CESifo

# AREA CONFERENCES 2021

## Economics of Education

**Munich, 3–4 September 2021**

# The Intergenerational Transmission of Skills: An Investigation of the Causal Impact of Families on Student Outcomes

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

CESifo

# The Intergenerational Transmission of Skills: An Investigation of the Causal Impact of Families on Student Outcomes*

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

August 22, 2021

## Abstract

The extensive literature on intergenerational mobility has highlighted the importance of family linkages but has failed to provide credible evidence about the underlying family factors that drive the pervasive correlations. Employing a unique combination of survey and Dutch registry data that links math and language skills across generations, we can identify the causal connection between parent-child skills. The upward bias in purely descriptive analyses of intergenerational persistence is as large as 60 percent. The between-subject estimation is robust to a range of dynastic differences among families. The data also permit novel IV estimation of the key intergenerational mobility parameter, which demonstrates the influence of school and peer quality on lasting educational outcomes. Finally, we show the strong influence of family skill transmission on subsequent choices of STEM fields.

Keywords: intergenerational mobility, parent-child skill transmission, causality, STEM

JEL classification: I24, I26, J12, J24, J62

---

*Acknowledgements: This paper is part of the Intergenerational Transmission of Skills (ITS) project carried out at the Research Centre for Education and the Labour Market (ROA: www.roa.nl). We gratefully acknowledge a grant received from the Dutch Ministry of Education, Culture and Science and the Netherlands Initiative for Education Research (NRO: grant 405-17-900).

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

# 1. Introduction

Understanding the fundamental causes of the persistence of economic outcomes across generations remains one of the most important topics of social science research. The existing research has taken both micro and macro perspectives, investigating not only equity of opportunities but also implications for overall educational and economic efficiency. A particularly important component of this research is the role of the education sector in furthering or breaking inertia in economic mobility. But a continuing analytical problem is separating causal influences from those reflecting selection and other determinants of intergenerational mobility. The development of a rich, new data set about the detailed transmission of cognitive skills within families enables us to make direct causal inferences about how status of family outcomes is preserved.

Much of the early work on intergenerational transmission of economic outcomes focused on obtaining valid estimates of the correlation of father and son incomes (e.g., Solon (1999), Haider and Solon (2006)). This task was difficult because of age differences at time of measurement, difficulties in estimating permanent incomes, and interpretative questions about the source of correlations. A natural evolution of these mobility investigations was an intensive focus on the correlations of educational attainment across generations, building on the extensive research into the value of human capital investments (Black and Devereux (2011), Björklund and Salvanes (2011)). This line of study has dealt extensively with some of the parent-offspring measurement issues and has opened the way for a variety of approaches illuminating the causal structure of the patterns.

Two concerns remain, however, in interpreting the intergenerational transmission of economic status in previous work. First, its frequent focus on school attainment proves to be problematic because this is a coarse and potentially misleading measure of economic capacity.[1] Second, even when identifying the causal aspects of correlations for school attainment, these studies leave a variety of interpretative questions about the more primitive sources – nature, nurture, neighborhoods, teachers, and a range of factors bundled in school completion.

[1] See, for example, the discussions in Hanushek and Woessmann (2008) and Hanushek, Schwerdt, Wiederhold, and Woessmann (2015, 2017).

This paper pursues a novel approach to identifying the causal impact of parent skills on children's skills. Using unique linked data, we estimate directly how the skills of parents – measured by math and language tests – affect the skills of their children on similar tests. The estimation exploits within-parent within-child variation in skills for identification, providing plausibly causal estimates of the key intergenerational skill transmission parameter.

For this analysis, we develop the Intergenerational Transmission of Skills (ITS) database.[2] The ITS database matches extensive data on parent skills in math and language at age 13 with register data from the Netherlands on their children's skills in the same subjects at approximately the same age (Jacobs, van der Velden, and Vermeulen (2021)). The survey data underlying the ITS database cover three cohorts of parents sampled when they were 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 between 17,000 (1982) and 37,000 (1977) students entering Dutch secondary education. The linkage to registry data minimizes the problems of sample attrition that plague attempts to investigate intergenerational linkages with survey-based panels (e.g., Brown, McIntosh, and Taylor (2011); de Coulon, Meschi, and Vignoles (2011)). Extensive additional information on grandparent characteristics such as highest level of education, social position, and household structure further permits a deeper look into dynastic effects (Adermon, Lindahl, and Palme (2021)).

By looking at how differences in a child's skills between math and language relate to their parents' differences in math and language, all observed and unobserved influences of family, school, and neighborhood that do not differentially affect the two skill domains are eliminated. Because the tests are taken during the same age period, a common concern about age-related measurement error in the literature on transmission of status is eliminated. Because of the different dimensions of achievement, skill transmission is well-identified, eliminating a host of confounding other aspects of background that have inhibited any causal interpretation of prior investigations of family transmission of status. And, because of the sampling of classmates of parents, it is possible to investigate the malleability of skills and to provide evidence about pre-birth versus post-birth aspects of skill transmission.

[2] The current paper is one of two inaugural papers for the ITS project. The companion paper is a sociological analysis by Jacobs and van der Velden (2021). They 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 between-subject models of skill transmission show that the skills of parents are strongly associated with the skills of their offspring. In terms of magnitude, a one-standard-deviation increase in parent skills increases the skills of children by 0.1 standard deviations. This estimate is about 60 percent lower than the simple subject-specific estimates, suggesting that not eliminating unobserved family heterogeneity leads to severely upward biased estimates of the skill transmission. Our estimates of the key intergenerational skill transmission parameter are consistent across a wide range of sensitivity and robustness tests that relax estimation assumptions. In particular, the striking stability of our results when accounting for various (grand-)parent characteristics suggests that remaining unobserved variables are unlikely to confound our estimates.

In unique instrumental variables (IV) estimations, we can also eliminate concerns of bias from dynastic patterns in genetic traits. We isolate determinants of parent skills outside the family by exploiting information about subject-specific achievement of the parents' classroom peers. Estimates derived from just the parent skill variation based on peer effects or school quality differences are very similar to those obtained from the total variation in parent skills in the between-subject model – thus, reinforcing a causal interpretation. Our IV results also demonstrate that exogenous policies that improve school quality will have lasting impact on family outcomes through the transmission of higher skills to children.

Differential math skills of parents also have an influence on the long run path of children. The relative pattern of parent skills directly relates to STEM choices of children both at school and after school, which we can observe in our administrative data. Interestingly, there is no gender bias in how parent skills relate to STEM choices.[3]

The next section provides an overview of the basic research on intergenerational mobility that is relevant to this paper. Section 3 describes the ITS data set, and Section 4 develops the empirical models. Section 5 presents the basic intergenerational transmission estimates. Section 6 discusses the IV model that exploits variation in parent skills explained by classroom quality. Section 7 shows that parent skills affect children's actual STEM choices. Section 8 concludes.

[3] Considerable academic and worldwide policy attention has focused on expanding students in science, technology, engineering, and math (STEM) fields of study and occupations (Stoet and Geary (2018), UNESCO (2017)). The role of families has received little attention (see, for example, the review in Altonji, Arcidiacono, and Maurel (2016))).

## 2. Dimensions of Intergenerational Mobility

An important perspective that underlies much of the relevant prior work is that parents have a huge influence on the subsequent success of their offspring, and patterns of economic success across families tend to be maintained over time. Economic mobility across generations then depends importantly on just how strong the influence of families is on child outcomes. If society has an interest in promoting economic opportunity and in furthering economic mobility, the possibilities depend importantly on the role of malleable factors, generally outside of the family, in determining child outcomes.

A straightforward analysis from this perspective is to look at a simple Galton (1889) regression such as

$$y^c = \alpha + \beta y^p + \nu \tag{1}$$

where $y^c$ and $y^p$ are measures of relevant economic outcomes of children (c) and parents (p) and $\nu$ denotes other influences. The focus in a variety of different versions of this relationship is how large $\beta$ and $\nu$ are. Although not relevant empirically, if $\nu \equiv 0$, parents are replicated in their children, and there is no intergenerational mobility.

The key parameter of interest in most existing applications is $\beta$, the measure of intergenerational persistence. Heuristically, the larger $\beta$ is the more family determines child outcomes, leading the empirical analysis to center on obtaining precise estimates of $\beta$.[4]

The general topic of family persistence in outcomes has been extensively researched, going back over a century. Economists have been heavily involved in the recent development of the field as related to economic outcomes, and there are now several detailed surveys and evaluations of different aspects of this analysis. Here we review the overall line of research that leads up to our analysis of the causal impacts of families on intergenerational mobility.

The most straightforward analysis of intergenerational mobility is how the income of children relates to the income of their parents, and the early investigations of this illustrate the common lines of research (Solon (1999)). Starting from OLS estimates of Eq. 1, initial analyses focused on adjusting for a variety of empirical impediments to obtaining more precise estimates

[4] Depending on the measurement of $y^c$ and $y^p$, $\beta$ can be interpreted as the parent-child correlation or the elasticity of child outcomes with respect of parent status, and with suitable normalization (1- $\beta$) becomes a measure of the amount of economic mobility.

of the income persistence parameter, $\beta$. Specifically, consider the case the $y^c$ and/or $y^p$ are measured with error as in:

$$y^{c*} = y^c + \varsigma_c \tag{2}$$

$$y^{p*} = y^p + \varsigma_p \tag{3}$$

This is motivated by the fact that historically data sets linking adult children to their parents have been limited and have suffered from some common shortcomings. Instead of measuring lifetime earnings, readily available data frequently observed incomes for single ages. Moreover, observations of children and parents came at different points in the life cycle, leading to uncertainty about the full evolution of incomes. And there were questions about whether individual income or family income should be used and whether mother-daughter analyses led to similar results as the more common father-son relationships. Errors in $y^p$ will generally bias $\beta$ downward, while errors in $y^c$ lead to imprecise estimates of $\beta$ and perhaps biases depending on the distribution of these errors. The initial investigations centered largely on dealing with these measurement errors in the transmission of income from parent to child (Solon (1999), Björklund and Jäntti (2011)).

As an alternative, other analyses moved to persistence of education (almost exclusively measured by school attainment or years of schooling) instead of incomes (Björklund and Salvanes (2011)). Because of the strong linkage of education to earnings, a focus on persistence of education fits naturally into a perspective of equalizing economic opportunities for individuals. Analytically, concentration on the intergenerational transmission of education offers several advantages over the prior focus on persistence of income differences. First, measurement errors ($\varsigma_c, \varsigma_p$) are generally smaller. Second, observations at different points in the life cycle are less important because educational attainment stabilizes at relatively young ages. Third, data are widely available within and across countries. Interestingly, estimates of persistence in education across countries tend to match estimates of persistence in incomes (Björklund and Salvanes (2011)).

