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

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Abstract

The standard economic model of occupational choice, following Roy (1951), emphasizes comparative advantages but seldom considers their source. We show that STEM field choices are strongly influenced by comparative advantage of math versus language skill that is passed down through families. Using unique Dutch survey and registry data, we identify the intergenerational transmission of comparative advantage from within-family between-subject variation in skills. IV estimation utilizing subject-specific variation in parental school factors supports a causal interpretation and shows the malleability of comparative advantages. The strong impact of comparative skill advantage on STEM field choices is evident both within and across generations.

Keywords: intergenerational mobility, parent-child skill transmission, comparative advantage, STEM
JEL classification: I24, I26, J12, J24, J62

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1. Introduction

Policy discussions related to expanding STEM education commonly reduce to ensuring that schools produce sufficient math skills. Such policy focus mirrors prior economic analysis of the role of skills in determining education investments and subsequent outcomes that, with few exceptions, have pursued a single-factor model captured by one dimension of ability. This formulation, however, generally ignores the fundamental role of *comparative* advantage in occupational choice, as initially formalized by Roy (1951) and subsequently applied to explaining, among others, college attendance decisions (Willis and Rosen (1979)), sectoral wage differences (Heckman and Sedlacek (1985)), and field of study choices (Kirkeboen, Leuven, and Mogstad (2016)). Nonetheless, this research assumes skill differences to be exogenously determined, a limitation when considering any policy application. In this paper, we address the key challenges of identifying the sources of individual differences in comparative advantages, of understanding the extent to which these advantages can be altered, and of investigating their role in STEM field choices.

We directly address both the measurement and sources of comparative advantages in occupational choice based on cognitive skill differences of individuals. We are ultimately interested in why some people prepare for STEM fields while others go in different directions such as law, business, or service occupations, but we are also interested in the source of these differences and specifically the role of families.

Our core analysis explores the sources of comparative advantages by studying how differences in math and language skills are transmitted from parents to children. An important component of this is understanding the extent to which this intergenerational transmission of comparative skills is predetermined or whether it can be influenced by the education system.

We build a unique data set providing comparable measures for different domains of cognitive skills of both parents and their children. Our data come from linking extensive Dutch survey data on parent skills in math and language to register data on their children's skills in the same domains on similar tests at a similar age. The parental survey data cover three cohorts of parents sampled as students in the first year of secondary education (1977 and 1989) or the last year of primary education (1982). The surveys are nationally representative covering 8–15% of all students entering Dutch secondary education. The combined dataset includes more than 25,000 parents and 40,000 of their children.

Comparative skill advantage as measured by early test scores plays a significant role in STEM choices. In both the parent and child generation, an increase in math relative to language skills increases the likelihood of obtaining a STEM degree in vocational education or at university. Importantly, the influence of comparative advantage in STEM choices is consistent across the math skill distribution, underscoring the critical role of relative, not absolute, math skills and supporting our focus on the intergenerational transmission of comparative skill advantages.[1]

We embed our analysis in a conceptual model that combines a Galton-inspired intergenerational transmission model with an educational production function considering how various inputs affect the cognitive skills of children. Empirically, we exploit within-family between-subject variation in cognitive skills, asking how differences in parents' skills between math and language relate to differences in math and language skills of their children. In this analysis, all observed and unobserved influences of family, school, and neighborhoods that do not differentially affect the two skill domains are eliminated.

We find that parents with a comparative advantage in math are significantly more likely to have children with a similar math skill advantage. In terms of magnitude, a difference of 10 percentile ranks between skills in math and language in the parent generation translates into a one-rank difference in the child generation. The strength of transmission remains virtually unchanged when we allow for various grandparent characteristics (i.e., education and occupational status) and for detailed regional factors to influence math and language skills differently. Moreover, our results are not affected by the variation in average skills across individuals as our measure of comparative skill

[1] We also show more generally that early-life test scores represent skills that have long-term economic value. Consistent with prior work (e.g., Chetty, Friedman, and Rockoff (2014), Aucejo and James (2021)), skills in math and language measured early in the education system are strongly correlated to hourly wages, income, and wealth three decades later.

advantage is orthogonal to average math and language skills by construction.

We investigate both the source of comparative skill advantage and its malleability with a novel instrumental variable (IV) estimation. We exploit differences between math and language skills of the parents' classroom peers to isolate variation in parents' comparative skill advantage developed outside the family. These differences in comparative skill advantages of peers reflect differences in the subject-specific quality of the early formal education environment of parents. Our IV estimates indicate clearly that nonfamily inputs in the production of skills affect comparative skill advantages that then carry over to future generations. This would not be the case if the observed skill transmission patterns just reflected innate differences in talent (e.g., a "math gene") or dynastic predispositions for specific subjects (e.g., arising through occupational legacies).[2] Overall, our IV results show that any policy that shifts focus from one skill domain to another not only affects the comparative skill advantage of the current students but has lasting impacts on subsequent generations. Robustness analyses and falsification checks show that our IV strategy suffers neither from non-random sorting to schools/classrooms nor from the reflection problem.

Given our findings on the sources of comparative skill advantages, we return to how parents influence the long-run path of children. In particular, although academic and policy attention has focused on increasing the number of individuals entering STEM fields of study and occupations (e.g., UNESCO (2017)), the role of families in influencing STEM choices has received little attention.[3] From the registry data for children, we observe patterns of course taking in secondary schooling and of choice of field of study in

[2] In addition to the substantive interpretation of the IV estimates, they provide a correction for any measurement error in the comparative skill advantages. Measurement error concerns are further addressed by a series of alternative corrections of such error.

[3] See, for example, the comprehensive review by Altonji, Arcidiacono, and Maurel (2016)). An exception is Altmejd (2024), who considers the intergenerational transmission of field of study in Sweden. While his design allows identification of how parental choices of fields of study lead to those of children, it does not consider the underlying sources of parental or child choices beyond the familial consistency.

post-secondary or tertiary education.[4] We show that children of parents with relatively higher math than language skills are more likely to choose STEM fields both at school and after school. Put differently, parents with comparative advantage in math (language) "produce" children who opt for STEM (non-STEM) fields, just as would be suggested by a simple Roy model of occupational choice. Parents influence the comparative skill advantages of both boy and girl offspring with no gender bias, leading to similar course choice patterns in secondary school. But ultimately comparative skill advantages have less influence on girls' subsequent choices of STEM field of study than on boys' choices, suggesting that gender-specific barriers during and after secondary education contribute to the consistently lower participation of girls in STEM fields.

Our results contribute to five strands of prior literature. First, we add evidence on the sources of comparative advantage to the well-established theoretical literature on the importance of comparative advantage in the labor market (e.g., Roy (1951), Lazear (2009), Acemoglu and Autor (2011), Eisenhauer, Heckman, and Vytlacil (2015)).[5]

Second, we broaden the perspective of the large literature on the intergenerational transmission of human capital by providing the first evidence on how comparative advantages in cognitive skills are transmitted from parents to children. This intergenerational literature has made important advances in understanding overall influences of families (e.g., Black and Devereux (2011), Adermon, Lindahl, and Palme (2021)) but has stopped short of addressing the important role of multiple cognitive skill dimensions.[6] Moreover, our IV results also speak to the nature-nurture debate by showing

[4] For recent analysis of intergenerational correlations in course taking, see Dahl, Rooth, and Stenberg (2024).

[5] The idea of comparative advantage has also been deeply embedded in a range of studies of other individual choice behavior, such as educational investment decisions (Willis and Rosen (1979)), immigration decisions (Borjas (1987)), the division of labor within households (Becker (1981)), and social interactions (Cicala, Fryer, and Spenkuch (2018)).

[6] A few studies have previously explored the intergenerational transmission of skills. Noteworthy among them are the works of Anger and Heineck (2010) and Dohmen, Falk, Huffman, and Sunde (2012), which leverage the German Socio-Economic Panel Study (SOEP) to analyze the transmission of cognitive skills and attitudes, respectively, across

that early comparative skill advantages do not arise just from genetic configurations but are shaped by pre-birth factors outside the family.[7]

Third, we directly insert the idea of differential cognitive skills into the growing literature on labor market returns to skills. Several recent studies suggest substantial wage returns to tested numeracy and literacy skills (e.g., Hanushek, Schwerdt, Wiederhold, and Woessmann (2015, 2017)), but they typically treat alternative tests as separate measures of a common cognitive factor.[8] Other research emphasizes the economic importance of specific kinds of skills, such as social skills (e.g., Deming (2017)), digital skills (e.g., Falck, Heimisch-Roecker, and Wiederhold (2021)), or technical skills (Barrera-Osorio, Kugler, and Silliman (2023)).[9] However, this literature either considers these skills in

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generations. Attanasio, de Paula, and Toppeta (2024) and Grönqvist, Öckert, and Vlachos (2017) examine the intergenerational correlation of both cognitive and non-cognitive skills. Attanasio et al. use data from the British Cohort Study, where parents at age 34 were linked to their children (of varying ages). Grönqvist et al. draw on military enlistment records of 18-year-old men.

Our analysis is unique in considering the transmission of multiple domains of cognitive skills, math and language, with clear implications for educational and occupational choices. The fact that these skills are elicited in a comparable fashion across generations – i.e., at the same age and using a similar test – and are linked to registry data further adds to the literature investigating the transmission of multiple skills.

[7] A variety of prior papers consider identification under varying assumptions about the effects of various pre-birth components and environmental components (e.g., Björklund, Lindahl, and Plug (2006), Sacerdote (2011a), Lundborg, Plug, and Rasmussen (forthcoming)) and about the direct influence of genetics (Houmark, Ronda, and Rosholm (2020)).

[8] If multiple test measures are available, studies mostly choose one to emphasize (e.g., Murnane, Willett, Duhaldeborde, and Tyler (2000)) or average the scores to deal with potential measurement errors (e.g., Lazear (2003)). Interestingly, however, when information on multiple test domains (e.g., math and language) is used in the labor market analysis, they are independently significant in determining earnings even though little attention has been drawn to this fact (Hanushek, Schwerdt, Wiederhold, and Woessmann (2015)).

[9] See Deming and Silliman (2024) and Woessmann (2024) for recent overviews. A complete description of individuals' early-career human capital is provided by Langer and Wiederhold (2023), who consider all skills developed through the German apprenticeship system. Aggregating more than 13,000 different skills to six broad skill categories, they

isolation or considers a horserace in returns between skill domains without recognizing the role of comparative advantage.

Fourth, by adding findings about the structure of skill production, we inform the continuing debates on STEM education. We show that comparative skill advantage significantly affects STEM field preparation and choices, both within a generation and across generations. This implies that changes in relative skills of today's generation, whether related to policy or otherwise, have ramifications for future generations.

Fifth, we contribute to the methodological discussion about measurement of cognitive skills. We focus throughout on the ordinal properties of the math and language assessments by analyzing child and parent skills as percentile ranks in the overall skill distributions. This addresses concerns about assuming cardinal properties for standard assessments as found in most economic analyses of test scores (Bond and Lang (2013)). The results are nevertheless robust to the more conventional analysis of scale scores.

2. Institutional Background and Data

2.1 The Dutch Education System

The Dutch education system is an early stratifying system (Bol and van de Werfhorst (2013)), where students are allocated to different tracks (low, middle, or high) after primary education (grade 6, at age 12). This allocation is largely based on the performance of students on the CITO test taken at the end of primary education.[10]

The CITO (Central Institute for Test Development) test is a national high-stakes test measuring school performance in math and language (along with other subjects).[11] This

show that cognitive, social, and digital skills have higher returns than manual or administrative skills.

[10] The other component that determines track allocation is the primary school teacher's advice, which is partly based on the objective results of the CITO test, and partly on the teacher's subjective expectations of students' success in secondary education.

[11] Before the 2014/15 school year, participation in the national test was not mandatory. However, around 85% of the schools in primary education have participated in the CITO test since its introduction. From 2014/2015 onwards, it is compulsory for students in grade 6 to take a final test. The government makes the CITO test available to all schools. Even though schools can also choose another final test approved by the

test, first employed in 1970, was introduced to ensure an objective, merit-based assignment to different tracks in subsequent schooling. The testing is done over a three-day period in spring of the final year of primary schooling. The test involves multiple choice items and is centrally scored.

After having been in secondary school for two years (for students attending the low track) or three years (for students attending the middle or high track), students decide on a course profile that will determine the type of courses they can take in upper-secondary or tertiary education.[12] After finishing secondary school, students can choose, depending on their track in secondary education, to enter upper secondary vocational education, tertiary vocational education, or university. They can also directly enter the labor force without additional schooling.

