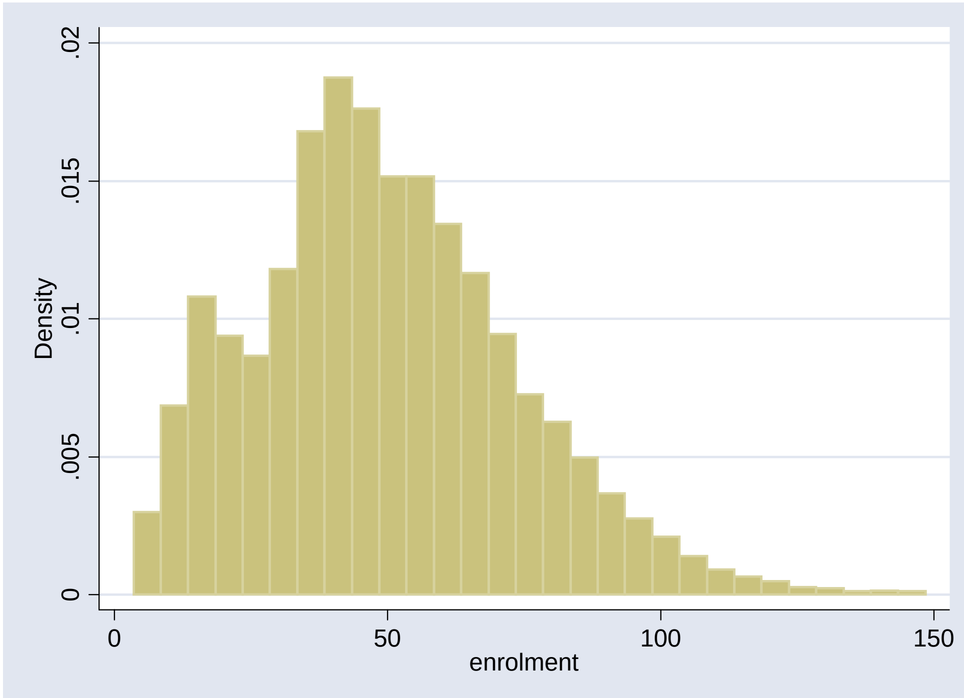


Figure 5 Distribution of rule given students per teacher hour at 8th grade

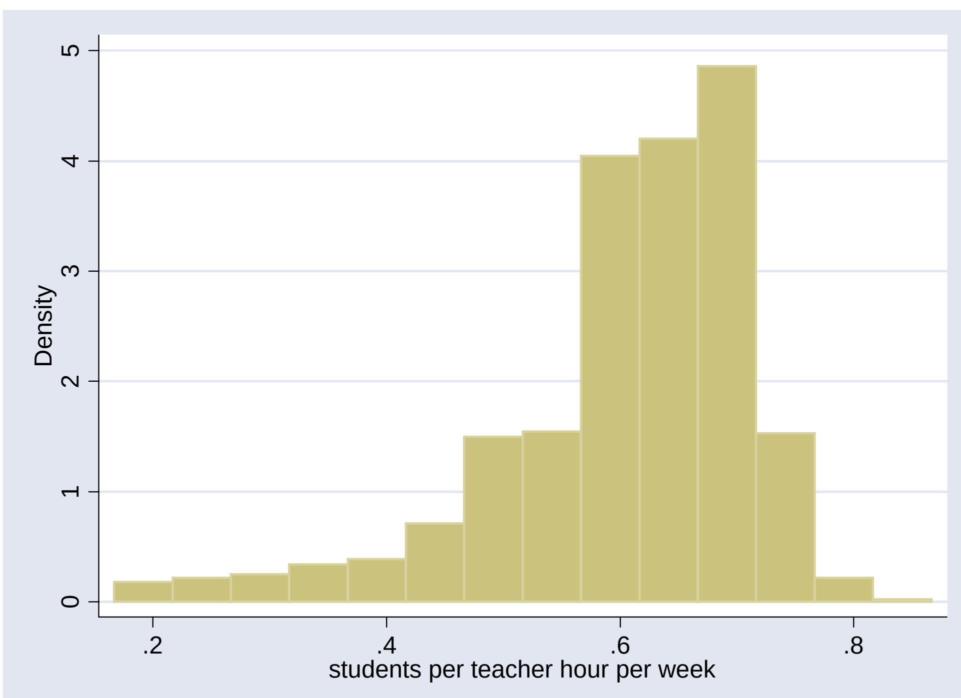


Figure 6   Distribution of (24 maximum) rule given class size at 8th grade

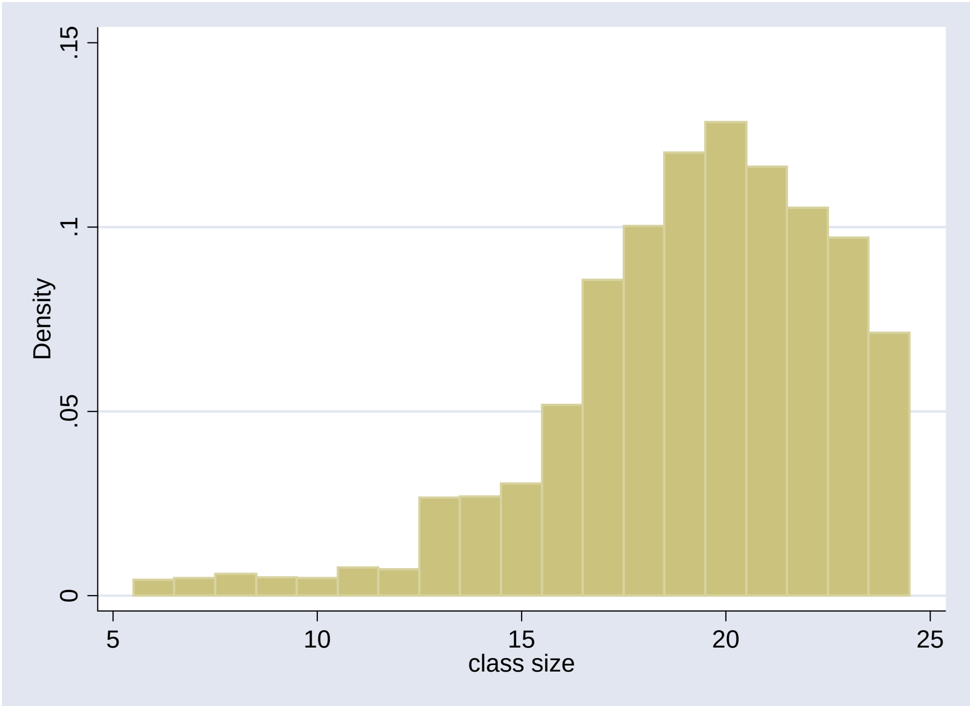
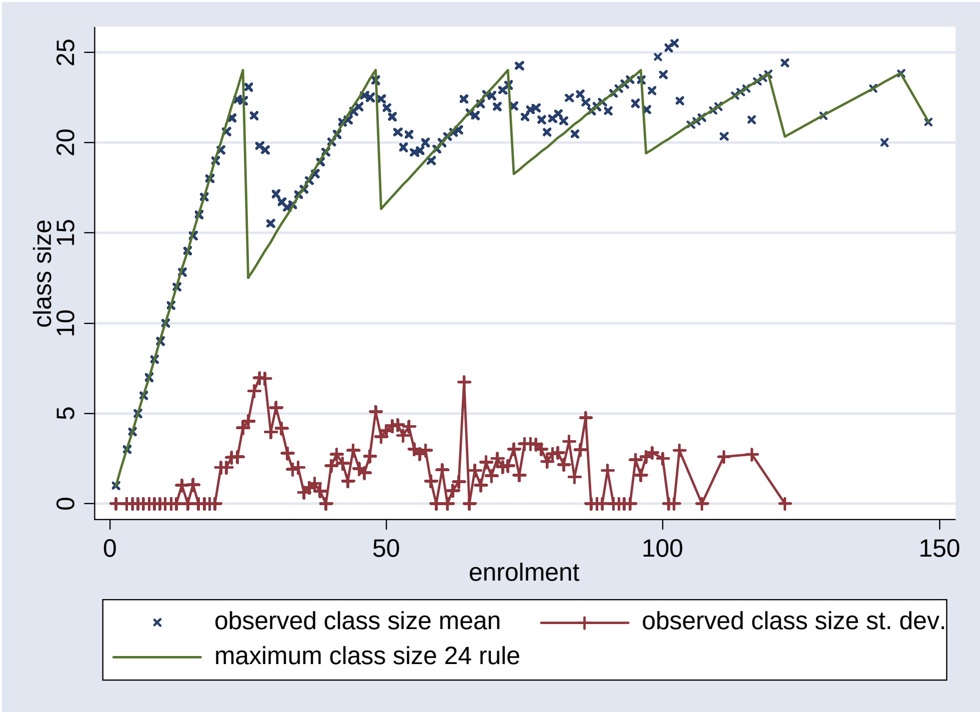


Figure 7. Class size rule and distribution of observed 8th grade class size 2000-2003



5. Estimation

Van der Klaauw (2002) uses a "fuzzy" regression-discontinuity design, albeit in a different context. With fuzzy rather than sharp designs, the treatment rule is non-deterministic. Van der Klaauw substitutes a non-parametric estimate of the conditional expectation of treatment for the endogenous regressor. In the present context, where true class size is not observed but enrolment is, expected class size, calculated from the above rule, is used. Similarly, in the case of students per teacher hour, rule-based expected students per teacher hour, is used. There are two important identifying assumptions:

1. Parents do not exploit administrative rules in order to place their children in schools with smaller classes or fewer students per teacher hour. This is the conditional independence assumption of Hahn, Todd and van der Klaauw (2001). This seems plausible, as parents could not know which side of a discontinuity their school-grade-year would fall until after having signed up and enrolment was calculated. Thereafter, changing school would require leaving the public system or moving to accommodation in another public school catchment area. The fixed costs of such changes make them seem unlikely.

2. In levels, treatment effects are only locally identified at the point where the treatment probability changes discontinuously. This motivates BH to use data close to discontinuities in their analysis. In differences, or within groups, treatment effects are only locally identified where treatment probability differs within group.

In such fuzzy discontinuities there is the potential that our imputed class size will be measured with error and that the sibling difference estimates are then contaminated by sizeable measurement error. Thus, our sibling difference estimates should be regarded as a lower bound because of the attenuation bias they may exhibit. However, the fuzziness that infects our data is considerably smaller in urban areas and we try to push this bound by investigating the class size and teacher hour effects on the outcome in the Copenhagen, Aarhus, and Alborg areas which are a single administrations and our rule should then be exact and measurement error disappears.

