Supplementary material to:

"Long-term effects of class size"

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This document provides supplementary material to our paper "Long-term effects of class size". Appendix I provides details of the construction of three central variables: parents' income, non-cognitive ability at age 13, and years of schooling. Appendix II inquires the robustness of the findings reported in the paper with regard to the restriction to one-school districts, the unit at which the standard errors are clustered, the choice of bandwidth, inclusion of municipality-by-cohort fixed effects and baseline covariates, the choice of instrument and the pooling of thresholds. Appendix III reports the correlations between log wages and cognitive and non-cognitive ability.

Appendix I: Construction of three central variables

Parents' income. Parents' income is generated by summing both parents' average non-zero earnings at prime age (35-45 years). The earnings in a given year consist of the annual gross wage earnings and compensation during temporary work absence (illness or parental leave), and are based on income statements made by employers.

Non-cognitive ability at age 13. The index of non-cognitive skills at age 13 is derived from survey questions for the pupils in 6th grade and from school administrative data. Details about the underlying questions and their availability across cohorts are shown in Table AI. To ensure that the index is comparable with the psychological evaluation at age 18, we formed the index by weighting the questions by the estimated parameters from a regression of non-cognitive skills at age 18 on the questions of non-cognitive skills at age 13. The regression also controlled for the measures of cognitive skills at age 13, fixed effects for enrollment segment; linear controls for enrollment which are interacted with threshold and segment, municipality-by-cohort fixed effects, gender, dummy variables for month of birth, dummy variables for mother's and father's educational attainment, parental income, mother's age at child's birth, indicators for being a first or second generation Nordic immigrant, indicators for being a first or second generation non-Nordic immigrants, an indicator for having separated parents, and the number of siblings.

Years of schooling. We convert the data on the highest education level attained to years of schooling using the Swedish Level of Living Survey (SLLS) conducted in 2000. The SLLS includes register information on educational attainment and survey information on time spent in school for a representative sample of individuals. We regress years of schooling (from the survey) on seven dummy variables for highest educational attainment (from the register) in the SLLS sample. Then we predict years of education for the ETF sample using the regression estimates derived from the SLLS data.

Table AI: Information used for construction of the index for non-cognitive ability at age 13

	Information available for cohorts			
	1967	1972	1977	1982
Survey questions for pupils in 6th grade				
"Do you give up if you get a difficult task in school?" (Persistence)	√	√		√
"Are you often worried about things happening in school?" (Anxiety)	√	√		√
"What occupation do you aim for?" (Aspirations)	√	√	√	√
"Do you think you do well in school?" (Self-confidence)	√	√	√	
"Do you often think about other things when you do arithmetic or write in school?" (Concentration)	√	√		
"Do you get disappointed if you get bad results on a test?" (Expectations)	√	√		
"Do you sometimes bring schoolmates home?" (Sociability)	√	√		
"Are you often alone during breaks?" (Sociability)	√	√		
"Do you like working together with your classmates?" (Cooperativeness)	√	√	√	
"Do you think you learn a lot of meaningless things in school?" (Motivation)	√	√		
"Do you do your best even with boring tasks?" (Effort)	√	√		
"Do you think it's scary to answer questions in school?" (Self-security)	√	√		√
"Do you think it's hard to get the answer right, even if you know it, when you are asked by the teacher?" (Coping with stress)	√			√
School administrative data in 6th grade				
Absenteeism	√	√		

Notes: The exact formulation of the questions varies slightly between cohorts. The answering alternatives vary somewhat between questions and cohorts.

Appendix II: Robustness

This appendix inquires the robustness of the findings reported in the paper with regard to the restriction to one-school districts, the unit at which the standard errors are clustered, the choice of bandwidth, the use of different instruments and control functions, the use of municipality-by-cohort fixed effects and baseline covariates, and the pooling of thresholds.

Restriction to one-school districts. For ease of comparison, column (1) of Table AII reproduces the IV estimates from Table V in the paper. Column (2) of Table AII shows analogous IV estimates obtained using the full sample, i.e., the estimates obtained when including districts with more than one school. This shows that the point estimates obtained from the two samples are very similar, and that the estimates from the one-school districts have smaller standard errors.

