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Abstract:
How does class size in compulsory school affect peoples' long run education and earnings? We use maximum class size rules and Norwegian administrative registries allowing us to observe outcomes up to age 48. We do not find any indication of beneficial effects of class size reduction in compulsory school. For a 1 person reduction in class size we can rule out effects on income as small as 0.087 percent in primary school and 0.12 percent in middle school. Population differences in parental background, school size or competitive pressure do not appear to reconcile our findings with previous studies.

Keywords: Class size, Schooling, Earnings, Regression Discontinuity

JEL classification: I21, I28, J24, C30

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Sammendrag

Påvirker klassestørrelse i grunnskolen elevenes utdanningslengde og inntekt som voksen? Ved å utnytte lovpålagte regler for maksimal klassestørrelse i barne- og ungdomsskolen isolerer vi variasjon i disse langtidsutfallene som utelukkende er forårsaket av klassestørrelse. Norske registerdata tillater oss å observere utfall for skolekohortene som fullførte ungdomsskolen fra 1978 til 2003 opp til en alder av 48 år. I denne studien finner vi ingen tegn til positive effekter av en reduksjon i klassestørrelse. Vi er i stand til å utelukke fordelaktige effekter av en 1-persons reduksjon i klassestørrelsen på inntekt som voksen på 0,087 prosent i barneskolen og 0,12 prosent i ungdomsskolen med 95-prosent sikkerhet. Disse funnene ser ikke ut til å påvirkes av foreldrebakgrunn, skolestørrelse eller konkurranse mellom skoler.

1 Introduction

The relationship between class size and human capital is one of the most researched and debated questions in education but evidence on this matter has been difficult to come by. The main challenge when estimating class size effects is to correct for potential omitted variable bias that may arise because of decisions by parents, schools and education authorities which may lead to sorting, and other confounding factors. The most credible studies either rely on social experiments such as the Tennessee STAR study (f.e. Krueger, 2003), or exploit quasi experimental setups that generate arguably exogenous variation in class size (f.e. Angrist and Lavy, 1999; Hoxby, 2000; Leuven et al., 2008).

Because of data limitations the large majority of existing class size studies has focused on short term outcomes such as test scores. Ultimately one would however like to know whether class size also affects outcomes on the longer run. One reason for focusing on long term outcomes is that fade-out is known to be a concern in education contexts, and in particular at earlier ages (Chetty et al., 2011). But short run outcomes such as test scores may also be a partial measure of the human capital that class size affects, and some have argued that in particular non-cognitive skills are important for outcomes later in life (f.e. Heckman and Cunha, 2007). A focus on test scores can therefore lead to substantial underestimates of actual gains. Finally, there is also substantial variation in the short term effects that are found in the literature (Wößmann and West, 2006), but little is known whether this is mirrored by similar effects on the longer term.

An early study that found persistent longer term effects of small classes is Krueger and Whitmore (2001). Using the STAR data they showed that those attending a small class in grades K-3 were more likely to take college-entrance exams. Similarly, Chetty et al. (2011) find that pupils that attended small classes in the STAR data are more likely to attend college, but do not find effects on earnings at age 27. Recently Fredriksson et al. (2013, 2014), using Swedish data, were the first to report statistically significant effects on earnings and find that a 1 pupil decrease leads to 1.5 percentage point increase in earnings. A recent study using Norwegian data by Falch et al. (2017) do not find any significant beneficial long-term effects from a middle school class size reduction, consistent with the absence of effects in Leuven et al. (2008).

The current paper presents long term impact estimates of average class size in compulsory school (grades 1 to 9) for Norway, as well as separate class size effects for primary school (grades 1 to 6) and middle school (grades 7 to 9). The data cover all cohorts graduating from middle school and go back to the late 1970s. Using Norway's registry data we can trace these cohorts' education and earnings up to 2014 when the oldest individuals are 48 years old. The empirical approach is standard, and exploits maximum class size rules that were effective up to the school year 2002/03. The maximum class size in primary school was 28 (and 30 in middle school). Our data are informative about class size in middle school, as well as class size in primary school for those that did their primary education in so-called combined schools that have both a primary and middle school department. About one out of four primary schools and close to two out of three middle schools in Norway are such combined schools.

We do not find beneficial effects of class size in primary school on earnings, and completed schooling. Our results for class size in middle school are in line with the results for primary school. Although the effects are statistically insignificant, we have enough precision to rule out beneficial effects on earnings as small as 0.12 percent for a 1 pupil reduction in class size with 95 percent certainty. Long run estimated impacts of class size in middle school on completed schooling are also small, statistically insignificant and also allow us to rule out even very small beneficial effects. These results for middle school are consistent with previous results of Leuven et al. (2008) who estimated short term effects of class size in middle school on test scores of middle exit exams for two cohorts in the early 2000s. They did not find significant effects on test scores, and could rule out effects as small as 1.5 percent of a standard deviation for a one student reduction in average class size during three years.

Our results contrast with the recent evidence from Sweden (Fredriksson et al., 2013,

2014) who find large negative and statistically significant effects from average class size in primary schools (grade 4-6) on earnings and, in some specifications, also for years of schooling. We investigate two possible explanations for these diverging results.

The first is differences in (compensating) parent behavior. Fredriksson et al. (2014) show that higher education parents compensate for larger classes while less educated parents do not. They also find that lowering class size improves achievement at age 13 for children from parents with low (below median) income, while children from high (above median) income families do not appear to benefit from smaller classes. That this also could be the case for Norway, is suggested by the results from the only Norwegian study on short run effects of class size in primary school by Bonesrønning and Iversen (2013) who have data on one cohort of fourth graders. They find effects for the subgroup of students with parents who are educated at or below the upper secondary school level, and for the subgroup of students from dissolved families. However, we find no evidence that smaller classes provide benefits in the long run for children from more disadvantaged backgrounds, whether it is by parental income, education or migrant background suggesting that parent behavior does not drive the different results.

The second explanation is differences in the school population. Compared to Sweden schools in Norway are relatively small. If school size mediates class size effects then this is another potential source of effect heterogeneity. A second difference in the population stems from the Swedish sample of school districts with only one school. Such schools are perhaps more shielded from competition, and competitive pressure could attenuate the effect of class size. When we approximate the Swedish sample by focusing on single-school municipalities none of the estimates provide evidence of class size effects.

The analysis proceeds by first briefly discussing the institutional context in Norway before introducing the data sources. In Section 3 the empirical approach is discussed before presenting the results in Section 4. The paper finishes with a discussion of the findings and conclusion.

2 Institutions and data

Compulsory education in Norway covers 9 years of schooling.[1] Children start their primary education in the year they turn seven and, after six years in primary school, transfer to middle schools that cover grade 7 to 9. After compulsory schooling students have the possibility to enroll in general or vocational upper secondary schools, where the general track leads to university. Compulsory schooling is free of charge, and nearly all schools are public (less than 3% is private). Although schools in Norway have to conform with the same laws and curriculum, the responsibility for providing education lies with municipalities who are responsible for operating schools. Schools have catchment areas, which are relatively strictly enforced.