6. Results

Here education length is measured up until 11 years after 8th grade. Our enrolment observation window spans the years 1981-1990 and the latest year for which we have educational institution registration is 2001, which dictates that 11 years is the longest time period for which we can be sure to observe all siblings after 8th grade. Thus, our data consists of observations for which we can observe a sibling whose age difference is no more than 9 years. In the first instance, we treat this data as a sample of family averages and the results are reported in Table 3[4]. We report specifications that include log class size, log teacher hours per student, both, and both plus their interaction. In each case we include controls for month of birth, number of classes, and start year. We also report the same specifications but also including a number of additional controls – child gender, whether the child was born after August 1st in the year, and whether the child is the first born child.

There is some multicollinearity between class size (i.e. the number of students in the 8th grade divided by the number of classrooms used by that grade) and teacher hours per student (which is essentially driven by the number of classes in the grade year). This is especially the case in the range immediately around a class size discontinuity since the teacher hours recommendation would then generate a discrete change in teacher hours. Indeed, the teacher hour recommendation is designed to partially compensate for the abrupt changes driven by the class size rule. This prevents BH from estimating the effect of class size, controlling for the number of teachers, so their estimates of class size should be interpreted as the effect of class size net of the effect of the compensation provided by the teacher hour recommendation. Indeed, if compensation were exact they would find no net class size effect.

Table 4 shows the same specifications, except that now the estimates relate to sibling differences.

[4] See Appendix for results that include families with just a singleton child as well as the siblings data used in Table 3. The coefficients here on class size and students/hour of 0.51 and 0.67 become 0.81 and 1.04.

Table 3 Post-compulsory education length: Family Averages

Log Class size	0.6958	-	-0.1644	-0.1500	0.5082	-	0.2052	0.04541
	0.0489		*0.1146*	*0.1305*	*0.0539*		*0.2818*	*0.3067*
Log students/hours	-	0.7485	0.8873	0.8009	-	0.6712	0.4061	0.8598
		0.0456	*0.1069*	*0.3899*		*0.0708*	*0.3708*	*0.4316*
Log Size * Log Students/hours	-	-	-	0.0315	-	-	-	-0.3788
				0.1371				*0.1843*
Male	-	-	-	-	-0.0337	-0.0336	-0.0337	-0.0366
					0.0149	*0.0149*	*0.0149*	*0.0149*
First child	-	-	-	-	1.9115	1.9117	1.9116	1.9117
					0.945	*0.0945*	*0.0945*	*0.0945*
Age 1 August	-	-	-	-	-0.1437	-0.1437	-0.1437	-0.1437
					0.0020	*0.0020*	*0.0020*	*0,.0020*
Intercept	5.0631	7.4717	8.0245	7.9844	28.3563	30.3210	30.3210	28.3677
	0.1466	*0.0204*	*0.3857*	*0.4321*	*0.3815*	*0.3547*	*0.3547*	*1.2726*
R-squared	0.0014	0.0019	0.0019	0.0019	0.0541	0.0541	0.0541	0.0541
# observations	299283	299283	299283	299283	299283	299283	299283	299283
# families	141186	141186	141186	141186	141186	141186	141186	141186

Note: Standard errors in italics.

Table 4 Post-compulsory education length: Sibling differences

Log Class size	-0.0454	-	0.1384	0.1193	-0.0808	-	0.1218	0.1196
	0.0292		*0.0634*	*0.0725*	*0.0314*		*0.0954*	*0.0972*
Log students/hours	-	-0.1241	-0.3047	-0.1651	-	-0.1392	-0.2998	-0.2716
		0.0430	*0.0932*	*0.2733*		*0.0439*	*0.1333*	*0.2764*
Log Size * Log Students/hours	-	-	-	-0.0528	-	-	-	-0.0117
				0.0971				*0.1012*
Male	-	-	-	-	-0.1946	-0.1946	-0.1946	-0.1946
					0.0066	*0.0066*	*0.0066*	*0.0066*
First child	-	-	-	-	0.3319	0.3319	0.3319	0.3319
					0.0099	*0.0099*	*0.0099*	*0.0099*
Age 1 August	-	-	-	-	-0.0518	-0.0519	-0.0518	-0.0518
					0.0010	*0.0010*	*0.0010*	*0.0010*
Intercept	7.2629	7.0730	6.5800	6.6302	15.9141	15.5871	15.1164	15.1187
	0.0876	*0.0187*	*0.2266*	*0.2448*	*0.2099*	*0.1839*	*0.4121*	*0.4126*
R-squared	0.6612	0.6612	0.6612	0.6612	0.6661	0.6661	0.6661	0.6661
# observations	299283	299283	299283	299283	299283	299283	299283	299283
# families	141186	141186	141186	141186	141186	141186	141186	141186

Note: Standard errors in italics.

Our preferred specifications control for observable heterogeneity and, like BH we find substantially larger effects of the resources variables when we do this. However, like them, we find that it is difficult to find robust estimates when we include both class size and students/hour together. The case for including their interaction seems weak since it was never significant. Thus, hereafter, like BH we concentrate on specifications that include only class size and only students/hour and not both together[5].