Level of clustering of standard errors. Table AIII reports standard estimates for three levels of clustering: the school by cohort level in column (1), the school level in column (2) and the enrollment count level in column (3). Column (3) thus contains the standard errors reported in the paper. The standard errors are very similar across the three levels of clustering. Consequently, significance levels of the estimated effects are hardly affected. Only for cognitive ability at age 13 and for the probability to have a bachelor degree, there is some movement between 5% and 10% significance levels.

Choice of bandwidth. The paper reports results obtained using a bandwidth of 15 around the thresholds. The last column of Table AIV repeats the reduced form results from the paper for the effect of the instrument on class size, cognitive ability at age 13 and log wages. The other columns report results for narrower bandwidths. The effects on class size are stable across the different bandwidths while the estimated effects on cognitive ability at age 13 and on log wages increase somewhat when the bandwidth gets smaller. The bottom rows report the number of districts by cohort and the number of clusters (enrollment counts) for different bandwidths. For bandwidths below 7, the number of clusters becomes rather small, which affects the reliability of the estimated standard errors.

Maimonides Rule. The paper reports results using an indicator for being above or below a threshold as instrumental variable. Following Angrist and Lavy (1999) we have also estimated the impact of class size on outcomes using class size predicted by the maximum

class size rule as the instrumental variable. The first column of Table AV repeats the results reported in column (1) of Table V in the paper. Columns (2) and (3) report results using predicted class size as the instrumental variable for different specifications of the running variable. Column (4) uses predicted class size as the instrumental variable but limits the sample to districts that have enrollment at most 5 pupils away from a threshold. The estimates in columns (2) and (4) are very similar to those in column (1). The estimates in column (3) are somewhat smaller than those in column (1). The effects on cognitive ability at age 13, academic achievement at age 16, and on log wages are still statistically significant.

Municipality-by-cohort fixed effects. Table V in the paper reports results from specifications that include municipality-by-cohort fixed effects. In the paper we argue that this is the correct specification given the non-random way in which municipalities were sampled. Nevertheless, Table AVI shows that results from specifications without these fixed effects are similar. The first column has no fixed effects or controls (except for the segment-based control functions). The point estimates are close to the specification with full controls (column 4), but the precision is low. The second column only controls for the basic covariates but does not include the fixed effects. This typically does not change the point estimates much, but the precision increases. The third and fourth column replicate the results in Table V in the paper (but in reversed order).

Effects at separate thresholds. The paper reports results based on pooled data from the four thresholds at 30, 60, 90 and 120. The reason for this is the small number of observations per threshold. To conduct a meaningful analysis of the effects at separate thresholds, we must be more restrictive in specifying the control function. To analyze effects at different thresholds we impose segment fixed effects and linear controls for enrollment that are allowed to vary by threshold (but not by segment). We also make use of school districts with less than 15 pupils, to have more data to estimate the slope before the first threshold. This results in 6,092 observations and 202 district×cohorts. Figure AI shows the relations between enrollment in one-school districts on the one hand, and parental education, average class size, cognitive ability at age 13 and log wages on the other hand. The top-left graph shows that there is no systematic relation between parental education and enrollment around the thresholds. The top-right graph shows that the first stage relationship between enrollment and average class size is present at the first three thresholds, but not at the fourth (note, however, that there are only two observations where enrollment exceeds 120). The bottom-left graph shows the relation between enrollment and cognitive ability at age 13. Ability is always higher just right

of the thresholds than just left of the thresholds. The bottom-right graph shows that the same is true for the relation between enrollment and log wages.

The top part of Table AVII shows reduced form estimates of being above the threshold on outcomes separately for the four thresholds. The table shows that when class size is affected by the rule (the first three thresholds), the reduced form effects on the outcomes have the expected signs. At the fourth threshold, where there is no effect on class size, there are no significant effects on any of the outcomes. For the first three thresholds, there is some variability of the estimated effects across thresholds, but these differences are in almost all cases not significantly different from zero. The reason for the variability in the reduced form effects is most likely that we have a rather limited number of observations around each threshold. The lack of observations around each threshold is the reason for pooling all thresholds in the paper.

