

Is Social Inequality in School-Age Achievement Generated before or during Schooling? A European Perspective

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Abstract

Social gaps in children's educational achievement emerge early in life and remain stable over schooling. Does social origin constantly shape achievement or is social inequality in school just an echo of inequality settled before schooling? We extend the previous research by studying the origins of social gaps in language achievement among primary-school students in Germany, the Netherlands, and the United Kingdom. Based on dynamic accounts of skill development, we expected social origin to shape school-age achievement not only directly but also indirectly via before-school achievement. Using longitudinal data (Cohort Study on Educational Careers, Millennium Cohort Study, and National Educational Panel Study) and applying an instrumental variable approach, we estimated the extent to which achievement gaps by parental education in school were generated before and during schooling. About 50–80 per cent of language gaps observed at end of primary school were explained by gaps settled before formal schooling in all three countries. Conversely, at most 20–50 per cent of school-age gaps were generated during schooling. These findings suggest that the roots of social inequality in school-age achievement must be sought primarily in processes transpiring before school life starts.

Introduction

Social gaps in achievement emerge and grow before school enrolment and remain by and large stable over schooling. This pattern was documented in many Anglophone countries, such as the United States, the United Kingdom, Canada, and Australia (Feinstein, 2003; Cheadle, 2008; Potter and Roksa, 2013; Bradbury *et al.*, 2015; von Hippel and Hamrock, 2019). Most recently, Skopek and Passaretta (2021) added generality

to this statement by documenting the evolution of social gaps in achievement from infancy to adolescence in Germany.

Such findings draw the attention of scholars and policymakers being concerned with inequality of educational opportunities towards the early years of life (see, e.g. Farkas and Beron, 2004). If achievement gaps are already visible prior to school enrolment and remain constant thereafter, does it imply that social inequality in school-age achievement is generated before schooling?

Simply inspecting the temporal evolution of achievement gaps by social origin cannot answer this question (Protopapas *et al.*, 2011; Baumert, Nagy and Lehmann, 2012).

To illustrate the point, we employ a thought experiment featuring two hypothetical scenarios. In scenario *A*, a child's rank in the distribution of achievement at preschool age perfectly predicted the same child's rank at school age. Social origin shaped school-age rank only indirectly, namely through earlier achievement, without having any additional direct impact on ranks afterwards. Hence, social gaps in school-age achievement were generated *before* school age and, due to rank persistency of achievement, carried forward in school. In the opposite scenario *B*, there was complete rank mobility (zero persistency) in achievement distributions. A child rank in preschool-age did not predict rank in school age and, therefore, social gaps formed before school did not carry over schooling. Instead, social origin operated directly on achievement in school age in such a way to reproduce the same inequality pattern found in preschool age. Hence, social inequality in school age was generated *during* schooling. The two hypothetical scenarios feature fundamentally different generative processes of achievement inequality by social origin. However, both scenarios feature the same empirical pattern of stable social gaps in achievement over schooling as observed in many countries. It is precisely the aim of our study to examine the empirical plausibility of either scenario.

In contrast to the previous studies that established the longitudinal profile of social gaps in achievement, we estimate the extent to which social inequality observed later in school age is generated earlier, namely *before* children entered primary schooling. This allows us to empirically assess the importance of social origin as a force that shapes achievement inequality *during* school age as a flip side of the coin (i.e. after children entered school). Our theoretical framework accounts for the possibility that inequality in achievement is a path-dependent process in which current states of inequality are (at least in part) inherited from previous states (DiPrete and Eirich, 2006). Such a dynamic model of inequality is not new in scholarship on educational disadvantage. Examining the United States, the United Kingdom, and Australia, Bradbury *et al.* (2015) adopted a similar perspective and found that most of the social inequalities in school achievement are produced before school life starts and simply carried over schooling.

Cross-national evidence including various European countries characterized by different institutional settings is lacking, however. Our study fills this gap by providing comparable and complementary evidence for the European

context. We apply our framework to two national contexts not analysed so far, Germany and the Netherlands, and to the previously analysed context of the United Kingdom as a benchmark. Germany and the Netherlands are theoretically interesting cases as they contrast in many institutional and sociocultural ways the typical liberal welfare state contexts previously studied. We focus on achievement related to language, which is one of the most critical dimensions of cognitive development in the early years of life and crucial for educational success and later life chances (Farkas and Beron, 2004; Alexander, 2008). We use data from the most detailed and reliable cohort studies available in the three countries (Millennium Cohort Study [MCS], Cohort Study on Educational Careers [COOL], and National Educational Panel Study [NEPS]). All three datasets provide high-quality and longitudinal measures of children's achievement across comparable age. We use parental education as a marker of social origin that is theoretically meaningful and empirically comparable across the three countries. Furthermore, we employ a harmonized longitudinal design that, for each country and child, entails one achievement measure before schooling (around age 5) and two measures over the course of primary schooling. To properly estimate persistency in achievement distributions in the presence of error-prone measures of achievement, we employ an instrumental variable estimation approach. Unlike achievement research based on pre-harmonized, cross-national, and cross-sectional assessment studies such as the Programme for International Student Assessment (PISA), our study builds on country-specific longitudinal child cohort studies. Thus, despite all ex-post-harmonization efforts, methodological differences between datasets inevitably impose limitations in interpreting country differences in findings. Notwithstanding, we highlight a striking similarity in our cross-national findings, which suggests that from 50 to 80 per cent of social-origin gaps in achievement found later in primary school can be attributed to achievement gaps right before school starts.