An advantage of our sibling differences method relative to BH's instrumental variable method is that we do not have to rely on the identifying assumption that class size is not influenced by parents. It seems possible that this is a real problem for their estimates because they exhibit large changes when control variables are added suggesting that the administrative rules are not doing a good job of randomising resources. The suspicion is that there will remain, despite the large number of controls that they include, important unobservable effects that may still cause their IV estimates to be biased. This is a feature of their estimates that apply the rules to their complete 10% sample, and it also applies to their sub-sample of pupils who are located close to the discontinuities. It is not clear what direction the remaining unobserved heterogeneity would bias the results. The effect of class size on low ability children is likely to be higher than for high ability children: high ability children are likely to be more robust to large class size. In which case, the bias will be negative. On the other hand, more able parents may have stronger preferences for low size and have more able children, in which case the bias would be positive.

However, a disadvantage of our method is that class size and teacher hours, as generated by the administrative rules, are a "fuzzy" measure of actual resources faced by a particular child. In particular, the actual practice of certain municipalities may differ from the federal rule. Densely populated municipalities will face lower variance in cohort sizes and so be able to adopt a practice that is closer to the national rule than a sparsely populated authority. To explore the sensitivity of the results we re-estimated our models using a variety of assumed maxima and we find, in Tables 5a and 5b, that a critical maximum of 24 actually does produce the most precise estimates.

[5] It should be noted that interactions of resources with gender, first child, and age at school entry were insignificant, indicating that there are no differential resource effects along these dimensions. Thus, we restrict ourselves to this simple specification in subsequent analysis.

Table 5a *Post-compulsory education length: Sibling differences by Assumed Class Size Maxima*

	21	**22**	**23**	**24**	**25**	**26**	**27**	**28**
Log Class size	-0.0640	-0.0726	-0.1162	-0.0863	-0.1102	-0.0751	-0.0189	-0.0050
	0.0345	*0.0038*	*0.0330*	*0.0325*	*0.0323*	*0.0318*	*0.0309*	*0.0303*
Male	-0.1948	-0.1947	-0.1947	-0.1947	-0.1945	-0.1946	-0.1947	-0.1947
	0.0066	*0.0067*	*0.0067*	*0.0067*	*0.0068*	*0.0068*	*0.0068*	*0.0068*
First child	0.3321	0.3322	0.3322	0.3319	0.3320	0.3321	0.3321	0.3321
	0.0099	*0.0099*	*0.0099*	*0.0099*	*0.0101*	*0.0101*	*0.0101*	*0.0102*
Age 1 August	-0.0519	-0.0519	-0.0519	-0.0519	-0.0519	-0.0519	-0.0519	-0.0519
	0.0011	*0.0011*	*0.0011*	*0.0011*	*0.0011*	*0.0011*	*0.0011*	*0.0011*
Intercept	15.852	15.814	15.998	15.932	16.022	15.907	15.726	15.676
	0.2133	*0.2132*	*0.2119*	*0.2116*	*0.2136*	*0.2132*	*0.2125*	*0.2129*
R-squared	0.5230	0.5303	0.5304	0.5302	0.5266	0.5263	0.5256	0.5236
# obs	299283	299283	299283	299283	299283	299283	299283	299283
# families	141186	141186	141186	141186	141186	141186	141186	141186

Note: Standard errors in italics.

Table 5b *Post-compulsory education length: Sibling differences by Assumed Students/hour Maxima*

	21	**22**	**23**	**24**	**25**	**26**	**27**	**28**
Log (students/ hour)	-0.1279	-0.1359	-0.1786	-0.1392	-0.1526	-0.1279	-0.0574	-0.0458
	0.0480	*0.0461*	*0.0442*	*0.0440*	*0.0432*	*0.0440*	*0.0439*	*0.0442*
Male	-0.1948	-0.1947	-0.1947	-0.1947	-0.1945	-0.1946	-0.1947	-0.1947
	0.0066	*0.0067*	*0.0067*	*0.0067*	*0.0068*	*0.0068*	*0.0068*	*0.0068*
First child	0.3321	0.3322	0.3322	0.3319	0.3320	0.3321	0.3321	0.3321
	0.0099	*0.0099*	*0.0099*	*0.0099*	*0.0101*	*0.0101*	*0.0101*	*0.0102*
Age 1 August	-0.0519	-0.0518	-0.0519	-0.0519	-0.0519	-0.0519	-0.0519	-0.0519
	0.0011	*0.0011*	*0.0011*	*0.0011*	*0.0011*	*0.0011*	*0.0011*	*0.0011*
Intercept	15.583	15.515	15.538	15.587	15.594	15.604	15.640	15.641
	0.1846	*0.1851*	*0.1845*	*0.1840*	*0.1860*	*0.1859*	*0.1861*	*0.1872*
R-squared	0.5303	0.5304	0.5302	0.5266	0.5263	0.5263	0.5256	0.5236
# obs.	299283	299283	299283	299283	299283	299283	299283	299283
# families	141186	141186	141186	141186	141186	141186	141186	141186

Note: Standard errors in italics

Measurement error is, nonetheless, a problem for any sibling difference analysis. The primary source of measurement error in the levels of resources is due to pooling across individual municipalities that choose rules that differ from each other and from the national requirements. Thus, in an attempt to explore how far this lower bound could be pushed, by re-estimating our models for separate large municipalities. This leaves only time variation in the rules as our only remaining source of measurement error in the differences would be due to time variation in the local rules. We minimise this by including time effects. We also investigate the stability of the estimates to the length of the sibling difference - the age gap between siblings. Siblings that are close in age are likely to have experienced less instability in the rules.