The most comprehensive work on intergenerational mobility has built upon the extensive registry data that have recently become available. These registries have not only provided clearer measurement over the life cycle of related generations but also have allowed a variety of extensions. The most complete investigation of educational persistence currently available demonstrates the power of using the extensive Swedish registry data: Adermon, Lindahl, and Palme (2021) expand measures of $y^p$ to cover education levels of extended families and show that the influence of full dynasties is considerably stronger than that of parents alone.

One significant extension of these persistence studies focuses on nature-nurture debates and attempts to separate the effects of family influences into a genetic (or fixed) component and an environmental component.[5] These studies are generally designed to decompose the variation in outcomes into that arising from variation in more fundamental components (Sacerdote (2011)) as in:

$$y^c = \alpha + \beta_G G + \beta_F F + \nu \tag{4}$$

where $G$ is the (constant) genetic component and $F$ is the family environment component.[6] The studies themselves approach the estimation from a range of perspectives, although a common approach is to decompose differences in outcomes among siblings based on genetic models of common inheritance. Thus, for example, comparisons of outcomes for brothers and sisters or for monozygotic and dizygotic twins provide estimates of the degree of common genetic influence. In related extensions, analysis of outcomes for separated siblings or for adopted children can help to break the variations in outcomes more accurately between common family factors and common genes. Under very strong assumptions, these provide causal estimates of the overall influence of families (Sacerdote (2011), Black and Devereux (2011), Adermon, Lindahl, and Palme (2021)).

The estimates of family influences from these sibling comparisons are open to several interpretive issues. First, the common estimates of the contribution to variance in outcomes from $F$ include not only the impacts of family background but also of any other experiences shared by the parent and child generation. Second, even if family inputs are the only relevant shared experience, these estimates do not indicate what factors in the family environment are driving the differences.

[5] There is a longer – and at times controversial – history of analyses of IQ differences and of the heritability of such differences. See, for example, the extended controversy surrounding Herrnstein and Murray (1994). For the most part these debates go beyond our analysis, although the analytical approaches underlying the debates are infused in some of the background literature referenced here.

[6] A better but nonstandard nomenclature from Adermon, Lindahl, and Palme (2021) is that $G$ represents pre-birth factors (mostly but not exclusively genetic) and $F$ is post-birth factors.

Most importantly, however, this entire line of analyses of status persistence, while providing solid descriptive pictures of the pattern of intergenerational mobility, does not provide causal evidence about the source of economic inertia in families. Specifically, these existing studies have significantly improved the measurement of $y^p$, thus reducing much of the downward bias in $\beta$ from measurement error, but they have not adequately identified the causal impact of parent income or education on the subsequent generation. A more general and more realistic model of intergenerational persistence would be:

$$y^c = \alpha + \beta y^p + \gamma \boldsymbol{X} + \nu \tag{5}$$

where $\boldsymbol{X}$ is a vector of other influences on $y^c$ that may be correlated with $y^p$ while having an independent influence on child outcomes. Incomes and education levels are correlated with a wide range of factors from school quality to neighborhood attributes to parenting approaches, entailing a correlation between outcomes across generations that cannot be interpreted causally. The prior refinements in the estimation of the persistence parameter $\beta$ do generally not solve the omitted variable problem.

As Black and Devereux (2011) note, extended analyses have developed IV estimates based on policy changes (e.g., the extension of compulsory schooling laws or exogenous changes in welfare programs), but they have not provided consistent estimates of the impact of specific family characteristics on child incomes and education. Indeed, more recent investigations have continued to yield inconsistent results (e.g., Dahl and Lochner (2012), Bleakley and Ferrie (2016)).

Another recent approach to the analysis of intergenerational mobility emphasizes geographical differences in mobility and draws on these differences to sort out some of the factors affecting mobility (Chetty, Hendren, Kline, and Saez (2014), Chetty and Hendren (2018)). This analysis employs administrative tax records to develop neighborhood differences in income mobility opportunities and concludes that neighborhoods have strong causal impacts on mobility.[7] This analysis nevertheless cannot say anything about any underlying family characteristics that enter into the patterns of intergenerational mobility.

[7] Mogstad and Torsvik (2021), however, raise questions about errors in the underlying estimation of neighborhood differences.

There are other lines of research that are not explicitly focused on intergenerational mobility but that are directly relevant to understanding the role of families in affecting the long-run outcomes of children. The extensive literature on child development provides insights into many early environmental factors that have long run implications. The most relevant portion is that focused on poverty in the early years of development and generally starts with a presumption of considerable persistence in poverty. An important part of this literature gives systematic attention to policies and programs that can alleviate poverty (e.g., Duncan and Le Menestrel (2019)), but the research has not provided clear results about the causal elements of families. A significant and growing portion of this child development literature is the analysis of early childhood investments including preschool experiences (Cunha, Heckman, and Schennach (2010), García, Heckman, Leaf, and Prados (2020), Gray-Lobe, Pathak, and Walters (2021)). Importantly, this area does not provide any general evidence on the causal impact of different aspects of the family that would relate to intergenerational mobility.

A second related line of inquiry investigates educational production functions and how families affect the skills of children. Beginning with the Coleman Report (Coleman et al. (1966)), the first large-scale quantitative study of skill formation in children, there has been ubiquitous recognition of the important role of family background in affecting student achievement. The general form of this analysis, which relates closely to our empirical analysis, is:

$$T^{c} = \alpha + \beta \boldsymbol{F} + \gamma \boldsymbol{S} + \eta \,. \tag{6}$$

Here, child outcomes are measured by test scores ($T^c$) and $\boldsymbol{F}$ and $\boldsymbol{S}$ are vectors of family attributes and school attributes, respectively. Yet, this study and follow-on research into the educational process has been almost exclusively concerned with the schooling inputs ($\boldsymbol{S}$) and has not addressed causal factors in families that yield these differences (Hanushek (2002)). Instead, it has stopped at descriptive studies that employ whatever measured family factors are available within each given dataset.

The production function literature has, however, introduced an additional dimension to the discussions. This literature has generally focused on student achievement and skills – as opposed to school attainment that has been central to the previous persistence analyses. The analysis of school attainment in the intergenerational analyses has been a pragmatic choice based on data availability, but its limitations are clear. For instance, the analysis of achievement leads to the simple and incontrovertible point that skills developed at any level of schooling vary widely. This finding dovetails with the evidence that skills are a noticeably better measure of labor market potential than the more traditional use of years of schooling (Hanushek, Schwerdt, Wiederhold, and Woessmann (2015, 2017), Hampf, Wiederhold, and Woessmann (2017)).[8] Through the labor market payoff to skills, the relationship between family inputs to the educational production process and intergenerational persistence becomes apparent.

Completing the circle, consideration of achievement impacts of families has slowly filtered into intergenerational mobility analyses but again in a descriptive form that has not clearly identified components of family influences. Two different studies use longitudinal British data to demonstrate significant correlations of parent and child achievement, but both caution against causal interpretation of their results (Brown, McIntosh, and Taylor (2011), de Coulon, Meschi, and Vignoles (2011)). Adermon, Lindahl, and Palme (2021) employ GPAs of Swedish children to introduce a qualitative schooling dimension, but the comparability of grades across schools is limited and no GPA data are available in the parent generation.

From the large body of existing research on intergenerational mobility there is no doubt that family inputs to child development are highly important in determining the long run outcomes of children. But the many descriptions of the correlational patterns fall short of identifying important causal factors driving the family transmission of income and status.

## 3. Data and Institutional Background

*The Dutch education system*

As our dataset is compiled from Dutch administrative data and survey data, we begin by providing brief institutional context. The Dutch education system is an early stratifying system (Bol and Werfhorst (2013)), where pupils are allocated to different tracks (low, middle, or high) in secondary education after primary education (grade 6, at age 12). This allocation is largely based on the performance of pupils on a national test at the end of primary education, the CITO

[8] Relatedly, measured skill differences across countries prove to be extraordinarily important in explaining cross-country differences in economic growth (Hanushek and Woessmann (2015)).

test (Central Institute for Test Development (CITO)) test.[9] CITO is a high-stakes test measuring school performance in math and language.[10] After two years (for students attending the low track) or three years (for students attending the middle or high track), students have to decide on a course profile that will determine the type of courses they can take in upper-secondary or tertiary education.[11] After graduating from secondary school, students can choose, depending on their track in secondary education, to enter upper secondary vocational education, tertiary vocational education or university, or the labor force.

*Data*

For this paper, we developed the Intergenerational Transmission of Skills (ITS) database. This database was constructed to be the foundation of an extensive research program on the intergenerational transmission of skills (for more information on this research program, see www.roa.nl). The construction of this unique database is extensively discussed in Jacobs, van der Velden, and Vermeulen (2021).

The ITS dataset is a combination of extensive survey data gathered in the 1970's and 1980's, containing the skill measures of the parent generation, linked to register data available at Statistics Netherlands, which include the skill measures of the children's generation. The combined dataset contains information on the math and language skills of 25,483 parents and 41,409 of their children. The survey data consist of two cohorts that were sampled in the first year of secondary education (1977 and 1989), and one cohort that was sampled in the last year of primary education (1982). Each of these longitudinal surveys are large, nationally representative panels of students (see Appendix A.4 for more details on the data).

[9] 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 pupils' success in secondary education.
[10] Before the 2014/15 school year, participation in the national test was not mandatory. However, around 85 percent of the schools in primary education have participated in the CITO test since its introduction in 1970. From 2014/2015 onwards, it is compulsory for pupils in grade 6 to take a final test. The government makes the CITO test available to all schools, but schools can also choose another final test approved by the Ministry of Education. Nonetheless, most schools participate in the CITO test (Jacobs, van der Velden, and van Vugt (2021)).
[11] In the low track, students can choose between four profiles: Technical, Agriculture, Economics, Health & Welfare, or a combination thereof or a general profile. In the middle and high tracks, students can choose between Nature & Technical, Nature & Health, Economics & Society, Culture & Society, or a combination thereof.

The math and language skills of the surveyed cohorts were assessed during the school year using a shortened version of the CITO test.[12] In addition, their parents (the grandparental generation in our analysis) filled in a survey answering background questions such as their highest level of education, socio-economic status, and number of children living at home. After the initial survey and assessment, individuals were followed annually over the course of their school career until leaving education. For most pupils in the original cohorts, basic identifying information is available including name and address at the time of the survey, allowing us to link these cohort data to register data from Statistics Netherlands. The data could be linked successfully in more than 80 percent of cases (1977 cohort: 81 percent; 1982 cohort: 88 percent). For the latest cohort a unique personal identifier made the linking process successful in 98 percent of the cases.  Parent test scores in each domain are standardized with a mean of zero and a standard deviation (SD) of one within each cohort, using the complete original dataset (i.e., parents and non-parents).