2.2 The Intergenerational Transmission of Skills (ITS) Database

For this paper, we developed the Intergenerational Transmission of Skills (ITS) database. This database, which provides CITO test scores for parents and their children, is the foundation of an extensive research program on the intergenerational transmission of cognitive skills (Jacobs, Vermeulen, and van der Velden (2021)).[13]

Ministry of Education, most schools participate in the CITO test (Jacobs, van der Velden, and van Vugt (forthcoming)).

[12] In the low track (called in Dutch 'VMBO'), students can choose between four profiles: Technical, Agriculture, Economics, and Health & Welfare, or a combination thereof. In the middle and high tracks (called in Dutch 'HAVO' and 'VWO', respectively), students can choose between Nature & Technical, Nature & Health, Economics & Society, Culture & Society, or a combination thereof.

[13] For more information on this research program and details of the construction of this database, see https://www.roa.nl/research/research-projects/intergenerational-transmission-skills-its-research-project. The inaugural papers in this project were Jacobs and van der Velden (2021) and our initial investigation of comparative cognitive skills, Hanushek et al. (2021). Jacobs and van der Velden (2021) estimate structural equation models to investigate the relative contribution of three mechanisms that underlie the intergenerational transmission of education from parents to children: human capital, cultural capital, and financial capital. Our previous analysis considered comparative cognitive skills in a different context and did not see the implications of comparative skills for testing the Roy model and for addressing the STEM policy debates. We incorporated the main insights of Hanushek et al. (2021), leading to the comprehensive investigation of comparative skill advantage found in this paper.

The ITS dataset combines extensive survey data gathered for three cohorts of students in the 1970's and 1980's with more recent register data on their children available at Statistics Netherlands. The survey data contain cognitive skill measures of the parent generation along with other descriptive information about the families. The register data contain cognitive skill measures of the children's generation as well as other information on their secondary schooling. Two cohorts of parents were sampled in the first year of secondary education (1977 and 1989), and one cohort was sampled in the last year of primary education (1982).[14] Each of these longitudinal surveys is a nationally representative panel of students: in the 1977 cohort, 37,280 students from 1,275 schools participated (15% of the student population at that time); in the 1982 cohort, 16,813 students from 669 schools participated (8% of the student population); and in the 1989 cohort, 19,524 students from 381 schools participated (10.5% of the student population).

Individual classrooms were selected within sampled schools, and all students in those classrooms were surveyed. The math and language skills of the surveyed cohorts were assessed during the school year using a shortened version of the CITO test.[15] In addition, background information on their parents (the grandparent generation in our analysis) including their highest level of education, socio-economic status, and number of children living at home was collected. After the initial survey, individuals were followed annually over the course of their school career until leaving education. Basic identifying information is available including name and address at the time of the survey for most students in the original cohorts, allowing us to link their data to register data from Statistics Netherlands. The data could be linked successfully in 80% to nearly 100% of cases, depending on the cohort (1977 cohort: 81%; 1982 cohort: 88%; 1989 cohort: 98%

[14] In the 1977 and 1989 cohort, parent cognitive skills were tested after tracking. Our results are robust to including controls for the school track attended and also hold within each cohort (see below), implying that they are not simply driven by track effects.

[15] Note that surveyed students took the full CITO tests for placement purposes, but the surveys were given at different times during the year and the official CITO scores were not linked to the surveys. In the 1977 and 1982 cohorts, the survey tests were taken at the start of the school year. In the 1989 cohort, students took the test 5–7 months after the start of the school year, during the first months of the 1990 calendar year.

when a unique personal identifier was available).[16] Unless both parents participated in one of the three surveys, we have one parent in each matched family.[17]

The combined dataset contains information on the math and language skills of 25,483 parents and 41,774 of their children. The sample sizes and average skills of parents and children differ by cohort (Table A1). The sample size differences across cohorts partly reflect the window for observed test-taking by children. Statistics Netherlands has register data of all schools that participated in the CITO test from school year 2005/2006 onwards. Because of COVID-19, our observation window concludes at the end of the 2018/2019 school year.[18] Thus, we only observe those parents whose children took the CITO test at the end of primary school between 2006 and 2019.[19] This implies that for the 1977 cohort, we observe parents who are relatively old when they had children, while for the 1989 cohort we observe relatively young parents.[20] The selectivity of our sample with respect to age also has implications for parent education and skills. Because more highly educated people tend to enter parenthood at a later age, the parents from the 1977 cohort whose children we can observe in our data are positively selected in terms of their education and skills. The parents from the third cohort entered parenthood relatively young and tend to have slightly lower educational attainment and skills. The parents from the second cohort (around age 12 in 1982) fall in between. However, since our main estimation model relies on variation in cognitive skills within-parent between-subjects and because our results

[16] The register data provide information on the legal parents of children. In most but not all cases, these are also the biological parents.

[17] The fact that we usually observe the cognitive skills of only one of the parents in the ITS data potentially induces measurement error in the parent skill variables. To address this, we analyze the 365 children in our data for whom we observe both parents. We randomly drop one of the parents and estimate the relationship between child and parent skills. The results are very similar to those in the two-parent sample (see Figure A1), indicating that our main findings are unlikely to be affected by generally having skill information for one of the parents.

[18] The CITO test was not taken in school year 2019/2020, the first COVID-19-year.

[19] At the time of test taking, 91.8% of children live in the same household as the parent whose cognitive skills we observe.

[20] In the year of birth of the children, the parents were on average 31.7 years old (33.6 years in the 1977 cohort, 30.7 years in the 1982 cohort, and 27.0 years in the 1989 cohort).

hold in each cohort, this sample selectivity has no major implications for our results.

Data on grandparent education, which we derive from the parent questionnaire in the original cohort studies, provide additional information about the long-run transmission of skills (e.g., Adermon, Lindahl, and Palme (2021)). In Table A1, we again observe that our parent subsample in the 1977 cohort is positively selected, with a relatively high share of tertiary educated grandparents. However, there are no apparent differences by cohort in the social background of grandparents, measured by the type of occupation that the main breadwinner in the household held when parents took the skill test.

In addition to test scores, the registry data also provide detailed information on children's educational careers, allowing us to observe children's STEM choices at school. These in-school choices have important long-term consequences, since enrollment into most upper-secondary or tertiary education programs is only possible with specific backgrounds in terms of courses taken. We also observe STEM choices in upper secondary vocational or tertiary education directly. We separately code outcomes as either STEM or non-STEM based on the type of courses taken at school and the subsequent field of study. We observe that 34% of children choose a STEM profile at school, while 23% study a STEM field in upper secondary vocational or tertiary education (Table A1).[21]

In Appendix A.1, we establish the economic significance of our test score measures. Table A2 demonstrates that these early assessments of math and language skills effectively capture variations in long-term economic outcomes. Our findings align with previous research indicating that early skill assessments are significant predictors of future educational attainment and labor market outcomes in diverse contexts (e.g., Chetty, Friedman, and Rockoff (2014), Aucejo and James (2021)).

2.3 Measuring Comparative Skill Advantage

We construct a straightforward measure of individual comparative cognitive skill advantage based on the test score math and language data. Test scores of children in a subject are measured in percentile ranks within each test year based on the universe of test

[21] See section 6 for the analysis of STEM outcomes of children. We also show there that our results are robust to applying different definitions of STEM.

data from administrative records.[22] Parent test scores in each subject are measured in percentile ranks within each cohort, using the complete survey data that includes parents and unmatched survey-takers). Within each generation, we interpret the difference between the percentile ranks in math and language as comparative skill advantage.[23]

Our measure of comparative skill advantage does not permit an absolute interpretation as there is no natural metric that would allow measurement of levels of math and language skills on the same scale. We define the comparative skill advantage in relative terms by anchoring the skills of an individual in each subject to the distribution of the entire assessed population.

There is a wide dispersion of math-language skill differences (Figure 1) despite the high underlying correlation of math and language skills in each generation (0.67 for children and 0.61 for parents). Comparative skill advantages reach plus and minus 50 percentile points with a standard deviation around 25 percentile points in the pooled sample for each cohort (Table A1).

Relatively early test scores as opposed to later-life test scores are particularly well suited for assessing the impacts of comparative skill advantages. First, the comprehensive and unified curriculum in all Dutch primary schools implies that our skill data are less contaminated by other influences including subsequent career paths. This is particularly important for our analysis that relates comparative skill advantages to subsequent study or occupational choices; concerns about reverse causality and omitted variables would arise with skills measured at an adult age. Second, as emphasized in models of field-of-study choice (Altonji, Arcidiacono, and Maurel (2016)), individual beliefs about own

[22] After the 2014/2015 school year, test suppliers other than CITO became available. For comparability over time, the calculation of rank positions is done based on the schools that participated in the CITO test throughout the entire period of observation. Results are robust to an alternative calculation of percentile ranks based on the universe of schools.

[23] The choice of calculating the math skill advantage or the language skill advantage has no impact on the analysis. Other plausible formulations of the comparative skill advantage include a simple binary measure (i.e., 1 if math skills exceed language skills, 0 otherwise) and a math-language skill ratio (see Goulas, Griselda, and Megalokonomou (forthcoming)). Our results are robust to these alternative formulations.

comparative advantage may be more important than actual comparative advantage, although perceived and actual comparative advantages can be assumed to be highly correlated.[24] Arguably, primary education is the formative period not only for the production of basic skills in math and language but also for the formation of individuals' perceptions of whether they are better in math than in language or vice versa.

2.4 Economic Importance of Comparative Skill Advantages

We begin by assessing the relationship of comparative skills with STEM field choices within both the parent and child generations. In the background is the common perspective that individuals with appropriately high math skills have a strong incentive to enter STEM fields – regardless of their language skills – and that this drives the observed skill-STEM patterns. To assess this directly, we also consider the role of comparative skills across terciles of the math skill distribution.

Table 1 shows clearly that comparative skill advantages are significantly linked to STEM choices. In the parent generation (panel A, col. 1), a 10-percentile rank increase in the difference between math and language skills raises the probability of choosing a STEM field by 2 percentage points – a 7.9% increase relative to the baseline probability of obtaining a STEM degree. Importantly, the influence of comparative skills is consistent across all terciles of the math skill distribution (cols. 2–4). Only in the upper tercile does the influence become somewhat weaker, potentially reflecting the relatively high labor market returns to STEM education.

Similarly, in the child generation – across all students taking the CITO test from 2006-2019 – we also observe that a comparative math skill advantage increases the likelihood to choose a STEM field of study – both in the full sample and within each tercile of the math skill distribution (Table 1, panel B). Coefficient magnitudes are quite like those in the parent generation, while the relative effects tend to be somewhat smaller due to the general increase in STEM participation over time.

Overall, we demonstrate that what matters for STEM choices in both the parent and

[24] Similar ideas have also entered in research on learning about comparative advantage across occupations (Papageorgiou (2014)).

child generations is not just the level of math skills, but the comparative skill advantage.[25] This finding motivates our deeper exploration of the role of families and comparative cognitive skills.

3. Conceptual Framework

We propose an analytical framework that links the comparative skill advantages of parents and their children. Our overarching conceptual framework integrates two distinct research traditions: the investigation of intergenerational mobility merged with the investigation of educational production functions. The extensive work on intergenerational persistence of economic and noneconomic outcomes, which started over a century ago by Francis Galton (1889), provides structure to the interaction of parents and children. The educational production function analyses address how parents combine with schools and other factors to affect the skills of their children. The combination of the two permits new insights into the influence of comparative skill advantages on STEM education.

To identify the roles of parents and of educational environments on comparative skills, we construct a simple linear measure of the difference between math (T_m) and language (T_l) skills that we interpret as a measure of comparative (math) skill advantage (CS):

$$CS = T_m - T_l \tag{1}$$

A key feature of our measure of the comparative skill advantage in eq. 1 is that it is orthogonal to average math and language skills by construction. To see this, consider that the covariance of CS and the average of math and language skills ($1/2\ (T_m + T_l)$) is given by: $Cov(T_m - T_l, 1/2\ (T_m + T_l)) = 1/2\big(Var\ (T_m) - Var\ (T_l)\big)$. Thus, the covariance between CS and average skills is zero if math and language skills are both measured by standardized test scores with identical variance.[26] This highlights that the analysis of the

[25] Supporting this interpretation, our results are robust in a non-parametric version of the analysis using a simple binary indicator of having higher math than language skills in place of the math-language skill difference (results not shown).

[26] In our preferred specification, we measure both skills in percentile ranks. Thus, in expectation the variance of these measures aligns with that of a uniformly distributed random variable spanning from 1 to 99 with a standard deviation of approximately 28.3.

effects of *CS* is separate from the analysis of the impact of average skill levels on any outcome.

