Where Do STEM Graduates Stem From? The Intergenerational Transmission of Comparative Skill Advantages

Eric A. Hanushek, Babs Jacobs, Guido Schwerdt,
Rolf van der Velden, Stan Vermeulen, and Simon Wiederhold[†]

Online Appendices

[†] Hanushek: Hoover Institution, Stanford University, CESifo, and NBER, hanushek@stanford.edu; Jacobs: Research Centre for Education and the Labour Market (ROA) at Maastricht University, bpja.jacobs@maastrichtuniversity.nl; Schwerdt: University of Konstanz, ifo Institute, CESifo, and IZA, guido.schwerdt@uni-konstanz.de; van der Velden: Research Centre for Education and the Labour Market (ROA) at Maastricht University, r.vandervelden@maastrichtuniversity.nl; Vermeulen: Research Centre for Education and the Labour Market (ROA) at Maastricht University, c.vermeulen@maastrichtuniversity.nl; Wiederhold: Halle Institute for Economic Research (IWH), University of Halle, ifo Institute, and CESifo, simon.wiederhold@iwh-halle.de

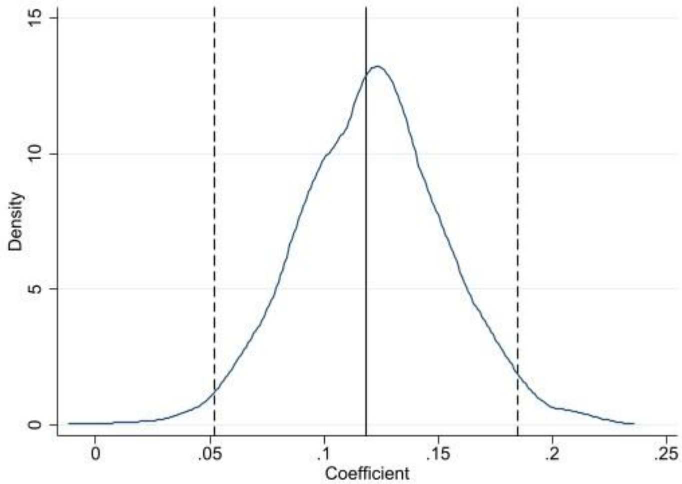
A.1 Appendix for Measurement error

Potential measurement error due to observing only one parent

We usually observe the cognitive skills of only one of the parents in our linked data, and this could potentially induce measurement error in the parent skill variables. To address this, we make use of the subsample of 365 students in the ITS dataset where we observe both parents. We perform the following analysis: In the two-parent sample, we randomly drop one of the parents and estimate the relationship between child and parent comparative skill advantages. Figure A1 shows the distribution of the coefficients on parents' comparative skill advantage when redrawing samples 1,200 times. The resulting estimates are close to the coefficient obtained in the two-parent sample (indicated by the solid vertical line). In fact, 96 percent of the bootstrapped coefficients are within the 95 percent confidence interval of the two-parent-sample coefficient (indicated by the dashed vertical lines). This exercise provides direct evidence that observing only one of the parents in the majority of our data is unlikely to affect our results.[57]

[57] In the two-parent sample, the cognitive skills of mothers and fathers are significantly positively correlated (correlation coefficients of 0.25 for math, 0.32 for language, and 0.14 for the difference between math and language). This corroborates previous evidence on positive assortative mating on educational attainment (e.g., Eika, Mogstad, and Zafar (2019), Educational Assortative Mating and Household Income Inequality, *Journal of Political Economy* 127, no. 6: 2795-2835).

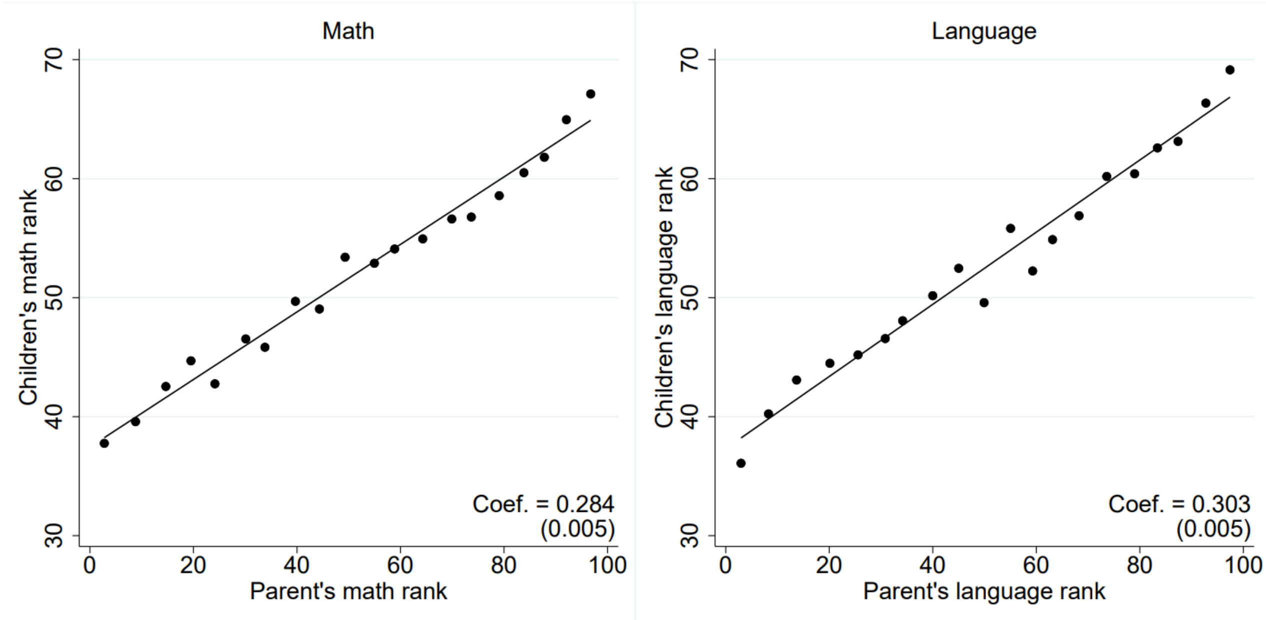
Figure A1: Randomly dropping one parent in two-parent sample