Theoretical Framework

The Dynamic Nature of Skill Formation

Understanding why there is an association between social origin and achievement among children in school age requires insights on the process through which inequality is generated starting from early childhood. Skill formation is often described as a process through which skills—rather than simply inherited—are dynamically produced starting from the early years. Heckman and Cunha (2007) describe the process of skill formation as a multistage and dynamic process in which the current

skill level (φ_t) is a positive function of previously acquired skills (φ_{t-1}) and past family environment and investment (I_{t-1}):

$$\varphi_t = f(\varphi_{t-1}, I_{t-1}).$$

This model conveys two important ideas regarding skill development at the individual level. First and foremost, skills are self-productive because the acquisition of skills (φ_t) is higher for higher stocks of previously accumulated skills (φ_{t-1}). This dynamic leads to path-dependencies in the sense that ‘skill begets skill’. Second, past family environment and investment (I_{t-1}) are not only effective in the short run (φ_t) but bear also long-lasting consequences due the indirect influence they have on subsequent skill levels (e.g. φ_{t+2}) through the development of skills in between (φ_t and φ_{t+1}). We focus on the dynamics of skill formation between two phases of children’s life courses in this article: the pre-school period—ranging from birth up to pre-primary education—and the school period—ranging from the school start towards the end of primary school.

Innate abilities (φ_0), determined at birth, may correlate with social origin and contribute to the social gaps in children’s achievement. However, abilities are not fully determined genetically, and gene expression is also a function of the environment (Nisbett *et al.*, 2012; Heckman and Mosso, 2014). Randomized experiments show that exposure to better environments during (early) childhood has the potential to improve achievement and long-term educational outcomes (Heckman *et al.*, 2010; Chetty, Hendren and Katz, 2016; Doyle, 2020). This evidence suggests that social origin, which shapes the family environment and parental investments, may generate social inequalities in skill development even when there are no differences in innate abilities.

Social causation models connect social origin and child outcomes through a variety of mechanisms, most of which relate to the quality of parent-child interactions and the financial resources. Better-off families can invest more material and immaterial resources to satisfy children’s biological and emotional needs and are less likely to experience economic hardship and distress, both of which create more favourable conditions for children’s cognitive and non-cognitive development (McLoyd, 1998; Conger and Donnellan, 2007). For example, highly educated parents may involve their children in educationally relevant activities (Lareau, 2006; Ermisch, 2008), and are less likely to experience financial constraints that strain routine parenting practices (Hoff, Laursen and Tardif, 2002; Dotterer, Iheoma and Pungello, 2012).

In the early years of life parental investment and family environment are particularly consequential for cognitive development. Indeed, children’s brains are malleable and most receptive to external stimuli at the early stages (Sowell *et al.*, 2003; Doyle *et al.*, 2009). Hence, short and long-run effects of social inequality in family environment and investment operating from birth up to the school entry contribute to the emergence of social inequality in children’s educational achievements already before school.

The dynamic model of skill formation postulates some resilience of social-origin gaps in achievement over time. According to the principle of self-productivity of skills at the individual level, if current skills are a positive function of past skills, then current disparities among children will be persistent throughout future achievement distributions. Because of such persistency in achievement ranks, it is likely that a certain fraction of social inequality in achievement settled by the end of the preschool phase is carried over to primary schooling. Moreover, ability sorting in school systems may exacerbate further the effects of skill self-productivity. For example, children with better early educational achievement may be channelled into schools with higher quality teachers (Borman *et al.*, 2005) or benefit from peer effects when matched into homogeneous classrooms or ability groups (Gamoran, 1992; Hoxby, 2000; Condron, 2008).

In addition to persistency in achievement inequality, parental investments, and family environments continue to shape children’s educational experiences and learning opportunities after they transited to school life. School and classroom segregation along social lines can foster unequal learning opportunities for children who started school life with similar intellectual resources. Hence, one would expect that social origin is associated with school-age achievement over and above achievement before school entry. For example, better-off families may be more efficient in detecting and compensating for a poor start of their children or even try to secure further advantages after a bright start (Bernardi, 2014). The mechanisms working towards the *compensation* for a poor-performing start or the *boosting* of a high-performing start relate to various forms of parental investment ranging from parents’ direct involvement—e.g. *via* stimulation or emotional support at home—to the exploitation of opportunities offered by primary school differentiation—e.g. *via* enrolment in high-quality schools, private education, or private tutoring. A related question is whether there is symmetry between compensatory of boosting dynamics or, to the contrary,

either of the two dominates. In the latter case, the impact of social origin on children's school achievement will be a function of their earlier (pre-school) achievement.