3.1 Intergenerational Transmission

A large literature focuses on intergenerational persistence of economic outcomes including income (Solon (1999), Björklund and Jäntti (2011)), educational attainment (Björklund and Salvanes (2011), Black and Devereux (2011)), and more recently cognitive skills (Adermon, Lindahl, and Palme (2021)). These generally follow the linear statistical approach rooted in Galton (1889), but with increased sophistication in dealing with a variety of issues including measurement error, nature vs. nurture, and the roles of extended families.[27]

We borrow the general framework of this prior work to study the intergenerational transmission of comparative skill advantages with the following model:

$$CS^c = \alpha + \beta CS^p + \varepsilon \tag{2}$$

where superscripts on CS^c and CS^p denote children and parents, respectively.

The key parameter of interest is β, the measure of intergenerational persistence. Heuristically, the larger β, the more the family determines child outcomes, leading the prior empirical analyses to focus on obtaining consistent estimates of β.

This model allows us to measure the strength of the transmission of comparative skill advantages across generations in the standard framework of the literature on intergenerational mobility. This is not, however, informative about how this correlation comes about. Analyzing this larger issue requires a richer conceptual model and a different empirical approach.

3.2 Skill Production

The central focus of this analysis is the formation of comparative advantage. The economic literature lacks a common framework for modeling the production of

[27] There is a parallel, more theoretical line of research following Becker and Tomes (1976, 1979). See the overview in Mogstad (2017) and related empirical analysis in Houmark, Ronda, and Rosholm (2020). Structural modeling of intergenerational effects is also related (e.g., Lee and Seshadri (2018)), including analysis of multiple types of ability (e.g., Guo and Leung (2021), Attanasio, de Paula, and Toppeta (2024)).

comparative skill advantages, and studies that analyze the impacts of comparative advantage on economic decisions typically take the basic ability differences as exogenously given. We develop a simple framework that characterizes the underlying production function for comparative skills. This provides structure for our thinking about potential confounders in the estimation of the intergenerational transmission of comparative skill advantages.

An important line of inquiry in the economics of education investigates education production functions and how families affect the skills of children. The Coleman Report (Coleman et al. (1966)), the first large-scale quantitative study of skill formation in children, led to the ubiquitous recognition of the importance of family background. Existing studies have, however, provided little evidence on the causal structure of family inputs and on the family's impact on different cognitive skills of their children.

The general form of a production function formulation of math or language skills that relates closely to our empirical analysis is:

$$\begin{aligned} T_i^c &= \lambda F_i + \varphi S_i + \eta_i \\ &= \lambda_1 G_i + \lambda_2 B_i + \varphi S_i + \eta_i \end{aligned} \tag{3}$$

Test scores of child i, T_i^c , are explained by family background factors (F_i) and environmental factors, which we refer to for expositional purposes simply as school factors (S_i). As argued in Björklund, Lindahl, and Plug (2006) and further developed in Adermon, Lindahl, and Palme (2021), it is insightful to partition the family background inputs further into pre-birth factors (G_i), i.e., factors that are determined before the child was born, and post-birth factors (B_i), i.e., inputs to educational production that are not fully determined at the time of birth. The error term, η_i, contains other influences on test scores and is assumed to be i.i.d. with mean zero.

To streamline our exposition, we focus on the pre-birth factors (G_i), which include the cognitive skills of parents, and we move all post-birth factors (contemporaneous family inputs, B_i, and environmental factors, S_i) into a new composite error term, μ_i:

$$T_i^c = \lambda_1 G_i + \mu_i \quad \textit{where}\ \mu_i = \lambda_2 B_i + \varphi S_i + \eta_i \tag{4}$$

We interpret eq. 4 as the reduced form effects of pre-birth factors. Most education production studies are primarily interested in the causal effects of either school factors or post-birth family inputs, considering pre-birth factors simply as further covariates. Our interest in intergenerational transmission, however, leads us to study the reduced form effect of inputs to educational production determined before the child was born. We think of these as primitives in the production of learning that is captured in the later test scores. With this focus any measured post-birth factors in our empirical model become potentially endogenous.

We extend the one-dimensional skill production model to a two-dimensional model of the production of separate skills in math and language as follows:

$$T_{idm}^{c} = \rho_{1m} T_{idm}^{p} + \rho_{2l} T_{idl}^{p} + \delta_m \psi_{id} + \mu_{idm} \tag{5}$$

$$T_{idl}^{c} = \rho_{1l} T_{idl}^{p} + \rho_{2m} T_{idm}^{p} + \delta_l \psi_{id} + \mu_{idl} \tag{6}$$

Domain-specific test scores, T_{ida}^{c}, of child i of dynasty d in domain a (either math or language) are explained by pre-birth factors, which we have further decomposed into parent skills in math, T_{idm}^{p}, and language, T_{idl}^{p}, as well as other pre-birth factors, ψ_{id}.[28] This framework allows for different main effects of parental skills on child skills, ρ_{1m} and ρ_{1l}, and for spill-over effects with parental math (language) skills also impacting a child's language (math) skills, ρ_{2m} and ρ_{2l}.

By differencing eq. 5 and 6, we arrive at a framework for the production of a comparative skill advantage that has its roots in a standard educational production model of specific skills:

$$\begin{aligned} CS_i^c &= T_{idm}^c - T_{idl}^c \\ &= (\rho_{1m} - \rho_{2m}) T_{idm}^p - (\rho_{1l} - \rho_{2l}) T_{idl}^p + (\delta_m - \delta_l)\psi_{id} + (\mu_{idm} - \mu_{idl}) \end{aligned} \tag{7}$$

As eq. 7 highlights, the comparative skill advantage depends on the net effects of the two subject-specific skills. The net effect, $\rho_{1a} - \rho_{2a}$, is the direct effect of parent skills on child skills in each subject, ρ_{1a}, minus the spill-over effect in subject a on child skills in the other subject.

[28] In the empirical analysis, we can link families over time going back to grandparents, as suggested by Adermon, Lindahl, and Palme (2021) and Moreno (2021).

A simple Galton-inspired intergenerational transmission model of comparative skill advantages as in eq. 2 can be readily derived from this model by making further assumptions about the effects of parent skills on the production of child skills. In particular, if the net effects are constant across domains, i.e., $\rho_{1a} - \rho_{2a} = \rho_1 - \rho_2 = \beta^*$, eq. 7 further simplifies to:

$$CS_i^c = \beta^* CS_i^p + (\delta_m - \delta_l)\psi_{id} + (\mu_{idm} - \mu_{idl}) \tag{8}$$

where β^* measures the effect of parents' comparative skill advantage on the comparative skill advantage of their children.[29]

3.3 Causality

Eq. 8 clarifies the identification problems that surround a simple Galton regression of child comparative skill advantage on parent comparative skill advantage. First, inputs that have the same impact on both skills cancel out. Pre-birth factors (ψ_{id}), such as genetic factors or characteristics of grandparents, do not confound the estimation of the intergenerational transmission of comparative skill advantages as long as they influence the production of math and language skills in the same way. Similarly, any post-birth inputs in the composite error term, $\mu_{idm} - \mu_{idl}$, with constant effects across skill domains cancel out, implying important determinants of education production such as school quality will not confound estimation of the intergenerational transmission of comparative skill advantages as long as children's subject-specific school quality is not correlated with relative parental skills.

Second, any bias in the estimation of β^* arises because parent comparative skill advantage is correlated either with other pre-birth factors, ψ_{id}, or with post-birth factors, $(\mu_{idm} - \mu_{idl})$. In terms of post-birth factors, variation in subject-specific school or teacher quality could be a potential confounder. However, it is only a problem if differences in subject-specific school or teacher quality are correlated with, but not caused by, parent

[29] Most studies exploiting within-student across-subject variation (e.g., Bietenbeck, Piopiunik, and Wiederhold (2018)) assume no spill-over effects and constant direct effects across subjects. Under these stronger assumptions, β^* identifies the direct effect of parent skills on child skills.

comparative skill advantage. If, for example, parents with a comparative skill advantage in one subject deliberately send their children to schools with higher subject-specific quality in this subject, this is simply a mediator of the reduced-form effect of parent comparative skill advantage, implying no bias. But a bias could arise if, for example, the correlation between parent comparative skill advantage and subject-specific school quality exists because of regional immobility of parents combined with persistent differences in subject-specific school quality across regions.

Pre-birth factors could reflect dynastic predispositions for specific subjects (e.g., arising through occupational legacies) or genetic differences in talent for a specific subject (e.g., a "math gene"). While there is uncertainty about genetically inherited differences in talent for specific subjects,[30] if they exist, they would affect skill advantages of both parents and children and would lead to a direct relationship of comparative skill advantages across generations. Thus, they are naturally included as a mechanism for intergeneration transmission.

Establishing the causal relationships of comparative skills is a central part of this analysis, but it is not the only important issue. For policy purposes, in order to address the availability of STEM-trained individuals through comparative skill advantages, it is important to know if comparative skill advantages are malleable. This leads to the question of whether any "shock" due to post-birth factors in the production of comparative skill advantages *of parents* also spills over to the next generation. An extreme alternative is that any observed correlation in comparative skills across generations is entirely predetermined.

4. Empirical Strategy

Our empirical strategy, following directly from the conceptual model, starts by estimating the simple regression of eq. 2. Since we rely solely on between-subject test score variation within children and within parents, any observed or unobserved

[30] Summarizing the state of the literature, Holden (2008) concludes that "…genius-type alleles, particularly for specific skills such as math ability, don't seem to exist." But, recent studies suggest math ability might be moderately heritable (e.g., Davis and al. (2014), Zhang et al. (2023)).

characteristic of children, parents, classrooms, or schools having a similar impact on math and language skills does not confound the estimated impact of parents' comparative skill advantage. However, to account for the possibility that covariates affect math and language skills differently, we make multivariate adjustments to the simple Galton correlational model using our parent survey data:

$$CS_I^c = \alpha + \beta CS_i^p + \gamma X_i + \varepsilon_i \tag{9}$$

The vector of covariates, X_i, in eq. 9 contains a set of parent and grandparent background characteristics, measured at the time when the parent took the skill test, along with a total of 799 municipality-of-residence fixed effects and parent and child test-year fixed effects (see Table A1). For parents, we include gender, migration background, and number of siblings. For grandparents, we include the age of either grandparent (in seven categories), educational attainment (in four categories for the highest level of education of the grandparents),[31] and social background (in seven categories of occupational status of the main breadwinner). These control for pre- and post-birth factors that possibly also influence the formation of children's comparative skill advantage in eq. 8.

We additionally pursue an IV strategy for causal identification and to address the malleability of comparative advantages. We consider the portion of parents' comparative skill advantage driven by between-subject differences in teachers or peer quality during the parent's early formal education – variation that is arguably exogenous to the formation of children's comparative skill advantage. This IV approach relies on a unique feature of the data: the parent cohort surveys use classrooms within school as the primary sampling unit. More specifically, in the two later cohorts (1982 and 1989), we have information on math and language test scores for (almost) all classmates of parents around age 12. Unfortunately, school and class identifiers for the 1977 cohort were removed by Statistics Netherlands and could not be restored. In total, the sample in the IV analysis consists of

[31] Results are robust to including educational attainment of either grandparent individually.

8,011 parents and 12,268 children across the two available cohorts.[32]

Formally, we instrument CS_i^p by comparative skills of parents' classroom peers:

$$CS_j^p = \theta + \pi \overline{CS}_{-j}^{class} + \vartheta_j \tag{10}$$

where $\overline{CS}_{-j}^{class}$ is measured as the difference between the average (leave-out mean) percentile ranks in math and in language of parents' classroom peers.[33] The between-subject difference in classroom ranks measures the relative quality of the formal education environment in math vs. language – whether from teachers, peers, or other elements of schools.[34]

Our IV approach isolates variation in the comparative skill advantage of parents that is independent of dynastic factors potentially impacting the formation of their children's skill advantage. The exclusion restriction is that our instrument is only correlated with children's comparative skill advantage because of its association with the comparative skill advantage of the parents.

The IV estimator directly addresses two potential issues. First, measurement error in the comparative advantage of parents could bias the estimates of intergenerational persistence. Second, omitted factors that differentially impact either math or language skills (and are not simply mechanisms by which parents influence children's comparative

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[32] For more details on the assignment of classrooms in the survey data for the 1982 and 1989 cohort, see Appendix A.3. A small number of observations (1% in the 1982 cohort and 5% in the 1989 cohort) could not be linked in the original dataset.

[33] Peer ranks are based on the country-wide skill distribution. In Appendix A.3, we show that our IV results are robust to several alternative ways of constructing an instrument based on peer performance in math and language.

[34] Students in the 1982 cohort were tested in the last year of primary school. Students in the 1989 cohort were tested about halfway through their first academic year in secondary school, implying that students had 5–7 months of exposure to their teachers and tested peers. Moreover, primary schools often feed into common secondary schools so that primary school students stay together with at least some of their classmates in secondary school. During the period 2006–2019, when we can observe school transitions in our administrative CITO data, a median share of 19% of a student's primary school peers attends the same secondary school-track combination. This share has been slightly decreasing over time, potentially reflecting more school choice in the Netherlands.

advantage) may bias the estimated influence of parents.