Notes: The figure depicts estimated coefficients on parents' comparative skill advantage in the least squares model (see eq. 10) when redrawing samples 1,200 times. Estimations are conducted based on 365 children for whom we observe both parents in the survey data. In each of the 1,200 iterations we randomly drop one of the parents for each child and estimate the relationship between child and parent comparative skill advantages. Solid vertical line indicates coefficient in the two-parent estimation, dashed lines indicate 95 percent confidence interval. *Data sources:* Administrative data; pooled ITS survey dataset.

A.2 Appendix for Section 6.1: OLS Models

Figure A2: Binned scatterplots of child cognitive skills and parent cognitive skills



Notes: The figure displays two binned scatterplots showing the strength of parent-child transmissions in math skills (left) and language skills (right). Child skills are measured as the percentile rank of test scores of linked children in full sample of children taking the test in a given year based on the administrative data. Parent skills are measured as the percentile rank of test scores of linked parents in full sample of parents and nonparents in an education cohort. To construct the figure, we divided the parent skill rank into 20 ranked equal-sized groups and plotted the mean of the children skill rank against the mean of the parent skill rank in each bin. The best-fit line, the coefficient, and the standard error (clustered at the parent level) are calculated from bivariate regressions on the micro data. *Data sources:* ITS dataset (linked administrative and pooled survey data).

Table A1: Intergenerational transmission of subject-specific skills

	Child math skill rank	Child language skill rank
	(1)	(2)
	Panel A: Math	
Math skill rank	0.260	0.234
	(0.006)	(0.006)
R-squared	0.121	0.124
Observations	41,774	41,774
	Panel B: Language	
Language skill rank	0.208	0.264
	(0.006)	(0.006)
R-squared	0.101	0.136
Observations	41,774	41,774
	Panel C: Math and language	
Math skill rank	0.209	0.125
	(0.007)	(0.007)
Language skill rank	0.089	0.193
	(0.007)	(0.007)
R-squared	0.125	0.144
Observations	41,774	41,774
	Control variables in all panels	
Grandparent education	yes	yes
Grandparent social background	yes	yes
Municipality fixed effects	yes	yes

Notes: Least squares regressions. Sample: Pooled sample of all matched parent-children observations in the three education cohorts. Dependent variables: Math skills of children in column (1); language skills of children in column (2). Children's cognitive skills are measured as the percentile rank of test score of children in full sample of children taking the test in a given year based on the administrative data. Parents' cognitive skills are measured as the percentile rank of test score of parents in full sample of parents in an education cohort. Grandparent education is measured by four categories of the highest level of education of both grandparents. Grandparent social background is measured by seven categories of occupational status of the main breadwinner in the parent household. Grandparent education, grandparent social background, and municipality fixed effects refer to time when parents took the skill test. All regressions control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, and children test year fixed effects. Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Table A2: Estimates of intergenerational skill transmission for each cohort separately

	Panel A: Math			
	Pooled	Cohort		
		1977	1982	1989
Parent skill rank	0.260	0.268	0.250	0.242
	(0.006)	(0.008)	(0.011)	(0.016)
R-squared	0.121	0.130	0.134	0.146
Observations (students)	41,774	22,417	12,930	6,427
	Panel B: Language			
Parent skill rank	0.264	0.288	0.224	0.251
	(0.006)	(0.008)	(0.010)	(0.016)
R-squared	0.136	0.149	0.141	0.164
Observations (students)	41,774	22,417	12,930	6,427
	Panel C: Math and language			
Parent comparative skill advantage	0.094	0.122	0.068	0.081
	(0.005)	(0.008)	(0.009)	(0.013)
R-squared	0.067	0.025	0.015	0.022
Observations	41,774	22,417	12,930	6,427
	Control variables in all panels			
Grandparent education	yes	yes	yes	yes
Grandparent social background	yes	yes	yes	yes
Municipality fixed effects	yes	yes	yes	yes

Notes: Least squares regressions. Sample: Pooled sample of all matched parent-children observations in the three education cohorts. Dependent variables: Math skill rank of children in Panel A; language skill rank of children in Panel B; skill rank difference between math and language in Panel C; rank is the percentile rank of test scores of linked children in full sample of children taking the test in a given year based on the administrative data. Parent skill rank is the percentile rank of test scores of linked parents in full sample of parents and nonparents in an education cohort; parent comparative skill advantage is the difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort. Grandparent education is measured by four categories of the highest level of education of both grandparents. Grandparent social background is measured by seven categories of occupational status of the main breadwinner in the parent household. Grandparent education, grandparent social background, and municipality fixed effects refer to time when parents took the skill test. All regressions control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, and children test year fixed effects. In Panel C: Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Table A3: Coefficients on control variables in the least squares model (Table 3, Col. 4)