The data in this study come from administrative registries collected by Statistics Norway that cover the complete population. From these registries we know from which middle school people graduate. We take these graduating cohorts from 1978 onwards and link them to various data sources. The tax registry provides information on earnings from work until 2013. Information on the highest observed education attainment comes from the national education registry. Finally, we also use the population registry to identify people's parents in order to obtain family background characteristics such as age and education, at age 16. We follow people up to age 50 which allows us to report average class size effects on schooling and earnings between age 27 and 42 following Fredriksson et al. (2013), but we will also estimate age specific class size effects from age 18 to age 48.

Class size

Information on class size also comes from administrative data. Schools have to report the number of classes and the number of pupils separately for each grade. Using this information, average class size at the school-grade level is calculated by dividing enroll-

[1]As of 1997, children start school the year they turn six and compulsory schooling lasts ten years. The cohorts used in the analysis were not affected by this reform.

ment by the number of classes. Following Leuven et al. (2008), the class size variable used in the analysis below is average class-size in primary school and average class size in middle school. The reason for using average class size rather than class size separate for each grade is that year-to-year class-sizes are highly correlated, and separate identification is not feasible. Using class size from a single grade is also problematic since it may capture effect of class-size in previous grades. These complications are avoided by taking the average of class size which delivers an interpretable and policy relevant effect.

Class size information is available back to the school year 1977/78. This left truncation implies that we cannot construct a complete class size history for all cohorts. For the graduating cohort in 1978 for example only class size in grade 9 (the final grade of middle school) is available, while information on class size in earlier grades is not. A complete class size history for middle school is observed for the 1980 cohort and onwards, while for the 1986 cohort and onwards we observe both complete primary and middle school class size histories. Our analysis retains however all cohorts to preserve as much data as possible, and average class size refers to average *observed* class size.[2]

Because we observe individuals' graduating middle school we have a class size measure for middle school for everyone, but this is not the case for primary school class size as the registries do not record which primary school people attended. Many middle schools in Norway have however an integrated primary school, and children that receive their primary schooling in such a combined school typically follow their complete compulsory schooling up to grade 9 there. This means that for this population we also have class size in the primary school grades 1–6.

One caveat is that larger combined middle schools often also take in pupils from other primary schools in the municipality.[3] Consequently, primary class size in these

[2]Estimates based on the cohorts with complete histories (not reported here) show that this does not affect our results. This should not come as a surprise as there is a lot of persistence in class size from grade to grade, and any remaining measurement error bias is likely reduced by our instrumental variables approach.

[3]This breaks the perfect correlation between class size in primary school and class size in middle school, and will help us below in separately identifying class size effects in primary and middle school.

middle schools will be measured with error for these incoming pupils. Our instrumental variable strategy should mitigate bias from measurement error problem if it is approximately classical. We will however err on the side of caution and base our baseline estimates on the subpopulation of students who graduated from combined middle schools that do not take-up students from other primary schools. This ensures that we observe the correct primary school class size for these individuals in our data. We operationalize this by stratifying our sample to those who attended combined schools where the difference in enrollment at the end of primary school differs no more than one pupil from enrollment at the start of middle in the following school year:

$$\text{take-up}_t = \text{enroll}_{\text{grade 7},t} - \text{enroll}_{\text{grade 6},t-1} \leq 1.$$

Table 1 reports average class sizes for everybody (All), for those who attended combined primary and middle schools (Combined), and our reference population of students who attended combined schools with take-up$\leq$1 in grade 7 (Baseline). Average class size in middle school is about 25 and slightly less, 23, in the combined schools which reflects their smaller size. Average class size in primary school is about 21 in our baseline sample. Since there is no take-up in our baseline sample, class size is essentially the same in primary and middle school. We will therefore estimate the effect of average class size in compulsory schooling (grades 1-9) for this population, while we will separate these effects using the full combined school population.

Table 1 also reports descriptive statistics on the background characteristics of the people in our data, as well as the outcomes we consider: educational attainment, and labor earnings both logarithmic and normalized relative to the total sample average.

The main thing to note here is the similarity, both in terms of background and outcomes, between those who attended combined schools and the population at large. Parents have somewhat less schooling in the combined and baseline sample, but overall the differences are minor.

Table 1. Descriptive statistics

	All	Attended combined school	
		Combined	Baseline (Take-up$\leq$1)
A. School characteristics:			
Average class size:			
- Compulsory school (grades 1–9)		21.5	21.3
- Primary school (grades 1–6)		20.8	21.0
- Middle school (grades 7–9)	24.8	22.9	21.8
PS Enrollment		34.8	34.8
MS Enrollment	92.7	50.3	35.7
B. Background characteristics			
Girl	0.49	0.49	0.49
Immigrant	0.08	0.09	0.07
Mother: College Degree	0.17	0.17	0.15
Mother: Years of Schooling	11.3	11.3	11.2
Father: College Degree	0.21	0.19	0.17
Father: Years of Schooling	11.8	11.7	11.6
Year of Graduation from MS	1988.3	1989.6	1989.5
Year of Birth	1973.3	1974.6	1974.5
C. Outcomes			
Years of Schooling	14.0	14.0	13.9
	(2.2)	(2.1)	(2.1)
Log-Earnings	12.25	12.30	12.30
	(0.57)	(0.55)	(0.56)
Earnings, normalized	0.86	0.83	0.83
	(0.54)	(0.51)	(0.51)
N Persons	1,313,474	171,174	45,670
N Combined schools	686	686	398
N Middle schools	1,813	667	380

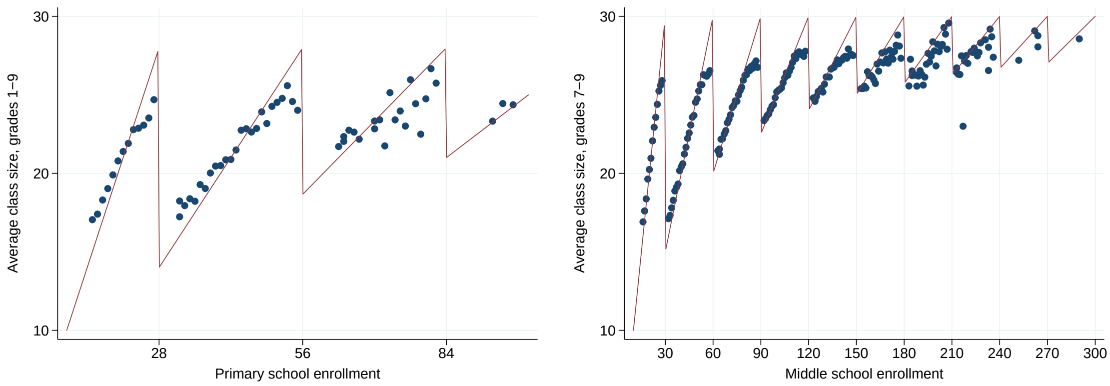


Figure 1. Maximum class size rules, Baseline Population

Maximum Class Size Rules

Until 2003 all schools were subject to maximum class size rules. In middle schools the maximum class size was 30, whereas in primary schools it was 30 up to the year 1985 when it was lowered to 28.[4] Figure 1 illustrates the maximum class size rule. The left panel plots average class size in compulsory schooling against enrollment at the start of school (or first observed enrollment if 1st grade enrollment is missing). The solid line is predicted class size given enrollment. It has the familiar saw-tooth shape with predicted class size increasing until a new classroom is opened after each multiple of the maximum class size when class size drops discontinuously. As can be seen from the figure, actual class size follows the pattern of predicted class size with discontinuous drops at multiples of 28. The right panel plots average class size in middle school against enrollment at the start of middle school.