5. Intergenerational Transmission of Comparative Skill Advantages

Parents directly transmit individual skills to their children. As easily shown in our data, parents with greater math skills have children with greater math skills and similarly for language (see Figure A2 and Table A3).[35] But our interest goes beyond the separate factors to look at whether comparative skill advantages are transmitted to children.[36]

The basic character of comparative advantage transmission is readily seen by the strong linkage of math-language skill differences across generations (Figure 2). Parents who perform relatively better in math than in language are significantly more likely to have children who are relatively better at math compared to language (and vice versa). Moreover, the relationship between the comparative skill advantages is linear.

This bivariate portrayal of intergenerational persistence in comparative skill advantages does speak to the distinct dimensions of cognitive skills. If there were a single ability factor such that the variations in comparative skills just represented measurement errors, we would not expect the clear intergenerational transmission of those differences.

It may nonetheless be that the observed persistence is affected by unobserved confounders. To address this, we consider the multivariate specification of eq. 9. The OLS results in the next subsection provide the basic persistence estimates. The subsequent IV estimates address causality more rigorously and point to the malleability of parental comparative advantage.

5.1. Persistence of Comparative Skill Advantage – Baseline Estimates

We observe a strong intergenerational transmission of comparative skill advantages

[35] The patterns of the two subject-specific relationships are remarkably similar: An increase in parent skills by one percentile is associated with an increase in child skills of 0.28 percentiles in math and 0.30 percentiles in language. These estimates are in the same ballpark as the parent-child human capital persistence parameter of 0.361 estimated in Adermon, Lindahl, and Palme (2021).

[36] An alternative interpretation of the single-subject relationships might be that there is a single latent factor (general cognitive ability) and that each of the subject measures is the true latent factor plus random error. If that were the case, however, one would not expect the close relationship of parent-child math and parent-child language to be significantly larger than that for the alternative parent skill (panel C of Table A3).

even after conditioning on a range of plausible inputs (Table 2).[37] Accounting just for basic sociodemographic characteristics of parents and grandparents, a 10 percentile rank difference in parental math and language skills translates into a one-rank difference of children (col. 1).

Estimates of the key transmission parameter are remarkably stable with the addition of more controls for family background including grandparent education (col. 2) and grandparent social status (col. 3). Likewise, the persistence parameter changes little with detailed regional variation captured by fixed effects for the municipality of residence when parents took the skill test (col. 4).[38]

Mathematically skilled parents are also more likely to have a comparative skill advantage in math (and analogously for language). Given the strong intergenerational transmission of absolute math and language skills (Figure A2), an alternative interpretation of these results might be that the observed persistence of comparative skill advantages is just a (noisy) reflection of the overall intergenerational transmission of math or language skills. However, as we showed, there is a mechanical zero correlation between the comparative skill advantage and the average of math and language skills, a fact confirmed by our persistence results being unaffected by the inclusion of the skill average (col. 5). Moreover, while statistically significant at the 1% level, the impact of skill average is just one-fifth the magnitude of that for comparative skill advantage. The negative estimate indicates that higher skilled parents tend to produce children with relatively higher language skills. This finding is consistent with language skills being nurtured within the family environment, whereas math skills are primarily cultivated at

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[37] Results for each cohort individually are reported in panel C of Table A3. Estimates are statistically significant in each cohort. Consistent with the subject-specific results in panels A and B, the estimate of parents' comparative skill advantage is largest in the first cohort. Coefficients on the control variables in the full model are shown in Table A5.

[38] The estimated strength of the intergenerational transmission is very similar when we use the difference between standardized math and language test scores to measure comparative skill advantages instead of percentile ranks (Table A6). This suggests that, at least with high-quality tests such as CITO, the standard implicit assumption of cardinality of previous studies does not distort the results.

school (see Hanushek and Rivkin (2010), Bacher-Hicks and Koedel (2023)).

Intriguingly, the strength of the intergenerational transmission of comparative skill advantages does not vary by the gender match of parents and their children (col. 6). This result differs from several papers on the intergenerational transmission of human capital that suggest a stronger influence of mothers (e.g., Black, Devereux, and Salvanes (2005), Holmlund, Lindahl, and Plug (2011), Piopiunik (2014), Attanasio, de Paula, and Toppeta (2024) , Lundborg, Plug, and Rasmussen (forthcoming). Altmejd (2024) also suggests that fields of study differ: daughters tend to follow mothers while sons follow fathers.[39]

Our estimation of the intergenerational transmission of comparative skill advantages accounts for all factors that similarly affect math and language skills, such as general motivation and ability, access to learning aids and opportunities, as well as the impacts of peers and neighborhoods. In the spirit of Altonji, Elder, and Taber (2005) and Oster (2019), the stability of the coefficient on parents' comparative skill advantage when we add various parent and grandparent characteristics suggests no major role for unobserved variables in confounding our estimates.

One obvious concern is that measurement error distorts our estimates of the persistence in comparative skill advantages. Any specific test will measure subject-specific skills with a varying degree of reliability. Measurement error in parent cognitive skills could be particularly damaging in the estimation of our differenced model (see, for example, Angrist and Krueger (1999)). We investigate this in two different ways. First, we consider alternative ways to measure comparative advantage that would lessen the impact of measurement error. Second, we employ an IV strategy in the next subsection that directly confronts any possible measurement error. Both checks suggest that measurement

[39] Several additional heterogeneity analyses are relegated to the appendix. Most noteworthy, we find that skill transmission tends to become stronger as the education level of grandparents increases, perhaps operating through more negative attitudes toward education in lower-educated families (Table A7, col. 1). The strength of transmission does not, however, vary systematically with grandparents' social background (Table A7, col. 2). Furthermore, in a simple mechanism analysis, we find that several parent outcomes measured after the skill assessment, such as highest educational degree or future income, cannot explain intergenerational transmission patterns (Tables A8 and A9).

error is unlikely to have a meaningful impact.

In our baseline analysis, we measure cognitive skills of parents and their children in percentile ranks (Table 3, col. 1). Any measurement error that is rank-preserving does not affect our estimates, but errors that lead to changed ranks will generally lead to attenuation of our estimates of persistence. In the spirit of the classical solution to such measurement error, we explore broader categories when defining rank measures.[40] This aggregation will reduce the likelihood that measurement error in the tests alters the rank positions of individuals. The estimated transmission parameter changes little when we measure math and language skills in decile ranks (col. 2). The extreme of this aggregation is creating a binary measure that indicates whether or not the rank in math is higher than in language (col. 3–6). Potential measurement errors in this binary specification are largest when all observations are used in calculating the comparative skill advantages (col. 3) and are reduced when we drop individuals with small differences in rank positions between math and language in order to reduce the possibility for misclassification. In col. 4-6, we progressively drop those with comparative skill advantages of less than 5, 10, or 15 ranks. The estimated transmission parameter quickly approaches the baseline estimates when dropping those with just a small math-language gap.

Two further concerns regarding measurement error remain. First, there may be potential "floor" and "ceiling" effects. If there is some measurement error in skills, then a measured top rank in one skill domain will by construction rarely be matched by a measured top rank in the other domain, and vice versa at the very bottom of the distribution. Second, one skill domain may be systematically measured with more error. To address these concerns, we conducted a simulation that accounted for the bounded nature of the skill measures (percentiles ranging from 0 to 100) and the possibility of

[40] The classical treatment of errors in variables aggregates data into two groups and yields consistent estimates of the slope as long as observations are not classified into the wrong group; by eliminating observations at the boundary of the groups, any inconsistency of estimates can be reduced (Wald (1940), Cochran (1968)).

differential measurement error across domains.[41] Each had just a minimal impact on our persistence estimate.

5.2 Persistence of Comparative Skill Advantage – IV Estimates

By knowing parents' classroom environments near the end of primary school, we can characterize the educational inputs outside of each parent's family. Differences in the comparative skill advantage of parents' classmates are indeed strong predictors of parents' own skill advantage (F-value>200) and form the basis for an instrumental variables approach. The first stage relationship, shown in col. 2 of Table 4, indicates that a classroom that scores ten percentile ranks higher in math than in language is associated with parents scoring about 3 percentile ranks higher in math than in language. The reduced form effect on the comparative skill advantage of children is also significantly positive (col. 3).

The IV estimates of the persistence of comparative advantage are significant and very stable even with a variety of controls in the model (col. 4 and 5).[42] The IV estimate with the instrumented parental comparative advantage indicates that an increase of relative math skills of parents by 10 percentile ranks leads to an increase in the relative math skills of children of 1.1 percentile ranks. This estimate is little affected by adding controls for grandparents' education and social status to the model. This suggests that the variation in classrooms' comparative skill advantage is unrelated to these characteristics of parental background – which also makes it more plausible that it is also unrelated to other unobservable characteristics. Moreover, the similarity of the estimated transmission parameters from OLS and IV approaches suggests only a limited confounding impact of unobserved subject-specific proclivities of families.

Our IV estimation addresses the possibility of bias from omitted subject-specific

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[41] We simulated a sample of 10,000 individuals and introduced random noise to the true skill ranks, assuming less measurement error in math (standard deviation of 5) than in language (standard deviation of 10), while ensuring the measured skills remained within the 0-100 percentile range. The results (not shown) indicated a strong correlation (0.967) between the true comparative skill advantage and the measured advantage.

[42] Results are similar across various ways of constructing the instrument (Table A10).

proclivities of families while also dealing with issues of measurement error in the parents' comparative advantage. Because these peer scores are correlated with true individual comparative advantage but not with the individual test errors, they are valid instruments to deal with the possibility of measurement error. The similarity of the IV and OLS estimates strongly reinforces the prior conclusion that measurement error from the test-based measures of comparative advantage does not significantly bias the persistence estimates.

The IV analysis of comparative advantage has a larger and more important implication. These estimates point to the malleability of family cognitive skill influences. Our IV results imply that the intergenerational transmission of comparative skill advantages within families is not entirely genetic in origin and is malleable, being partly shaped by the formal education system. These outside factors include some combination of peer influences and of teacher and school influences, and we cannot distinguish between impacts on comparative advantage coming from a particularly good teacher or from the influence of peers per se with our data.[43] But, importantly, these estimates provide direct confirmation that there is room for policy to affect performance not only of the current generation but also of future generations.

5.3 Persistence of Comparative Skill Advantage – IV Identification

The necessary exclusion restriction in the IV approach is that the comparative skill advantages of parents' classroom peers are correlated with the skill advantages of children only through their impact on parents. While we cannot exploit random assignment of students to schools and classrooms, we argue – and show below – that intuition about possible selection biases from analyses of skill levels do not present similar problems when looking at comparative skills.

In our setting, we see three potential threats to identification. We address these concerns through an extensive set of robustness analyses, which consistently support the

[43] The extensive research on peer effects and on educational production functions provides direct contemporaneous evidence on the varying influences on student achievement that is more suitable for dissecting the relevant components of the formal educational system (see, for example, Sacerdote (2011b), Woessmann (2016), Handel and Hanushek (2023)).

validity of our IV approach. The details of these tests are provided in Appendix A.3, and we summarize both the issues and the evidence here.

First, bias might arise from particular forms of school selection. Student selection based on average (i.e., subject-invariant) school quality would not bias our results since the comparative skill advantage is, by construction, orthogonal in expectation to average skills. And school choice based on subject-specific quality differences across schools would simply be a mediating factor when parents make this choice because of their own comparative skill advantage.

School choice becomes a concern if a direct correlation of the instrument with children's sorting into schools arises because of other reasons (e.g., regional stickiness across generations). To account for the relative quality of schools in math versus language, we directly control for the comparative skill advantage of the *children's* school peers. While such analysis shuts down one potential explanation for the intergenerational persistence of comparative skill advantages (i.e., parents with a comparative advantage in math select schools for their children that are known to be better at math), our IV estimates of the transmission parameter change only little (Table A11).[44] The IV results are also very similar when we exclude children who are more likely to know their parents' primary school classroom peers personally, either because they are in the same school as the children of their parents' former classmates or because they still live in the same municipality as their parents did during their early formal education (Table A13).