Variables	(1)	Variables	(2)
Parent comparative skill advantage	0.094	Other	-1.771
	(0.005)		(0.606)
		No answer	0.374
			(1.068)
Parent characteristics		*Grandparent characteristics*	
Female	0.936	Age grandfather at time of parent birth: 21-25	0.682
	(0.258)		(1.176)
Migrant	-0.208	Age grandfather at time of parent birth: 26-30	0.310
	(0.444)		(1.200)
Number of siblings: 1	-0.090	Age grandfather at time of parent birth: 31-35	0.544
	(0.533)		(1.232)
Number of siblings: 2	-0.328	Age grandfather at time of parent birth: 36-40	0.204
	(0.547)		(1.289)
Number of siblings: 3 or more	0.885	Age grandfather at time of parent birth: 41 and above	0.102
	(0.566)		(1.376)
Number of siblings: missing	-1.074	Age grandfather at time of parent birth: missing	-0.112
	(0.902)		(2.207)
Grandparent education		Age grandmother at time of parent birth: 21-25	-0.851
Grandparent education: lower secondary	-0.655		(0.635)
	(0.372)	Age grandmother at time of parent birth: 26-30	-0.840
Grandparent education: upper secondary	-0.762		(0.684)
	(0.399)	Age grandmother at time of parent birth: 31-35	-1.647
Grandparent education: tertiary	-1.520		(0.764)
	(0.503)	Age grandmother at time of parent birth: 36-40	-0.589
Grandparent education: missing	-1.097		(0.891)
	(0.988)	Age grandmother at time of parent birth: 41 and above	-1.346
Grandparent social background			(1.241)
Blue-collar worker	-1.721	Age grandmother at time of parent birth: missing	9.805
	(0.535)		(6.948)
Employer with staff	-1.618		
	(0.728)		
Lower white-collar worker	-2.318		
	(0.611)		
Middle white-collar worker	-2.287		
	(0.576)		
Professionals	-2.067		
	(0.633)		
Municipality fixed effects		yes	
R-squared	0.018	Observations	41,774

Notes: Least squares regressions. Sample: All matched parent-children observations in the three education cohorts. Dependent variable: Difference between the percentile ranks of linked children's math and language test scores in full sample of children taking the test in a given year based on the administrative data. Parent comparative skill advantage is measured as the difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort. Omitted categories: Gender: male; migration background: native; number of siblings: none; grandparent education: primary; grandparent social background: employer without staff; age grandfather at time of parent birth: 20 years or lower; age grandmother at time of parent birth: 20 years or lower. Grandparent education, grandparent social background, and municipality fixed effects refer to time when parents took the skill test. All regressions control for parent survey indicators and children test year fixed effects. Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Table A4: Intergenerational transmission of comparative skill advantage (cardinal skill measures)

	(1)	(2)	(3)	(4)
Parent comparative skill advantage	0.098	0.098	0.097	0.096
	(0.005)	(0.005)	(0.005)	(0.005)
Grandparent education		yes	yes	yes
Grandparent social background			yes	yes
Municipality fixed effects				yes
R-squared	0.013	0.014	0.014	0.018
Observations	41,774	41,774	41,774	41,774

Notes: Least squares regressions. Sample: All matched parent-children observations in the three education cohorts. Dependent variable: Difference between math and language test scores of linked children; test scores are standardized with mean zero and SD one in full sample of children taking the test in each test year. Parent comparative skill advantage is measured as the difference between math and language test scores of linked parents; test scores are standardized with mean zero and SD one in full sample of parents and nonparents in each education cohort. Grandparent education is measured by four categories of the highest level of education of both grandparents. Grandparent social background is measured by seven categories of occupational status of the main breadwinner in the parent household. Grandparent education, grandparent social background, and municipality fixed effects refer to time when parents took the skill test. All regressions control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, parent survey indicators, and children test year fixed effects. Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Table A5: Effect heterogeneity

	(1)	(2)
Parent comparative skill advantage	0.062	0.097
	(0.012)	(0.009)
Grandparent education		
× Lower secondary	0.035	
	(0.015)	
× Upper secondary	0.046	
	(0.015)	
× Tertiary	0.048	
	(0.017)	
× Missing education information	0.019	
	(0.026)	
Grandparent social background		
× Independent contractor		-0.016
		(0.020)
× Employer with staff		0.031
		(0.023)
× Lower white-collar worker		-0.011
		(0.017)
× Middle white-collar worker		0.018
		(0.015)
× Professionals		-0.013
		(0.017)
× Other		-0.028
		(0.016)
× No answer		0.007
		(0.028)
Municipality fixed effects	yes	yes
R-squared	0.019	0.018
Observations	41,774	41,774

Notes: Least squares regressions. Sample: All matched parent-children observations in the three education cohorts. Dependent variable: Difference between the percentile ranks of linked children's math and language test scores in full sample of children taking the test in a given year based on the administrative data. Parent comparative skill advantage is measured as the difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort. The coarser definition of grandparent education used in this table combines primary and lower secondary education to the lower education category, while upper secondary and tertiary education are referred to as medium and tertiary education, respectively. The coarser definition of parent social status lumps together "employer without staff" and "employer with staff" in the "employer" category, and the "other" and "unknown" in the "other" category. Omitted category in column (1) is low education (at most lower secondary); omitted category in column (2) is blue collar worker. Grandparent education, grandparent social background, and municipality fixed effects refer to time when parents took the skill test. All regressions further control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, parent survey indicators, and children test year fixed effects. Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Potential Mechanisms

Our estimates of the intergenerational transmission of comparative skill advantages still leave several open questions. In particular, it would be valuable to understand why parents with different cognitive skill mixes when they finished primary education produce offspring with similar skill mixes. Linking the ITS data with administrative data on parents' future outcomes, we pursue an exploratory investigation of possible mediators of the skill transmission. Specifically, we observe the highest obtained educational degree and current income of parents, as well as household income and wealth – each of which is a plausible contributor to child skills.