Figure 2 shows the distribution of class size in primary and middle school. Conform the maximum class size rule, hardly any classes are observed beyond 28 in primary school and 30 in middle school. Modal class size is 22 in primary school and 26 in middle school, but we also observe many smaller classes because quite a few schools in Norway are small or of moderate size.

[4]The new rule was not supposed to affect the children who were already enrolled in primary school, but in practice most schools enforced the new class size rule for all grades in primary school.

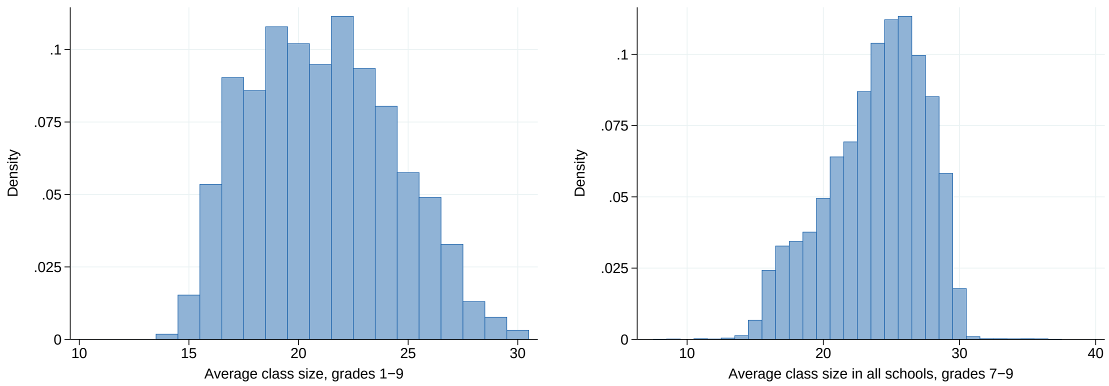


Figure 2. The distribution of class size in compulsory schooling

3 Empirical approach

The main challenge when estimating the causal relationship between class size and subsequent outcomes lies is addressing potential omitted variable bias. Parents, but also teachers, and or schools may make choices that result in a non-random allocation of students to classrooms in such a way that class size correlate with unobserved determinants of performance. In this case estimation approaches that rely on unconfoundedness assumptions such as OLS will be inconsistent.

Because of this concern our analysis exploits quasi-random variation in class size generated by the maximum class size rules discussed above, following the seminal paper by Angrist and Lavy (1999). The idea is that while class size discontinuously drops at the multiples of the maximum class size, nothing else will as long as people's position relative to these cutoffs is as-if random. We exploit this so-called fuzzy regression discontinuity design using an instrumental variable strategy.

3.1 Instrumental variable (IV) estimation of class size effects

The basic setup is as follows. Consider the following outcome equation

$$y = \beta_0 + \beta_1 cs + f(\text{enroll}) + u \tag{1}$$

where outcome y (education or earnings) depends on average class size (cs), and a function $f(\cdot)$ of enrollment. We abstract here from control variables for clarity (but not generality), but discuss our controls below. Class size can potentially correlate with unobservables, $E[u|cs, \text{enroll}] \neq 0$, in which case OLS estimation of (1) gives an inconsistent estimate of the class size effect β_1. We tackle this by instrumenting class size. Let the corresponding first stage be

$$cs = \pi_0 + \pi_1 z + g(\text{enroll}) + e \tag{2}$$

where the instrument z satisfies $e, u \perp z|\text{enroll}$ and $\pi_1 \neq 0$. Here we also control for enrollment through $g(\cdot)$. Instrumenting cs with z in (1) allows us to consistently estimate β_1.

We follow Fredriksson et al. (2013) and assign individuals to segments with a window of ± 15 pupils around each discontinuity. For schools at the first discontinuity we therefore take everybody with school enrollment between 16 and 45, for the second segment everybody with enrollment between 46 and 75, etc. Conditional on segment we then instrument class size with an indicator variable which is 1 above the segment's discontinuity and 0 below. The first stage coefficient is allowed to vary across segments, the period during which the maximum class size rule was 30, the transition period, and the final period with the maximum class size rule was 28.

The baseline specification for $f(\cdot)$ and $g(\cdot)$ consists in segment dummies, and a segment specific linear spline in enrollment with the kink at each discontinuity. These segment dummies and enrollment splines take the possibly confounding effects of the running variable, enrollment at the start of school (enroll), into account. In robustness checks we estimate more flexible specifications and show that this does not matter for our results. We further control for gender, month-of-birth, immigrant background, mother's and father's years of schooling, age at school start, and cohort dummies. All standard errors are clustered at the school level.

3.2 *Measurement error in class size*

It is well known that IV will still consistently estimate β_1 with classical measurement error in cs. While measurement error in class size is not an issue in our baseline sample, cs is observed with error for some students in the broader combined school sample and the population as a whole. We denote these students by $a = 1$ and 0 otherwise:

$$cs^* = cs + a\theta$$

$a \in \{0, 1\}$ is unobserved and θ – the difference in class size between the combined school and the other primary school – is measurement error in class size. In practice we will therefore be estimating

$$y = \beta_0 + \beta_1 cs^* + f(\text{enroll}) + u^* \tag{3}$$

where $u^* = u - \beta_1 a\theta$, and

$$cs^* = \pi_0 + \pi_1 z + g(\text{enroll}) + e^* \tag{4}$$

where $e^* = e + a\theta$. This means that we now need to assume that e^*, $u^* \perp z|\text{enroll}$ which will hold if the observed instrument is orthogonal to the components generating measurement error: $a, \theta \perp z|\text{enroll}$. Since in the current application z is locally random, IV estimation of equation (3) will thus give a consistent estimate of β_1.

While the above implies that our IV approach addresses the measurement error in class size, we observe class size without error for our sample of students that attended grades 1-6 and grades 7-9 in the same school. We therefore start our analysis on this baseline sample. This also generates a check in the sense that the estimated class size effect on this sample should be very similar to the estimated effect on the extended sample of everyone that attended a combined school where class size is for some measured with error.

3.3 *Separating class size in grades 1-6 and grades 7-9*

Although we start out by estimating the effect of average class size in compulsory schooling in our baseline sample, it is also interesting to separately estimate the effect of class size in primary and middle school. In the broader population that attended any combined school there will be schools that take in children from other primary schools. This breaks the near perfect correlation between enrollment in primary and middle school grades. We can therefore use these schools to separately estimate the effect of class size in primary and middle school. To do so we estimate the following equation with 2SLS

$$y = \delta_{\text{PS}} \cdot \text{cs}^{\text{PS}} + \delta_{\text{LS}} \cdot \text{cs}^{\text{MS}} + f(\text{enroll}^{\text{PS}}, \text{enroll}^{\text{MS}}) + \mathbf{x}'\beta + \phi_{\text{cohort}} + \tau_{\text{time}} + \epsilon \quad (5)$$

The effects of interest are δ_{PS} and δ_{LS}, the coefficients on class size in primary school, cs_i^{PS}, and class size in middle school, cs_i^{MS}. All specifications control for segment dummies and segment splines, separately for primary and middle school. Control variables $\mathbf{x}$ include as above gender, month-of-birth, immigrant background, mother's and father's years of schooling, age at school start, cohort dummies, and time dummies (if necessary).