Analytical Framework and Objectives

To operationalize our theoretical considerations into an empirical setup, we draw upon an analytical mediation framework that distinguishes two main channels through which social origin operates on language outcomes of school-age children (see Figure 1). An *indirect* channel, which runs through opportunities of early cognitive development and language acquisition from infancy to preschool age up to the school start. And a *direct* channel, which operates *via* the additional role of social origin unfolding over schooling. The *indirect* channel requires that social origin played a role before the school enrolment (*A*) and that achievement levels are directly associated over time (*B*). The latter induces rank persistency in achievement which may reflect either a causal process of path-dependency ('skill begets skill'), the presence of a stable underlying ability that causes child achievement outcomes at different times, or a mixture of both. In any of those scenarios, the indirect channel (path *AB*) captures mechanisms of social inequality in achievement that operate mostly *before* the exposure to formal schooling but are consequential for the social inequality in the classroom.

The *direct* channel requires that, over and above a child's preschool-age achievement, social origin predicts additional (dis)advantage in development and learning (*C*). Hence, the direct channel captures mechanisms of social inequality unfolding *during* school age, that is, after children had entered formal schooling. The dashed line indicates that the direct channel might be a function of prior achievement, which results in a more complex scenario of moderated mediation. For example, if there is more compensation at the bottom and less boosting at the top, the direct channel of social reproduction will be

larger at the lower end and smaller at the upper end of the distribution of preschool-age achievement.

Our primary goal is to assess the relative strength of the two analytical channels, which mirror the overall outcome of the various mechanisms of social inequality operating *before* (indirect) and *during* (direct) school life. Note that such analytical distinction cannot be made by simply looking at the evolution of social inequality in achievement from early childhood to schooling. It is beyond the scope of the paper to specify and test the complex array of specific mechanisms of the social origin-achievement association. Neither our paper can test the causal validity of the skill-begets-skill hypothesis. Rather, our study aims to estimate the empirical manifestations of an entire system of causal mechanisms that jointly operate in linking social origin and child achievement at different stages of children's development. Our study provides an overarching framework to determine the relative importance of two distinct institutional stages in children's early lives—preschool and school age—when it comes to the formation of social inequality in educational opportunities. In the following, we outline general expectations about cross-country variations in the strength of the links depicted in Figure 1.

Institutional Contexts and Expectations

We expect to detect evidence for both the *direct* and the *indirect* channels in all three countries. The differences in the institutional configurations motivate some more nuanced hypotheses, however. First, we expect Path *A* (Figure 1) to be strongest in the United Kingdom. The United Kingdom resembles a 'liberal' welfare state regime characterized by larger inequality in living conditions of families compared with the generous welfare state regimes in Germany and the Netherlands. Moreover, in the United Kingdom, early childhood education and care (hereafter: ECEC, 0–3) and kindergarten (3–4 to 6) are quite costly, scarcely available, and often

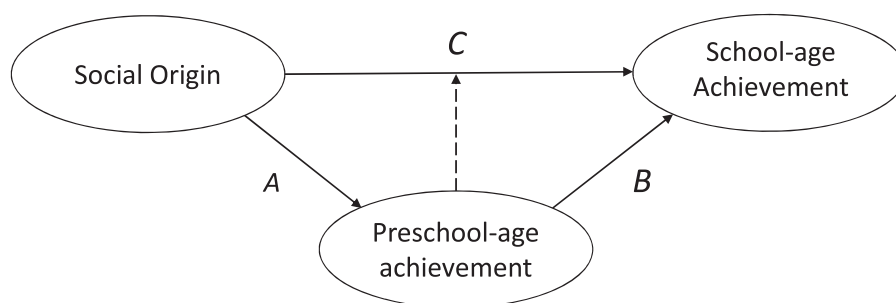


Figure 1. Direct and indirect paths to social inequality in school-age achievement

run by low-qualified staff (West, Blome and Lewis, 2020). In contrast, the Netherlands and Germany are characterized by relatively high attendance rates in ECEC and nearly universal kindergarten systems (Blossfeld *et al.*, 2017; Leseman *et al.*, 2017). A Dutch peculiarity is nation-wide programmes aimed at reducing early educational and developmental disparities (e.g. the Dutch Educational Disadvantage Policy *Onderwijsachterstandenbeleid*). The comparably higher level of economic inequality of the United Kingdom may magnify social inequality in achievement in the early years of life. At the same time, a relatively low utilization of ECEC in the United Kingdom may limit its potential to mitigate social inequality in early skill development. Conversely, higher uptake of ECEC and kindergarten services in Germany and especially in the Netherlands may effectively compress social gaps in early achievement. Summing up, we expect social inequalities in preschool-age achievement to be smallest in the Netherlands, highest in the United Kingdom, and medium-level in Germany.