Since actual resources are measured with error, sibling differences in resources may be measured with considerable error and this may lead to attenuation bias. Thus, in Tables 6a and 6b, we also estimate our sibling difference model for ten of the largest municipalities and different assumptions about the rule. We can see that our estimated class size and teacher hour inputs have a larger effect in the single municipality datasets, where there is little or no measurement error, than in the complete datasets where we have, undoubtedly incorrectly, assumed that the same maximum class size applies to all municipalities. It also seems to be the case that a 25 class maximum rule may be more appropriate. Our estimates class size effects are now much larger than the -0.09 figure from Tables 4 and 5a. Similarly, the students/hour effects are also much larger compared to -0.14 in Tables 4 and 5b.

One might argue that, even within an municipality, the practice may have changed over time and leave our sibling differences remain contaminated by some measurement error associated with changes in rules within municipalities. Thus, in Tables 7a and 7b, we estimate the models again by cutting the data into siblings whose age difference is 1, 2, 3….9 years. The group with the larger age difference faces a larger probability of the maximum class size practice having changed between siblings and so be more subject to measurement error in the class size change. Thus, we would expect the input effects to be subject to less attenuation bias, as so be larger, in the short difference case. This turns out to be the case: the Copenhagen estimates here should be compared to -0.11 for class size and -0.17 for students/hour; while the Aarhus figures should be compared to -0.26 and -0.47.

Table 6a Education length sibling differences estimates by largest 10 municipalities and different assumed class size maxima: Log class size

	24		25		26		Families	Observations
Copenhagen	-0.1056	*0.1679*	-0.2081	*0.1686*	0.0044	*0.1671*	13056	6336
Aarhus	-0.2635	*0.1634*	-0.6813	*0.1669*	-0.2128	*0.1616*	8973	4733
Odense	0.0315	*0.2031*	-0.4620	*0.2007*	-0.2043	*0.1965*	8530	4175
Aalborg	-0.2020	*0.2039*	-0.3493	*0.1924*	-0.5495	*0.1859*	8165	3984
Esbjerg	-0.9794	*0.2525*	-1.3146	*0.2545*	-0.9403	*0.2315*	4742	2303
Herning	0.4475	*0.3335*	0.2011	*0.3156*	0.2608	*0.2723*	4160	1977
Kolding	-0.3172	*0.2936*	-0.5703	*0.2927*	-0.4000	*0.2947*	3496	1723
Horsens	-0.0422	*0.3697*	0.1432	*0.3886*	-0.0341	*0.3883*	3267	1602
Silkeborg	-1.0884	*0.3590*	-1.0891	*0.3189*	-0.8916	*0.3114*	3136	1522
Randers	-0.1040	*0.3180*	-0.1773	*0.3168*	-0.1976	*0.3121*	3117	1524

Note: Standard errors in italics.

Table 6b Education length sibling differences estimates by largest 10 municipalities and different assumed class size maxima; Log students/hour

	24		25		26		Families	Observations
Copenhagen	-0.1733	*0.2281*	-0.2546	*0.2278*	-0.1233	*0.2308*	13056	6336
Aarhus	-0.4697	*0.2148*	-0.8724	*0.2179*	-0.4109	*0.2196*	8973	4733
Odense	-0.2093	*0.2832*	-0.6955	*0.2735*	-0.4649	*0.2749*	8530	4175
Aalborg	-0.2975	*0.2823*	-0.4726	*0.2634*	-0.8126	*0.2687*	8165	3984
Esbjerg	-1.3910	*0.3419*	-1.7004	*0.3420*	-1.2751	*0.3251*	4742	2303
Herning	0.6124	*0.4395*	0.3371	*0.4145*	0.3229	*0.3740*	4160	1977
Kolding	-0.6008	*0.3945*	-0.8749	*0.3933*	-0.6963	*0.4022*	3496	1723
Horsens	-0.1870	*0.5184*	0.0795	*0.5225*	-0.0141	*0.5519*	3267	1602
Silkeborg	-1.4274	*0.4829*	-1.5056	*0.4345*	-1.2418	*0.4262*	3136	1522
Randers	-0.3316	*0.4458*	-0.3414	*0.4271*	-0.5235	*0.4458*	3117	1524

Note: Standard errors in italics

Table 7a Post-compulsory education length: Sibling differences: Copenhagen

Max age difference	Log class size		Log (students/hour)		# observations	# families
1	-1.5960	*0.4891*	-2.1839	*0.6818*	1567	782
2	-0.5173	*0.2614*	-0.6392	*0.3549*	5212	2560
3	-0.3583	*0.2037*	-0.4280	*0.2759*	8904	4348
4	-0.2563	*0.1817*	-0.3213	*0.2475*	11126	5402
5	-0.0756	*0.1747*	-0.1037	*0.2373*	12163	5896
6	-0.1271	*0.1705*	-0.1770	*0.2315*	12679	6145
7	-0.1075	*0.1687*	-0.1671	*0.2292*	12950	6281
8	-0.1106	*0.1680*	-0.1762	*0.2283*	13024	6325
9	-0.1056	*0.1679*	-0.1733	*0.2281*	13056	6336

Note: Standard errors in italics.