A second potential threat to identification is endogenous switching between schools or classrooms in the parent generation. However, powerful predictors of school choice, such as grandparents' education or social status, are virtually uncorrelated with skill differences in parents' classrooms (Table A14). The results align more closely with models of school and classroom choice where only the average educational quality of

[44] Since we cannot identify classroom within schools in the administrative child data, there could potentially be student sorting within schools. If we restrict the sample to children in schools with at most 30 students in grade six in a given year, likely implying only one classroom, the estimated transmission parameter remains very similar (Table A12). This also suggests that within-school sorting is not a significant problem.

schools is relevant, as we do observe a strong correlation between grandparental characteristics and skill *levels*. It is also highly unlikely that, in the 1970s and 1980s, schools or classrooms were selected by grandparents based on specific school or teacher performance in math relative to language, as no information on subject-specific school quality was publicly available in the Netherlands at the time. This is further supported by evidence showing that such sorting on subject-specific school quality is not apparent in the current generation, even with better information.[45] Finally, when examining contemporary school quality data from the Dutch school inspectorate, we find no relationship between a school's comparative skill advantage and the likelihood of receiving a more favorable rating (which would be observable to parents).[46]

A third potential worry is the well-known reflection problem, where a student's comparative skill advantage affects the average comparative skill advantage of her classroom peers. Our estimates would be confounded if the variation used in the IV estimation is (in part) driven by the reflection problem. We can get some insight into the possible importance of this by tracing within-classroom peer correlations from a longitudinal cohort study covering Dutch primary school students between 2013 and 2021.[47] We consider the correlation between a child's comparative skills advantage in the first grade and the comparative skill advantage of the child's classroom peers for all grade levels until grade 5 (Table A16). Expectedly, we observe a strong correlation in grade 1, reflecting the basic idea of our IV strategy. However, the correlation becomes substantially weaker in later grades, and vanishes entirely when we consider children who move to a different school. Assuming persistent peer effects, this evidence is clearly at

[45] Specifically, Table A11 clearly shows that parents with relatively higher math skills do not systematically choose schools for their children that perform relatively better in math.

[46] With our baseline controls: coef. = 0.0004, p=0.406. Inspectorate ratings are available for the period 2012–2018. Conditional on having received a rating, the share of schools with an "insufficient" rating is 10.7%. However, not all schools are visited by the inspectorate, as only 18.4% of schools have received a rating.

[47] This so-called NCO-LVS dataset contains information on the performance of primary school students on standardized tests in math, language, and spelling for each grade. For information on the NCO-LVS dataset, see Haelermans et al. (2022).

odds with the reflection problem concern that a child's comparative skill advantage influences the skill advantage of the classroom peers.[48]

6. Children's STEM Choices: The Role of Parental Comparative Skill Advantages

The Dutch education system provides an ideal setting for evaluating the role of comparative skill advantages in determining STEM choices and participation. Students in lower secondary school choose a course profile – a set of courses covering specific areas of study – that guides their work in upper secondary school. Because subsequent fields of study in post-secondary education require specific courses for entry, these profiles have a strong influence on fields of study and, ultimately, occupational choices. The importance of profile choice is clear from the raw data on progression to STEM fields of study. Of students with a STEM profile in school, 61% go on to a STEM field of study, compared to just 14% of those with other profiles.

Our primary objective is understanding the role of parental transmission of skills in determining subsequent STEM choices of their children. We estimate linear probability models that relate an indicator for children's choice of STEM at school or after school to comparative math skill advantage of parents. Estimation focuses on the 1977 parent cohort, where we have data on STEM choices for the majority of children.[49]

Results in Table 5 highlight the significant influence of parent skills on STEM participation of their children. Including our baseline controls, we find that a parent that scores ten percentile ranks higher in math than in language is associated with her child being 0.9 percentage points (2 percent) more likely to choose a STEM profile at school (col. 2) and being 0.5 percentage points (1.6 percent) more likely to opt for a STEM field

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[48] We do observe a strong and persistent correlation between the *level* of child skills in grade 1 and the level of skills of classroom peers in later grades, suggesting that classroom peers' skills may be endogenous to child skills when considering levels instead of comparative advantages.

[49] In the 1977 cohort, we can follow two-thirds (66.5%) of children in the post-school activities, allowing us to observe both STEM profile choice and STEM field of study choice. In the later cohorts, this share is substantially smaller (1982 cohort: 43.3%; 1989 cohort: 12.2%).

in vocational or tertiary education (col. 4). This finding, of course, is not very surprising, given that comparative skill advantages of parents filter through to their children, but it underscores the important consequences for children's educational pathways of the intergenerational transmission of comparative skills.

While parents strongly affect STEM profile choice of girls, there is a weaker influence of comparative skill advantages by the time field of study decisions are made (also see Figure 4). For boys, there is little difference in the strength of the relation between parents' comparative skill advantages and STEM choices throughout the educational career. Thus, ultimately, parents' comparative skill advantages have somewhat less influence on girls' choices of STEM field of study than on boys' choices, independent of preparation.

7. Conclusions

The role of comparative advantage for economic choices has been extensively studied, and as developed by Roy (1951) is the foundation of occupational choices. But most analyses stop short of indicating where differences in comparative advantages come from and how malleable they are. Our analysis shows that comparative advantages in cognitive skills are transferred across generations: Parents who were relatively better at math (vs. language) in childhood are more likely to have children with a similar comparative skill advantage in math. Notably, we show that parents' comparative skill advantage is a strong predictor of STEM choices – both their own and those of their children.

While it is common, often for data reasons, to treat all achievement data as simply alternative measures of a common factor, we show that tests in different domains have meaningful implications for individual choices and outcomes. This is easiest to see in the choice of STEM fields. Our analysis clearly shows that STEM choices are made in the context of alternatives, and somebody with comparatively high language skills might rationally opt for a non-STEM career even if that person has high absolute math skills.

The new Intergenerational Transmission of Skills (ITS) database that we develop permits matching skills of Dutch parents and children derived from similar tests taken at similar ages. We measure comparative skill advantage as the ordinal difference between math and language skills in the parent and child generation, respectively, each assessed by the percentile position in the nationwide skill distribution. Our empirical strategy exploits within-family between-subject variation in cognitive skills, thus eliminating all family, school, and neighborhood factors that are not specific to either math or language performance. The estimates of the intergenerational transmission of comparative skill advantage prove very robust to a variety of specification and robustness exercises.

Comparative skill advantages are also shown to be malleable, implying that the intergenerational transmission of skills is not entirely driven by factors that are fixed within family dynasties. Our IV estimation strategy, based on comparative skills of the parents' classroom peers, indicates that nonfamily inputs in the production of skills significantly affect comparative skill advantages that then carry over to future generations. Therefore, educational policies that shift focus from one skill domain to another not only affect comparative skill advantages of current students but also have lasting impacts on future generations. Similarly, evaluations of school programs that ignore such achievement spillovers on future generations will understate the full program impact.

Comparative skills influence long-run career patterns, as predicted by a Roy model of occupational choice. Relatively high math skills of parents promote greater choice of STEM paths both by them and by their children. While the influence of parents' comparative advantage in math on STEM choices is observable for boys and girls, it appears to be a stronger determinant of STEM field-of-study choice for boys, potentially contributing to the observed underrepresentation of women in STEM occupations.

Our results carry an important message regarding policies aimed at increasing the number of STEM-trained workers. The importance of skill-based comparative advantages in determining STEM choices, together with its malleability through environmental factors, suggest that any policy changing the relative cognitive skills of students today will spill-over to future generations, having a lasting impact on the sorting into STEM (and other) fields.

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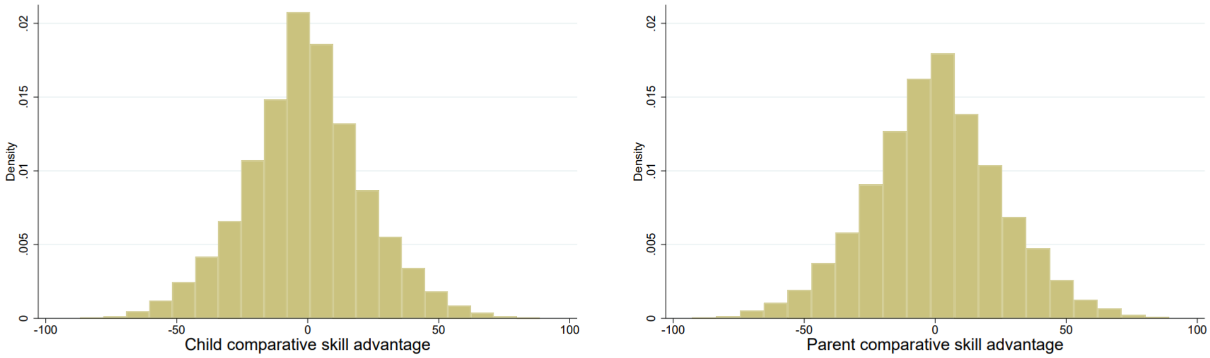
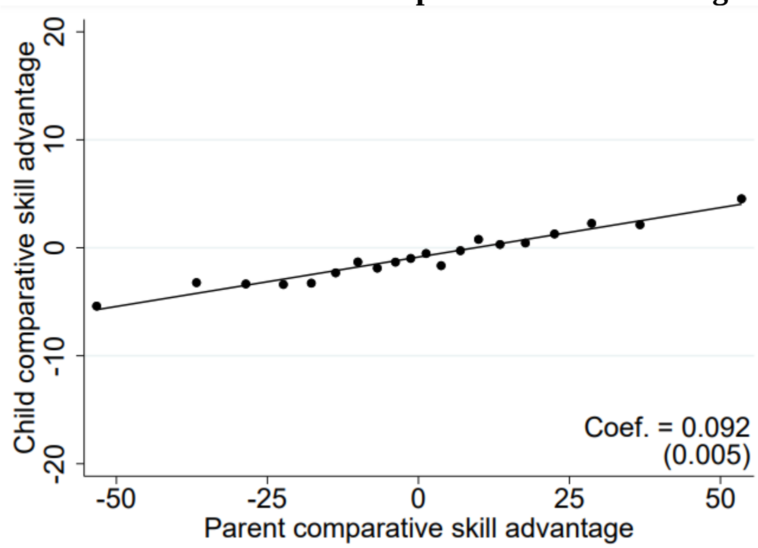
Figure 1: Histogram of comparative skill advantages



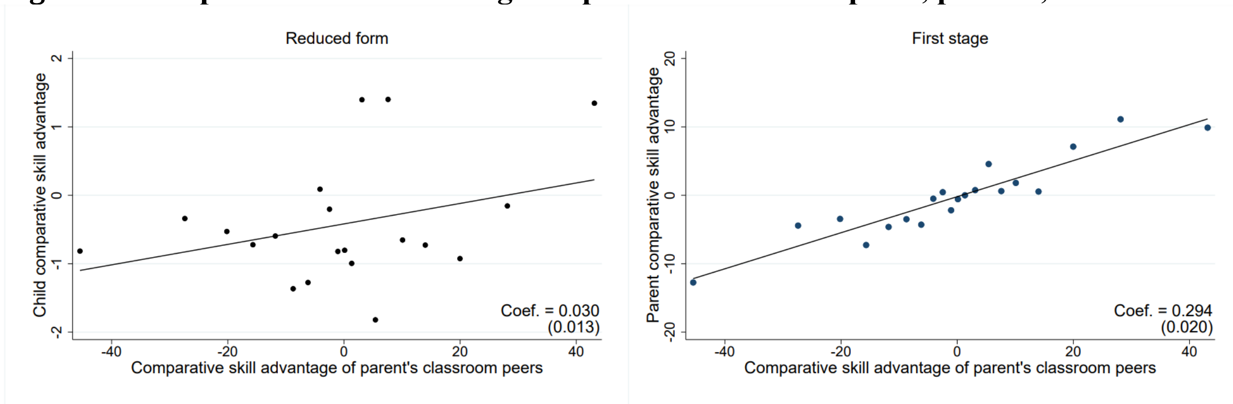
Notes: The figure depicts the comparative skill advantage for children (left) and parents (right). The comparative skill advantage of children is measured as the difference between the percentile ranks of linked children's math and language test scores in full sample of children taking the test in a given year based on the administrative data. For parents, the 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. *Data sources:* ITS dataset (linked administrative and pooled survey data).