The bottom panel of Table AVII shows IV estimates obtained by exploiting the thresholds in another fashion than in the main analysis reported in the paper. Here we estimate the overall effect of class size on outcomes using the four thresholds as separate instruments. Relative to the estimates reported in column (1) in Table V of the paper, the estimates are very similar and slightly more precise.

Graphs not conditioned on baseline covariates. Figures VI and VII in the paper show the relations between normalized enrollment in grade 4 and residual cognitive ability and residual ln wages, respectively. The residuals come from regressions of cognitive ability and ln wages on fixed effects for enrollment segments, municipality-by-cohort fixed effects and baseline covariates. Figure AII shows similar graphs when we omit baseline covariates from the conditioning set. The lines in the graphs show the linear best fits at either side of the threshold. These lines are very similar as those in Figures VI and VII in the paper. There is more dispersion, however. All in all, inclusion of baseline covariates increases precision without affecting the estimates.

Table AII: IV estimates of class size in 4th-6th grade, one-school districts and full sample

Outcome [# individuals]	One-school districts (1)	Full sample (2)
Ability measures		
Cognitive ability, age 13	-0.0330**	-0.0440***
$[N = 5,116; N = 25,856]$	(0.0146)	(0.0167)
Non-cognitive ability, age 13	-0.0265**	-0.0258
$[N = 4,681; N = 23,864]$	(0.0118)	(0.0179)
Academic achievement, age 16	-0.0233**	-0.0172
$[N = 5,318; N = 26,081]$	(0.0101)	(0.0139)
Educational attainment (age 27-42)		
Years of schooling	-0.0545**	-0.0494
$[N = 5,588; N = 27,771]$	(0.0256)	(0.0319)
P(bachelor's degree)	-0.0076*	-0.0077
$[N = 5,920; N = 29,371]$	(0.0043)	(0.0054)
Labor market outcomes (age 27-42)		
Earnings (effect relative to the average)	-0.0117*	-0.0129
$[N = 5,920; N = 29,371]$	(0.0061)	(0.0103)
ln(wage)	-0.0063*	-0.0102**
$[N = 3,185; N = 16,283]$	(0.0033)	(0.0047)
P(earnings>0)	-0.0016	-0.0018
$[N = 5,920; N = 29,371]$	(0.0024)	(0.0037)
No. of clusters	77	165
No. of districts×cohort	191	697

Note: The estimates are based on representative samples of individuals born in 1967, 1972, 1977 or 1982. All ability measures are standardized. The educational outcomes are measured in 2009, while the labor market outcomes have been averaged over the 2007-2009 period. Earnings effects (and their standard errors) are divided by the average earnings level to facilitate interpretation. The ln(wage) estimates are restricted to wage-earners. Class size in grades 4-6 is instrumented with Above threshold (=1 if school district enrollment in 4th grade exceeds the class size rule threshold in the enrollment segment). All models include the following controls for school district enrollment in grade 4: fixed effects for enrollment segment; linear controls for enrollment which are interacted with threshold and segment. In addition all models include the following baseline controls: municipality-by-cohort fixed effects, gender, dummy variables for month of birth, dummy variables for mother's and father's educational attainment, parental income, mother's age at child's birth, indicators for being a first or second generation Nordic immigrant, indicators for being a first or second generation non-Nordic immigrant, an indicator for having separated parents, and the number of siblings. Standard errors adjusted for clustering by enrollment count are in parentheses. ***/**/*=the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Table AIII: IV-estimates of class size on different outcomes; different levels of clustered SE