With two endogenous variables we need two separate first stages: one for average class size in primary school and one for middle school. The first stages are following

$$\text{cs}^{\text{PS}} = \pi_{\text{PS}} \cdot \text{above}^{\text{PS}} + \varphi_{\text{PS}} \cdot \text{above}^{\text{MS}} + g_{\text{PS}}(\text{enroll}^{\text{PS}}, \text{enroll}^{\text{MS}}) + \mathbf{x}'\gamma_{\text{PS}} + \xi_{\text{cohort}} + \chi_t + u$$

and

$$\text{cs}^{\text{MS}} = \pi_{\text{LS}} \cdot \text{above}^{PS} + \varphi_{\text{LS}} \cdot \text{above}^{\text{MS}} + g_{LS}(\text{enroll}^{\text{PS}}, \text{enroll}^{\text{MS}}) + \mathbf{x}'\gamma_{\text{MS}} + \tilde{\xi}_{\text{cohort}} + \tilde{\chi}_t + v$$

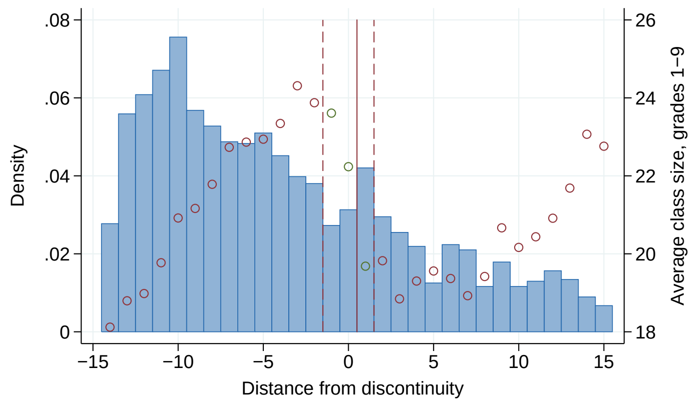
where the instruments above^{PS} and above^{MS} are indicator variables for being above the predicted class size discontinuity within a segment based on enrollment in primary and middle school. All standard errors are again clustered at the school level.

Finally note that we estimate the effect of class size in primary (middle) school while keeping predicted class size in middle (primary) school constant. Primary class size estimates in most existing studies may pick up correlated class size effects in middle school or vice versa.

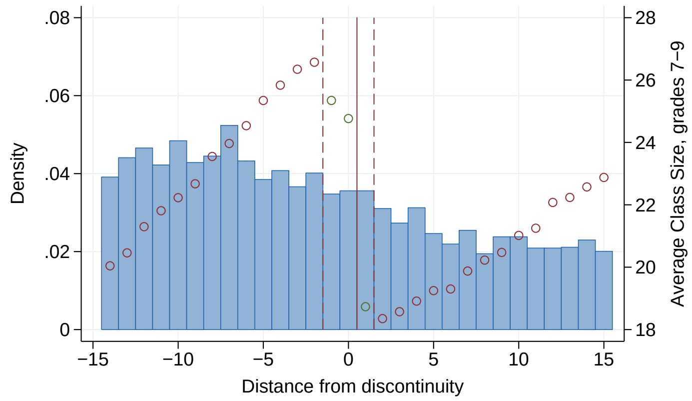
3.4 Instrument validity

One concern in regression discontinuity designs is that agents may place themselves systematically on the left or right side of the kinks. Urquiola and Verhoogen (2009) found a particularly stark example of this in Chile. As a first check, Figure 3 shows histograms of distance from the discontinuity – enrollment normalized to zero at each segment's discontinuity – pooled across segments. This allows for a visual inspection of whether bunching takes place. The figure also plots average class size as a function of normalized enrollment. This is done separately for our baseline sample (the graph at the top), those graduating from any combined school (the graph in the middle), and for all individuals (the graph at the bottom).

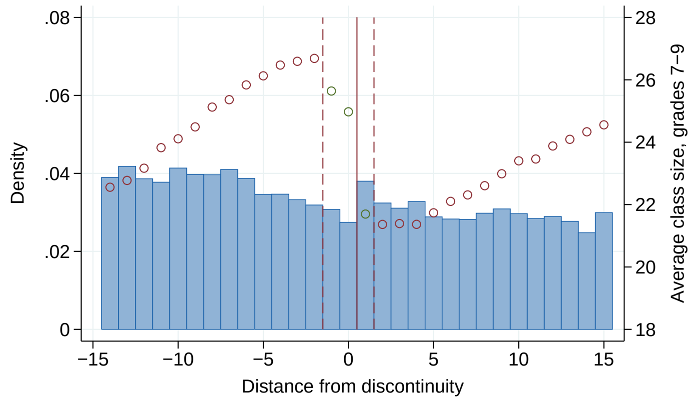
The top panel in Figure 3 shows normalized primary school enrollment for our baseline sample. Because schools in this population are relatively small, the data consist mostly of enrollment around the first two discontinuities. The distribution is relatively smooth and downward sloping, but there appears to be some indication of bunching right after the discontinuity which is indicated by the solid vertical line. We will implement our IV strategy using a so-called donut, and from now on exclude observations where the distance to the discontinuity is no more than one. This will strengthen our first stage and addresses potential imbalance around the cutoff. The donut is indicated by the two vertical dashed lines. The middle and bottom panels show qualitatively the same picture for normalized middle school enrollment in the larger population that attended any combined school (middle panel), and for all individuals (bottom panel). The histograms are flatter because schools in these samples are larger.



Baseline



Combined schools



All schools

Figure 3. The distribution of enrollment distance from discontinuity

The reason for worrying about bunching is that it may lead to a violation of the exogeneity of the instrument. A direct check of such a violation is to see whether the instrument correlates with predetermined variables that are strong predictors of the outcome variables under consideration. From the registry data we can link pupils' background characteristics. We have information on the age and education for both parents, pupils' gender, immigrant status and year and month of birth. The first four columns of Table 2 regress the the main outcome variables on these predetermined background characteristics in the population. The regressions further controls for time and cohort dummies and includes the reference specification for $f(\text{enroll}_i)$. The table shows that the background characteristics are economically important predictors of the outcomes, statistically significant, and we strongly reject the null hypothesis that these predetermined characteristics do not matter.

The last four columns of Table 2 replaces the outcomes with our instruments for primary and middle school class size for everyone, those who attended a combined school, and our baseline population. If there is no confounding bunching/selection around the discontinuities our instruments should be orthogonal to the pupils predetermined background characteristics. This is indeed what we observe: most of the regressors are not significantly different from zero, and the p-value on the joint test that the coefficients are equal to zero are all highly insignificant.[5] Moreover, all the coefficients are very small. This is also the case for the significant effect of mothers years of schooling on the primary school threshold.[6] Finally, the exogeneity of our threshold crossing instruments is also confirmed by specification checks which show that our point estimates are insensitive to the inclusion of these background variables.

[5]Without the donuts, we do not find any indications that the regressors have predictive power on the the placement of the schools on either side of the maximum class size threshold.

[6]A graphical representation of this effect is shown in Figure A1 in the appendix.