We observe that parents who performed relatively better in math than in language at school advance farther in the education system, earn more, and accumulate more wealth (Table A6). However, the role of these economic factors in explaining the extent to which comparative skill advantages are transmitted from one generation to the next is very limited. Adding the parental economic variables to the baseline transmission model leaves the parent skill coefficient virtually unchanged (Table A7). This reflects the fact that the considered measures of parent economic success are only weakly, if at all, correlated with child comparative skill advantages after conditioning on parent skill advantages.[58]

Our simple analysis of mechanisms has two important caveats. First, interpreting the results in Table A7 as showing the effect of parents' comparative skill advantages net of the mediator hinges on additional conditional independence assumptions with respect to unmeasured mediators and confounders correlated with both the included mediator and the outcome. Second, a straightforward decomposition of the effect of parent skill advantages on child skill advantages into shares attributed to one or several mediators can only be achieved when imposing additional assumptions (see Heckman, Pinto, and Savelyev (2013)).[59]

If parent education, income, and wealth do not drive intergenerational skill transmission, what might? Plausible alternative mechanisms are factors that affect subject-specific informal learning in the family, such as role model effects (leading by example), passion for a subject, or

[58] In an unreported subject-specific mediation analysis, we find that the considered mediators (in particular, the highest obtained educational degree of parents) are relevant in explaining the subject-specific skill transmission from parents to their children. However, the mediators affect math and language skills similarly, so they cannot meaningfully explain the transmission of comparative skill advantages.

[59] More advanced decomposition methods could be contemplated (e.g., Heckman, Pinto, and Savelyev (2013), Heckman and Pinto (2015)). However, because the observed potential mediators explain very little of the intergenerational transmission of comparative skill advantages, we stop at the basic analysis in Table A7.

pedagogical skills. It seems likely that parents with particularly high skills in one subject will also be more willing and more able to transmit these skills to their children. Unfortunately, our data do not allow to test this presumption directly.

Table A6: Potential mediators of intergenerational transmission of comparative skill advantages

	Parent higher education (1)	Parent income (2)	Household income (3)	Household wealth (4)
Parent comparative skill advantage	0.0003	0.0199	0.0156	0.0292
	(0.0001)	(0.006)	(0.005)	(0.007)
Grandparent education	yes	yes	yes	yes
Grandparent social background	yes	yes	yes	yes
Municipality fixed effects	yes	yes	yes	yes
R-squared	0.161	0.426	0.103	0.184
Observations	41,774	38,957	41,134	36,973

Notes: Least squares regressions. Sample: Pooled sample of all matched parent-children observations in the three education cohorts. Dependent variables: Binary variable taking a value of 1 if parents obtained a degree in higher vocational education or university education; 0 otherwise (column 1). Parent income including income from labor, income from owned companies, unemployment and social security, measured as the percentile of the parent in the Dutch personal income distribution in the child's test-taking year (column 2). Sum of the personal incomes of all household members measured as the percentile of the household in the Dutch household distribution in terms of yearly spendable income in the child's test-taking year (column 3). Household wealth, measured as the percentile of the household in the Dutch household distribution in terms of the household's total wealth, determined by assets minus debts in the child's test-taking year (column 4). Parent comparative skill advantage is measured as the difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort. Grandparent education is measured by four categories of the highest level of education of both grandparents. Grandparent social background is measured by seven categories of occupational status of the main breadwinner in the parent household. Grandparent education, grandparent social background, and municipality fixed effects refer to time when parents took the skill test. All regressions further control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, parent survey indicators, and children test year fixed effects. Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Table A7: Analysis of potential mechanisms

	(1)	(2)	(3)	(4)	(5)
Parent comparative skill advantage	0.094	0.094	0.094	0.094	0.094
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Parent education					
Medium		-0.168			
		(0.327)			
High		-1.182			
		(0.377)			
Missing		0.616			
		(0.528)			
Parent income			0.016		
			(0.054)		
Household income				0.137	
				(0.058)	
Household wealth					0.232
					(0.053)
Grandparent education	yes	yes	yes	yes	yes
Grandparent social background	yes	yes	yes	yes	yes
Municipality fixed effects	yes	yes	yes	yes	yes
R-squared	0.018	0.019	0.018	0.018	0.019
Observations	41,774	41,774	41,774	41,774	41,774