Level of clustering	School by cohort	School	Enrollment
Ability measures			
Cognitive ability, age 13	-0.0330**	-0.0330**	-0.0330**
[N=5,116]	(0.0172)	(0.0172)	(0.0146)
Non-cognitive ability, age 13	-0.0265*	-0.0265*	-0.0265**
[N=4,681]	(0.0139)	(0.0142)	(0.0118)
Academic achievement, age 16	-0.0233**	-0.0233**	-0.0233**
[N=5,318]	(0.0116)	(0.0111)	(0.0101)
Educational attainment (age 27-42)			
Years of schooling	-0.0545**	-0.0545**	-0.0545**
[N=5,588]	(0.0243)	(0.0241)	(0.0256)
P(Bachelor's degree)	-0.0076**	-0.0076*	-0.0076*
[N=5,920]	(0.0039)	(0.0040)	(0.0043)
Labor market outcomes (age 27-42)			
Earnings (effect relative to average)	-0.0117*	-0.0117*	-0.0117*
[N=5,920]	(0.0067)	(0.0065)	(0.0061)
ln(Wage), ages 27-42	-0.0063*	-0.0063*	-0.0063*
[N=3,185]	(0.0036)	(0.0035)	(0.0033)
P(Eearnings>0), ages 27-42	-0.0016	-0.0016	-0.0016
[N=5,920]	(0.0025)	(0.0026)	(0.0024)
Number of clusters	191	156	77

Note: The estimates are based on representative samples of individuals born in 1967, 1972, 1977 or 1982 in one-school districts. All ability measures are standardized. The educational outcomes are measured in 2009, while the labor market outcomes have been averaged over the 2007-2009 period. Earnings effects (and their standard errors) are divided by the average earnings level to facilitate interpretation. The ln(wage) estimates are restricted to wage-earners. Class size in grades 4-6 is instrumented with Above threshold (=1 if school district enrollment in 4th grade exceeds the class size rule threshold in the enrollment segment). All models include the following controls for school district enrollment in grade 4: fixed effects for enrollment segment; linear controls for enrollment which are interacted with threshold and segment. In addition all models include the following baseline controls: municipality-by-cohort fixed effects, gender, dummy variables for month of birth, dummy variables for mother's and father's educational attainment, parental income, mother's age at child's birth, indicators for being a first or second generation Nordic immigrant, indicators for being a first or second generation non-Nordic immigrant, an indicator for having separated parents, and the number of siblings. Standard errors adjusted for clustering at the levels indicated in the table are in parentheses. ***/**/*=the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Table AIV: Reduced form estimates, different bandwidths

	Bandwidth				
	7	9	11	13	15
Class size grades 4-6	-5.6493***	-5.8616***	-5.959***	-6.2937***	-6.6254***
	(0.7607)	(0.8750)	(0.9619)	(0.8960)	(0.7523)
Cognitive ability, age 13	0.3811**	0.4323***	0.2643***	0.2322**	0.2144**
	(0.1664)	(0.1424)	(0.1010)	(0.0965)	(0.0876)
ln(wage), age 27-42	0.0648***	0.0696***	0.0756***	0.0683***	0.0428**
	(0.0219)	(0.0255)	(0.0186)	(0.0172)	(0.0212)
Number of clusters	38	49	58	68	77
No. of districts×cohort	90	112	140	166	191

Note: The estimates are based on representative samples of individuals born in 1967, 1972, 1977 or 1982 in one-school districts. The table shows the estimates of Above threshold (=1 if school district enrollment in 4th grade exceeds the class size rule threshold in the enrollment segment). Cognitive ability at age 13 is standardized. The wage measure is an average during 2007-2009 and is restricted to wage-earners. All models include the following controls for school district enrollment in grade 4: fixed effects for enrollment segment; linear controls for enrollment which are interacted with threshold and segment. In addition all models include the following baseline controls: municipality-by-cohort fixed effects, gender, dummy variables for month of birth, dummy variables for mother's and father's educational attainment, parental income, mother's age at child's birth, indicators for being a first or second generation Nordic immigrant, indicators for being a first or second generation non-Nordic immigrant, an indicator for having separated parents, and the number of siblings. Standard errors adjusted for clustering by enrollment count are in parentheses. ***/**/*=the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Table AV: IV-estimates of class size, using different instruments and control functions