Figure 2: Intergenerational transmission of comparative skill advantages



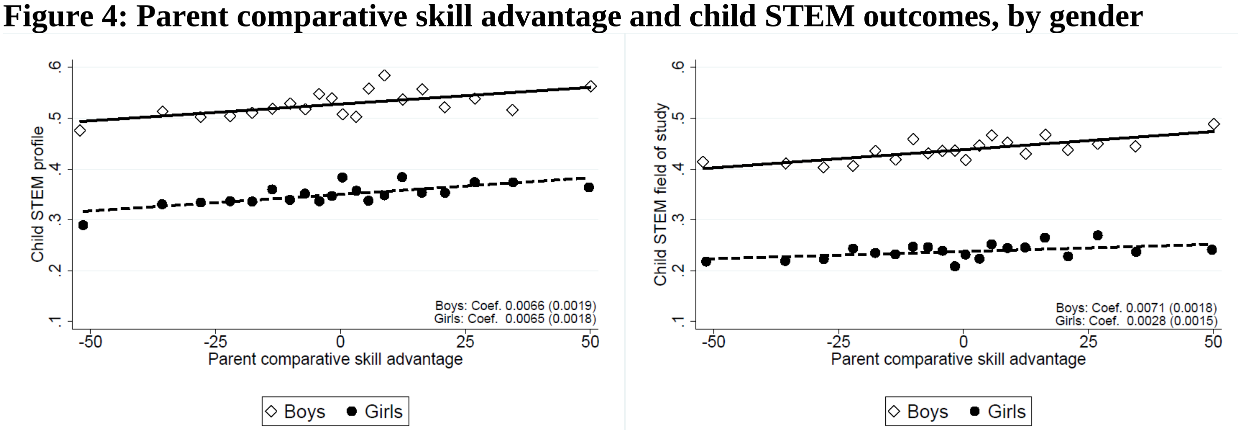
Notes: The figure displays a binned scatterplot showing the strength of parent-child transmissions in comparative skill advantages. Child comparative skill advantage is measured as the difference between the percentile ranks of linked children's math and language test scores in full sample of children 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 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:* Administrative data; pooled ITS survey dataset.

Figure 3: Comparative skill advantages of parents' classroom peers, parents, and children



Notes: The figure displays two binned scatterplots showing the strength of the relationship between the comparative skill advantage of parents' classroom peers and the comparative skill advantage of children (left) and parents (right), respectively. Child comparative skill advantage is measured as the 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 comparative skill advantage of parents' classroom peers is measured as the difference between the percentile ranks in math and language test scores of parents' classrooms peers within an education cohort. The best-fit line, the coefficient, and the standard error (clustered at the classroom level) are calculated from bivariate regressions on the micro data. *Data sources:* Administrative data; pooled ITS survey dataset.

Figure 4: Parent comparative skill advantage and child STEM outcomes, by gender



Notes: The figure displays two binned scatterplots showing the strength of the relationship between parent comparative skill advantage and child STEM course choice in high school (left panel) and STEM field-of-study choice (right panel), respectively, by child gender. See Table 5 for STEM definitions. Sample is restricted to children of individuals in the first survey cohort (1977) for whom we observe course- and study profile choices. 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 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:* Administrative data; pooled ITS survey dataset.

Table 1: Comparative skill advantage and STEM field of study choice

		Math tercile		
	All	Low	Middle	High
Outcome: STEM Field of Study (0/1)	(1)	(2)	(3)	(4)
Panel A: Parents				
Parent CSA (/10)	0.020	0.018	0.020	0.016
	(0.001)	(0.002)	(0.002)	(0.002)
Outcome mean	0.252	0.221	0.236	0.291
R-squared	0.013	0.010	0.014	0.005
Observations	28,264	8,796	8,785	10,683
Panel B: Children				
Children CSA (/10)	0.026	0.024	0.019	0.014
	(0.002)	(0.001)	(0.001)	(0.001)
Outcome mean	0.331	0.258	0.328	0.412
R-squared	0.017	0.011	0.009	0.003
Observations	1,161,307	402,607	373,489	385,211

Notes: Least squares regressions. Sample: Pooled sample of all individuals (parents and nonparents) in the three survey cohorts in Panel A; pooled sample of all children that took the CITO test at the end of primary education between 2006–2019 for whom we observe course- and study profile choices in Panel B. In cols. (2)–(4), the sample is split by terciles of the math test score distribution in the parent generation (Panel A) and the child generation (Panel B), respectively. 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 field (Panel A); binary variable indicating the choice of a STEM field of study after secondary school in Panel B. 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, Agriculture, as well as Medicine and Nursery were classified as a STEM choice of study. Parent comparative skill advantage is measured as the difference between the percentile ranks of math and language test scores in full sample of parents and nonparents in an education cohort. Child comparative skill advantage is measured as the difference between the percentile ranks of math and language test scores in full sample of children taking the test in a given year based on the administrative data. Standard errors (in parentheses) are clustered at the individual level in Panel A and at the school level in Panel B. Data sources: Administrative data; pooled ITS survey database.

Table 2: Intergenerational transmission of comparative skill advantages (OLS)

	(1)	(2)	(3)	(4)	(5)	(6)
Parent comparative skill advantage	0.098	0.097	0.096	0.094	0.094	0.098
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.010)
Parent skill average					-0.019	
					(0.005)	
Parent-child gender match						
× Male parent & female child						-0.001
						(0.013)
× Female parent & male child						-0.004
						(0.013)
× Female parent & female child						-0.005
						(0.013)
Grandparent education		yes	yes	yes	yes	yes
Grandparent social background			yes	yes	yes	yes
Municipality fixed effects				yes	yes	yes
R-squared	0.013	0.013	0.014	0.018	0.038	0.123
Observations	41,774	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 skill average is measured as the average of 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 also 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 database.

Table 3: Addressing measurement error

	Ranks in percentiles (baseline)	Ranks in deciles	Binary CSA indicator			
			All	w/o 5 ranks	w/o 10 ranks	w/o 15 ranks
	(1)	(2)	(3)	(4)	(5)	(6)
Parent comparative skill advantage	0.094	0.092	0.065	0.079	0.091	0.103
	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)	(0.008)
Further controls	yes	yes	yes	yes	yes	yes
R-squared	0.016	0.017	0.008	0.011	0.014	0.016
Observations	41,774	41,774	41,774	33,478	27,099	21,521

Notes: Least squares regressions. Sample: All matched parent-children observations in the three education cohorts. Col. (1) replicates the baseline model from col. (4) of Table 2. Comparative skill advantages of children and parents are measured in decile ranks in col. (2) and as binary variables in col. (3), taking a value of one if the percentile rank in math skills is equal or larger than the percentile rank in language skills, and zero otherwise. Using the indicators of comparative skill advantages from col. (3), col. (4), (5), and (6) drop parents who are in the range of 5, 10, or 15 percentile positions in the difference between math and language skills, respectively. For children, ranks are calculated in full sample of children taking the test in each test year; for parents, ranks are calculated in full sample of parents and nonparents in an education cohort. Further controls include grandparent education, grandparent social background, and municipality fixed effects (all referring to the time when parents took the skill test). All regressions also 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 4: Intergenerational transmission of comparative skill advantages (IV)

	OLS model	First stage IV	Reduced form	Second stage IV	
	(1)	(2)	(3)	(4)	(5)
Parent comparative skill advantage	0.083			0.106	0.110
	(0.009)			(0.046)	(0.047)
Classroom comparative skill advantage		0.290	0.031		
		(0.019)	(0.013)		
Further controls					yes
F-statistic excluded instrument				225.01	212.58
R-squared	0.01	0.09	0.002	0.01	0.02
Observations	12,268	12,268	12,268	12,268	12,268

Notes: Least squares and two-stage least squares regressions. Sample: All matched parent-children observations in the education cohorts of 1982 and 1989. 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 col. (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 col. (2). Col. (1) replicates least squares model from col. (1) of Table 2 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. Further controls include grandparent education and grandparent social background (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 5: Parents' comparative skill advantage and STEM choices of children

	Child (survey) STEM profile	Child (survey) STEM profile	Child (survey) STEM field of study	Child (survey) STEM field of study
	(1)	(2)	(3)	(4)
	Panel A: Full sample			
Parent comparative skill advantage (/10)	0.0078	0.0090	0.0057	0.0054
	(0.0014)	(0.0015)	(0.0013)	(0.0013)
Further controls	no	yes	no	yes
Outcome mean	0.439	0.439	0.338	0.338
R-squared	0.002	0.016	0.001	0.011
Observations	28,665	28,665	28,665	28,665
	Panel B: Male sample			
Parent comparative skill advantage (/10)	0.0088	0.0090	0.0080	0.0070
	(0.0020)	(0.0020)	(0.0019)	(0.0020)
Further controls	no	yes	no	yes
Outcome mean	0.527	0.527	0.438	0.438
R-squared	0.002	0.019	0.003	0.026
Observations	14,358	14,358	14,358	14,358
	Panel C: Female sample			
Parent comparative skill advantage (/10)	0.0069	0.0092	0.0032	0.0041
	(0.0019)	(0.0019)	(0.0016)	(0.0017)
Further controls	no	yes	no	yes
Outcome mean	0.351	0.351	0.238	0.238
R-squared	0.004	0.025	0.002	0.014
Observations	14,307	14,307	14,307	14,307

Notes: Least squares regressions. Sample: Children of individuals in the first survey cohort (1977) for whom we observe both their course- and study profile choice. Dependent variables: Binary variable indicating the choice of a STEM (Science, Technology, Engineering, and Mathematics) course profile at secondary school in col. (1) and (2); binary variable indicating the choice of a STEM field of study after secondary school in col. (3) and (4). Students are designated as following a STEM-course profile if they take the Technical or Agriculture course profile (low academic track) or the Nature & Technical or Nature & Health 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, Agriculture, as well as Medicine and Nursery were classified as a STEM choice of study. Baseline values are calculated based on observations with non-missing information on STEM choices. Further controls include grandparent education and grandparent social background (referring to the time when parents took the skill test), as well as fixed effects for the parent municipality-of-residence (measured at the time of test-taking). All regressions additionally control for parent gender, parent migration background, number of siblings of parents, and age of grandparents at the time of parent birth. Standard errors (in parentheses) are clustered at the parent level. *Data sources*: Administrative data; pooled ITS survey database.

Online Appendices

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

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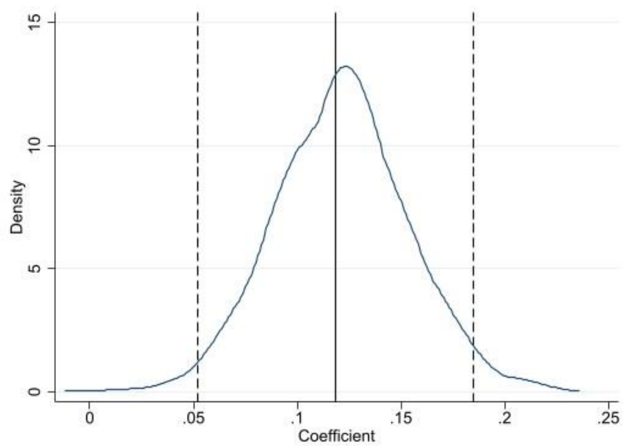
A.1 Appendix for Section 2.2: ITS Data

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% of the bootstrapped coefficients are within the 95% 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.[51]

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% confidence interval. *Data sources:* Administrative data; pooled ITS survey dataset.

[51] 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).

Table A1: Descriptive statistics

Variable		Pooled	Cohort 1977	1982	1989
		(1)	(2)	(3)	(4)
Child Characteristics					
Math rank	Mean	51.71	53.80	50.61	46.62
	SD	28.06	27.87	28.05	28.00
Language rank	Mean	52.57	55.03	51.21	46.76
	SD	28.00	27.62	28.05	28.13
Comparative skill advantage	Mean	-0.86	-1.23	-0.60	-0.14
	SD	22.86	23.30	22.41	22.20
Course profile	STEM	0.36	0.37	0.36	0.30
	Non-STEM	0.48	0.47	0.49	0.52
Field of study	STEM	0.25	0.29	0.23	0.14
	Non-STEM	0.51	0.59	0.47	0.33
Gender	Female	0.50	0.50	0.51	0.51
Parent Characteristics					
Math rank	Mean	50.33	53.94	47.21	44.00
	SD	28.28	27.61	28.69	27.92
Language rank	Mean	50.26	54.09	47.65	42.16
	SD	28.53	27.87	28.81	27.92
Comparative skill advantage	Mean	-0.07	0.15	0.44	-1.83
	SD	25.10	23.92	27.39	24.22
Classroom math rank	Mean	49.48	n/a	54.04	45.34
	SD	28.80	n/a	28.79	28.18
Classroom language rank	Mean	49.61	n/a	53.07	46.46
	SD	28.33	n/a	28.22	28.05
Classroom comparative skill adv.	Mean	-0.13	n/a	0.97	-1.12
	SD	17.88	n/a	22.90	11.46
Personal income percentile	Mean	63.29	66.36	61.67	55.72
	SD	28.84	28.77	28.65	27.79
Household income percentile	Mean	72.50	74.38	72.18	66.54
	SD	21.84	21.54	21.64	22.18
Household wealth percentile	Mean	58.08	63.29	56.05	43.42
	SD	25.86	24.82	25.33	24.51
Gender	Female	0.53	0.48	0.57	0.63
Education	Low	0.24	0.21	0.25	0.30
	Medium	0.44	0.48	0.41	0.40
	High	0.25	0.28	0.25	0.17
Migration background	Yes	0.08	0.07	0.08	0.15
Number of siblings	0	0.06	0.06	0.05	0.05
	1	0.37	0.34	0.41	0.40
	2	0.28	0.30	0.26	0.23
	3+	0.23	0.25	0.21	0.19

continued on next page

Grandparent Characteristics					
Education	Primary education	0.19	0.14	0.26	0.20
	Lower secondary education	0.31	0.30	0.34	0.27
	Higher secondary education	0.29	0.33	0.19	0.34
	Tertiary education	0.17	0.19	0.14	0.14
Social background	Blue collar worker	0.28	0.28	0.29	0.28
	Employer – without staff	0.08	0.09	0.07	0.05
	Employer – with staff	0.05	0.06	0.05	0.04
	Lower white-collar worker	0.11	0.12	0.11	0.09
	Middle white-collar worker	0.19	0.21	0.16	0.17
	Professionals	0.12	0.13	0.11	0.12
	Other	0.13	0.12	0.13	0.21
Age at time of birth grandfather	Mean	30.57	31.47	29.76	29.06
Age at time of birth grandmother	Mean	27.99	28.81	27.36	26.42
Observations	Total number	41,774	22,417	12,930	6,427