Notes: Least squares regressions. Sample: All matched parent-children observations in the three education cohorts. Dependent variable: Difference between the percentile ranks of linked children's math and language test scores in full sample of children taking the test in a given year based on the administrative data. Parent comparative skill advantage is measured as the difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort. Parent education is measured as the highest educational degree obtained by the observed parent (omitted category: low education); low education: at most lower secondary; medium education: higher secondary and upper secondary vocational education; high education: tertiary education, consisting of higher vocational education and university. Household income is based on the percentile of the household in the Dutch household distribution in terms of yearly spendable income in the child's test-taking year. Parent personal income is based on the percentile of the parent in the Dutch personal income distribution (including income from labor, income from owned companies, unemployment and social security) in the child's test-taking year. Household wealth is based on the percentile of the household in the Dutch household distribution in terms of the household's total wealth, determined by assets minus debts in the child's test-taking year. Missing values for parent education (3.5 percent), parent income (6.7 percent), household income (1.5 percent), and household wealth (11.5 percent) are imputed (imputation dummies added to the regression models). Grandparent education is measured by four categories of the highest level of education of both grandparents. Grandparent social background is measured by seven categories of occupational status of the main breadwinner in the parent household. Grandparent education, grandparent social background, and municipality fixed effects refer to time when parents took the skill test. All regressions further control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, parent survey indicators, and children test year fixed effects. Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

A.3 Appendix for Section 6.2: Instrumental Variable Approach

Identification of classrooms

Sampling was done at the classroom level in all three parent cohorts. However, for the 1977 cohort school and class identifiers were removed by Statistics Netherlands and could not be retrieved. In the 1989 cohort, classroom identifiers are directly available. For the 1982 cohort, which is sampled in the last year of primary school, a classroom identifier was collected but the identifier is no longer available. In this cohort, however, we can approximate students' classmates by combining available information at the school and municipality level that is consistently available for all students. At the school level, we have religious denomination and number of grade 6 classrooms. Together with the municipality code of students' place of residence, this provides an indication of which students were potentially classmates. For example, if 20 students resided in the same municipality and attended the same protestant primary school with one grade 6 classroom, they can reasonably be assumed to have been classmates. However, for larger municipalities and more common denominations, this combined information is not sufficient to uniquely identify classrooms. Hence, we put a lower- and an upper-bound on class size to include only those students in the sample for whom we can be reasonably certain that they were indeed classmates.

In the main IV analyses for the 1982 cohort, minimum class size has been restricted to 15 students, and maximum class size to 30 students. We used these values because a class size of 15 students corresponds to the 10th percentile and a class size of 29 students to the 90th percentile of the class-size distribution in the 1989 cohort.[60] The minimum class size restriction is introduced because classmates are partly identified based on municipality code of residence, not on municipality code of school attendance. An unreasonably small number of students from a certain municipality likely implies that they attend a school in a different municipality. While they still may attend the same school as their peers from the same municipality, they will also share a classroom with other students whom we are not able to identify. The reason for a maximum class size is that in large municipalities, the combination of number of grade 6

[60] For comparison, the first percentile of the class-size distribution in the 1989 cohort corresponds to a class with 9 students, while the 99th percentile corresponds to class with 32 students.

classrooms and denomination does not uniquely identify schools.[61] There are likely to be more schools with the same profile from the same municipality that participate in the survey, and assigning all these students to the same 'classroom' would not be appropriate.

Our class size restrictions could introduce selectivity in the type of schools and students for whom we can implement our IV approach in the 1982 cohort. This might affect our estimated average effect if effect heterogeneity is large. We address this concern in two ways. First, we extend our class size restrictions to include a range of class sizes from 10 to 35 in the 1982 cohort. The IV estimate on parent comparative skill advantage in the full IV sample drops from 0.110 in the baseline to 0.071 when we use the extended class-size range for the 1982 cohort but remains significant at the 10 percent level. The decrease in coefficient magnitude is not surprising when considering that the broader range of included class sizes introduces some measurement error. Second, we impose a class size restriction of 15 to 30 students also in the sample of the 1989 cohort, for which we have perfectly reliable class identifiers. We find that this restriction has virtually no effect on our IV estimate.

Furthermore, to benchmark the quality of our classroom assignment procedure in the 1982 cohort, we apply the same procedure to the data of the 1989 cohort. The correlation coefficient between the comparative skill advantages of the actual classroom and the predicted classroom (based on our procedure) is 0.72. The correlation coefficient between the class ranks in math (language) of the actual and predicted classroom are 0.86 (0.88). The corresponding IV estimates of the intergenerational transmission of comparative skill advantages based on the actual classroom and the predicted classroom are not statistically significantly different from each other.

Robustness to other definitions of the comparative skill advantage of classroom peers

The core idea behind the IV approach is that differences in parent classroom environments affect parents' comparative skill advantage, but do not have an independent impact on children's skill advantage. In operationalizing this idea, we have some leeway of how to construct the instrument. In our baseline specification, we use the difference between the percentile ranks in math and language tests of parents' classroom peers. That is, we calculate for every parent the

[61] Note that we identify 'schoolmates' in cases where we can uniquely identify a school, but know that the number of surveyed classrooms in this school is larger than one. However, the vast majority of schools have only one classroom.

average performance of classmates, while excluding the parent's test score in the calculation of the average (i.e., leave-out mean). This is a straightforward and intuitive way to measure the quality of the classroom environment, but there are also other plausible approaches.