Model	(1)	(2)	(3)	(4)
Ability measures				
Cognitive ability, age 13	-0.0330**	-0.0218*	-0.0175*	-0.0424**
[N=5,116]	(0.0146)	(0.0123)	(0.0105)	(0.0163)
Non-cognitive ability, age 13	-0.0265**	-0.0206**	-0.0119	-0.0376**
[N=4,681]	(0.0118)	(0.0103)	(0.0123)	(0.0138)
Academic achievement, age 16	-0.0233**	-0.0239**	-0.0212**	-0.0240**
[N=5,318]	(0.0101)	(0.0096)	(0.0102)	(0.0104)
Educational attainment (age 27-42)				
Years of schooling	-0.0545**	-0.0381	-0.0137	-0.0254
[N=5,588]	(0.0256)	(0.0267)	(0.0233)	(0.0242)
P(Bachelor's degree)	-0.0076*	-0.0063	-0.0042	-0.0031
[N=5,920]	(0.0043)	(0.0046)	(0.0048)	(0.0058)
Labor market outcomes (age 27-42)				
Earnings (effect relative to the average)	-0.0117*	-0.0105**	-0.0066	-0.0128*
[N=5,920]	(0.0061)	(0.0049)	(0.0051)	(0.0069)
ln(Wage), ages 27-42	-0.0063*	-0.0058**	-0.0053**	-0.0079**
[N=3,185]	(0.0033)	(0.0028)	(0.0022)	(0.0032)
P(Earnings>0)	-0.0016	-0.0011	-0.0033	-0.0001
[N=5,920]	(0.0024)	(0.0020)	(0.0021)	(0.0029)
Instrument				
Above threshold	√			
Predicted class size		√	√	√
Enrollment controls				
Segment fixed effects	√	√		
Polynomials:				
- 1st order	√	√		
- 2nd order			√	
Interacted with segments and thresholds	√	√		
± 5 pupils from thresholds				√
Number of districts×cohorts	191	191	191	64

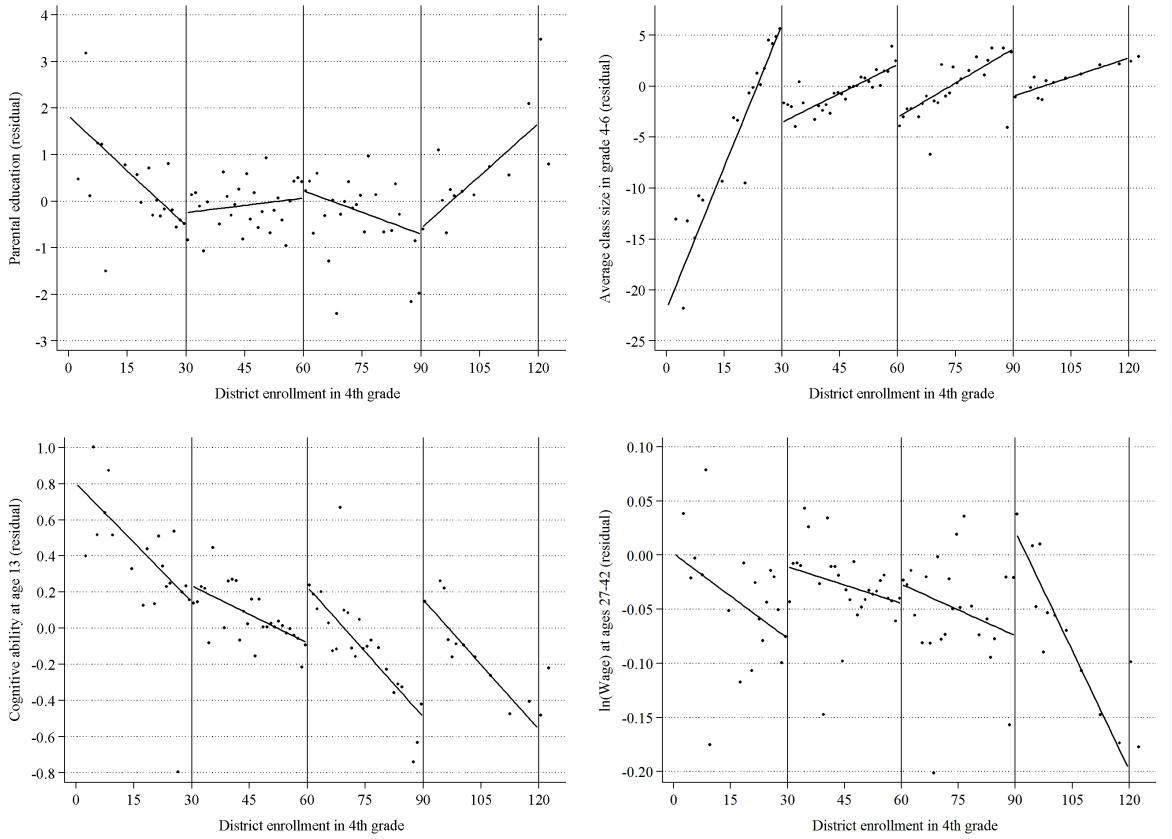
Note: The estimates are based on representative samples of individuals born in 1967, 1972, 1977 or 1982 in one-school districts. All ability measures are standardized. The educational outcomes are measured in 2009, while the labor market outcomes have been averaged over the 2007-2009 period. Earnings effects (and their standard errors) are divided by the average earnings level to facilitate interpretation. The ln(wage) estimates are restricted to wage-earners. In column (1), average class size in grades 4-6 is instrumented with Above threshold (=1 if school district enrollment in 4th grade exceeds the class size rule threshold in the enrollment segment). In columns (2)-(4), class size is instrumented by the predicted class size according to the maximum class size rule. All models include the following controls for school district enrollment in grade 4: fixed effects for enrollment segment; linear controls for enrollment which are interacted with threshold and segment. In addition all models include the following baseline controls: municipality-by-cohort fixed effects, gender, dummy variables for month of birth, dummy variables for mother's and father's educational attainment, parental income, mother's age at child's birth, indicators for being a first or second generation Nordic immigrant, indicators for being a first or second generation non-Nordic immigrant, an indicator for having separated parents, and the number of siblings. Standard errors adjusted for clustering by enrollment count are in parentheses. ***/**/*=the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Table AVI: IV-estimates of class size, using different control variables