Notes: Table reports means, SD, and shares for the pooled sample and by survey cohort. If neither mean nor SD is specified, the reported statistic refers to the share of the respective variable. 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. Comparative skill advantage is measured as the difference between the percentile ranks in math and language. Classroom skills are measured as the percentile rank of average test score of parents' classroom peers (leave-out mean) in full sample of parents and nonparents in an education cohort. Classroom comparative skill advantage is measured as the difference between the percentile ranks in math and language of parents' classroom peers. Children's gender, course profile, and field of study are taken from administrative data. Students are designated as following a STEM course profile if they take the Technical or Agriculture profile (low academic track) or the Nature & Technical or Nature & Health profile (middle/high academic track). STEM study choice is determined based on the 1-digit ISCED97 fields of education classification. Study programs in the Science, Mathematics and Computing, Engineering, Manufacturing and Construction, Agriculture, and Medicine and Nursery were classified as a STEM choice of study. Students who chose a 'combination' course profile, where its STEM-component is unknown, have been coded as non-STEM. Only students progressed far enough in the education system can be assigned a STEM/non-STEM profile/field of study. Parent personal income (from labor, owned companies, or social security benefits) is measured as percentile in the Dutch income distribution. Parent household income is measured as percentile in the Dutch distribution of yearly spendable income. Household wealth is measured as percentile in the Dutch distribution of the household's total wealth. Income and wealth data are taken from the administrative data in the child's test-taking year. Parent education is measured as the highest educational degree obtained by the parent observed in the survey data; "low" denotes maximum lower secondary education (ISCED 1 or 2); "medium" denotes higher secondary or upper secondary vocational education (ISCED 3 or 4); "high" denotes tertiary education, consisting of higher vocational education and university (ISCED 5 and above). Grandparent education is the highest level of education of both grandparents. Social background is based on the occupation type of the main breadwinner in the parent household at the time of the parent's skill assessment. The "other" category includes, among others, grandparents who are unemployed, pensioned, disabled, or work in their household. For exposition, mean age of grandparents at the time of the parent's birth is shown; in the regressions, we control for the following age groups: below 21, 21-25, 26-30, 31-35, 36-40, 41+. Other than income and wealth, all (grand-)parent characteristics stem from the survey data. (Grand-)parent characteristics are reported at the child level. Statistics on missing categories are not reported. *Data sources:* Administrative data; pooled ITS survey database.

Early Life Assessments of Cognitive Skills and Long-Run Outcomes

We validate our early-life math and language skill measures with Dutch economic performance data. For the parent generation, we link test scores in math and language assessed around age 12 to administrative records on wages, household income, and household wealth measured 30 years later (i.e., in 2007 for 1977 cohort; in 2012 for 1982 cohort; in 2019 for 1989 cohort). Table A2 reports results of three specifications of regression models for five different long-run outcomes in the parental generation. Regression models in panel A (panel B) include only math (language) skills, while both skills are included simultaneously in panel C. All regressions control for a rich set of covariates for family background, measured at the time of the skill assessment (see Section 4).

The results demonstrate that the level of both early math and early language skills are strongly and consistently related to long-run success measured by educational attainment, hourly earnings, personal income, household income, and household wealth. In terms of magnitude, the wage returns to math (language) skills are very similar to the estimates for grade 6 test scores reported in Chetty, Friedman, and Rockoff (2014). Importantly, when both skill domains are jointly analyzed, math and language skills are independently significant in determining future educational and labor market outcomes.

These correlations between test scores at school and economic outcomes in adulthood clearly show that our measures of cognitive skills are economically meaningful. An equal-percentile move in math performance systematically has a larger impact on economic outcomes compared to a language move, but both skills independently contribute to outcomes despite their high correlation. Since information on later life outcomes is obtained from reliable administrative records, the strong correlations of our test score measures with these outcomes also lessen concerns about measurement error in the parent skill measures.

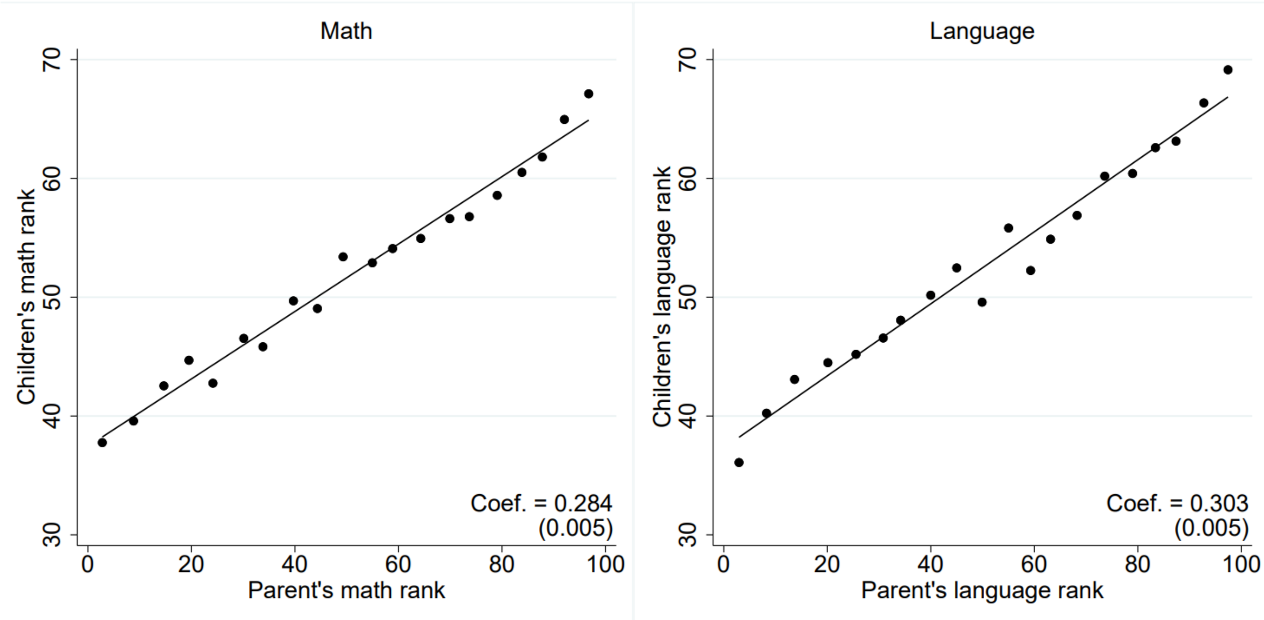
Table A2: Parent cognitive skills and long-term outcomes

	Higher education	Log hourly wage	Personal income (Percentile)	Household income (Percentile)	Household wealth (Percentile)
	(1)	(2)	(3)	(4)	(5)
	Panel A: Math				
Math skill rank	0.0049	0.0039	0.187	0.140	0.179
	(0.0001)	(0.0001)	(0.004)	(0.004)	(0.004)
Further controls	yes	yes	yes	yes	yes
R-squared	0.236	0.276	0.316	0.079	0.133
Observations	61,756	41,928	53,099	55,320	53,963
	Panel B: Language				
Language skill rank	0.0047	0.0035	0.160	0.110	0.140
	(0.0001)	(0.0001)	(0.004)	(0.004)	(0.004)
Further controls	yes	yes	yes	yes	yes
R-squared	0.229	0.262	0.307	0.070	0.119
Observations	61,756	41,928	53,099	55,230	53,963
	Panel C: Math and language				
Math skill rank	0.0033	0.0029	0.143	0.115	0.147
	(0.0001)	(0.0001)	(0.005)	0.005	(0.005)
Language skill rank	0.0028	0.0018	0.077	0.044	0.055
	(0.0001)	(0.0001)	(0.005)	(0.005)	(0.005)
Further controls	yes	yes	yes	yes	yes
R-squared	0.255	0.286	0.320	0.080	0.135
Observations	61,756	41,928	53,099	55,230	53,963

Notes: Least squares regressions. Sample: Pooled sample of all individuals (parents and nonparents) in the three survey cohorts. All wage, income, and wealth variables are measured 30 years after the skill assessment took place (i.e., 2007 for 1977 cohort; 2012 for 1982 cohort; 2019 for 1989 cohort); higher education degree completion is based on the highest educational degree obtained by the individual observed in the survey data. Dependent variables: Binary variable taking a value of 1 if surveyed individuals obtained a degree in higher vocational education or university education (col. 1); log gross hourly wage, trimmed at the 1st and 99th percentile (col. 2); personal income from labor, owned companies, or social security benefits, measured as percentile in the Dutch personal income distribution (col. 3); sum of the personal incomes of all household members, measured as percentile in the Dutch distribution of yearly spendable household income (col. 4); household assets minus debts, measured as percentile in the Dutch distribution of total household wealth (col. 5). Individuals' cognitive skills are measured as the percentile ranks of test scores in the full sample in each survey cohort. All regressions control for individual's gender, migration background, number of siblings, survey indicators, and municipality-of-residence fixed effects (measured at the time of test-taking). Regressions also control for education; social status, and age of individuals' parents at the time of the skill assessment (age refers to individuals' birth). Standard errors (in parentheses) are clustered at the individual level. *Data sources*: Administrative data; pooled ITS survey database.

A.2 Appendix for Section 5.1: 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 A3: 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 col. (1); language skills of children in col. (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 A4: Estimates of intergenerational skill transmission for each cohort separately

	Pooled	Cohort 1977	1982	1989
	Panel A: Math			
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 A5: Coefficients on control variables in the least squares model (Table 2, Col. 4)

Variables	(1)	Variables	(2)
Parent comparative skill advantage	0.094	*Grandparent characteristics*	
	(0.005)	Age grandfather at time of parent birth: 21-25	0.682
Parent characteristics			(1.176)
Female	0.936	Age grandfather at time of parent birth: 26-30	0.310
	(0.258)		(1.200)
Migrant	-0.208	Age grandfather at time of parent birth: 31-35	0.544
	(0.444)		(1.232)
Number of siblings: 1	-0.090	Age grandfather at time of parent birth: 36-40	0.204
	(0.533)		(1.289)
Number of siblings: 2	-0.328	Age grandfather at time of parent birth: 41+	0.102
	(0.547)		(1.376)
Number of siblings: 3 or more	0.885	Age grandmother at time of parent birth: 21-25	-0.851
	(0.566)		(0.635)
Grandparent education		Age grandmother at time of parent birth: 26-30	-0.840
Grandparent education: lower secondary	-0.655		(0.684)
	(0.372)	Age grandmother at time of parent birth: 31-35	-1.647
Grandparent education: upper secondary	-0.762		(0.764)
	(0.399)	Age grandmother at time of parent birth: 36-40	-0.589
Grandparent education: tertiary	-1.520		(0.891)
	(0.503)	Age grandmother at time of parent birth: 41+	-1.346
Grandparent social background			(1.241)
Blue-collar worker	-1.721		
	(0.535)		
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)		
Other	-1.771		
	(0.606)		
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. Coefficients on missing categories are not reported. 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 A6: 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 A7: 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 col. (1) is low education (at most lower secondary); omitted category in col. (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 A8). 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 A9). 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.[52]

Our simple analysis of mechanisms has two important caveats. First, interpreting the results in Table A9 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)).[53]

[52] 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.

[53] 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 A9.