Second, the self-productivity of skills is a general principle and there is little *a priori* reason to expect cross-country differences in the dynamics of achievement from preschool to school age (Path B in Figure 1). Despite strong differentiation among secondary school tracks, primary education in Germany is highly standardized in terms of curricula, organizational structures, and teachers' education (Allmendinger, 1989). In the United Kingdom, primary education is comprehensive in all constituent countries, although England has a somewhat stronger emphasis on parental choice and school autonomy. In other constituent countries, however, school autonomy is more limited (West *et al.*, 2010). As in England, the Dutch primary education system is highly decentralized, but school autonomy is balanced by a strong role of the inspectorate of education (OECD, 2016). Indeed, the national funding rules apply equally to all Dutch schools (both public and private) and schools with a higher share of disadvantaged children are allocated substantially more funding from the national education budget. Despite these notable differences among school systems, primary education is comprehensive and ability sorting quite uncommon in primary school systems of all three countries. For these reasons, we expect to find rather similar persistency in achievement hierarchies (Path B) across countries.

Third, in relation to the direct channel of social reproduction (Path C in Figure 1), we expect to find the largest effect size for the United Kingdom. The reasons behind this expectation are similar to those stated in the case of the formation of early social inequality in

achievement. A comparably larger inequality in living standards in the United Kingdom might magnify the role of mechanisms favouring the influence of social origin on educational achievement over school age. When compared with United Kingdom's liberal, means-tested welfare state model, the more generous German and Dutch welfare systems may do better in limiting social-origin effects unfolding over the school years.

Despite specific country differences in the organization and the timing of ECEC and kindergarten, children in all countries are enrolled in some form of pre-primary education (ISCED 0) at around age 5, while starting primary schooling in the form of ISCED 1 only at around age 6–7. Hence, even though the Dutch kindergarten and the British reception class are formally part of the primary school systems, formal teaching in the classroom (ISCED 1) starts at approximately the same age in all countries. In the remaining of the text, we refer to the preschool-age period as the period ranging from birth up to age 5, which is a biographical point in which children have not yet been exposed to formal teaching in primary school (ISCED 1) in all countries.

Data and Methods

Data and Sample Selection

Our analysis draws on longitudinal data from the NEPS in Germany, the COOL in the Netherlands, and the MCS in the United Kingdom. NEPS and COOL data have a multi-cohort sequence design in which children are followed longitudinally starting from different stages in their lives. We use the *Kindergarten Cohort* of the NEPS and the *Cohort 1* of the COOL, which collected longitudinal information on samples of children enrolled in kindergarten (around age 5) in 2010/2011 and 2007/2008 (respectively) up to the end of primary school. The MCS is a birth-cohort study that collected longitudinal information on one single cohort of children born in the United Kingdom in 2000/2001 and followed up until 15 years of age (Hansen, 2014). For the sake of comparability, we limit the analyses between age 5—when most of the children are not yet exposed to formal teaching in primary school—and age 11—the end of primary education approximately. While there are minor differences in the historical periods of the data collection in the three datasets, it is unlikely that cohort effects bias our cross-national comparisons significantly: all children are observed for the first time between 2005/2006 and 2010/2011.¹

All three child cohort studies are representative at the national level² and contain detailed information on children's language abilities and their parental background. The MCS, NEPS, and COOL data include an overall

Table 1. Observation window and competence assessments for the three countries

	Preschool = age 5	Grade 1 = age 7	Grade 3 = age 8/9	Grade 5/6 = age 11
United Kingdom	BAS naming vocabulary	BAS word reading		BAS verbal similarities
The Netherlands	Language/emergent literacy		Vocabulary	Vocabulary
Germany	Vocabulary	Vocabulary	Vocabulary	

sample of 19,243, 2,996, and 7,075 children, respectively. From these samples, we removed children with no details on either social origin or migration background. From the NEPS and COOL samples, we additionally removed first-generation migrants because they are excluded by design in the MCS. Furthermore, we used balanced samples by including children only for whom we have data on all waves within the observation window. The balanced analytical samples include 9,866 children in the United Kingdom, 420 in Germany, and 1,115 in the Netherlands.

Attrition in NEPS occurred mostly in the transition from kindergarten to grade 1 because of the school-based sampling design (only a subset of the kindergarten sample attended schools that were sampled in following rounds, for documentation, see Würbach, 2019). Hence, although not in a socially selective fashion, the sample size dropped substantially after the transition to primary education. Attrition is mainly a result of school dropouts after first and second waves in COOL. In both NEPS and COOL, dropout risks are higher if children switch school or are retained in a grade. These issues are less severe in the MCS due to child-based sampling design; but there is still attrition over waves.

Attrition can induce bias in our case if it is a function of achievement and social origin. We tackled selective attrition *via* inverse probability weighting, which is a commonly used method to correct bias in longitudinal data (Seaman and White, 2013). Experimenting with multiple imputation as an alternative strategy to tackle attrition yielded very similar findings. The weighting strategy differed slightly across countries due to survey-specific circumstances (in the MCS, inverse probability weighting was combined with multiple imputation). Details on the procedure used to account for selective attrition are in Supplementary Appendix A1.