Model	(1)	(2)	(3)	(4)
Ability measures				
Cognitive ability, age 13	-0.0229	-0.0133	-0.0327	-0.0330**
[N=5,116]	(0.0236)	(0.0124)	(0.0230)	(0.0146)
Non-cognitive ability, age 13	-0.0456**	-0.0394**	-0.0263**	-0.0265**
[N=4,681]	(0.0194)	(0.0166)	(0.0119)	(0.0118)
Academic achievement, age 16	-0.0178	-0.0175	-0.0211	-0.0233**
[N=5,318]	(0.0223)	(0.0123)	(0.0180)	(0.0101)
Educational attainment (age 27-42)				
Years of schooling	-0.0628	-0.0502	-0.0480	-0.0545**
[N=5,588]	(0.0581)	(0.0329)	(0.0459)	(0.0256)
P(Bachelor's degree)	-0.0064	-0.0049	-0.0063	-0.0076*
[N=5,920]	(0.0080)	(0.0053)	(0.0066)	(0.0043)
Labor market outcomes (age 27-42)				
Earnings (effect relative to the average)	-0.0090	-0.0104	-0.0099	-0.0117*
[N=5,920]	(0.0168)	(0.0143)	(0.0066)	(0.0061)
ln(Wage), ages 27-42	-0.0026	-0.0040	-0.0043	-0.0063*
[N=3,185]	(0.0075)	(0.0061)	(0.0037)	(0.0033)
P(Earnings>0), age 27-42	-0.0040	-0.0027	-0.0011	-0.0016
[N=5,920]	(0.0034)	(0.0025)	(0.0029)	(0.0024)
Controls				
Municipality-by-cohort FE			√	√
Control variables		√		√
Number of districts×cohorts	191	191	191	191

Note: The estimates are based on representative samples of individuals born in 1967, 1972, 1977 or 1982 in one-school districts. All ability measures are standardized. The educational outcomes are measured in 2009, while the labor market outcomes have been averaged over the 2007-2009 period. Earnings effects (and their standard errors) are divided by the average earnings level to facilitate interpretation. The ln(wage) estimates are restricted to wage-earners. Average class size in grades 4-6 is instrumented with Above threshold (=1 if school district enrollment in 4th grade exceeds the class size rule threshold in the enrollment segment). All models include the following controls for school district enrollment in grade 4: fixed effects for enrollment segment; linear controls for enrollment which are interacted with threshold and segment. The control variables are: gender, dummy variables for month of birth, dummy variables for mother's and father's educational attainment, parental income, mother's age at child's birth, indicators for being a first or second generation Nordic immigrant, indicators for being a first or second generation non-Nordic immigrant, an indicator for having separated parents, and the number of siblings. Standard errors adjusted for clustering by enrollment count (77 clusters) are in parentheses. ***/**/*=the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Figure AI: The relation between district enrollment and (1) parental education, (2) average class size, (3) cognitive ability and (4) log wages