Table 2. Instrument Validity

	Outcomes			Population & Instrument			
				All	Combined		Baseline
	Schooling	Log-Earnings	Earnings	Above^{MS}	Above^{PS}	Above^{MS}	Above^{PS}
Girl	0.3861***	-0.2533***	-0.2843***	0.0002	-0.00002	0.0004	0.0013
	(0.0034)	(0.0009)	(0.0009)	(0.0004)	(0.00088)	(0.0011)	(0.0016)
Month of Birth / 12	-0.0061***	-0.0028***	-0.0021***	-0.00002	0.00005	-0.0003	-0.0001
	(0.0005)	(0.0001)	(0.0001)	(0.00006)	(0.00015)	(0.0002)	(0.0002)
Years of Schooling: Father	0.2033***	0.0057***	0.0048***	-0.0003	0.0007*	-0.0001	0.0002
	(0.0008)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0005)	(0.0007)
Years of Schooling: Mother	0.2077***	0.0018***	0.0006***	-0.0002	0.0003	-0.0001	0.0012**
	(0.0008)	(0.0002)	(0.0002)	(0.0002)	(0.0004)	(0.0005)	(0.0005)
Immigrant	-0.1410***	-0.0508***	-0.1032***	-0.0002	0.0041	-0.0039	-0.0038
	(0.0068)	(0.0018)	(0.0017)	(0.0022)	(0.0039)	(0.0049)	(0.0063)
Joint F-test	53772	16541	21486	0.61	0.77	0.66	1.20
Joint p-value	0	0	0	0.69	0.57	0.65	0.31
N Schools	1 813	1 813	1 813	1 813	686	686	398
N Students	1,231,797	1,111,277	1,313,474	1,181,827	180,519	180,519	45,670

Note: Standard errors in parentheses. (* $p<0.10$, ** $p<0.05$, *** $p<0.01$).

Table 3. The effect of class size compulsory school on average outcomes from age 27 to 42 – Baseline population

	Outcome		
	Schooling	Log-Earnings	Earnings
A. First Stage			
Above threshold	-8.92**	-8.88**	-8.89**
	(0.45)	(0.44)	(0.44)
F-statistic	397	399	401
B. Reduced Form			
Above threshold	0.094	-0.003	-0.018
	(0.072)	(0.013)	(0.018)
C. 2SLS			
Average class size grades 1-9	-0.0077	0.0003	0.0018
	(0.0079)	(0.0015)	(0.0021)
N Schools	394	395	398
N Students	43,258	43,766	45,670

Notes: */** = p-value < 0.10/0.05. Standard errors clustered at the school level in parentheses.

4 Results

4.1 The average effect of class size in compulsory schooling

Before discussing the class size effect estimates, first consider the first stage estimates reported in panel A of Table 3. Although our 2SLS estimates are based on first stages where the first stage coefficient is allowed to vary across segments and time periods, Table 3 reports for ease of interpretation the average first-stage coefficient. The instrument, being above the maximum class size threshold in a segment, is a strong predictor of class size in compulsory school. Crossing the discontinuity reduces average class size by about 8 pupils, and this large drop is highly significant with an F-statistic of about 400.

Panel B reports the corresponding pooled reduced form estimates for average schooling and earnings between age 27 and 42. We see no evidence that being above the threshold has any effect on average completed years of schooling, nor on on average earnings.

Panel C reports the corresponding 2SLS class size estimates. Although these are based on first stages where the first stage coefficient is allowed to vary across segments and time periods, they are close to the IV estimate implied by the pooled first-stage and reduced form estimates in Table 3. The first column reports the effect of class size on education. There is no evidence that class size in compulsory school matters for educational attainment. The point estimate implies that a one pupil increase throughout compulsory school reduces average educational attainment by less than 1/100th of a year, and is not statistically significant. The second column in Panel C reports the estimate for Log-Earnings. Consistent with the finding for education, there is also no evidence either that class size affects log-earnings. Even though the point estimate is statistically insignificant, it is precise enough to rule out negative effects of a one pupil increase of class size throughout compulsory school as small as -0.0026 with 95 percent likelihood. To investigate whether labor supply at the extensive margin matters, the third and final column reports the class size effect for average earnings (in levels) normalized by the population average. This variable is set to zero for individuals without income from work and we therefore do not exclude any individual from the estimation sample for this reason. It turns out that unemployment and labor force participation are not affected. The point estimate for earnings in levels is nearly identical to that for Log-Earnings, estimated with similar precision and we thus find no evidence that class size in compulsory schooling matters on average for earnings between age 27 and 42.

Tables A1-A3 in the Appendix investigate the sensitivity of the estimates to the specification of the enrollment controls in the reference 2SLS estimation. All columns in these tables control for segment dummies, and what varies is how enrollment is taken into account. The first column in these tables controls linearly for enrollment and the second column adds a quadratic term. The third and fourth columns repeat these specifications, but introduce interactions with segments. The fifth column allows for segment specific linear splines, which is the reference specification used in the paper. The sixth column relaxes this specification even further by allowing for segment specific quadratic splines. The seventh column shows the estimates without individual and family controls, and the eighth column drops the donut around the discontinuities. .

The main conclusion from this analysis is that the estimates are insensitive to how we control for enrollment. Only in the sixth and the eighth specification do the estimates change, but they remain small and statistically insignificant. Here we also see a sharp drop in the first stage F-statistics from about 400 to 46. Specification (6) shows that the flexible specification in enrollment starts to capture part of the variation generated by the discontinuities, and specification (7) confirms the exogeneity of our instrument. The final specification (8) shows that the fuzziness inside the donut weakens the first-stage.

The effect of class size by age

After graduating from middle school students usually enroll into an upper secondary school.[7] If small class sizes increase the probability of a person taking a long education, it should also decrease income and labor market participation while being educated, while later in life these students could be expected to collect higher earnings than their less educated peers. The outcomes analyzed in the previous subsection are the average of outcomes measured when the individuals were 27-42 years of age. We can however also estimate the age-by-age impact of class size. Examining the age profiles of class size impacts on different long-term outcomes for a wider age range allows us to see whether dynamic adjustments are important. In Figure 4 the average impact of a 1 pupil increase in middle school class size for years of schooling and Log-Earnings are reported separately for each age between 18 and 48. Since the sample size decrease as age increases, we typically lose some precision as age increases.[8]

The left panel of Figure 4 shows that the class size impact on years of schooling

[7]Upper secondary school is optional but almost all children in the Norwegian school system enroll for the first year. It is nevertheless common to drop out or spend more than the nominal duration before matriculating.

[8]The outcomes are observed for each cohort at ages 18 through 27, while they are observed for only one cohort at age 48.