As indicated earlier, this CITO test is taken in the final year (grade 6) of primary education. Statistics Netherlands has register data of all schools that participated in the CITO test from school year 2005/2006 onwards. The latest data on the CITO test available is for the year 2018/2019, as the test was not taken in the COVID-19-year 2019/2020. Thus, it is possible to link the original cohort data to their children's test score information in the register data if they fall in this observation window. Test scores of children in each domain are standardized with a mean of zero and an SD of one within each test year in the full administrative data.[13]

Part of the ITS dataset is also administrative data providing detailed information on children's educational careers. More specifically, we observe children's STEM (Science, Technology, Engineering, and Mathematics) choices at school, which have important long-term consequences, as 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

[12] In the 1977 and 1982 cohorts, the 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.

[13] After the 2014/2015 school year other test suppliers became available. Since it might not be random which schools switched to a different test supplier (Jacobs, van der Velden, and van Vugt (2021)), the standardization is done based on the schools that participated in the CITO test every year. All results are robust to an alternative standardization based on the universe of schools.

upper secondary vocational or tertiary education directly. We separately code outcomes as either a STEM or non-STEM based on the type of courses taken and the subsequent field of study.[14]

The sample sizes and average skills of both parents and children differ by cohort, as seen in Table 1.[15] In total, we have 41,774 observations in our linked child-parent sample (we observe both parents for 365 children). The sample size differences across cohorts partly reflect the window of observed test-taking by children. We only observe those parents whose children took the CITO test at the end of primary school between 2006 and 2019. 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. 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 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 therefore tend to have slightly lower educational attainment and skills, while the parents from the second cohort (around aged 12 in 1982) fall somewhere in between. However, we show that our results hold in each individual cohort, so this sample selectivity has no major implications for our results.

Data on grandparent education, which we derive from the parent questionnaire in the cohort studies, provide additional information about the long run transmission of skills. For our main analysis, we take the highest level of obtained education of the grandmother or grandfather.[16] We again observe that our parent subsample in the 1977 cohort is positively selected, with a relatively high share of tertiary educated grandparents. Parent social background is based on the socio-economic status of the main breadwinner in the parent household, again derived from the parent questionnaire in the cohort studies.

Finally, we usually observe the skills of only one of the parents in the ITS data. This potentially induces measurement error in our parent skill variable. To address this, we make use of the fact that for a subsample of the ITS dataset we actually observe both parents. This is the

[14] In section 7, we show that our results are robust when applying more or less restrictive definitions of STEM.

[15] Details on the sample selection procedure, as well as the representativeness of the analytical sample relative to the original cohort studies can be found in Appendix A.4.

[16] Results are unaffected when either taking grandfather or grandmother's level of education or when including both jointly.

case for 365 children in our data. The results indicate that our main findings are not affected by the fact that we observe only one parent (for more details, see Appendix A.1).

## 4. Empirical Strategy

Our objective is to estimate the causal effect of parent skills on skills of their offspring. To focus on the relevant issues, we consider a composite conceptual model that combines a Galton-inspired intergenerational transmission model with an educational production function (Eq. 6). We start our discussion of the conceptual model by focusing on two separate transmission channels and assuming additive separability:

$$T_{ida}^{c} = \alpha + \beta_a \boldsymbol{F}_{ida} + \gamma_a \boldsymbol{S}_{ida} + \eta_{ida} \tag{7}$$

The test score, $T_{ida}^{c}$, of child $i$ of dynasty $d$ in subject assessment $a$ is explained by family factors ( $\boldsymbol{F}_{ida}$ ) and environmental factors that we refer to for expositional purposes simply as school factors ( $\boldsymbol{S}_{ida}$ ). (Note, while we speak in terms of parent-child linkages, we actually consider longer dynasties ($d$) linking families over time and going back to grandparents, as suggested by Adermon, Lindahl, and Palme (2021)). The error term, $\eta_{ida}$, contains all unobservable influences on child test scores and is assumed to be i.i.d. with mean zero. For our focus on the transmission channel of parent skills, we decompose family influences as:

$$\boldsymbol{F}_{ida} = T_{ida}^{p} + \psi_{ida} + \xi_{ida} \tag{8}$$

The family inputs ( $\boldsymbol{F}_{ida}$ ) include the direct transmission from skills of the child's parent ( $T_{ida}^{p}$ ) in the relevant skill domain for observed child skills and both pre-birth factors ( $\psi_{ida}$ ) and post-birth factors ( $\xi_{ida}$ ) for members of dynasty $d$.

By combining Eq. 7 and 8, the identification problems that surround a simple Galton regression of child test scores on parent test scores are immediately clear. To the extent that parent skills $T_{ida}^{p}$ are correlated with $\psi_{ida}$ or $\xi_{ida}$, the estimates of the skill transmission parameter, $\beta_a$, will be biased. Prior analyses, recognizing these problems, have pursued various estimation strategies. The most common strategy has been OLS estimation that includes a range of available measures for family characteristics, but prior work has also included instrumenting parent skills (e.g., Brown, McIntosh, and Taylor (2011)) and considering adoptees (e.g., Adermon, Lindahl, and Palme (2021)). Nonetheless, it is difficult to find credible instruments for parent skills that are independent of the various other influences of families. And, even if adoptees can plausibly break the correlations with pre-birth influences by coming from different dynasties (*d*), they are unlikely to break the influence of post-birth factors $\xi_{ida}$.

To identify the skill transmission parameter ($\beta_a$) with intergenerational data on skills of children and parents from the same dynasty, we exploit the fact that our data contain measures of skills in math (*m*) and language (*l)* for children as well as their parents. This allows us to specify the following subject-specific empirical models:

$$T_{idm}^{c} = \alpha_m + \beta_m T_{idm}^{p} + \gamma_m \boldsymbol{S}_{idm} + \beta_m \psi_{idm} + \beta_m \xi_{idm} + \eta_{idm} \tag{9}$$

$$T_{idl}^{c} = \alpha_l + \beta_l T_{idl}^{p} + \gamma_l \boldsymbol{S}_{idl} + \beta_l \psi_{idl} + \beta_l \xi_{idl} + \eta_{idl} \tag{10}$$

Without fully measuring pre-birth and post-birth factors, estimating Eq. (9) and (10) by ordinary least squares produces biased estimates if any unobserved components are correlated with parent test scores, $T_{ida}^{p}$.[17] But accurately characterizing the elements of $\boldsymbol{S}_{idm}$, $\psi_{idm}$, and $\xi_{idm}$ is precisely the larger problem of understanding the causal elements of intergenerational mobility. As an example, the quality of the child's school (***S***) is likely to be important in determining child achievement and to be correlated with parent skills through school selection for children and through intergenerational persistence in school choice that also goes back to parent school quality. Yet, such differences in school quality have proven difficult to measure with existing survey data (Hanushek (2002)). Or, as another example, common genetic factors that affect skills of all members of a dynasty would be part of $\psi$ and would have a confounding impact.

To eliminate these omitted variable concerns, we difference child and parent test scores between math and language:

$$\Delta T_{id}^{c} = \alpha_m - \alpha_l + (\beta_m - \beta_l)\Delta T_{id}^{p} + (\gamma_m - \gamma_l)\Delta \boldsymbol{S}_{id} + (\beta_m - \beta_l)\Delta \psi_{id} + (\beta_m - \beta_l)\Delta \xi_{id} + (\eta_{idm} - \eta_{idl}) \tag{11}$$

where

$$\Delta T_{id}^{c} = T_{idm}^{c} - T_{idl}^{c},\ \Delta T_{id}^{p} = T_{idm}^{p} - T_{idl}^{p},\ \Delta \boldsymbol{S}_{id} = \boldsymbol{S}_{idm} - \boldsymbol{S}_{idl}.\ \Delta \psi_{id} = \psi_{idm} - \psi_{idl},\ \Delta \xi_{id} = \xi_{idm} - \xi_{idl}.$$

While possible to relax later, we assume that coefficients on all observed and

[17] Note that reverse causality is not an issue because parents were assessed before their children were even born.

unobserved variables are equal across subjects. In particular, we assume (a.i) $\beta_a = \beta$ and (a.ii) $\gamma_a = \gamma$. With these assumptions, we have:

$$\Delta T^c_{id} = \alpha + \beta \Delta T^p_{id} + \gamma \Delta \boldsymbol{S}_{id} + \beta \Delta \psi_{id} + \beta \Delta \xi_{id} + (\eta_{idm} - \eta_{idl}) \tag{12}$$

In the baseline, we estimate a simplified version:

$$\Delta T^c_{id} = \alpha + \beta \Delta T^p_{id} + \varepsilon_{id} \tag{13}$$

where $\varepsilon_{id} = \gamma \Delta \boldsymbol{S}_{id} + \beta \Delta \psi_{id} + \beta \Delta \xi_{id} + (\eta_{idm} - \eta_{idl})$

The question then becomes whether $\varepsilon_{id}$ and $\Delta T^p_{id}$ are correlated. There is reason to believe that they are not, and we can partially test these presumptions later. Within-person differences in skills are arguably less likely to be systematically related to potentially important confounding factors. For example, while school factors/quality both affect individual achievement and are likely to be correlated with parent skills in Eq. 9 and 10, it is unlikely that parents are selecting schools on the specific quality of the school in either math or language as opposed to overall quality. This presumption is reinforced by the fact that parents have no information on the school's subject-specific quality as opposed to the school's overall quality. Moreover, parents can choose a school, but not the individual teacher. Similarly, conditional on observing the parent skill differences, it is reasonable to assume that other pre-birth and post-birth family factors tend to influence the level of performance in Eq. 9 and 10 as opposed to subject differences in Eq. 13. Rather, the fact that an individual is better at math than at language (or vice versa) is more likely related to personal predispositions for a certain subject or subject-specific differences in the quality of formal education (e.g., an exceptionally good math teacher). Thus, we argue that these sources of variation in within-parent skill differences are unlikely to have independent impacts on subject-specific skill-production of children.

Note that the estimation of Eq. 13 relies solely on between-subject variation within-child and within-parent, that is, observed or unobserved characteristics of children, parents, classrooms, or schools do not confound the estimate on parent skills as long as they have a similar impact on math and language skills. An alternative way to see this is by directly estimating a pooled model combining Eq. 9 and 10 while adding a family fixed effect. The family fixed effect absorbs all measured and unmeasured influences on the child's test scores as long as these other influences have the same effect on both math and language scores.