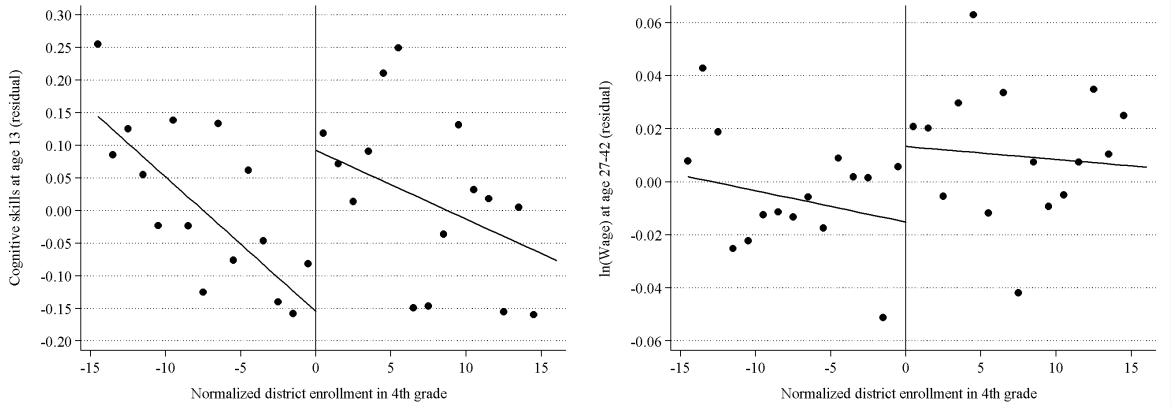
Notes: The figures show the relations between enrollment and (1) residual parental education, (2) residual class size, (3) residual cognitive ability at age 13, and (4) residual log wages at age 27-42, after controlling for fixed effects for enrollment segments and municipality-by-cohort fixed effects, by school district enrollment in grade 4 (1-student bins). The residuals for cognitive ability and log wages come from regressions that also controls for gender, dummy variables for month of birth, dummy variables for mother's and father's educational attainment, parental income, mother's age at child's birth, indicators for being a first or second generation Nordic immigrant, indicators for being a first or second generation non-Nordic immigrant, an indicator for having separated parents, and the number of siblings. The data pertain to representative samples of individuals born in 1967, 1972, 1977 or 1982 in one-school districts. The regression lines were fitted to individual data.

Table AVII: Estimates of class size on different outcomes, exploiting separate thresholds

Outcome	Class size grades 4-6	Cognitive ability age 13	Non-cogn. ability age 13	Academic achievement age 16	Years of schooling age 27-42	Earnings age 27-42 (%)	ln(Wage) age 27-42
				Reduced form effects			
Above 1st threshold	-10.0425***	0.0942	0.0382	0.2553**	0.0010	0.0954**	0.0501**
	(0.9002)	(0.1054)	(0.1112)	(0.1049)	(0.2419)	(0.0370)	(0.0222)
Above 2nd threshold	-5.2010***	0.3034***	0.2152**	0.1189	0.3888**	0.1100**	0.0395
	(1.0690)	(0.1100)	(0.0936)	(0.0880)	(0.1770)	(0.0851)	(0.0361)
Above 3rd threshold	-4.6842***	0.6487***	0.5611***	0.3145*	1.1312***	0.2565**	0.1259***
	(1.3048)	(0.1333)	(0.1689)	(0.1805)	(0.3518)	(0.1099)	(0.0344)
Above 4th threshold	-0.1784	0.0910	-0.2841	-0.0421	0.2466	0.0912	-0.0286
	(1.0580)	(0.1427)	(0.1888)	(0.1529)	(0.3621)	(0.1294)	(0.0435)
				IV estimates of class size			
Class size grades 4-6	.	-0.0272**	-0.0191*	-0.0269***	-0.0324	-0.0156***	-0.0070***
	.	(0.0117)	(0.0114)	(0.0085)	(0.0231)	(0.0059)	(0.0026)
N observations	6,092	5,260	4,798	5,455	5,745	6,092	4,605
N districts×cohorts	202	202	202	202	202	202	202