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 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 A8: Potential mediators of intergenerational transmission of comparative skill advantages

	Parent higher education	Parent income (Percentile)	Household income (Percentile)	Household wealth (Percentile)
	(1)	(2)	(3)	(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 (col. 1). Parent income from labor, owned companies, or social security benefits, measured as percentile in the Dutch personal income distribution in the child's test-taking year (col. 2). Sum of the personal incomes of all household members, measured as percentile in the Dutch distribution of yearly spendable household income in the child's test-taking year (col. 3). Household wealth (i.e., assets minus debts), measured as percentile in the Dutch distribution of total household wealth in the child's test-taking year (col. 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 A9: 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. Parent income from labor, owned companies, or social security benefits, measured as percentile in the Dutch personal income distribution in the child's test-taking year. Household income is the sum of the personal incomes of all household members, measured as percentile in the Dutch distribution in terms of yearly spendable household income in the child's test-taking year. Household wealth (i.e., assets minus debts) is measured as percentile in the Dutch distribution of total household wealth in the child's test-taking year. Missing values for parent education (3.5%), parent income (6.7%), household income (1.5%), and household wealth (11.5%) 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 5.3: 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 10^{th} percentile and a class size of 29 students to the 90^{th} percentile of the class-size distribution in the 1989 cohort.[54] 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

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

classrooms and denomination does not uniquely identify schools.[55] 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% 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

[55] 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 A10, we show that the IV results are robust to various other ways of constructing the instrument. All estimates of parents' comparative skill advantage in col. (1) to (6) are not statistically significantly different from each other. In col. (1), we report our baseline estimate. In col. (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. Col. (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 col. (4), we construct the dummy instrument using absolute (i.e., level) differences in leave-out means instead of differences in ranks. Col. (5) directly uses the absolute differences in leave-out means as an instrument, which again implies making a linearity assumption. Finally, col. (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 A10: 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: Col. (1): Difference between the percentile ranks of classroom peers in math and language within a parent's education cohort; col. (2): difference between the percentile ranks of full classroom in math and language within a parent's education cohort; col. (3): Binary indicator for higher ranked classroom peers (math vs. language) within the parent's education cohort; col. (4): Binary indicator for better performing classroom peers (math vs. language); col. (5): Test scores in math and language of classroom peers; col. (6): Like col. (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).

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 generations because of endogenous sorting of children within schools. Table A11 shows that the IV estimates are robust to controlling for skill differences between math and language of children's classroom peers. In Table A12, we replicate this analysis in one-classroom schools. 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 A13, 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 col. (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 col. (5) of Table 4.[56]

In col. (2) of Table A13, 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 A13, we restrict the sample even further to child-parent matches where children attend a school that is at least 50 (col. 3) or 100 (col. 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 col. (2) (p=0.214) and col. (5) (p=0.282).

[56] 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 A11: Controlling for children's school quality

	OLS model	First stage IV	Reduced form	Second stage IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Parent comparative skill advantage	0.082			0.092	0.097	0.096
	(0.009)			(0.044)	(0.045)	(0.046)
Classroom comparative skill advantage		0.289	0.027			
		(0.019)	(0.013)			
Children's school quality	0.147	0.017	0.148	0.147	0.143	
(ranks)	(0.012)	(0.014)	(0.012)	(0.012)	(0.012)	
Children's school quality						13.218
(absolute)						(1.022)
Further controls					yes	yes
F-statistic excluded instrument				224.91	211.67	211.73
R-squared	0.02	0.04	0.02	0.02	0.03	0.03
Observations	12,241	12,241	12,241	12,241	12,241	12,241

Notes: Least squares and two-stage least squares regressions. Sample: All matched parent-children observations in the education cohorts of 1982 and 1989; 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 col. (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 col. (2). Col. (1) replicates baseline least squares model (see col. 1 of Table 2) 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 A12: 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 A11 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 col. (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 col. (2). Col. (1) replicates baseline least squares model (see col. (1) of Table 2) 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 A13: 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: Col. (1): Excluding children who attend the same school and whose parents have been classmates in the education cohorts of 1982 and 1989; col. (2): as in col. (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; col. (3) (col. 4): as in col. (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); col. (5): as in col. (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 col. (2) and (5) contain only children with a valid municipality or province identifier (92.06% of the total IV sample). Results in col. (3) and (4) contain only children and parents with available municipality longitude and latitude coordinates (88.52% 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).

Table A14: Grandparental background characteristics and parental classroom skills

	Class skill difference	Class math skills	Class lang. skills
	(1)	(2)	(3)
Grandparent education			
Grandparent education: lower secondary	0.409	6.769	6.360
	(0.633)	(0.953)	(0.937)
Grandparent education: upper secondary	-0.926	9.460	10.386
	(0.650)	(1.072)	(1.075)
Grandparent education: tertiary	-1.053	16.816	17.869
	(0.856)	(1.403)	(1.366)
Grandparent social background			
Blue-collar worker	0.238	-1.642	-1.881
	(1.237)	(1.935)	(1.771)
Employer with staff	-0.042	2.702	2.744
	(1.443)	(2.072)	(1.970)
Lower white-collar worker	-0.501	1.214	1.715
	(1.522)	(2.095)	(1.975)
Middle white-collar worker	0.496	4.234	3.738
	(1.369)	(2.101)	(2.007)
Professionals	-0.117	6.127	6.245
	(1.355)	(2.187)	(2.087)
Other	-0.509	-4.903	-4.394
	(1.266)	(1.987)	(1.886)
Grandparent age			
Age grandfather at time of parent birth: 21-25	1.700	0.193	-1.507
	(1.778)	(2.792)	(2.742)
Age grandfather at time of parent birth: 26-30	1.659	1.649	-0.011
	(1.857)	(2.847)	(2.807)
Age grandfather at time of parent birth: 31-35	2.096	2.848	0.752
	(2.005)	(2.937)	(2.853)
Age grandfather at time of parent birth: 36-40	1.992	3.225	1.233
	(2.000)	(3.127)	(3.025)
Age grandfather at time of parent birth: 41+	3.896	5.895	1.999
	(2.366)	(3.412)	(3.292)
Age grandmother at time of parent birth: 21-25	1.002	4.551	3.549
	(0.846)	(1.365)	(1.355)
Age grandmother at time of parent birth: 26-30	0.750	5.943	5.193
	(0.946)	(1.492)	(1.494)
Age grandmother at time of parent birth: 31-35	-0.546	5.819	6.365
	(1.051)	(1.813)	(1.795)
Age grandmother at time of parent birth: 36-40	-0.875	5.052	5.927
	(1.516)	(2.331)	(2.325)
Age grandmother at time of parent birth: 41+	-0.835	3.632	4.467
	(2.428)	(3.895)	(3.699)
R-squared	0.006	0.103	0.103
Observations	8,011	8,011	8,011
Clusters	1,138	1,138	1,138
F-statistic all coefficients	0.97	16.15	16.58

Notes: Least squares regressions. Sample: All parent observations used in the IV regressions. Dependent variable: Classroom comparative skill advantage, measured as the difference between the percentile ranks in math and language of parents' classroom peers in an education cohort (col. (1)), classroom leave-out-mean math or language percentile rank (col. (2) and (3)). Omitted grandparent categories: education: primary; social background: employer without staff; age at time of parent birth: 20 years or lower. Coefficients on missing categories are not reported. All regressions control for parent survey indicators. Standard errors clustered at the classroom level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

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 A15 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 (col. 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 col. (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 col. (3), the IV estimate on parent comparative skill advantage in this subsample even tends to be somewhat larger than the baseline estimate. Col. (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 col. (5) and (6) of Table A15, 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 A15: 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: Col. (1): All matched parent-children observations in the education cohorts of 1982 and 1989; col. (2): Matched parent-children observations form rural schools in the education cohorts of 1982 and 1989; col. (3): Matched parent-children observations from schools with exactly one classroom in the education cohorts of 1982 and 1989; col. (4): Matched parent-children observations from rural schools with exactly one classroom in the education cohorts of 1982 and 1989; col. (5): All matched parent-children observations in the education cohort of 1982; col. (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).

Table A16: Child skills and grade peer skills over the course of primary education

	Grade 1	Grade 2	Grade 3	Grade 4	Grade 5
	(1)	(2)	(3)	(4)	(5)
	Leave-out-mean classroom CSA				
	Panel A: Full sample				
Child CSA in grade 1	0.180	0.040	0.023	0.009	-0.004
	(0.008)	(0.006)	(0.005)	(0.005)	(0.004)
R-squared	0.054	0.044	0.002	0.001	0.005
Observations	61,243	61,243	61,243	61,243	61,243
	Panel B: Movers				
Child CSA in grade 1		0.028	0.019	-0.009	0.009
		(0.032)	(0.019)	(0.015)	(0.012)
R-squared		0.015	0.004	0.001	0.003
Observations		750	1,614	2,343	2,904
	Leave-out-mean classroom math skills				
	Panel A: Full sample				
Child math skills in grade 1	0.318	0.194	0.164	0.155	0.145
	(0.012)	(0.013)	(0.012)	(0.013)	(0.012)
R-squared	0.102	0.045	0.025	0.023	0.023
Observations	61,243	61,243	61,243	61,243	61,243
	Panel B: Movers				
Child math skills in grade 1		0.146	0.151	0.147	0.148
		(0.037)	(0.027)	(0.025)	(0.025)
R-squared		0.020	0.023	0.020	0.022
Observations		750	1,614	2,343	2,904
	Leave-out-mean classroom reading skills				
	Panel A: Full sample				
Child reading skills in grade 1	0.327	0.214	0.203	0.195	0.188
	(0.013)	(0.013)	(0.012)	(0.013)	(0.013)
R-squared	0.102	0.046	0.036	0.036	0.033
Observations	61,243	61,243	61,243	61,243	61,243
	Panel B: Movers				
Child reading skills in grade 1		0.123	0.149	0.162	0.164
		(0.039)	(0.029)	(0.026)	(0.024)
R-squared		0.019	0.019	0.022	0.024
Observations		750	1,614	2,343	2,904

Notes: Least squares regressions. Sample: Dutch primary school students for who we observe their end-of-year test scores in both math and reading from grade 1 to grade 5 in the period 2013 to 2021 (Panel A) and a subsample of these students who switched schools between grades 1 and 2 (col. (2), 1 and 3 (col. (3), 1 and 4 (col. (4), or 1 and 5 (col. (5) (Panel B). Dependent variable: Classroom comparative skill advantage, measured as the difference between the percentile ranks in math and reading of children's classroom peers within a school year (upper part); classroom math or reading skills, measured in percentile ranks of children's classroom peers within a school year (middle and lower part). All regressions control for grade 1 school year dummies. *Data sources:* Administrative data, National Cohort Study.

A.4 Appendix for Section 6: STEM Definition

Students are designated as following a STEM-course profile if they take the Technical or Agriculture course profile (low academic track) or the Nature & Technical or Nature & Health 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, Agriculture, as well as Medicine and Nursery were classified as a STEM choice of study.

Table A17 considers a narrower definition of STEM, defining course profiles and study programs in the agricultural and medical fields as non-STEM. Results are robust to applying this more restrictive definition. While effect heterogeneity by gender gets more pronounced, this partly reflects the lower baseline probabilities of women choosing these narrowly defined STEM fields.

Table A17: Parents' comparative skill advantage and STEM choices of children – Narrow STEM definition

	Child (survey) STEM profile	Child (survey) STEM profile	Child (survey) STEM field of study	Child (survey) STEM field of study
	(1)	(2)	(3)	(4)
	Panel A: Full sample			
Parent comparative skill advantage (/10)	0.0056	0.0064	0.0049	0.0046
	(0.0012)	(0.0012)	(0.0011)	(0.0011)
Controls				
Outcome mean	0.250	0.250	0.221	0.221
R-squared	0.001	0.014	0.001	0.010
Observations	28,665	28,665	28,665	28,665
	Panel B: Male sample			
Parent comparative skill advantage (/10)	0.0088	0.0095	0.0083	0.0079
	(0.0019)	(0.0020)	(0.0018)	(0.0019)
Further controls				
Outcome mean	0.379	0.379	0.364	0.364
R-squared	0.003	0.020	0.003	0.023
Observations	14,358	14,358	14,358	14,358
	Panel C: Female sample			
Parent comparative skill advantage (/10)	0.0025	0.0032	0.0015	0.0014
	(0.0012)	(0.0013)	(0.0010)	(0.0010)
Further controls				
Outcome mean	0.120	0.120	0.078	0.078
R-squared	0.001	0.016	0.000	0.018
Observations	14,307	14,307	14,307	14,307

Notes: Least squares regressions. Sample: Children of individuals in the first survey cohort (1977) for whom we observe both their course- and study profile choice. Dependent variables: Binary variable indicating the choice of a STEM (Science, Technology, Engineering, and Mathematics) course profile at secondary school in col. (1) and (2); binary variable indicating the choice of a STEM field of study after secondary school in col. (3) and (4). 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. Further controls include grandparent education and grandparent social background (referring to the time when parents took the skill test), as well as fixed effects for the parent municipality-of-residence (measured at the time of test-taking). All regressions additionally control for parent gender, parent migration background, number of siblings of parents, and age of grandparents at the time of parent birth. Standard errors (in parentheses) are clustered at the parent level. *Data sources*: Administrative data; pooled ITS survey database.