Language Achievement

We focus on language skills for theoretical and pragmatic reasons. First, language is a crucial cognitive domain in the early life course. Language and the knowledge of vocabulary are necessary to communicate effectively with teachers and peers (Dockrell, Lindsay and Palikara, 2011) and mediate learning in a variety of competence domains, such as reading, writing and even mathematics

(Shaftel *et al.*, 2006; Mercer and Sams, 2008; Snow, Lawrence and White, 2009). Second, language skills have substantial returns in the labour market (e.g. Hanushek *et al.*, 2015), and evidence on the persistence of social gaps in language is therefore relevant for our understanding of intergenerational social mobility. Third, our focus on language enables the cross-national comparison as the national datasets had a similar timing in the measurement.

Table 1 reports details on the nature and timing of language-related tests administered to children in each country and wave. In the MCS, language-related skills were measured by standard tests based on the British Ability Scales (BAS) in terms of expressive language abilities (age 5), reading abilities (age 7), and verbal reasoning and knowledge (age 11). NEPS used an adjusted version of the Peabody Picture Vocabulary Test (PPVT) to measure children's receptive vocabulary. Like NEPS, COOL tested receptive vocabulary/emergent literacy (age 5) and vocabulary knowledge (ages 8 and 11).³ Although there are differences in timing and type of tests, all tests aim to measure language-related abilities and cover a similar observation window in the three countries.

Following standards in the literature (e.g. Feinstein, 2003; Bradbury *et al.*, 2015), we measure *relative* test score differences rather than *absolute* test score differences. Comparing achievement gaps in absolute terms (e.g. competence or proficiency scores) is sensible only if test score scales are strictly comparable between different ages and countries. In the context of our data, such metric comparability is given neither within nor between countries. Relative measures express gaps in relation to the overall variation of test scores and are useful for gauging inequality in achievement across groups even when absolute comparisons are not possible (Reardon, 2008; Skopek and Passaretta, 2021).⁴ An obvious limitation is that the relative approach is mute about the magnitude of gaps in the actual levels of proficiency. Furthermore, based on relative measures we estimate the persistency in terms of distributional position rather than the predictive relationship between competence levels. Although facilitating comparisons of achievement gaps at different educational stages, relative

comparisons between countries hinge on additional assumptions such as similar reliability and validity between different tests (e.g. the BAS versus the German adaption of the PPVT) which we cannot directly test with our data. This caveat should be kept in mind when comparing achievement gaps between countries.

We z -standardized test scores within each wave to have a mean of 0 and a standard deviation of 1, separately for each country. Note that our z -score measures assume that test scores are interval scaled. However, we ran all analyses using percentile ranks, which do not require interval scaling assumptions, and obtained very similar results (see Supplementary Appendix C). To increase precision of our estimation, we adjusted test scores for slight age differences among children in the same life stage. Following Bradbury *et al.* (2015), the procedure involved a z -standardization of the residuals from a linear regression of raw test scores on a cubic function of age at test day (see Supplementary Appendix A2 for details).

Covariates

Like some of the previous studies (e.g. Bradbury *et al.*, 2015; Linberg *et al.*, 2019), we focus on achievement gaps by parental education. Parental education is a crucial and rather stable resource shaping the socioeconomic conditions of the origin family. From a life-course view, educational attainment is usually antecedent to occupational attainment and parenthood; from a measurement view, it is less transitory and less subject to measurement error compared with alternative measures based on income. We measured parental education by the highest years of full-time education among the parents. In Germany and the Netherlands, years of education reflect the years that are necessary to acquire the educational certificates. In the United Kingdom, the indicator reflects the actual years spent in full-time education.

In addition to a metric parental education variable, we also constructed a categorical variable by distinguishing low- (12 years or less), medium- (12 to 14), and high- (15 or more) educated parents. In the German case, we only distinguish medium-low- (15 years or less) from high- (16 or more) educated parents due to the small sample size. We used 16 rather than 15 years of schooling as a threshold to define highly educated parents because 15 years fall below tertiary education in the German system (that is *Abitur* plus additional non-tertiary training). In contrast, 15 years correspond to a tertiary degree in the Netherlands. The metric and the group-based approach are complementary. On the one hand, a single metric allows parsimonious models which

bring advantages when working with the relatively small samples of COOL and especially NEPS. On the other hand, categorical variables enable a better examination of potential non-linear relationships between parental education and children's achievement. In addition, a group-based approach might be more relevant when considering targeted policy interventions.

We controlled for migration background in all analyses. Migration background is a separate dimension shaping achievement inequality but may correlate with parental education. Migration background is defined by having at least one parent not born in the country. Note that, for our purpose, we did not control for mediating variables that reflect specific social inequality mechanisms—such as parenting styles, school choice, or family demographic characteristics. Although uncorrelated with parental education in our representative samples, we did control for gender to increase the precision of our estimates.