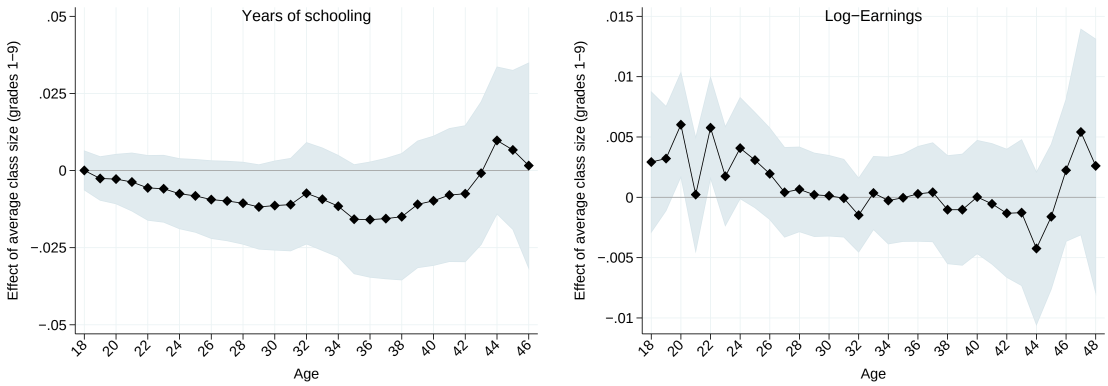


Figure 4. Effect by age

is almost always small and negative and never significantly different from zero. The effect starts out at zero at the age of 18, which can be interpreted as meaning that class size does not impact the probability of graduating from high school at nominal duration. The class size effect gradually grows more negative until the mid-thirties, before steadily shrinking towards zero as age increases. The effect size corresponds to the average estimate of -0.0077 over the age 27–42 presented in Table 3. It should be kept in mind that the effect sizes are very small at all ages, and that the 95 percent confidence interval excludes negative effects smaller than -0.035 years of schooling (less than 13 days).

The right panel of Figure 4 shows that the effect of class size on Log-Earnings is slightly positive when people are in their early twenties, before falling to zero when approaching thirty. From then onwards the effect size is close to zero and statistically insignificant. Notice that the class size impact between the age of 27 and 42 is in line with the 0.0003 average impact size reported in Table 3.

While age profiles are of interest if we want to comment on the dynamic effects class size has on labor market behavior, for the most part there are few signs of the effects changing with age. The most striking conclusion we can draw from these figures are they support the point estimates from Table 3 and that these estimates are stable across different ages.

4.2 The average effect of class size in primary and middle school

As explained in Section 3.3 above, many combined schools take in children from other primary schools which breaks the near perfect correlation between (predicted) class size in primary and middle school. We use this feature of the broader population that attended a combined school to decompose the class size effect estimate for compulsory schooling into that for primary school and that for middle school. Table 4 reports the results.

As before, our 2SLS estimates are based on first-stages that interact PS and MS threshold crossing with segment dummies, and regime dummies to take into account that maximum class size decreased from 30 to 28 in 1985. Also, for ease of interpretation, Panel A and B of Table 4 report the pooled first-stage coefficients. As expected the primary school threshold crossing indicator loads on class size in primary school, while middle school threshold crossing predicts middle school class size best. This also shows that there is sufficient independent variation in our data to separate the effects of class size in primary and middle school.

For completeness, Panel C reports the (pooled) reduced form effects, while Panel D of Table 4 reports the 2SLS class size effect estimates for the long term outcomes that we consider: education, log(earnings), and average earnings. The first row reports class size effects for the primary school grades 1–6. In the first column we see a small class size effect estimate for years of schooling which is also highly insignificant. In the second column we report the estimated class size effect in primary school for Log-Earnings which is also small and very similar to the average point estimate for average class size in compulsory schooling (grades 1-9) in our baseline sample reported in Table 3. The third column reports the primary school class size effect estimate for earnings, which includes zeros for those without work or outside the labor force. As above this estimate is very close to that for Log-Earnings. The final row of Table 4 reports the estimates of the long run impact of class size in the secondary school grades 7–9. We again find no evidence for any class size effects for years of schooling, Log-Earnings or

Table 4. The effect of class size in primary school (PS) and middle school (MS) on average outcomes from age 27 to 42 – Combined Schools

	Schooling	Log-Earnings	Earnings
A. First Stage, PS			
Above PS threshold	-9.98**	-9.96**	-9.95**
	(0.32)	(0.32)	(0.32)
Above MS threshold	-0.92**	-0.92**	-0.92**
	(0.20)	(0.19)	(0.19)
F-test PS instrument	933	943	949
F-test MS instrument	22	22	22
B. First Stage, MS			
Above PS threshold	-0.70**	-0.69**	-0.71**
	(0.29)	(0.28)	(0.28)
Above MS threshold	-8.29**	-8.36**	-8.32**
	(0.30)	(0.29)	(0.30)
F-test PS instrument	6	6	6
F-test MS instrument	761	806	788
C. Reduced form			
Above PS threshold	0.0173	-0.0064	-0.0178*
	(0.0403)	(0.0076)	(0.0103)
Above MS threshold	0.0107	0.0010	0.0021
	(0.0328)	(0.0049)	(0.0067)
D. 2SLS			
Average class size grades 1-6 (PS)	-0.0020	0.0007	0.0017
	(0.0040)	(0.0008)	(0.0011)
Average class size grades 7-9 (MS)	0.0004	-0.00003	-0.0003
	(0.0036)	(0.00060)	(0.0008)
N Schools	682	682	686
N Students	162,103	163,786	171,174

*/** = p-value < 0.10/0.05. Standard errors clustered at the school level in parentheses. All long term impacts are estimated on outcomes when the pupils are 27-42 years old.

Earnings.

To summarize, we find small effect estimates on long run schooling and earnings for class size in compulsory schooling. Decomposing these effects into class size impacts in primary and middle school gives even smaller and more precise point estimates, and we can rule out beneficial effects on Log-Earnings of a one student decrease in class size as small as 0.087 percent in primary school and 0.12 percent in middle school.

5 Heterogeneity and interpretation of the results

Our results contrast with the evidence from Sweden (Fredriksson et al., 2013, 2014) which shows large negative and statistically significant effects from average class size in primary schools (grade 4-6) on earnings and, in some specifications, also for years of schooling. In their preferred specification (Fredriksson et al. (2014), Table A3) a one pupil increase in class size in primary school reduces average earnings between age 27 and 47 by 1.5 percent, while our point estimate of the same effect is very close to zero, and we can rule out effects as small as 0.087 percent. A natural question to ask is whether we can understand this difference in the class size effects between Norway and Sweden.

One explanation lies with parent behavior. Fredriksson et al. (2014) find that lowering class size improves achievement at age 13 for children from parents with low (below median) income, while children from high (above median) income families do not appear to benefit from smaller classes. Fredriksson et al. (2014) also show that high income families help more with homework if their children are in larger classes, while low income families do not appear to adjust their homework help to class size. This suggests that compensating behavior by parents may explain why there is no evidence for a class size effect for children from high income families in Sweden.

If Norwegian parents are like Swedish ones we could expect similar differential class size effects. To investigate this hypothesis, Table 6 reports effect heterogeneity by parental background characteristics that are strong predictors of children's outcomes and are arguably correlated with parental inputs. The lack of evidence for class size effects in the overall population suggest that Norwegian parents compensate more quickly, we therefore investigate heterogeneity more in the lower tail of the parental background distribution. Also, since the average effects are approximately zero, there is little scope for interaction effects of a significant magnitude across broad groups because a negative effect must be offset by a positive one.

In line with Fredriksson et al. (2014), the first panel of Table 5 reports class size effects for children from low (1st quartile) and high (4th quartile) income families. The second panel shows class size effects separately for children of families where none of the parents have more than compulsory schooling (about 18 percent of our population), and children from more highly educated backgrounds, while the last panel of Table 5 shows how the effect of class size differs between immigrants (7 percent of our population) and non-immigrants.