A remaining concern might be the existence of long-lasting dynastic predispositions or genetic advantages in the production of skills in a certain subject within dynasties, which would lead to subject-specific pre-birth factors ($\Delta\psi_{id}$) correlated with subject-specific parent skills. To test for the importance of such concerns, we can further isolate the variation of within-parent skill differences across subjects that is driven by subject-specific differences in the quality of the formal education environment of the parent. We introduce an IV strategy that exploits variation in subject-specific skills of parents' classroom peers. This design makes use of another unique feature of our data, namely, the sampling procedure of the parent cohort surveys, which uses the school and within school the class level as the primary sampling unit. Thus, we have information on math and language scores for (almost) all classmates of parents at age 12 for two of the three sampled cohorts.[18] Separately for math and language, we calculate the percentile rank of the average skills in the parents' classroom in the country-wide distribution of classroom skills. The difference in class ranks across subjects is a suitable measure of the subject-specific differences in the quality of the formal education environment – whether from teachers, peers, or other elements of schools. We argue, and provide extensive evidence, that these are unlikely to be related to dynastic predispositions for a certain subject within families.[19]

Note, that this IV analysis does not effectively separate nature from nurture elements of intergenerational skill transmission. Significant portions of nurture remain within the veil of the family, which we do not penetrate. It does, however, indicate the potential malleability of skills from outside factors, particularly ones that can be manipulated by policy.

## 5. Main Results

As highlighted in the discussion of the previous work into intergenerational mobility, the standard approach of each research line is combining common measures of parent and child success – income, education, or achievement –to assess how much inertia exists in socio-

[18] A small number of observations is missing (1 percent in the 1982 cohort and 5 percent in the 1989 cohort) because not all classmates were tested, or were tested but failed to be linked in the original data set. We cannot construct the instrument in the 1977 cohort as the school and class identifier in that dataset was removed by Statistics Netherlands and could not be restored.

[19] The 1982 cohort has students in the last year of primary school where the peers indicate relevant peer and school quality. In the 1989 cohort, students were sampled about halfway through the academic year of their first year in secondary school. Thus, students had 5-7 months of exposure to their teachers and peers in secondary school. Moreover, primary schools often feed into secondary schools, with the consequence that primary school students stay together with at least some of their classmates when entering secondary school. In fact, in the period 2006-2019, where we can observe school transitions in our administrative CITO data, a median share of 19 percent of a student's primary school peers attend the same secondary school *x* track combination. This percentage is slightly decreasing over time, potentially reflecting more school choice.

economic outcomes. We begin by replicating this basic approach with the ITS data using the math and language skill data as two separate, although not necessarily independent, observations of skill transmission across generations. The survey data about the parents also permit multivariate adjustment of the simple Galton correlational model of skills to obtain a richer picture of the source and strength of skill transmission.

But, as emphasized in the conceptual discussion, it is difficult to interpret these separate skill-transmission estimates as causal even with detailed survey data or estimation approaches designed to lessen the impact of various omitted factors. We move from these descriptive estimates to our baseline causal estimates of the effect of parent skills on skill production of their children that come from our between-subject model. We follow this with evidence on effect heterogeneities and mechanisms.

### 5.1. Skill-Specific Transmission Models

In most discussions of skills, little attention is given to the precise assessments and subjects that are employed, implicitly and pragmatically treating alternative tests as separate measures of some common factor. Figure 1 follows this standard model and provides a visualization of the potential strength of the parent-child skill transmission from the CITO math and language tests. The two panels show that the relationship between domain-specific skills of parents and their children is well described by a linear model for both math and language. The patterns of the two subject-specific relationships are also remarkably similar: An increase in parent skills by one SD is associated with an increase in children's skills of 0.28 (0.30) SD in math (language).

The subject-specific parent-child relationships mirror the historic findings of strong correlations of education across generations, but the interpretive questions related to possible omitted variables remain. For example, the similarity in the strength of the parent-child transmission across skill domains raises the question of whether the relationships might be driven entirely by some omitted component of the family environment for parents and children. This omitted factor may drive or just be correlated with the skill production in math and language, pointing to an entirely different cause for family status inertia than the simple transmission of skills.

A common approach for dealing with such interpretation problems is to adjust this bivariate relationship for a variety of additional or alternative driving factors. For this, the ITS data contain several potentially useful variables that might provide more assurance that the descriptive patterns are closer to causal relationships. The results in Table 2 from multivariate regressions adjusting for plausible explanatory variables illustrate this common approach. The underlying regression models estimated with child-level data are various versions of Eq. 9 and 10 controlling for differences among parents.[20] All regressions control for the gender, the migration background, and the number siblings of the observed parent, for the age of either grandparent at the birth of the observed parent, and for parent cohort as well as children test year fixed effects. In column (2), we additionally control for grandparent education, measured by four categories of the highest level of education of both grandparents. Column (3) adds controls for parent social background as measured by seven categories of occupation types of the main breadwinner in the parent household (at the time parents took the skill test). Finally, we control for regional variation by adding at total of 799 municipality fixed effects in column (4).[21]

The results in Table 2 show that the intergenerational transmission of skills is strong in both subjects even after conditioning on a large set of control variables. Panel A has results for parent-child math scores, and panel B has parent-child language scores. Accounting only for basic sociodemographic characteristics of parents and grandparents, we find that an increase in parent subject-specific skills by one SD are associated with an increase in the skills of their offspring of 0.27 SD in math and 0.29 SD in language.

While coefficient estimates are quite stable across specifications, we observe a somewhat larger drop (6-8 percent) when including grandparent education. This finding is consistent with results in Adermon, Lindahl, and Palme (2021) suggesting that grandparent human capital affects the human capital accumulation of their grandchildren over and above any impact on human capital accumulation of parents. Adding parent social background and municipality fixed effects leaves the coefficient estimates almost unchanged.

The intergenerational transmission of subject-specific skills is strong in magnitude. An increase in parent skills by one SD is associated with about a quarter of a standard deviation

[20] Results for each cohort individually are reported in Panels A and B of Table A1. We observe statistically significant parent skill estimates in each cohort.

[21] These fixed effects refer to the municipality of residence when parents took the skill test.

increase in skills of their children in the same subject. To benchmark this effect size, we can make use of our own data and relate skill differences of children to other characteristics of parents that are easier to grasp. It turns out, for example, that the mean difference in skills of children whose grandparents worked as blue-collar vs. white-collar workers is also about a quarter of a standard deviation. Likewise, this effect size is similar to the skill advantage boys have in math (0.23 SD) and girls have in language (0.27 SD), respectively. Our estimate also lies in the same ballpark as a parent-child human capital persistence parameter of 0.361 estimated in Adermon, Lindahl, and Palme (2021).

However, as argued in section 4, these subject-specific coefficient estimates are likely to be upward-biased estimates of the causal effect of parent skills on skill development of their children. In the next section, we move to the between-subject model of estimation, but an assumption embodied in this model is that that the effect of parent skills on child skills is similar across subjects. The results in Table 2 support this assumption as coefficient estimates of parent skills in math and language are almost identical. A cross-equation test indicates that one cannot reject the equality of parent coefficients in math and language (in the full-control model in column 4); the p-value is 0.714.

The parent skill estimates in Table 2 are still likely to be biased due to unobserved factors affecting parent skills and children skills. For example, in families that emphasize the importance of good education, both parents and children may have higher skills even if there is no direct effect of parent skills on children skills. The previous estimates necessarily assume that the errors in estimating the subject-specific models are orthogonal to parent skills, conditional on the measured variables. But factors such as the dynastic educational attitude are difficult to measure, so there is no assurance that the control variables included in the Table 2 estimates adequately capture the key omitted factors.

### 5.2. Causal Estimates of the Intergenerational Transmission of Skills

To address omitted variable bias, we exploit the fact that both children and parents in the ITS data were tested in two subjects, math and language.  Before estimating the transmission models, however, it is useful to consider both how much variation exists between subjects and whether there is reason to believe that the variation across domains relates to meaningful skill differences as opposed to just noise.

Unsurprisingly, math and language skills are highly correlated within a generation. The simple correlations are 0.67 for children and 0.61 for parents. These correlations, while high, are nonetheless consistent with meaningful skill differences for individuals. Figure 2 provides a histogram of the difference between math and language skills for children and for parents. In both parent and child generations, we find that the share of those with math skills greater than language skills is roughly similar to the share of those for which the reverse is true. The differences reach plus and minus two SD. While we intend to exploit this variation in skill difference in the between-subject model, it is useful to look at the raw patterns of differences.

The simplest form of the between-subject model is a plot of the difference in child scores against the difference in parent scores. As seen in Figure 3, the skill differences of parents and their children are strongly related. Put differently, 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 (or vice versa).

The relationship between parent and child skills again appears to be linear. Consider two parents: one parent has average skills in both subjects (i.e., the normalized score differences that are depicted would have math=0 and language=0); the other parent has higher math than language skills (e.g., math=1, language=0). Suppose that the math skill of each parent improves by one SD. Then, the relative math skill (vs. language skill) of the children of both parents will increase by an equal amount.

Importantly, Figure 3 shows clearly that the slope of the bivariate relationship in the skill-difference analysis is considerably flatter than for the skill levels seen previously in Figure 1. In fact, the strength of the simple parent-child transmission of math or language skills is about three times stronger than the transmission of the math-language skill difference. This indicates that a substantial part of the correlation between skills of parents and their children is driven by factors that directly affect the development of skills in both subjects across generations in a similar way.

With this *prima facie* evidence about the information content of skill differences, we move to more formal and complete models of between-subject differences in parent and child test scores as depicted in Eq. 13. We implement this model by pooling the two subjects and add a family fixed effects in the pooled regression.[22] Table 3 presents the baseline results. As in

[22] Since each child is assigned to his or her parent, the estimation also accounts for parent fixed effects, thus exploiting only variation within children and within parents.

Table 2, we account for the possibility that exogenous covariates affect math and language performance differently – here by estimating a model in which parent and grandparent background characteristics are interacted with a subject dummy.[23]

The results in Table 3 show that higher parent skills lead to higher children skills. We find that an increase in parent skills by one SD is associated with an increase in the skills of the offspring of 0.10 SD (Column 1). This association remains remarkably stable when adding grandparent education (Column 2), parent social background (Column 3), and municipality fixed effects (Column 4).[24] In terms of magnitude, the effect of a one-standard-deviation increase in parent skills on child skills is similar to the gender gap in math test scores, which amounts to 0.10 SD in favor of boys.[25]

A key conclusion is that estimates of the intergenerational transmission of skills are severely upwardly biased when not accounting for unobserved family characteristics correlated with both children and parent skills. Comparing the baseline effect sizes in columns 4 of Tables 2 and 3, we observe that the parent skill estimate in the between-subject model decreases by about 60 percent as compared to the simple subject-specific estimations. In other words, the standard descriptive estimates of skill transmission cannot be taken as close to the underlying causal impacts of parent skill.