Analytical Strategy

We estimated three main sets of models for each country. The first set estimates the overall achievement gap (α_{1t}) by parental education (EDU) at each time point ($t = 1, 2, 3$) adjusted for migration background (MIG) and child sex (SEX):

$$z_{it} = \alpha_{0t} + \alpha_{1t}\text{EDU}_i + \alpha_{2t}\text{MIG}_i + \alpha_{3t}\text{SEX}_i + \epsilon_{it} \quad (1)$$

where z_{it} refers to the z -score of the child i at time point t . A second set estimates the separate effects of the initial achievement (z_{i1}), measured at the end of the preschool-age period ($t = 1$), and EDU on later achievement in school age ($t = 2$ and 3):

$$z_{it} = \beta_{0t} + \beta_{1t}\text{EDU}_i + \beta_{2t}\text{MIG}_i + \beta_{3t}\text{SEX}_i + \beta_{4t}z_{i1} + \epsilon_{it} \quad (2)$$

Parameter β_{4t} models the conditional persistence of achievement hierarchies over time. We use a parsimonious linear specification to model the persistency of achievement ranks but experimented also with more consumptive specifications and obtained similar results. Note that parameter β_{1t} identifies the additional, direct association of EDU with school-age achievement that comes on top of inequality observed before the school period began. The ratio between parameters β_{1t} and α_{1t} —the direct and the overall EDU-achievement associations—indicates the relative importance of the direct (and implicitly the indirect) path leading to inequality by parental education in primary school achievement.

In a third set of models, we allow for an interaction between EDU and initial achievement. Equation 3 specifies the direct EDU-achievement association to be a

function of achievement measured in the end of preschool age:

$$z_{it} = \gamma_{0t} + \gamma_{1t}\text{EDU}_i + \gamma_{2t}\text{MIG}_i + \gamma_{3t}\text{SEX}_i + \gamma_{4t}z_{i1} + \gamma_{5t}z_{i1}\text{EDU}_i + \epsilon_{it} \quad (3)$$

This model tests our theoretical ideas of social origin (parental education) acting either as a *compensator* for poor initial achievement or as a *booster* allowing high initial achievers to gain further advantage from their social origin over the school years. In case compensation prevails, that is if $\gamma_{5t} < 0$, additional EDU advantages would be stronger among initially low-achieving children. Conversely, in case boosting prevails, that is if $\gamma_{5t} > 0$, additional EDU advantages would be stronger on the upper end of the distribution of initial achievement. If $\gamma_{5t} = 0$, Equation 3 reduces to Equation 2 stating that additional (dis-)advantages related to EDU are equal over the distribution of preschool-age achievements (provided $\gamma_{1t} > 0$). Thus, estimating Equation 3 allows us to assess whether either compensation or boosting prevails or whether both dynamics have similar strength.

Equations 1 and 2 are estimated using parental education as both a metric and a group variable. For the sake of parsimony, Equation 3 is estimated only based on the metric variable.

Measurement Error and Regression to the Mean

Ignoring measurement error in initial test scores (z_{i1}) on the right-hand side of Equations 2 and 3 may lead to artefactual estimates driven by regression-to-the-mean (RTM). Past literature discusses explicitly RTM issues in relation to estimating achievement gaps conditioning on error-prone measures of earlier achievement (Jerrim and Vignoles, 2013; Bynner, 2015). In our case, standard ordinary least squares (OLS) models that ignore measurement error in preschool-age test scores underestimate the conditional persistence in relative achievement (e.g. β_{4t} in Equation 2) and overestimate the direct association of parental education with achievement (β_{1t} in Equation 2). Note that the issue is different from bias arising from the use of standardized and error-prone scores of achievements at the left-hand side of the equation, which implies that social origin gaps decrease with test reliability (Reardon, 2011).

We deal with measurement error in preschool-age tests (when estimating Equations 2 and 3) by using an instrument variable (IV) approach that minimizes biases arising from RTM (Bradbury *et al.*, 2015).⁵ The IV strategy involves using another preschool-age score to predict the actual preschool-age score of interest and, in a second stage, plug the predicted rather than the observed values

into Equations 2 and 3. These predictions yield error-free signals of preschool-age achievements under the conditions that (i) the instrument is relevant, e.g. highly correlated with the instrumented variable; and (ii) measurement error in scores is uncorrelated between different tests. We satisfy these assumptions by using instruments which are highly correlated with language scores in preschool age—such as scientific literacy, vocabulary, and arithmetics—that are taken on a different day (NEPS and COOL) or even in a previous wave (in the case of MCS). The temporal distance between the instrument test and the focal test ensures the elimination of day-specific sources of measurement error (e.g. having good or bad luck on a day). Moreover, measurement error due to familiarity with the specific set of test items is accounted for because test items in the primary test do not overlap with test items in the test used as an instrument. In all countries, we applied two-stage least square for the IV estimation. More details on our IV strategy, including the exact specification of first- and second stages, can be found in Supplementary Appendix A3.

Even if we cannot rule out entirely other possible sources of measurement error, we are confident that the adopted IV strategy addresses the major issues. Nevertheless, we will interpret estimates on the conditional persistence and on the relative importance of the indirect channel as lower bound estimates, and estimates for the residual EDU effect—the direct channel—as upper bound estimates.