In Table A8 we show that the IV results are robust to various other ways of constructing the instrument. All estimates of parents' comparative skill advantage in columns (1) to (6) are not statistically significantly different from each other. In column (1), we report our baseline estimate. In column (2), we construct differences in performance ranks between math and language of the entire classroom (i.e., including the parents). However, with this specification of the instrument, the strong first-stage relationship is partly mechanical because the class rank instrument also includes parent cognitive skills. Column (3) presents a non-parametrical version of the leave-out mean class rank instrument, which relaxes the functional form assumption of linearity. This instrument simply indicates whether the leave-out mean class rank is higher in math or language. In column (4), we construct the dummy instrument using absolute (i.e., level) differences in leave-out means instead of differences in ranks. Column (5) directly uses the absolute differences in leave-out means as an instrument, which again implies making a linearity assumption. Finally, column (6) takes into account that children in the 1989 cohort were tested in their first year in secondary school, that is, after tracking. Thus, we construct our baseline class rank instrument for the 1989 cohort separately by track, which addresses the potential concern that differences in the rank of math and language skills may be track-specific.

Table A8: Different definitions of classroom's comparative skill advantage

	Rank Class Leave-Out (Main)	Rank Class	Rank Class Dummy Leave-Out	Level Class Dummy Leave-Out	Level Class Absolute Leave-Out	Rank Class Track-Specific
	(1)	(2)	(3)	(4)	(5)	(6)
Parent comparative skill advantage	0.110	0.096	0.094	0.099	0.082	0.122
	(0.047)	(0.029)	(0.057)	(0.051)	(0.044)	(0.054)
Further controls	yes	yes	yes	yes	yes	yes
F-statistic excluded instrument	212.58	612.56	93.24	122.53	217.96	144.55
R-squared	0.016	0.016	0.016	0.016	0.016	0.015
Observations	12,268	12,268	12,268	12,268	12,268	12,268

Notes: Two-stage least squares regressions. Sample: All matched parent-children observations in the education cohorts of 1982 and 1989. Dependent variable: Difference between the percentile ranks of linked children's math and language test scores in full sample of children taking the test in a given year based on the administrative data. Parent comparative skill advantage is measured as the difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort. Instruments: Column (1): Difference between the percentile ranks of classroom peers in math and language within a parent's education cohort; column (2): difference between the percentile ranks of full classroom in math and language within a parent's education cohort; column (3): Binary indicator for higher ranked classroom peers (math vs. language) within the parent's education cohort; column (4): Binary indicator for better performing classroom peers (math vs. language); column (5): Test scores in math and language of classroom peers; column (6): Like column (1), but rank of math and language classrooms in the 1989 cohort (where children were sampled in the first year of secondary school) calculated by track, distinguishing between 11 different tracks. Further controls include grandparent education and grandparent social background (all referring to the time when parents took the skill test). All regressions additionally control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, parent survey indicators, and children test year fixed effects. Standard errors clustered at the classroom level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset (1982 and 1989 cohort).

Subsample analysis: Addressing potential violations of the exclusion restriction

In this section, we address various concerns about potential violations of the exclusion restriction of our IV approach by estimating the IV model based on child-parent matches in subsamples that are arguably less prone to such concerns.

Addressing correlated intergenerational peer composition

We start by addressing the concern that peer quality may be correlated across the parent and child generation because of endogenous sorting of children within schools. To this end, we replicate the analysis from Table 6 in one-classroom schools, controlling for skill differences between math and language of children's *classroom* peers (Table A9). While skill differences of children's classroom peers are strongly related to the skill differences of children, they hardly affect the estimated strength of the intergenerational transmission of comparative skill advantages. However, the transmission is less precisely estimated due to the reduction in sample size.

In Table A10, we account in various ways for potential effects of parents' classroom peers on the formation of children's skills that are not running through parent skills. In column (1), we exclude parents who have been classmates in early formal education and whose children are schoolmates today. For children who attend the same school as children of their parents' former classmates, parents' peers could directly affect children's skill development. Reassuringly, the IV estimate in this sample is very similar to our baseline IV estimate in column (5) of Table 5.[62]

In column (2) of Table A10, we further restrict the sample to children whose school is located in a municipality different from the parents' municipality of school attendance. In the further specifications of Table A10, we restrict the sample even further to child-parent matches where children attend a school that is at least 50 (column 3) or 100 (column 4) kilometers away from their parent's former school, or where children attend a school in a different province than the parent's school. Throughout all subsamples, the IV estimates remain sizeable, but fail to capture statistical significance in column (2) (p=0.214) and column (5) (p=0.282).

[62] A related concern might be that in our full sample we have 365 children for which we observe both parents in our data. In most of these cases, both parents attended the same school or even class. We can address this concern by excluding these 365 children from our sample and estimate the IV model based on a sample of children for which only one parent got sampled in any class of the survey. Our IV results are not affected by this sample restriction.