Of course, the identifying assumption of the between-subject model is that there are no important skill determinants that vary by subject and that are correlated with both parent skills and children skills. While we address this point more rigorously in the instrumental variable estimation in section 6, the stability of the coefficient on parent skills when accounting for various (grand-)parent characteristics suggests that remaining unobserved variables are unlikely to confound our estimates.[26]

---

[23] Results for each cohort individually are reported in Panel C of Table A1. We observe statistically significant parent skill estimates in each cohort. Consistent with the subject-specific results in Panels A and B, the parent skill estimate is largest in the first cohort.

[24] Coefficients on the control variables in the full model are shown in Table A2.

[25] The estimated strength of the intergenerational skill transmission is very similar when we use the difference in the percentile ranks in the overall distributions of math and language in the child or parent generation instead of the difference in skill levels (see Table A3).

[26] In the spirit of Altonji, Elder, and Taber (2005) and Oster (2019), the fact that the intergenerational transmission coefficient does not change with the addition of measured exogenous factors would not signal a significant role for unmeasured factors.

### 5.3. Effect Heterogeneity

In this section, we explore differential skill transmission within our between-subject model. The estimation in Table 4 interacts the skill difference of parents with potential factors modifying the intergenerational skill transmission: the parent and child gender match (column 1), grandparent education levels (column 2), and parent social background (column 3).[27]

Intriguingly, the strength of the intergenerational skill transmission does not vary by the gender match of parents and their children. This result differs from a number of previous papers that have tended to suggest a stronger influence of mothers, particularly for sons (e.g., Black, Devereux, and Salvanes (2005), Piopiunik (2014), Holmlund, Lindahl, and Plug (2011)). The difference in results may reflect the fact that the prior work is entirely based on school attainment that does not reflect any underlying differences in relative skills between mothers and fathers. Skill transmission tends to be modestly lower for children with low-educated grandparents compared to those with better educated grandparents. This effect is driven by very low grandparent education (see Table A4), perhaps operating through negative attitudes toward education in general. There is no effect heterogeneity with respect to parents' social background.

### 5.4. Potential Mechanisms

The causal estimates of the key intergenerational skill parameter still leave several open questions. In particular, it would be valuable to understand why parents with different skill mixes when they were in lower secondary school 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 do know 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 (see Table A5). However, the role of these economic factors in explaining the extent relative skills are transmitted from one generation to the next is very limited. Adding the economic variables to

[27] For expositional purposes, grandparent education and social status are summarized in coarser categories than available and used in the remaining tables. See Table A4 for the results with the more detailed categories.

the baseline between-subject model leaves the parent skill coefficient virtually unchanged (Table 5). This is due to the fact that the considered measures of parent economic success are only weakly, if at all, correlated with child skill differences after conditioning on parent skill differences.[28]

Our simple analysis of mechanisms has two important caveats. First, interpreting the parent skill coefficients in Table 5 as the effect of parent skills 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 skills on child skills into shares attributed to one or several mediators can only be achieved when imposing additional assumptions (see Heckman, Pinto, and Savelyev (2013)).[29]

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 knowledge in one subject will also be more willing and more able to transmit that knowledge to their children. Unfortunately, our data do not allow to test this presumption directly.

## 6. Exploiting Variation in Subject-Specific Quality of Formal Education of Parents

A key question is 'why are some parents better in math than in language or vice versa?'[30] Our approach uses differences in the domain-specific quality of schools attended by parents to develop unique instrumental variables that relate to the relative skill differences of parents. This

[28] 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 skill transmission from parents to their children. However, the mediators similarly affect math and language skills, so they cannot meaningfully explain skill *differences*.

[29] 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 skills in the between-subject model, we stop at the basic level in Table 5.

[30] The thrust of this question has arisen previously. For example, in the large literature on returns to schooling based on estimates involving twins, it has frequently been asked why the schooling of monozygotic twins differs since they are arguably facing the same optimal human capital investment (Behrman (2016)).

line of estimation demonstrates that parent skill differences are directly related to school quality differences that are skill specific.

In the baseline model of section 5.2, we assumed that there were no pre-birth factors ($\Delta\psi_{id}$) that affect the skill difference of parents and also independently affect the skill difference of children. However, subject-specific family predispositions arising because of subject-specific differences in genetic endowments or subject-specific dynastic traditions might violate this assumption (Sigmundsson, Polman, and Lorås (2013)).

To address such concerns, we turn to differential school quality that the parents were exposed to when they were in school. We exploit the fact that our survey data on parents is sampled at the level of classrooms and create an instrument for parent skill differences that is arguably exogenous.[31] Specifically, we compute a measure of classroom quality in math and language based on the measured skills of classroom peers. These subject-specific measures allow us to rank math and language classroom quality within the sample of all students (also including non-parents) in the respective cohort. We then use the differences in the ranking of math and language classrooms to extend the between-subject model of section 5.2 by an instrumental variable approach.[32]

We argue that, on the one hand, differences in classroom quality, which may reflect differences in teacher as well as peer quality, are very likely to be a main driver of between-subject skill differences of parents. On the other hand, such differences in the quality of math and language classrooms are extremely unlikely to be systematically related to family predispositions that affect between-subject differences in within-family skill production across generations.

Figure 4 previews the results of our IV approach. The left graph portrays the reduced-form relationship between differences in parent classroom quality and children's skills. This relationship is positive and statistically significant. It is the key assumption of our IV approach that this significant relationship arises only because of the strong first-stage relationship between

[31] Note, however, that for this analysis, we can only use the parents tested in the cohorts of 1982 and 1989. No information on the classroom of students is available for the 1977 cohort. For more details on the identification of classrooms in the survey data for the cohorts of 1982 and 1989, see Appendix A.2.

[32] While we consider differences in classroom performance ranks to be the most straightforward and intuitive measure of the quality of different classroom environments, there are, of course, also other plausible ways of operationalizing the core idea behind this identification strategy. In Appendix A.2 we show that the findings of the IV approach are robust to several alternative ways of constructing an instrument based on peer performance in math and language.

differences in classroom quality and differences in parent skills depicted in the right graph of Figure 4.

Table 4 shows the results of the instrumental variable estimation. As the instrument varies only across the 1138 classrooms in the sample, we cluster standard errors at the classroom level. In Column (1), we start by showing the estimate of our baseline between-subject model of section 5.2 based on the reduced sample used in the IV analysis. The first stage and reduced form effects of the IV approach in a model without further covariates are reported in columns (2) and (3). Columns (4) and (5) then show the instrumental variable estimate of models without and with further controls. The F-statistic on the excluded instrument is large and indicates that our instrument is very strong.

Differences in the classroom environment of parents are strong predictors of both parent skill differences as well as differences in skills of their children. The first stage effect indicates that a classroom that is ranked one percentile higher in the math than in the language ranking is significantly associated with parents scoring about 0.02 SD higher on the math than the language test. The reduced form effect on skill differences of their children is also positive and significant, but about one-tenth the magnitude.

In this just-identified model in column (3) of Table 4, the corresponding IV estimate is close to 0.10 SD The estimate is hardly affected by adding further controls (grandparent education, grandparent social background, and municipality fixed effects) to the model. This suggests that the variation in classroom quality is unrelated to such important observable characteristics of the parent background, which makes it more plausible that it is also unrelated to other unobservable characteristics.

Endogenous switching between schools or classrooms is an obvious threat to identification in this approach. However, as already argued in section 4, it is extremely unlikely that in the 1980s schools or classrooms within a school were selected by parents based on the specific skills in teaching math *relative* to language of the school or of a teacher as opposed to overall educational quality. Nonetheless, we can even further mitigate concerns of between-school or within-school sorting in a set of robustness checks presented in Appendix A.2 that all confirm our main findings. There, we also test directly whether peer compositions are correlated across generations.

Strikingly, the magnitude of the IV estimate is almost identical to the previous estimate of the between-subject model. Whether we exploit for identification the entire variation in parent skill differences or just the part of this variation that can be explained by differences in classroom quality makes almost no difference in the strength of the intergenerational skill transmission. While this result may be surprising at first glance, it is exactly what one would expect to find if the between-subject model is already correctly specified. Thus, we can interpret our IV results as additional evidence supporting the internal validity of the results obtained from the between-subject model.

## 7. Long-Term Outcomes

Finally, we investigate whether the effect of parent skills on child skills also carries over to later life outcomes. To do so, we use administrative data on children's STEM choices, reflecting the prominent role of STEM in both the academic and public discussion.[33] Table 7 shows that the relative pattern of parent skills directly relates to STEM choices of children. Children of parents whose math skills are one SD above their language skills have a 2.7 percentage points (6.4 percent) higher probability of choosing a STEM profile at school (column 1). Given that enrollment into most upper secondary vocational or tertiary education programs is only possible with specific backgrounds in terms of courses taken, it is not surprising that parent skills also affect children's STEM choices later in life. If math skills of parents are one SD above their language skills, children are 1.1 percentage points (3.4 percent) more likely to choose a STEM field in vocational or university education (column 3).

The relationship between parent skills and STEM choices does not vary significantly by gender (columns 2 and 4 of Table 7). This is even more striking when considering that women are generally less likely to choose STEM tracks at or after school (see bottom of Table 7). These descriptive results suggest that there is no gender bias in how parent skills relate to actual STEM choices.[34]

[33] Research and policy have considered not only overall issues of attracting more people into STEM fields but also the large gender disparities in these choices (Altonji, Arcidiacono, and Maurel (2016), Stoet and Geary (2018), UNESCO (2017), Deming and Noray (2020)).

[34] Table A8 considers a narrower definition of STEM, which defines course profiles and study programs in the agricultural and medical fields as non-STEM. Results are robust to applying this more restrictive definition. While

## 8. Conclusions

While the role of parents is generally viewed as significant if not dominant in determining child outcomes, there is a dearth of evidence about what aspects of families drive these results. Our analysis provides clear and credibly causal estimates of the strong influence of parental skills measured by math and language tests on the skills of children. Moreover, it documents that purely descriptive estimates of the transmission of skills are biased by as much as 60 percent.