Empirical Results

Overall Social Origin Gaps in Achievement Before and During School Age

Tables 2 and 3 report the total association (TOTAL) between social origin (parental education) and children's language-related skills before primary schooling (age 5) and over the school age (ages 7–8 and 9–11) for the three countries. In line with the previous research, we find positive and statistically significant associations between parental education and achievement in all countries and time points. Notably, social inequality in achievement is already substantial when children are only 5 years of age, a point in time by which they are not yet in school in Germany and have just started school in the United Kingdom and the Netherlands. Overall, we find the social gradient at age 5 to be highest in Germany (see Table 2). Estimates suggest that each additional year of parental education in the German context is associated with roughly 13 per cent of a standard deviation increase in language achievement ($b = 0.129$). Over the full range—8 years versus 18 years of highest

Table 2. Total and direct effects of social origin on achievement by age: parental years of education

	TOTAL (Equation 1)			DIRECT (Equation 2)			
	Preschool	School		School			
		Age 7	Age 11	United Kingdom (N = 9,866)			
				Age 7		Age 11	IV
Parental education				OLS	IV	OLS	IV
Age-5 score	0.105 (0.005) ^{***}	0.109 (0.005) ^{***}	0.096 (0.005) ^{***}	0.075 (0.005) ^{***} 0.330 (0.012) ^{***}	0.050 (0.006) ^{***} 0.564 (0.026) ^{***}	0.060 (0.005) ^{***} 0.338 (0.015) ^{***}	0.038 (0.005) ^{***} 0.547 (0.030) ^{***}
				The Netherlands (N = 1,115)			
		Age 8	Age 11	OLS	IV	OLS	IV
Parental education							
Age-5 score	0.044 (0.015) ^{***}	0.073 (0.018) ^{***}	0.100 (0.016) ^{***}	0.057 (0.017) ^{**} 0.366 (0.039) ^{***}	0.043 (0.017) [*] 0.677 (0.076) ^{***}	0.085 (0.015) ^{***} 0.340 (0.032) ^{***}	0.070 (0.016) ^{***} 0.677 (0.072) ^{***}
				Germany (N = 420)			
		Age 7	Age 9	OLS	IV	OLS	IV
Parental education							
Age-5 score	0.129 (0.021) ^{***}	0.158 (0.024) ^{***}	0.144 (0.024) ^{***}	0.074 (0.023) ^{**} 0.650 (0.046) ^{***}	0.056 (0.031) [*] 0.788 (0.119) ^{***}	0.066 (0.022) ^{**} 0.600 (0.071) ^{***}	0.024 (0.029) ^{***} 0.925 (0.136) ^{***}

Notes: Dependent variables are standardized test scores at different age (see Table 1). Parental education = parents' highest years of education. TOTAL: total effect of parental education on achievement. DIRECT: conditional parental education effect after controlling for standardized achievement scores at age 5 (preschool age). All models control for migration background (at least one foreign-born parent) and gender. IV models: results from second stage estimation shown. *F*-statistic for instrument relevance test: United Kingdom = 1,432; The Netherlands = 288; and Germany = 61. Full models for Equations 1 and 2 (IV only) are available in Supplementary Appendix Tables B1 and B3. Standard errors in parentheses.

Significance: * $P < 0.05$, ** $P < 0.01$, and *** $P < 0.001$.

Table 3. Total and direct effects of social origin on achievement by age: Parental educational groups

	TOTAL (Equation 1)			DIRECT (Equation 2)			
	Preschool		School		School		
	Age 5	Age 7	Age 11	United Kingdom (N = 9,866)	Age 7	Age 11	IV
Parental education				OLS	IV	OLS	IV
Medium	0.289 (0.027) ^{***}	0.292 (0.028) ^{***}	0.247 (0.031) ^{***}	0.196 (0.027) ^{***}	0.128 (0.029) ^{***}	0.149 (0.028) ^{***}	0.088 (0.029) ^{***}
High	0.594 (0.031) ^{***}	0.634 (0.030) ^{***}	0.552 (0.029) ^{***}	0.437 (0.030) ^{***}	0.298 (0.033) ^{***}	0.350 (0.026) ^{***}	0.226 (0.030) ^{***}
Age-5 score				0.333 (0.012) ^{***}	0.566 (0.026) ^{***}	0.340 (0.015) ^{***}	0.549 (0.030) ^{***}
The Netherlands (N = 1,115)							
Parental education				OLS	IV	OLS	IV
Medium	0.270 (0.085) ^{**}	0.218 (0.102) [*]	0.283 (0.080) ^{***}	0.119 (0.098)	0.035 (0.102)	0.191 (0.068) ^{***}	0.101 (0.075)
High	0.363 (0.092) ^{***}	0.494 (0.122) ^{***}	0.679 (0.111) ^{***}	0.361 (0.115) ^{**}	0.249 (0.119) [*]	0.557 (0.119) ^{***}	0.435 (0.106) ^{***}
Age-5 score				0.366 (0.040) ^{***}	0.676 (0.076) ^{***}	0.339 (0.031) ^{***}	0.674 (0.071) ^{***}
Germany (N = 420)							
Parental education				OLS	IV	OLS	IV
High	0.442 (0.091) ^{***}	0.575 (0.120) ^{***}	0.560 (0.101) ^{***}	0.271 (0.100) ^{**}	0.215 (0.120)	0.282 (0.095) ^{**}	0.154 (0.102)
Age-5 score				0.689 (0.048) ^{***}	0.816 (0.112) ^{***}	0.631 (0.075) ^{***}	0.919 (0.123) ^{***}