Table 7b Post-compulsory education length: Sibling differences: Aarhus

Max age difference	Log class size		Log (students/hour)		# observations	# families
1	-0.5383	*0.3957*	-0.8052	*0.5188*	1707	879
2	-0.1773	*0.2349*	-0.3850	*0.3080*	4278	2214
3	-0.2450	*0.1916*	-0.4748	*0.2529*	6530	3402
4	-0.3216	*0.1757*	-0.6173	*0.2323*	7878	4125
5	-0.1952	*0.1680*	-0.3826	*0.2209*	8490	4459
6	-0.1861	*0.1652*	-0.3753	*0.2170*	8776	4618
7	-0.2580	*0.1641*	-0.4643	*0.2157*	8908	4693
8	-0.2446	*0.1635*	-0.4448	*0.2150*	8950	4720
9	-0.2635	*0.1634*	-0.4697	*0.2148*	8973	4733

Note: Standard errors in italics.

A further issue for us (and BH) is that of censoring in education length. We are taking data that is at least 11 years post grade 8 and no older than 20 years post grade 8, i.e. between the ages of 25 and 35. Many (15%) observations remain in education beyond even the age of 25 and so there is some censoring in the data. Table 8 presents the headline coefficients, using just the specification that contains both class size and teacher hours, for education length measured up to different numbers of years after the beginning of 8th grade. That is, this table acknowledges that there is censoring in our education length data – we only observe completed education for those whose education is less than the 2001 minus the year that they were in 8th grade. This could be as large as 20 years for those in 8th grade in 1981 and as little as 10 for those who took 8th grade in 1990. Since many students do not complete their education until even older than 25 there is certainly some censoring in this data and the table shows the effects of resources on completed education using subsets of the data with different degrees of censoring. The distribution of the dependent variable for Table 8 is shown in Figure 8.

The first row corresponds to observations where individuals are followed until just one year out of 8th grade and subsequent years are ignored. Row 11 follows individuals up until 11 years out of 8th grade. This is the last year for which we can, with certainty observe all members of the family for the same number of years. Row 12 may contain families with a mixture of some individuals for 12 years (8th graders 1981-1989) and perhaps one for 11 years who was an 8th grader in 1990. Therefore, row 11 is the last row without differential censoring within family. The last row follows individuals up until at most 20 years out of 8th grade. Here only those in 8th grade in 1981 are observed 20 years later in 2001, those in 8th grade in 1981 are observed 19 years later in 2001, etc.

A further concern in research based on sibling differences is family size. Table 9 shows estimations performed separately by number of siblings. Surprisingly, it can be seen that our estimated resource effects are essentially being driven by 3 and 4 sibling households.

Finally, Table 10 investigates the importance of our chosen dependent variable. The presumption in our earlier results (and in BH) is that the outcome of interest is the number of years of post-compulsory schooling reflected in age at which individuals leave the education system. In fact, many young Danes take a break in their education, usually, between upper secondary and higher education. Moreover, there is a significant variance in the duration of secondary education even controlling for 3 or 5 year degree. In Table 10 we redefine the dependent variable to be a dummy variable which takes the value 1 if the individual had at least the indicated number of years of post-compulsory education. The mean shows that almost all Danes have some post-8th grade education while 25% end at or before 12th grade. Class size does seem to have a significant effect of getting to at least to 12th grade and there also seems to be an effect much later corresponding to the distinction between 3 year and 5 year degree. Students/hour does seem to have a beneficial effect throughout.

Figure 8. Distribution of education length for different observations windows after 8th grade

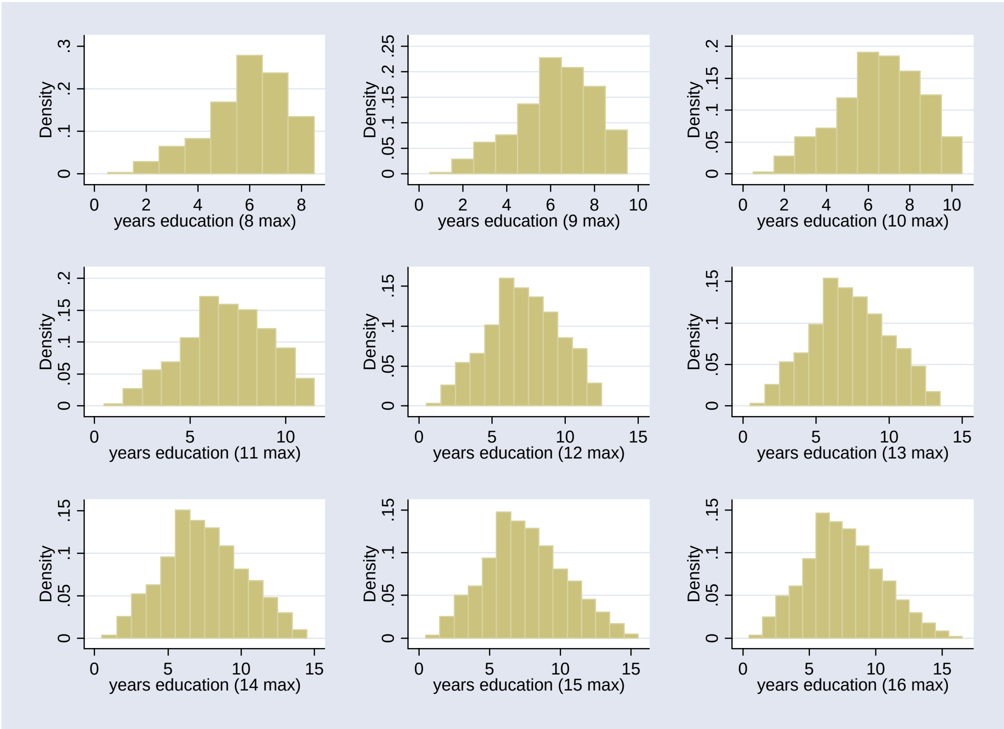


Table 8 *Coefficients on class size and students/hour by number of years after which the education length is censored*