The new Intergenerational Transmission of Skills (ITS) database that we develop permits matching skills of Dutch parents and children on similar tests taken at similar times, thus circumventing some of the serious problems with prior investigations of intergenerational linkages. We use between test variance for parents and children to develop an estimation strategy that eliminates all family, school, and neighborhood factors that are not specific to either math or language performance. Our estimates prove very stable when subjected to a variety of specification and robustness exercises.

We also develop a novel IV estimation strategy based on classroom peers of the parents. This estimation, which recognizes the differences of skills across test domains, shows that skills within dynasties are not just genetically determined but are significantly affected by environmental factors.

Family skills also influence long run career patterns. Relatively high math skills by parents promote greater choice of STEM paths by children. And, despite the fact that females are generally less likely to choose STEM tracks than males, the strength with which parental skills translate to STEM choices does not differ by gender.

Our results carry an important policy message regarding the long-run value of good educational environments with respect to educational inequalities. Strong persistence in the transmission of human capital across generations is often seen as an obstacle to equality of opportunity. But this might only be partly true. Our IV results clearly show that the part of parent skills that is malleable by educational quality also carries over to future generations. Thus, the

effect heterogeneity by gender gets more pronounced, this partly reflects the lower baseline probabilities of women choosing these narrowly defined STEM fields.

crucial challenge for education policy remains to guarantee equal access to good education. If children in families with more favorable pre-birth and post-birth factors also predominantly get access to better educational environments, educational inequalities across generations will be aggravated. However, if policy succeeds in providing better education to children in families with less favorable pre-birth factors and post-birth factors, the benefits of this will also spill-over to future generations.

## References


Adermon, Adrian, Mikael Lindahl, and Mårten Palme. 2021. "Dynastic Human Capital, Inequality, and Intergenerational Mobility." *American Economic Review* 111, no. 5 (May): 1523-48.

Altonji, Joseph G., Peter Arcidiacono, and Arnaud Maurel. 2016. "The Analysis of Field Choice in College and Graduate School: Determinants and Wage Effects." In *Handbook of the Economics of Education, Vol. 5*, edited by Eric A. Hanushek, Stephen Machin, and Ludger Woessmann. Amsterdam: North Holland: 305-396.

Altonji, Joseph G., Todd E. Elder, and Christopher R. Taber. 2005. "Selection on observed and unobserved variables: Assessing the effectiveness of Catholic schools." *Journal of Political Economy* 113, no. 1: 151-184.

Behrman, Jere R. 2016. "Twins Studies in Economics." In *The Oxford Handbook of Economics and Human Biology*, edited by John Komlos and Inas R. Kelly. Oxford, UK: Oxford Handbooks Online.

Björklund, Anders, and Markus Jäntti. 2011. "Intergenerational Income Mobility and the Role of Family Background." In *The Oxford Handbook of Economic Inequality*, edited by Brian Nolan, Wiemer Salverda, and Timothy M. Smeeding. Oxford: Oxford University Press.

Björklund, Anders, and Kjell G. Salvanes. 2011. "Education and family background: Mechanisms and policies." In *Handbook of the Economics of Education, Vol. 3*, edited by Stephen Machin Eric A. Hanushek and Ludger Woessmann. Amsterdam: North Holland: 201-247.

Black, Sandra E., and Paul J. Devereux. 2011. "Recent developments in intergenerational mobility." In *Handbook of Labor Economics, Vol. 4, Part B*, edited by Orley Ashenfelter and David Card. Amsterdam: North Holland: 1487-1541.

Black, Sandra E., Paul J. Devereux, and Kjell G. Salvanes. 2005. "Why the apple doesn't fall far: Understanding intergenerational transmission of human capital." *American Economic Review* 95, no. 1 (March): 437-449.

Bleakley, Hoyt, and Joseph Ferrie. 2016. "Shocking Behavior: Random Wealth in Antebellum Georgia and Human Capital Across Generations." *The Quarterly Journal of Economics* 131, no. 3 (August): 1455-1495.

Bol, Thijs, and Herman G. van de Werfhorst. 2013. "Educational Systems and the Trade-Off between Labor Market Allocation and Equality of Educational Opportunity." *Comparative Education Review* 57, no. 2 (May): 285-308.

Brown, S., S. McIntosh, and K. Taylor. 2011. "Following in Your Parents' Footsteps? Empirical Analysis of Matched Parent-Offspring Test Scores." *Oxford Bulletin of Economics and Statistics* 73, no. 1 (February): 40-58.

Chetty, Raj, and Nathaniel Hendren. 2018. "The Impacts of Neighborhoods on Intergenerational Mobility I: Childhood Exposure Effects." *Quarterly Journal of Economics* 133, no. 3 (August): 1107-1162.

Chetty, Raj, Nathaniel Hendren, Patrick Kline, and Emmanuel Saez. 2014. "Where is the land of opportunity? The geography of intergenerational mobility in the United States." *Quarterly Journal of Economics* 129, no. 4 (November): 1553-1623.

Coleman, James S., Ernest Q. Campbell, Carol J. Hobson, James McPartland, Alexander M. Mood, Frederic D. Weinfeld, and Robert L. York. 1966. *Equality of educational opportunity*. Washington, D.C.: U.S. Government Printing Office.

Cunha, Flavio, James J. Heckman, and Susanne M. Schennach. 2010. "Estimating the Technology of Cognitive and Noncognitive Skill Formation." *Econometrica* 78, no. 3: 883-931.

Dahl, Gordon B., and Lance Lochner. 2012. "The Impact of Family Income on Child Achievement: Evidence from the Earned Income Tax Credit." *American Economic Review* 102, no. 5 (August): 1927-1956.

de Coulon, Augustin, Elena Meschi, and Anna Vignoles. 2011. "Parents' skills and children's cognitive and non-cognitive outcomes." *Education Economics* 19, no. 5 (December): 451-474.

Deming, David J, and Kadeem Noray. 2020. "Earnings Dynamics, Changing Job Skills, and STEM Careers." *Quarterly Journal of Economics* 135, no. 4: 1965-2005.

Duncan, Greg, and Suzanne Le Menestrel, eds. 2019. *A Roadmap to Reducing Child Poverty*. Washington, DC: The National Academies Press.

Galton, Francis. 1889. *Natural Inheritance*. London: MacMillan.

García, Jorge Luis, James J. Heckman, Duncan Ermini Leaf, and María José Prados. 2020. "Quantifying the Life-Cycle Benefits of an Influential Early-Childhood Program." *Journal of Political Economy* 128, no. 7: 2502-2541.

Gray-Lobe, Guthrie, Parag A. Pathak, and Christopher R. Walters. 2021. "The Long-Term Effects of Universal Preschool in Boston." NBER Working Paper No. 28756. Cambridge, MA: National Bureau of Economic Research (May).

Haider, Steven, and Gary Solon. 2006. "Life-cycle variation in the association between current and lifetime earnings." *American Economic Review* 96, no. 4: 1308-1320.

Hampf, Franziska, Simon Wiederhold, and Ludger Woessmann. 2017. "Skills, earnings, and employment: exploring causality in the estimation of returns to skills." *Large-scale Assessments in Education* 5, no. 12: 1-30.

Hanushek, Eric A. 2002. "Publicly provided education." In *Handbook of Public Economics, Vol. 4*, edited by Alan J. Auerbach and Martin Feldstein. Amsterdam: North Holland: 2045-2141.

Hanushek, Eric A., Guido Schwerdt, Simon Wiederhold, and Ludger Woessmann. 2015. "Returns to skills around the world: Evidence from PIAAC." *European Economic Review* 73: 103-130.

Hanushek, Eric A., Guido Schwerdt, Simon Wiederhold, and Ludger Woessmann. 2017. "Coping with change: International differences in the returns to skills." *Economics Letters* 153: 15-19.

Hanushek, Eric A., and Ludger Woessmann. 2008. "The role of cognitive skills in economic development." *Journal of Economic Literature* 46, no. 3: 607-668.

Hanushek, Eric A., and Ludger Woessmann. 2015. *The knowledge capital of nations: Education and the economics of growth*. Cambridge, MA: MIT Press.

Heckman, James J., and Rodrigo Pinto. 2015. "Econometric Mediation Analyses: Identifying the Sources of Treatment Effects from Experimentally Estimated Production Technologies with Unmeasured and Mismeasured Inputs." *Econometric Reviews* 34, no. 1-2 (2015/02/07): 6-31.

Heckman, James J., Rodrigo Pinto, and Peter Savelyev. 2013. "Understanding the Mechanisms through Which an Influential Early Childhood Program Boosted Adult Outcomes." *American Economic Review* 103, no. 6: 2052-2086.

Herrnstein, Richard J., and Charles Murray. 1994. *The Bell Curve: Intelligence and Class Structure in American Life*. New York, NY: The Free Press.

Holmlund, Helena, Mikael Lindahl, and Erik Plug. 2011. "The causal effect of parents' schooling on children's schooling: A comparison of estimation methods." *Journal of Economic Literature* 49, no. 3: 615-651.

Jacobs, Babs, and Rolf van der Velden. 2021. "Exploring the Uncharted Waters of Inequality of Educational Opportunities: On the Role of Skills." Research Centre for Education and the Labour Market (ROA) Maastricht University.

Jacobs, Babs, Rolf van der Velden, and Stan Vermeulen. 2021. "Construction of the Intergenerational Transmission of Skills Database. Technical report." Research Centre for Education and the Labour Market (ROA) Maastricht University.

Jacobs, Madelon, Rolf van der Velden, and Lynn van Vugt. 2021. "Does lowering the bar help? Results from a natural experiment in high-stakes testing in Dutch primary education." ROA Research Memoranda No. 004. Research Centre for Education and the Labour Market (ROA): Maastricht University (June 17).

Mogstad, Magne, and Gaute Torsvik. 2021. "Family Background, Neighborhoods and Intergenerational Mobility." NBER Working Paper No. 28874. Cambridge, MA: National Bureau of Economic Research (May).

Oster, Emily. 2019. "Unobservable Selection and Coefficient Stability: Theory and Evidence." *Journal of Business & Economic Statistics* 37, no. 2 (April): 187-204.

Piopiunik, Marc. 2014. "Intergenerational transmission of education and mediating channels: Evidence from a compulsory schooling reform in Germany." *Scandinavian Journal of Economics* 116, no. 3: 878-907.

Sacerdote, Bruce. 2011. "Nature and Nurture Effects On Children's Outcomes: What Have We Learned From Studies of Twins And Adoptees?" In *Handbook of Social Economics*, edited by Jess Benhabib, Alberto Bisin, and Matthew O. Jackson: North-Holland: 1-30.