Table A9: Controlling for children's school quality (one-classroom schools)

	OLS model	First stage IV	Reduced form	Second stage IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Parent comparative skill advantage	0.074			0.086	0.099	0.097
	(0.013)			(0.071)	(0.075)	(0.075)
Classroom comparative skill advantage		0.263	0.023			
		(0.027)	(0.018)			
Children's school quality	0.118	0.013	0.119	0.118	0.113	
	(0.015)	(0.018)	(0.015)	(0.015)	(0.015)	
Children's school quality						10.427
(absolute)						(1.214)
Further controls					yes	yes
F-statistic excluded instrument				97.76	86.27	86.36
R-squared	0.02	0.04	0.01	0.02	0.03	0.03
Observations	5,620	5,620	5,620	5,620	5,620	5,620

Notes: Table replicates Table 6 for children whom we observe in a school with at most 30 grade-six students in a given year; this is our proxy for one-classroom schools, as classroom identifiers are not available in the administrative CITO data. Least squares and two-stage least squares regressions. Sample: All matched parent-children observations in the education cohorts of 1982 and 1989 in school-year combinations with 30 or less total observations; children with missing school information are excluded. Dependent variables: Difference between the percentile ranks of linked children's math and language test scores in full sample of children taking the test in a given year based on the administrative data in columns (1), (3), (4), (5), and (6); difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort in column (2). Column (1) replicates baseline least squares model (see column (1) of Table 3) in the IV sample. Classroom comparative skill advantage is measured as the difference between the percentile ranks in math and language of parents' classroom peers within a parent's education cohort. Children's school quality (ranks) is measured as the difference between the percentile ranks in math and language of children's school peers in the national test score distribution in a given year. Children's school quality (absolute) is measured as the test-year-standardized test score difference between math and language of children's school peers. Further controls include grandparent education and grandparent social background based on the occupation type of the main breadwinner in the parent household (all referring to the time when parents took the skill test). All regressions additionally control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, parent survey indicators, and children test year fixed effects. Standard errors clustered at the classroom level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset (1982 and 1989 cohort).

Table A10: Regional movers

	Without children of parent's classmates	Child & parent school not in same municipality	Child & parent school not in same municipality (distance >50 km)	Child & parent school not in same municipality (distance >100 km)	Child & parent school not in same province
	(1)	(2)	(3)	(4)	(5)
Parent comparative skill advantage	0.092	0.080	0.209	0.255	0.119
	(0.050)	(0.065)	(0.110)	(0.147)	(0.111)
Further controls	yes	yes	yes	yes	yes
F-statistic excluded instrument	176.63	134.69	25.91	20.65	34.71
R-squared	0.017	0.017	0.042	0.056	0.030
Observations	10,970	6,414	1,360	585	2,311

Notes: Two-stage least squares regressions in the sample of matched parent-children observations in the education cohorts of 1982 and 1989. Samples: Column (1): Excluding children who attend the same school and whose parents have been classmates in the education cohorts of 1982 and 1989; column (2): as in column (1), while keeping only children whose school is located in a different municipality than the parent's school in the education cohorts of 1982 and 1989; column (3) (column 4): as in column (2), while keeping only children whose school is located in a municipality that is more than 50 km (100 km) away from the municipality of the parent's school in the education cohorts of 1982 and 1989 (using the municipality centroid); column (5): as in column (1), while keeping only children whose school is located in a different province than the parent's school in the education cohorts of 1982 and 1989. Results in columns (2) and (5) contain only children with a valid municipality or province identifier (92.06 percent of the total IV sample). Results in columns (3) and (4) contain only children and parents with available municipality longitude and latitude coordinates (88.52 percent of the total IV sample). Dependent variable: Difference between the percentile ranks of linked children's math and language test scores in full sample of children taking the test in a given year based on the administrative data. Parent comparative skill advantage is measured as the difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort. The instrument is classroom comparative skill advantage, measured as the difference between the percentile ranks in math and language of parents' classroom peers within a parent's education cohort. Further controls include grandparent education and grandparent social background (all referring to the time when parents took the skill test). All regressions additionally control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, parent survey indicators, and children test year fixed effects. Standard errors clustered at the classroom level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset (1982 and 1989 cohort).

Addressing potential between- or within-school sorting of parents

Our estimation already accounts for potential sorting of parents to schools or teachers based on factors that similarly affect the formation of math and language skills. However, the estimates might be biased if sorting is based on factors that affect subject-specific skill production over generations within families. Our IV estimation results could be biased upward if, for instance, parents belonging to mathematically gifted families systematically attended schools with more knowledgeable math teachers, or if principals tended to assign parents from mathematically gifted families to teachers with high math knowledge.

Table A11 suggests that subject-specific sorting when parents attended school is unlikely to drive our results. We first address between-school sorting by restricting the sample to students living in rural areas (column 2). In this case, students likely have little choice between different schools, because there is usually only one relevant school in rural areas. The estimated IV effect for students in rural areas is very similar to our baseline effect, reported in column (1). To address the concern of within-school sorting, we focus on a subsample of schools with only one classroom, implying that principals cannot assign students to teachers based on their subject-specific ability or preferences. As shown in column (3), the IV estimate on parent comparative skill advantage in this subsample even tends to be somewhat larger than the baseline estimate. Column (4) shows that our results hold even when we restrict the sample to one-classroom schools in rural areas, simultaneously addressing across-school and within-school sorting. This is remarkable because this restricted sample is only one-third the size of the full sample.

In columns (5) and (6) of Table A11, we show the IV results separately for students in the 1982 cohort, who were tested at the end of primary school, and for students in the 1989 cohort, where testing took place at the beginning of secondary school. While still positive and sizable, the IV estimate in the 1989 cohort is not statistically significant. One plausible explanation is that parents in this cohort took the test in the first year of secondary school (i.e., after tracking), so they had considerable less exposure to peers or teachers than parents in the 1982 cohort. This is also reflected in the weaker first stage in the 1989 cohort.