	No additional controls				With additional controls			
# years later	class size		students/hour		class size		students/hour	
1	0.0011	*0.0015*	0.0021	*0.0021*	0.0018	*0.0014*	0.0036	*0.0021*
2	0.0074	*0.0043*	0.0128	*0.0061*	0.0092	*0.0040*	0.0126	*0.0059*
3	0.0003	*0.0078*	0.0065	*0.0109*	0.0024	*0.0072*	0.0060	*0.0106*
4	-0.0197	*0.0113*	-0.0222	*0.0158*	-0.0138	*0.0105*	-0.0292	*0.0154*
5	-0.0271	*0.0150*	-0.0270	*0.0209*	-0.0358	*0.0139*	-0.0802	*0.0205*
6	-0.0376	*0.0181*	-0.0497	*0.0253*	-0.0452	*0.0169*	-0.1033	*0.0248*
7	-0.0515	*0.0212*	-0.0779	*0.0296*	-0.0517	*0.0197*	-0.1321	*0.0289*
8	-0.0617	*0.0240*	-0.0998	*0.0335*	-0.0538	*0.0222*	-0.1470	*0.0327*
9	-0.0661	*0.0266*	-0.1131	*0.0371*	-0.0531	*0.0247*	-0.1548	*0.0362*
10	-0.0673	*0.0292*	-0.1180	*0.0407*	-0.0575	*0.0271*	-0.1689	*0.0398*
11	-0.0809	*0.0315*	-0.1392	*0.0440*	-0.0455	*0.0293*	-0.1241	*0.0430*
12	-0.1019	*0.0335*	-0.1729	*0.0467*	-0.0413	*0.0311*	-0.0998	*0.0458*
13	-0.1250	*0.0350*	-0.2078	*0.0489*	-0.0353	*0.0326*	-0.0664	*0.0480*
14	-0.1424	*0.0361*	-0.2325	*0.0504*	-0.0254	*0.0337*	-0.0289	*0.0496*
15	-0.1540	*0.0368*	-0.2500	*0.0514*	-0.0250	*0.0344*	-0.0196	*0.0506*
16	-0.1544	*0.0372*	-0.2511	*0.0520*	-0.0152	*0.0349*	0.0024	*0.0513*
17	-0.1562	*0.0374*	-0.2534	*0.0523*	-0.0094	*0.0351*	0.0211	*0.0516*
18	-0.1579	*0.0376*	-0.2550	*0.0524*	-0.0040	*0.0352*	0.0428	*0.0517*
19	-0.1595	*0.0376*	-0.2568	*0.0525*	0.0017	*0.0352*	0.0624	*0.0518*
20	-0.1595	*0.0376*	-0.2568	*0.0525*	0.0017	*0.0352*	0.0624	*0.0518*

Note: Standard errors in italics.

Table 9 Education Length Model Estimates and standard errors: by number of siblings

2 siblings				
Log class size	-0.0006	*0.0360*		
Log students/hour			-0.0192	*0.0488*
Male	-0.1630	*0.0073*	-0.1630	*0.0073*
First child	0.4289	*0.0123*	0.4289	*0.0123*
Age August 1	-0.0538	*0.0012*	-0.0538	*0.0012*
Intercept	15.8818	*0.2346*	15.8711	*0.2042*
R-squared	0.6800		0.6800	
# obs/families	251050 / 125525		251050 / 125525	
3 siblings				
Log class size	-0.3534	*0.0802*		
Log students/hour			-0.5172	*0.1078*
Male	-0.3142	*0.0169*	-0.3142	*0.0169*
First child	0.1721	*0.0267*	0.1721	*0.0267*
Age August 1	-0.0510	*0.0026*	-0.0510	*0.0026*
Intercept	16.5896	*0.5246*	15.2047	*0.4554*
R-squared	0.5960		0.5961	
# obs/families	43578 / 14526		43578 / 14526	
4 siblings				
Log class size	-0.7945	*0.2508*		
Log students/hour			-0.9837	*0.3315*
Male	-0.4175	*0.0544*	-0.4174	*0.0544*
First child	0.0641	*0.0980*	0.0624	*0.0980*
Age August 1	-0.0512	*0.0080*	-0.0514	*0.0080*
Intercept	17.6174	*1.6299*	14.5965	*1.4059*
R-squared	0.5802		0.5801	
# obs/families	4136 / 1034		4136 / 1034	
5 siblings				
Log class size	-0.7120	*0.7594*		
Log students/hour			-0.7349	*0.9478*
Male	-0.2217	*0.1727*	-0.2247	*0.1727*
First child	0.4031	*0.3655*	0.4104	*0.3658*
Age August 1	-0.0716	*0.0243*	-0.0722	*0.0243*
Intercept	21.1268	*4.8543*	18.5644	*4.2663*
R-squared	0.5685		0.5683	
# obs/families	435 / 87		435 / 87	
6 siblings				
Log class size	-2.2225	*1.9279*		
Log students/hour			-2.6249	*2.2470*
Male	0.5417	*0.4190*	0.5443	*0.4181*
First child	2.1489	*1.1142*	2.1459	*1.1140*
Age August 1	0.0314	*0.0063*	0.0297	*0.0661*
Intercept	2.8710	*12.2264*	-5.2294	*11.3748*
R-squared	0.6948		0.6949	
# obs/families	84 / 14		84 / 14	

Note: Standard errors in italics.