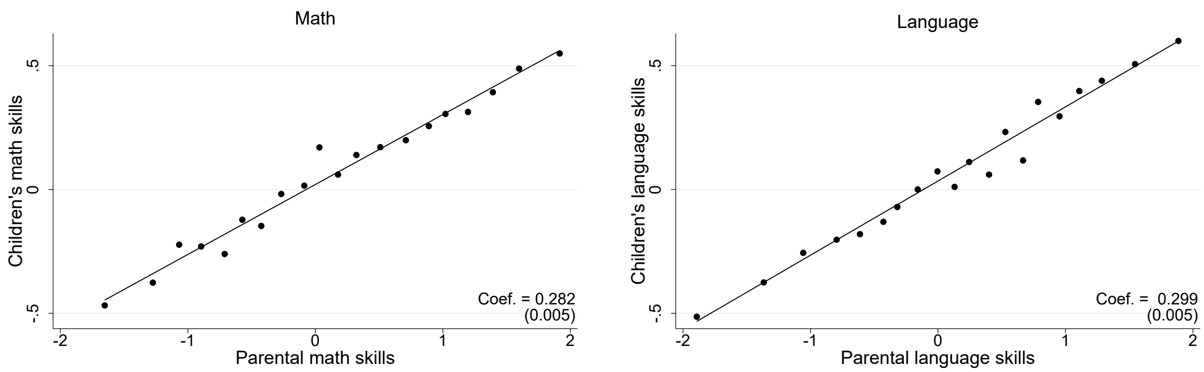
Sigmundsson, H., R. C. J. Polman, and H. Lorås. 2013. "Exploring Individual Differences in Children's Mathematical Skills: A Correlational and Dimensional Approach." *Psychological Reports* 113, no. 1 (August): 23-30.

Solon, Gary. 1999. "Intergenerational mobility in the labor market." In *Handbook of Labor Economics*, edited by Orley Ashenfelter and David Card. Amsterdam: Elsevier: 1761-1800.

Stoet, Gijsbert, and David C. Geary. 2018. "The Gender-Equality Paradox in Science, Technology, Engineering, and Mathematics Education." *Psychological Science* 29, no. 4 (April): 581-593.

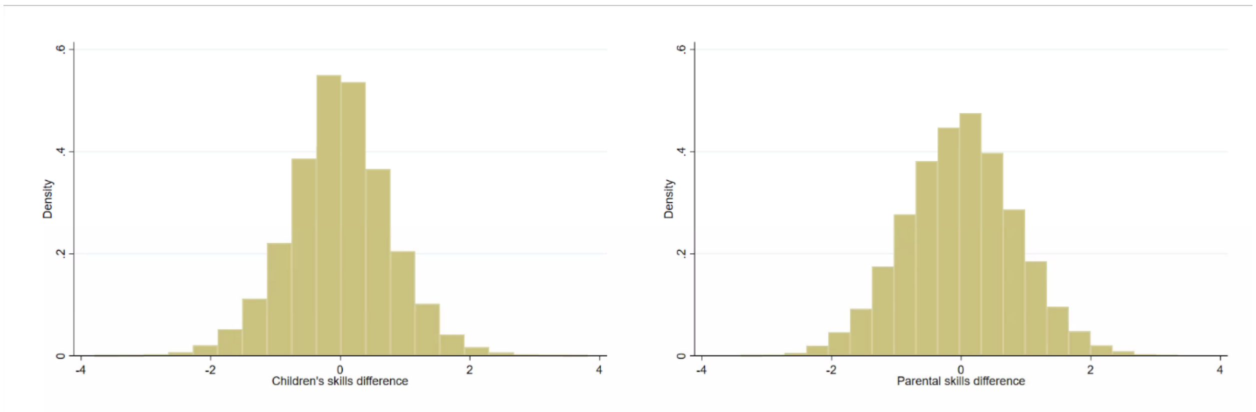
UNESCO. 2017. *Cracking the code: Girls' and women's education in science, technology, engineering and mathematics (STEM)*. Paris: United Nations Educational, Scientific and Cultural Organization.

Figure 1: Binned scatterplots of child skills and parent skills



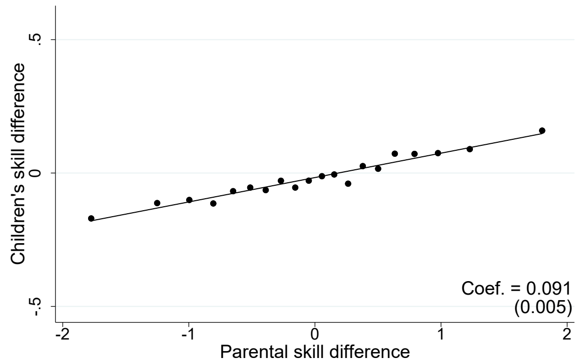

*Notes:* The figure displays two binned scatterplots showing the strength of parent-child transmissions in math skills (left) and language skills (right). To construct the figure, we divided parent skills into 20 ranked equal-sized groups and plotted the mean of the children skills against the mean of the parent skills 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:* Administrative data; pooled ITS survey dataset.

Figure 2: Histogram of the math-language skill difference



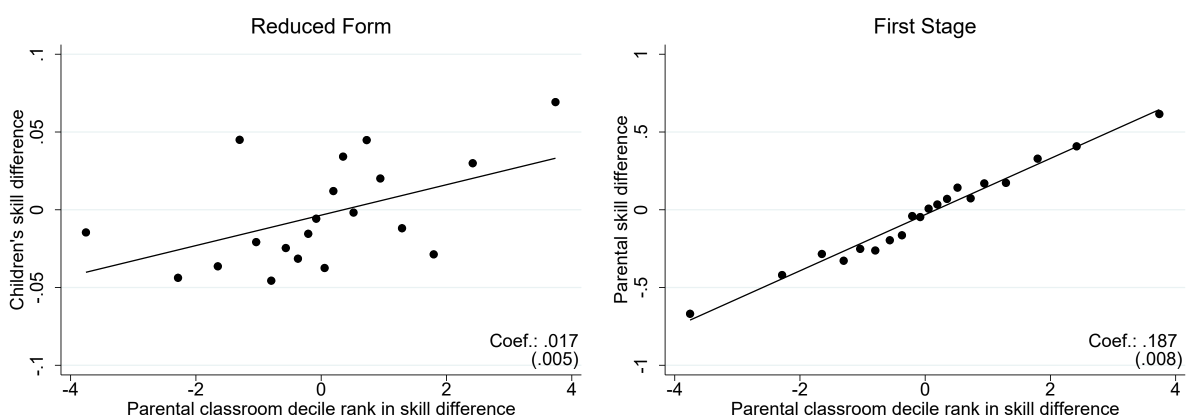

*Notes:* The figure depicts the difference between math and language skills for children (left) and parents (right). *Data sources:* Administrative data; pooled ITS survey dataset.

Figure 3: Binned scatterplot of child and parent skill differences




*Notes:* The figure displays a binned scatterplot showing the strength of parent-child transmissions in math-language skill differences. To construct the figure, we divided parent skills into 20 ranked equal-sized groups and plotted the mean of the children skills against the mean of the parent skills 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:* Administrative data; pooled ITS survey dataset.

Figure 4: Differences in parent classroom quality, parent skills, and children's skills




*Notes:* The figure displays two binned scatterplots showing the strength of the relationship between differences in parent classroom quality and differences in children's skills (left) and differences in parent skills (right), respectively. To construct the scatterplots, we divided differences in parent classroom quality into 20 ranked equal-sized groups and plotted the mean of the differences in parent classroom quality against the mean of the differences in children/parent skills in each bin. 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.

Table 1: Descriptive Statistics

| Variables | | Pooled | Cohort 1977 | 1982 | 1989 |
|---|---|---|---|---|---|
| | | (1) | (2) | (3) | (4) |
| **Child Characteristics** | | | | | |
| Math skills | Mean | 0.048 | 0.120 | 0.009 | -0.123 |
| | SD | 0.97 | 0.95 | 0.97 | 0.99 |
| Language skills | Mean | 0.066 | 0.149 | 0.019 | -0.132 |
| | SD | 0.95 | 0.92 | 0.96 | 1.00 |
| Math-language skill difference | Mean | -0.017 | -0.029 | -0.010 | 0.009 |
| | SD | 0.78 | 0.78 | 0.77 | 0.78 |
| Course profile | STEM | 0.34 | 0.37 | 0.34 | 0.24 |
| | Non-STEM | 0.46 | 0.46 | 0.46 | 0.44 |
| Field of study | STEM | 0.23 | 0.28 | 0.20 | 0.11 |
| | Non-STEM | 0.48 | 0.57 | 0.43 | 0.27 |
| Gender | Female | 0.50 | 0.50 | 0.51 | 0.51 |
| **Parent Characteristics** | | | | | |
| Math skills | Mean | 0.100 | 0.220 | 0.027 | -0.173 |
| | SD | 0.98 | 0.97 | 0.99 | 0.97 |
| Language skills | Mean | 0.105 | 0.202 | 0.036 | -0.092 |
| | SD | 0.96 | 0.93 | 0.99 | 0.98 |
| Math-language skill difference | Mean | -0.006 | 0.018 | -0.009 | -0.081 |
| | SD | 0.86 | 0.82 | 0.93 | 0.84 |
| Personal income percentile | Mean | 58.08 | 63.29 | 56.05 | 43.42 |
| | SD | 25.86 | 24.82 | 25.33 | 24.51 |
| 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 | 63.29 | 66.36 | 61.67 | 55.72 |
| | SD | 28.84 | 28.77 | 28.65 | 27.79 |
| 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 siblings | 0.06 | 0.06 | 0.05 | 0.05 |
| | 1 sibling | 0.37 | 0.34 | 0.41 | 0.40 |
| | 2 siblings | 0.28 | 0.30 | 0.26 | 0.23 |
| | >2 siblings | 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 variables indicated in the first column for the pooled sample as well as the three survey cohorts separately. The type of statistic reported is indicated in column 2. If neither Mean, SD, or Total number is specified, the reported statistic refers to the share with in the sample indicated in the top row. Children's skills are standardized with mean zero and SD one in the full sample of children taking the test in their cohort based on administrative data. Parent skills standardized with mean zero and SD one in the full sample of participants from each survey cohort. 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 (UNESCO, 2003). 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. Not all students can be assigned a STEM/non-STEM course profile/field of study as they have not progressed far enough into the education system. Household income is based on the percentile of the household in the Dutch distribution in terms of yearly spendable income. Parent personal income is based on the percentile of the parent in the Dutch income distribution (sources include: labor income, owned companies, unemployment benefits and social security). Household wealth is based on the percentile of the household in the Dutch distribution in terms of the household's total wealth, determined by assets minus debts. 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. In parent education, "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 measured based on the highest level of education of both grandparents. Grandparent social background is based on the occupation type of the main breadwinner in the parent household. For expositional reasons, mean age of grandparents at the time of the parent's birth is shown in the table; in the regressions, we control for each of the following age groups: below 21, 21-25, 26-30, 31-35, 36-40, 41 and above. Apart from income and wealth, which are taken from administrative data, all (grand-)parent characteristics stem from the survey datasets. (Grand-)parent characteristics are reported at the child level. *Data sources:* Administrative data; pooled ITS survey dataset.