As can be seen from Table 5, there is no evidence that smaller classes provide benefits in the long run for children from more disadvantaged backgrounds, whether it is by parental income, education or migrant background. In fact, none of the reported class size effect estimates are statistically significant at the 10 percent level, and none of the differences across demographic groups are close to being significantly different from zero. The absence of class size effects in Norway is consistent with all Norwegian parents systematically compensating for the class size of their children by increasing their own investments.

A second explanation for the contrast between Norway and Sweden may lie in differences in the school population. In contrast to Sweden, Norway has strong regional policies that aim to sustain populating relatively rural and remote areas. Consequently many schools in Norway are relatively small. Average enrollment in the Swedish sample is 63 pupils, compared to 35 pupils in our baseline sample. This means that in our sample most schools will be in the segment around the first discontinuity, while in Sweden most schools will be in the 2nd segment. If school size mediates class size

Table 5. Effects of average class size in compulsory school (grades 1–9) by parental background – Baseline population

	Schooling	Log-Earnings
A. Fathers' income		
1st quartile	-0.0170	0.0013
	(0.0168)	(0.0030)
2nd & 3rd quartile	0.0061	0.0009
	(0.0147)	(0.0023)
4th quartile	-0.0242	-0.0035
	(0.0244)	(0.0043)
B. Parental education		
Low-education	-0.0142	0.0011
	(0.0120)	(0.0029)
Other families	-0.0056	0.0001
	(0.0089)	(0.0016)
C. Immigrant status		
Immigrant	-0.0056	-0.0093
	(0.0283)	(0.0057)
Non-immigrant	-0.0079	0.0008
	(0.0081)	(0.0016)

*/** = *p*-value < 0.10/0.05. Standard errors clustered at the school level in parentheses. All long term impacts are estimated on outcomes when the pupils are 27-42 years old.

effects then this is another potential source of effect heterogeneity. To investigate this possibility, Table 6 reports class size effect estimates separately by discontinuity.[9] For years of schooling the effects of class size are indeed slightly larger and more negative around the second discontinuity, but neither effects are statistically or economically significant. For Log-Earnings we do not find any indication that an increase in class size reduces average earnings between age 27 and 42. Our point estimates for the effects on education and earnings around the second discontinuity are still nowhere close to the effects, both in size and statistical insignificance, reported in Fredriksson et al. (2014).

Finally, a difference in the population stems from the Swedish sample of school districts with only one school.[10] Such schools are perhaps more shielded from com-

[9]Few schools have enrollment around the higher discontinuities, and these stratified estimates are relatively noisy. We therefore do not report them here.

[10]Fredriksson et al. (2013, 2014) need to restrict their sample in this way to tackle the problem of

Table 6. Effects of average class size in compulsory school (grades 1-9) by school characteristics size – Baseline population

	Schooling	Log-Earnings
A. School size		
1st Segment	-0.0006	0.0009
	(0.0087)	(0.0015)
2nd Segment	-0.0134	0.0025
	(0.0164)	(0.0029)
B. Single school municipality		
Average class size grades 1-9	0.0065	-0.0030
	(0.0266)	(0.0038)

Note: ** = p-value < 0.05. Standard errors clustered at the school level in parentheses. All long term impacts are estimated on outcomes when the pupils are 27-42 years old. The results in panel A use all available data around each discontinuity to estimate the class size effect at each threshold, not only the observations with enrollment within ± 15. The results in Panel B are based on 32 municipalities and 4,100 individuals.

petition, while competitive pressure could attenuate the effect of class size. To investigate whether this difference can explain the diverging results we need to approach the Swedish setup in our sample. In Norway municipalities function as the de-facto school districts. This means that the only way to approximate the Swedish sample is by focusing on single-school municipalities. One caveat here is that while in Sweden these single school districts may lie in a large municipality, this is not the case in Norway.

Panel B of Table 6 presents the estimation results from the sample of one school municipalities. None of the estimates provide evidence of class size effects, suggesting that differences in competitive pressure do not explain the diverging results between Norway and Sweden. Unfortunately we cannot cut our data in such a way to increase both school size and have single school districts as well, which would be necessary to completely match the Swedish population.

flexible catchment areas in Sweden.

6 Conclusion

This paper presents long run impact estimates of average class size in compulsory school for Norway. Many Norwegian middle schools also have a primary school department, allowing us to separately estimate the effects of average class size in grades 1–6 (primary school), and grades 7–9 (middle school). Thanks to exhaustive administrative registries we have information on earnings and schooling for graduation cohorts for the school years 1978/79 to 2002/03, allowing us to observe wages up to age 48 for the oldest cohort.

We do not find any evidence of substantive beneficial effects of class size – neither in primary school nor in middle school – and our most precise estimates rule out effects on income as small as 0.26 percent for a one person reduction in class size throughout compulsory schooling (a 9 year class size reduction). Decomposing these effects into class size impacts in primary and middle school gives even smaller and more precise point estimates, and we can rule out beneficial effects on Log-Earnings of a one student decrease in class size as small as 0.087 percent in primary school and 0.12 percent in middle school. This finding stands in contrast with findings from Sweden Fredriksson et al. (2013) who find substantial impacts on long run wages and educational attainment, and Chetty et al. (2011) who found substantial but statistically insignificant estimates for the United States. We investigate whether the differences between Norway and Sweden may be driven by school size and, possibly, competitive pressure/choice. But we are unable to reconcile the differences between Norway and Sweden.

Our findings emphasize that the effect of class size is not a structural parameter, and are in line with substantial heterogeneity in short run effects across school institutions and populations documented elsewhere (Wößmann and West, 2006). Little is known when and why class size is effective. Further opening up the black box of education production is necessary to make progress on these issues.