Table 10 Sibling difference linear probability model on "at least" years of post compulsory schooling

Dep var:	Mean of dep var	Log class size			Log students/hour		
		Coeff	Std error	R squared	Coeff	Std error	R squared
1 more years	0.9701	-0.0019	0.0025	0.5725	-0.0012	0.0034	0.5725
2 more years	0.9157	-0.0136	0.0040	0.5960	-0.0168	0.0055	0.5960
3 more years	0.8497	-0.0149	0.0052	0.6026	-0.0206	0.0070	0.6026
4 more years	0.7482	-0.0098	0.0063	0.5936	-0.0155	0.0086	0.5936
5 more years	0.5881	-0.0040	0.0071	0.6015	-0.0096	0.0096	0.6015
6 more years	0.4401	-0.0116	0.0071	0.6076	-0.0215	0.0096	0.6076
7 more years	0.3038	-0.0041	0.0067	0.5985	-0.0092	0.0090	0.5985
8 more years	0.1859	-0.0058	0.0058	0.5832	-0.0133	0.0078	0.5832
9 more years	0.1001	-0.0123	0.0046	0.5602	-0.0199	0.0062	0.5602
10 more years	0.0283	-0.0070	0.0026	0.5195	-0.0091	0.0036	0.5195

7. Conclusions and Further Research

Our sibling differences analysis suggests that class size and students per teacher hour rules do have statistically and economically significant effects on education length in Denmark. Smaller classes and more teacher hours have been shown to increase length of education. Our simplest results, for the population as a whole, suggests that reducing 8th grade class size by 5% (moving from a current mean class size of 20 to 19) would increase mean schooling by about 0.005 years. These results are considerably smaller than BH. However, more complex analysis shows that the results are largest, and most significant, for households with three or four children. For three sibling households, mean schooling is predicted to increase by 0.017 years and for four siblings 0.038 years. In contrast, our analyses for smaller households suggest insignificant effects.

However, these headline results are probably biased downwards by measurement error. The main source of error is from pooling across municipalities which may apply different rules for determining resource allocation. We do indeed find much larger effects of resources when we conduct the analysis at a municipality level - which we are able to do because our data covers the whole population. Moreover, for the very largest municipalities we can even conduct the analysis at each level of sibling age difference and here we find much stronger effects when we use just closely spaced siblings.

Our conclusion, therefore, is that education resources do indeed have significant effects on education length, and such effects may even be quite large.

There are several avenues for development of this work. Firstly, individual 9th grade test scores and teacher assessments for the years 2002-4 has recently been made available. This will enable us to place our measures in the wider literature on immediate test score outcomes. Moreover, this data contains information on actual class size so we would be able to exploit the rules as instrumental variables to contrast IV results with sibling differences. However, this data is too recent to enable us to look also at completed education length.

Secondly, while our sibling differences controls for unobservable family effects, and limiting ourselves to siblings that attended the same school allows us to control for school *fixed* effects, we have not exploited the information that we have about *peer* parental background which is not removed by differencing even holding the school constant. In particular, we would like to know the effect of being young or old, or more or less able, relative to the average class member, since teachers may focus their attention on the average or, alternatively, teachers might focus on the youngest, or most able. Moreover, we would like to identify the effects of the parental backgrounds of other children in the class - for example, the proportion with working mothers, or the distribution of parental education levels.

Thirdly, "normalised" education length would adjust for late completers, and would be an outcome measure that is more obviously positive. Fourthly, future earnings is also a more direct outcome measure that is of relevance to policy.

The outcome measure we have concentrated on is length of education and it would be relatively straightforward to cost the effect of more resources, subject to a number of additional assumptions. We would need to account for the fact that greater teaching input costs more money and that, since the Danish government pays for years of teaching, late completers actually cost more.

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Appendix

Table 3A *Post-compulsory education length: Family averages only singletons*

Log Class size	1.4698	-	-1.4388	-1.3647	0.8142	-	-0.1954	-0.2052
	0.0177		*0.0460*	*0.0494*	*0.0194*		*0.0884*	*0.0971*
Log students/hour	-	1.7044	2.9476	2.5551	-	1.0368	1.2709	1.2605
		0.0166	*0.0431*	*0.1047*		*0.0238*	*0.1085*	*0.1167*
Log Size*Student/hr	-	-	-	0.1588	-	-	-	0.0125
				0.0386				*0.0514*
Male	-	-	-	-	-0.0337	-0.0811	-0.0807	-0.0807
					0.0149	*0.0062*	*0.0062*	*0.0062*
Age 1 August	-	-	-	-	-0.1437	-0.1206	-0.1204	-0.1204
					0.0020	*0.0008*	*0.0008*	*0.0008*
Intercept	2.4024	7.5635	12.417	12.229	28.356	31.456	34.416	34.455
	0.0528	*0.0084*	*0.1566*	*0.1621*	*0.3815*	*1.1045*	*1.1994*	*1.1010*
R-squared	0.0109	0.0167	0.0182	0.0183	0.0681	0.0691	0.0692	0.0692
# observations	392010	392010	392010	392010	392010	392010	392010	392010

Note: Standard errors in italics.