Table 2: Intergenerational transmission of skills in math and language

| | Panel A: Math | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Parent skills | 0.273 | 0.257 | 0.255 | 0.258 |
| | (0.005) | (0.005) | (0.005) | (0.005) |
| R-squared | 0.090 | 0.094 | 0.096 | 0.120 |
| Observations (students) | 41,774 | 41,774 | 41,774 | 41,774 |
| | Panel B: Language | | | |
| Parent skills | 0.286 | 0.264 | 0.261 | 0.261 |
| | (0.005) | (0.005) | (0.005) | (0.005) |
| R-squared | 0.102 | 0.109 | 0.110 | 0.135 |
| Observations (students) | 41,774 | 41,774 | 41,774 | 41,774 |
| | Control variables in Panels A + B | | | |
| Grandparent education | | yes | yes | yes |
| Parent social background | | | yes | yes |
| Municipality fixed effects | | | | yes |

*Notes:* Least squares regressions. Sample: Pooled sample of all matched parent-children observations in the three survey cohorts. Dependent variables: Math score of children in Panel A; language score of children in Panel B; test scores standardized with mean zero and SD one in full sample of children taking the test in each test year. Parent skills standardized with mean zero and SD one in full sample of parents in each survey cohort. Grandparent education is measured by four categories of the highest level of education of both grandparents. Parent social background is measured by seven categories of occupation types of the main breadwinner in the parent household. 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 3: Between-subject model of the intergenerational transmission of skills

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Parent skills | 0.098 | 0.098 | 0.097 | 0.096 |
| | (0.005) | (0.005) | (0.005) | (0.005) |
| Grandparent education | | yes | yes | yes |
| Parent social background | | | yes | yes |
| Municipality fixed effects | | | | yes |
| R-squared | 0.088 | 0.090 | 0.089 | 0.067 |
| Observations | 83,548 | 83,548 | 83,548 | 83,548 |

*Notes:* Least squares regressions with family fixed effects. Sample: Sample of all matched parent-children observations in the three survey cohorts pooled over math and language. Dependent variable: Test score of children standardized with mean zero and SD one in full sample of children taking the test in each test year. Parent skills standardized with mean zero and SD one in full sample of parents in each survey cohort. Grandparent education is measured by four categories of the highest level of education of both grandparents. Parent social background is measured by seven categories of occupation types of the main breadwinner in the parent household. 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. All control variables are interacted with a subject indicator. Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Table 4: Effect heterogeneity in the intergenerational transmission of skills

| | (1) | (2) | (3) |
|---|---|---|---|
| Parent skills | 0.099 | 0.086 | 0.099 |
| | (0.010) | (0.070) | (0.009) |
| Parent-child gender interaction | | | |
| x Male parent & female child | -0.002 | | |
| | (0.013) | | |
| x Female parent & male child | -0.003 | | |
| | (0.013) | | |
| x Female parent & female child | -0.003 | | |
| | (0.013) | | |
| Grandparent education | | | |
| x Medium | | 0.026 | |
| | | (0.011) | |
| x High | | 0.021 | |
| | | (0.014) | |
| x Missing education information | | -0.011 | |
| | | (0.024) | |
| Parent social background | | | |
| x Employer | | | -0.001 |
| | | | (0.016) |
| x Low white-collar worker | | | -0.005 |
| | | | (0.017) |
| x Medium white-collar worker | | | 0.016 |
| | | | (0.015) |
| x Professionals | | | -0.015 |
| | | | (0.016) |
| x Other | | | -0.018 |
| | | | (0.015) |
| Grandparent education | yes | yes | yes |
| Parent social background | yes | yes | yes |
| Municipality fixed effects | yes | yes | yes |
| R-squared | 0.051 | 0.067 | 0.067 |
| Observations | 83,548 | 83,548 | 83,548 |

*Notes:* Least squares regressions with family fixed effects. Sample: Sample of all matched parent-children observations in the three survey cohorts pooled over math and language. Dependent variable: Test score of children standardized with mean zero and SD one in full sample of children taking the test in each test year. Parent skills standardized with mean zero and SD one in full sample of parents in each survey cohort. The coarser definition of grandparent education used in this table combines primary and lower secondary education to the lower education category, while upper secondary and tertiary education are referred to as medium and tertiary education, respectively. The coarser definition of parent social status lumps together “employer without staff” and “employer with staff” in the “employer” category, and the “other” and “unknown” in the “other” category. Omitted category in column (1) is male parent & male child; omitted category in column (2) is low education (at most lower secondary); omitted category in column (3) is blue collar worker. 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 5: Analysis of mechanisms in the between-subject model

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Parent skills | 0.096 | 0.097 | 0.096 | 0.096 | 0.095 |
| | (0.005) | (0.005) | (0.005) | (0.005) | (0.005) |
| Parent education | | | | | |
| Medium | | -0.002 | | | |
| | | (0.011) | | | |
| High | | 0.030 | | | |
| | | (0.013) | | | |
| Missing | | -0.012 | | | |
| | | (0.019) | | | |
| Parent income (/10) | | | -0.001 | | |
| | | | (0.002) | | |
| Household income (/10) | | | | -0.007 | |
| | | | | (0.002) | |
| Household wealth (/10) | | | | | -0.009 |
| | | | | | (0.002) |
| Grandparent education | yes | yes | yes | yes | yes |
| Parent social background | yes | yes | yes | yes | yes |
| Municipality fixed effects | yes | yes | yes | yes | yes |
| R-squared | 0.067 | 0.070 | 0.067 | 0.064 | 0.062 |
| Observations | 83,548 | 83,548 | 83,548 | 83,548 | 83,548 |

*Notes:* Least squares regressions with family fixed effects. Sample: Sample of all matched parent-children observations in the three survey cohorts pooled over math and language. Dependent variable: Test score of children standardized with mean zero and SD one in full sample of children taking the test in each test year. Parent skills standardized with mean zero and SD one in full sample of parents in each survey cohort. Parent education is measured as the highest educational degree obtained by the observed parent (omitted category: low education); low education: at most lower secondary; medium education: higher secondary and upper secondary vocational education; high education: tertiary education, consisting of higher vocational education and university. Household income is based on the percentile of the household in the Dutch household distribution in terms of yearly spendable income in the child's test-taking year. Parent personal income is based on the percentile of the parent in the Dutch personal income distribution (including income from labor, income from owned companies, unemployment and social security) in the child's test-taking year. Household wealth is based on the percentile of the household in the Dutch household distribution in terms of the household's total wealth, determined by assets minus debts in the child's test-taking year. Missing values for parent education (3.5%), parent income (6.7%), household income (1.5%), and household wealth (11.5%) are imputed (imputation dummies added to the regression models). All regressions control for grandparent education, parent social background based on the occupation type of the main breadwinner in the parent household, and municipality fixed effects. 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. All control variables are interacted with a subject indicator. Standard errors clustered at the parent level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Table 6: Between-subject IV model of the intergenerational transmission of skills

| | FE model | First stage IV | Reduced form | Second stage IV | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Parent skills | 0.084 | | | 0.095 | 0.098 |
| | (0.008) | | | (0.028) | (0.029) |
| Parent classroom quality | | 0.187 | 0.018 | | |
| | | (0.007) | (0.005) | | |
| Further controls | | | | | yes |
| F-statistic excluded instrument | | | | 656.21 | 614.70 |
| R-squared | 0.08 | 0.32 | 0.05 | 0.08 | 0.08 |
| Observations | 24,536 | 24,536 | 24,536 | 24,536 | 24,536 |

*Notes:* Least squares and two-stage least squares regressions with individual fixed effects. Sample: Sample of all matched parent-children observations in the survey cohorts of 1982 and 1989 pooled over math and language. Dependent variables: Test score of children standardized with mean zero and SD one in full sample of children taking the test in each test year in columns 2, 3, and 4; Test score of parents standardized with mean zero and SD one in column1; Parent skills standardized with mean zero and SD one in full sample of parents in each survey cohort. Parent classroom quality is measured by the rank of math and language classrooms within the sample. Further controls include grandparent education (4 categories), Parent social background based on the occupation type of the main breadwinner in the parent household (7 categories), and municipality fixed effects (365 categories). 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. All control variables are interacted with a subject indicator. Standard errors clustered at the classroom level in parentheses. *Data sources:* Administrative data; pooled ITS survey dataset.

Table 7: Parent skills and children's STEM choices

| | STEM Profile Choice (y/n) | | STEM Study Choice (y/n) | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Parent skill difference | 0.027 | 0.025 | 0.011 | 0.017 |
| | (0.003) | (0.005) | (0.003) | (0.005) |
| x child female | | 0.004 | | -0.009 |
| | | (0.006) | | (0.006) |
| Grandparent education | yes | yes | yes | yes |
| Parent social background | yes | yes | yes | yes |
| Municipality fixed effects | yes | yes | yes | yes |
| Baseline outcome all | 0.425 | | 0.323 | |
| Baseline outcome female | 0.359 | | 0.217 | |
| Baseline outcome male | 0.494 | | 0.428 | |
| R-squared | 0.046 | 0.064 | 0.040 | 0.088 |
| Observations | 33,414 | 33,414 | 29,686 | 29,686 |

*Notes:* Least squares regressions. Sample: Pooled sample of all matched parent-children observations in the three survey cohorts. Dependent variables: Binary variable indicating the choice of a STEM (Science, Technology, Engineering, and Mathematics) course profile at secondary school in columns (1) and (2); binary variable indicating the choice of a STEM field of study after secondary school in columns (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 (see Section 3 for details). Baseline values are calculated based on observations with non-missing information on STEM choices. Parent skill difference is math – language; parent skills are standardized with mean zero and SD one in full sample of parents in each survey cohort. Grandparent education is measured by four categories of the highest level of education of both grandparents. Parent social background is measured by seven categories of occupation types of the main breadwinner in the parent household. 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.