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Appendix

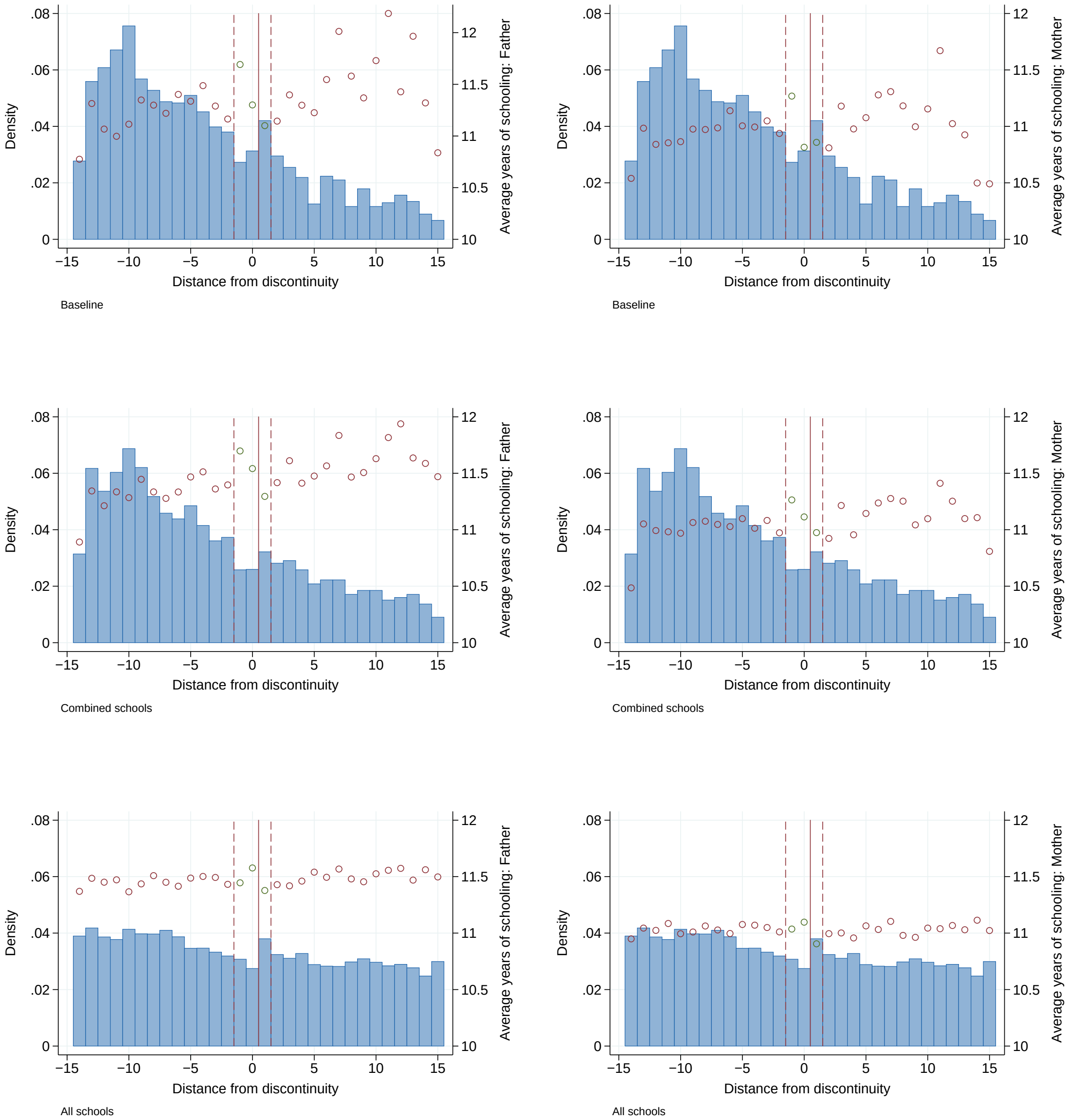

Figure A1. The distribution of enrollment distance from discontinuity in primary school– combined schools

These figures show the distribution of enrollment distance from the discontinuity in primary schools, and the mean values of years of fathers education (first column) and years of mothers education (second column). The figures in the first row are based on the baseline sample, the second row is based on the sample of all combined schools, while the figures in the last row are based on all schools.

Table A1. Specification tests: Schooling

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. First Stage, PS								
Above threshold	-8.66**	-8.65**	-8.82**	-8.81**	-8.92**	-6.42**	-8.91**	-6.31**
	(0.48)	(0.43)	(0.47)	(0.45)	(0.45)	(0.95)	(0.45)	(0.45)
F-statistic	333	397	346	391	397	46	392	201
B. Reduced form								
Above threshold	0.124*	0.136**	0.094	0.104	0.094	0.340**	0.161*	0.135**
	(0.068)	(0.068)	(0.068)	(0.072)	(0.072)	(0.149)	(0.086)	(0.051)
C. 2SLS								
Class size 1-9	-0.0105	-0.0128	-0.0074	-0.0082	-0.0077	-0.0282	-0.0122	-0.0163**
	(0.0077)	(0.0078)	(0.0075)	(0.0079)	(0.0079)	(0.0204)	(0.0091)	(0.0077)
Specification:								
- Linear	Yes		Yes		Yes		Yes	Yes
- Quadratic		Yes		Yes		Yes		
- Segment interactions			Yes	Yes	Yes	Yes	Yes	Yes
- Spline					Yes	Yes	Yes	Yes
- Individual Controls							No	
- Donut								No

Standard errors in parentheses. (* $p<0.10$, ** $p<0.05$). Long term impacts are estimated on years of education when the pupils are 27-42 years old. All these specifications use the baseline sample with 43,258 persons (specification 8 use 51,037). The standard errors that are clustered on 394 schools.

Table A2. Specification tests: ln(Earnings)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. First Stage, PS								
Above threshold	-8.64**	-8.61**	-8.79**	-8.77**	-8.88**	-6.29**	-8.87**	-6.27**
	(0.47)	(0.43)	(0.47)	(0.44)	(0.44)	(0.94)	(0.45)	(0.43)
F-statistic	338	400	349	392	399	45	394	209
B. Reduced form								
Above threshold	-0.0040	-0.0025	-0.0035	-0.0041	-0.0033	0.0062	-0.0066	-0.0006
	(0.0127)	(0.0128)	(0.0129)	(0.0132)	(0.0134)	(0.0292)	(0.0152)	(0.0082)
C. 2SLS								
Class size 1-9	0.0005	0.0004	0.0004	0.0004	0.0003	-0.0004	0.0009	0.0009
	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0015)	(0.0043)	(0.0017)	(0.0013)
Specification:								
- Linear	Yes		Yes		Yes		Yes	Yes
- Quadratic		Yes		Yes		Yes		
- Segment interactions			Yes	Yes	Yes	Yes	Yes	Yes
- Spline					Yes	Yes	Yes	Yes
- Individual Controls							No	
- Donut								No

Standard errors in parentheses. (* $p<0.10$, ** $p<0.05$). Long term impacts are estimated on log(Earnings) when the pupils are 27-42 years old. All these specifications use the baseline sample with 43,766 persons (specification 8 use 50,914). The standard errors that are clustered on 395 schools.

Table A3. Specification tests: Earnings

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. First Stage, PS								
Above threshold	-8.64**	-8.63**	-8.80**	-8.79**	-8.89**	-6.39**	-8.88**	-6.28**
	(0.47)	(0.43)	(0.47)	(0.44)	(0.44)	(0.94)	(0.45)	(0.43)
F-statistic	339	403	352	395	401	47	397	210
B. Reduced form								
Above threshold	-0.0126	-0.0110	-0.0137	-0.0174	-0.0182	0.0007	-0.0225	-0.0029
	(0.0169)	(0.0171)	(0.0172)	(0.0176)	(0.0178)	(0.0382)	(0.0202)	(0.0107)
C. 2SLS								
Class size 1-9	0.0015	0.0013	0.0015	0.0018	0.0018	0.0002	0.0025	0.0011
	(0.0019)	(0.0020)	(0.0020)	(0.0021)	(0.0021)	(0.0054)	(0.0023)	(0.0017)
Specification:								
- Linear	Yes		Yes		Yes		Yes	Yes
- Quadratic		Yes		Yes		Yes		
- Segment interactions			Yes	Yes	Yes	Yes	Yes	Yes
- Spline					Yes	Yes	Yes	Yes
- Individual Controls							No	
- Donut								No

Standard errors in parentheses. (* $p<0.10$, ** $p<0.05$). Long term impacts are estimated on earnings when the pupils are 27-42 years old. All these specifications use the baseline sample with 45,670 persons (specification 8 use 54,388). The standard errors that are clustered on 398 schools.

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