Note: The estimates are based on representative samples of individuals born in 1967, 1972, 1977 or 1982 in one-school districts. All ability measures are standardized. The educational outcomes are measured in 2009, while the labor market outcomes have been averaged over the 2007-2009 period. Earnings effects (and their standard errors) are divided by the average earnings level to facilitate interpretation. The ln(wage) estimates are restricted to wage-earners. Above 1st/2nd/3rd/4th threshold (the instruments for class size) are indicators equalling unity if school district enrollment in 4th grade exceeds the 1st/2nd/3rd/4th threshold of the class size rule, respectively. In the reduced form effects, the outcomes are regressed on the indicators for being above the class size rule thresholds. In the IV-estimates, average class size in grades 4-6 is instrumented by the indicators for being above 1st, 2nd, 3rd or 4th threshold of the class size rule. All models include the following controls for school district enrollment in grade 4: fixed effects for enrollment segment; linear controls for enrollment which are interacted with threshold. In addition all models include the following baseline controls: municipality-by-cohort fixed effects, gender, dummy variables for month of birth, dummy variables for mother's and father's educational attainment, parental income, mother's age at child's birth, indicators for being a first or second generation Nordic immigrant, indicators for being a first or second generation non-Nordic immigrant, an indicator for having separated parents, and the number of siblings. Standard errors adjusted for clustering by enrollment count are in parentheses. ***/**/*=the estimates are significantly different from zero at the 1/5/10 percent level, respectively.

Figure AII: The relation between district enrollment in grade 4 and (1) cognitive ability and (2) log wages, without controlling for baseline covariates



Notes: The figures show residual cognitive ability (left hand) and log wages (right hand), by normalized enrollment in grade 4 (1-student bins). The residuals come from regressions which include fixed effects for enrollment segments and municipality-by-cohort fixed effects. The data pertain to one-school districts for cohorts born 1967, 1972, 1977 and 1982. The regression lines were fitted to individual data. Discontinuities at threshold: 0.252 (s.e. 0.135) for cognitive ability and 0.029 (s.e. 0.020) for wages.

Appendix III: Correlation wage earnings and ability scores at age 13

The paper reports estimates of the effect of class size on adult wage earnings using an indirect method as in Krueger (2003), Schanzenbach (2007) and Chetty et al. (2011). To apply this method, we need estimates of the correlations between wage earnings and ability scores at age 13. These correlations are reported in Table AVIII. Columns (1) to (3) report correlations of ability measures with log wages, columns (4) to (6) report correlations of ability measures with earnings (relative to the average). The results show strong correlations between wage earnings and both cognitive and non-cognitive ability.

Table AVIII: The correlation between adult wage earnings and cognitive/non-cognitive scores at age 13

	ln(wage)			Earnings (eff. rel. to avg.)		
	(1)	(2)	(3)	(4)	(5)	(6)
Cognitive ability	0.0821***		0.0719***	0.1178***		0.0980***
	(0.0021)		(0.0022)	(0.0052)		(0.0051)
Non-cognitive ability		0.0552***	0.0328***		0.0929***	0.0620***
		(0.0021)	(0.0021)		(0.0045)	(0.0043)
Number of individuals	14,246	14,246	14,246	25,254	25,254	25,254

Note: Each column reports estimates from OLS regressions based on representative samples of individuals born in 1967, 1972, 1977 or 1982. The outcomes have been averaged over the 2007-2009 period. Earnings effects (and their standard errors) are divided by the average earnings level to facilitate interpretation. The ln(wage) estimates are restricted to wage-earners. All models control for municipality-by-cohort fixed effects. Robust standard errors in parentheses. ***=the estimates are significantly different from zero at the 1 percent level.

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