Table A11: School sorting in the parent generation

	Main	Rural schools	One-classroom schools	Rural & one-classroom schools	Cohort 1982	Cohort 1989
	(1)	(2)	(3)	(4)	(5)	(6)
Parent comparative skill advantage	0.110	0.121	0.157	0.142	0.140	0.052
	(0.047)	(0.054)	(0.063)	(0.069)	(0.060)	(0.078)
Further controls	yes	yes	yes	yes	yes	yes
F-statistic excluded instrument	212.58	139.52	158.86	116.83	163.83	45.56
R-squared	0.016	0.020	0.010	0.021	0.015	0.019
Observations	12,268	5,525	6,648	3,670	5,841	6,427

Notes: Two-stage least squares regressions. Samples: Column (1): All matched parent-children observations in the education cohorts of 1982 and 1989; column (2): Matched parent-children observations form rural schools in the education cohorts of 1982 and 1989; column (3): Matched parent-children observations from schools with exactly one classroom in the education cohorts of 1982 and 1989; column (4): Matched parent-children observations from rural schools with exactly one classroom in the education cohorts of 1982 and 1989; column (5): All matched parent-children observations in the education cohort of 1982; column (6): All matched parent-children observations in the education cohort of 1989. Dependent variable: Difference between the percentile ranks of linked children's math and language test scores in full sample of children taking the test in a given year based on the administrative data. Parent comparative skill advantage is measured as the difference between the percentile ranks of linked parents' math and language test scores in full sample of parents and nonparents in an education cohort. The instrument is classroom comparative skill advantage, measured as the difference between the percentile ranks in math and language of parents' classroom peers within a parent's education cohort. Further controls include grandparent education and grandparent social background (all referring to the time when parents took the skill test). All regressions further control for parent gender, parent migration background, number of siblings of parents, age of grandparents at the time of parent birth, parent survey indicators, and children test year fixed effects. Standard errors clustered at the classroom level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset (1982 and 1989 cohort).

A.4 Appendix for Section 7: Parents' Comparative Skill Advantage on Children's STEM Choices

Table A12: Parents' comparative skill advantage and STEM choices of parents and children – Narrow STEM definition

	Parent STEM field of study	Child (all) STEM profile	Child (all) STEM field of study	Child (survey) STEM profile	Child (survey) STEM field of study
	(1)	(2)	(3)	(4)	(5)
	Panel A: Full sample				
Parent comparative skill advantage (/10)	0.0064			0.0064	0.0046
	(0.0009)			(0.0012)	(0.0011)
Child comparative skill advantage (/10)		0.0166	0.0115		
		(0.0002)	(0.0002)		
Further controls	yes	yes	yes	yes	yes
Baseline outcome	0.183	0.232	0.220	0.250	0.221
R-squared	0.129	0.101	0.128	0.014	0.010
Observations	28,264	1,161,303	1,161,303	28,665	28,665
	Panel B: Male sample				
Parent comparative skill advantage (/10)	0.0097			0.0095	0.0079
	(0.0016)			(0.0020)	(0.0019)
Child comparative skill advantage (/10)		0.0168	0.0159		
		(0.0003)	(0.0003)		
Further controls	yes	yes	yes	yes	yes
Baseline outcome	0.311	0.349	0.364	0.379	0.364
R-squared	0.039	0.036	0.022	0.021	0.023
Observations	14,236	576,031	576,031	14,358	14,358
	Panel C: Female sample				
Parent comparative skill advantage (/10)	0.0038			0.0032	0.0014
	(0.0008)			(0.0013)	(0.0010)
Child comparative skill advantage (/10)		0.0162	0.0069		
		(0.0002)	(0.0002)		
Further controls	yes	yes	yes	yes	yes
Baseline outcome	0.053	0.117	0.078	0.120	0.078
R-squared	0.012	0.042	0.010	0.016	0.018
Observations	14,028	585,272	585,272	14,307	14,307

Notes: Least squares regressions. Sample: Pooled sample of all individuals (parents and nonparents) in the three survey cohorts in column (1); pooled sample of all students that took the CITO test at the end of primary education between 2006-2019 for who we observe both their course- and study profile choice in columns (2) and (3). Children of individuals in the first survey cohort (1977) for whom we observe both their course- and study profile choice in columns (4) and (5). Dependent variables: Binary variable taking a value of 1 if surveyed individuals' highest obtained degree 30 years after participating in the survey is in a STEM (Science, Technology, Engineering, and Mathematics) field (column 1); Binary variable indicating the choice of a STEM course profile at secondary school in columns (2) and (4); binary variable indicating the choice of a STEM field of study after secondary school in columns (3) and (5). Students are designated as following a STEM-course profile if they take the Technical course profile (low academic track) or the Nature & Technical course profile (middle/high academic track). STEM study choice is determined based on the 1-digit ISCED97 fields of education classification (UNESCO, 2003), where study programs categorized as Science, Mathematics and Computing, Engineering, Manufacturing and Construction, were classified as a STEM choice of study. Baseline values are calculated based on observations with non-missing information on STEM choices. Regressions in column (1) control for individual's gender, migration background, number of siblings, age of parents at the time of individual's birth, survey indicators, education and social background of grandparents, as well as municipality fixed effects. Regressions in columns (4) and (5) additionally include child test year fixed effects. Regressions in columns (2) and (3) control for student gender, migration background, student test year and school fixed effects. Standard errors (in parentheses) are clustered at the individual level in column (1), at the school level in columns (2) and (3), and at the parent level in columns (4) and (5). *Data sources*: Administrative data; pooled ITS survey database.