Notes: Dependent variables are standardized test scores at different age (see Table 1). Parental education = parents' highest years of education grouped to low, medium, and high (low-medium and high in Germany); reference category is low (medium-low in Germany). TOTAL: total effect of parental education on achievement. DIRECT: conditional parental education effect after controlling for standardized achievement scores at age 5 (preschool age). All models control for migration background (at least one foreign-born parent) and gender. IV models: results from second stage estimation shown. F-statistic for instrument relevance test: United Kingdom = 1,435; The Netherlands = 279; and Germany = 60. Full models for Equations 1 and 2 (IV only) are available in Supplementary Appendix Tables B2 and B4. Standard errors in parentheses.

Significance: * $P < 0.05$, ** $P < 0.01$, and *** $P < 0.001$.

parental education—the estimate implies a gap of 1.3 standard deviations. Conversely, gaps are the smallest in the Netherlands ($b=0.044$). Gap estimates for the United Kingdom are in between ($b=0.105$). A similar cross-national pattern is found in the analyses using the categorical measurement of parental education (see Table 3). Gaps are larger in the United Kingdom and Germany compared with the Netherlands. These results are only partly in line with our theoretical expectations but consistent to a recent study that detected larger social gaps in achievement in Germany when compared with the liberal context of the United States (Linberg *et al.*, 2019). However, these results should be interpreted with some caution since the exact underlying tests—even though related to the same cognitive domain—differed between countries and the statistical uncertainty (standard errors) are comparably large for the German case.

How does social inequality in language achievement develop over the years of primary schooling? Social gaps remain constant throughout primary schooling in the United Kingdom, and this holds true for both the results from the metric and the group-based approach to the measurement of parental education (effect size 10–11 per cent of a standard deviation for each additional year of parental education; 25–29 per cent of a standard deviation comparing children with medium- and low-educated parents; 55–63 per cent of a standard deviation comparing children with high- and low-educated parents). In contrast, point estimates suggest slightly increasing gaps in Germany and stronger increases in the Netherlands. Comparing the first (age 5) with the last time point (age 9), the social gradient increases only from 13 to 14–15 per cent of a standard deviation in Germany (from 44 to 56 per cent comparing children with high- and medium-low educated parents). However, these differences over time are within the range of estimation error ($P=0.581$ and 0.245 , respectively). When looking at the Netherlands, we observe a stronger and statistically significant increase between age 5 and 11: the social gradient more than doubles from about 4 to 10 per cent of a standard deviation ($P=0.002$). A similar pattern of increase is found between children with high- and low-educated parents (from 36 to 68 per cent; $P=0.004$) but not between children with medium- and low-educated parents. Taken together, the data at hand does not provide compelling evidence for systematically growing or shrinking gaps in Germany and the United Kingdom. Instead, there is some evidence for social gaps in language achievement to increase in the Netherlands.

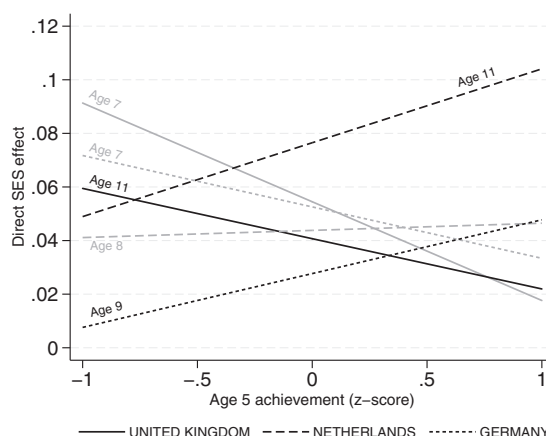


Figure 2. IV regression models (Equation 3): Direct social origin-achievement association over primary schooling along the distribution of preschool-age achievement, separately by country

Notes: interaction terms between preschool-age achievement and parental education are statistically significant only in the United Kingdom (see Supplementary Appendix Table B5).

The Role of Preschool-Age Achievement and Social Origin For School-Age Achievement

Column DIRECT in Tables 2 and 3 reports estimates for the direct association of parental education with language achievement over the school age once achievement levels at age 5 are accounted for. We report both OLS and IV estimates. The latter estimates adjust for measurement error in age-5 scores. We comment first on the results obtained by measuring parental education as a metric variable (Table 2) and then on the results from the group-based approach (Table 3).

Estimates in Table 2 reveal a strong and statistically significant association between preschool and school-age achievement in all countries. As the IV estimates suggest, achievement hierarchies settled before school entry tend to persist over primary schooling. However, there is still room for an additional role of parental education during school-age. The comparison between OLS and IV estimates clearly demonstrates that ignoring measurement error would severely underestimate persistency in relative achievement (e.g. in the United Kingdom, 0.330 for OLS versus 0.564 for IV at age 7) and, as a result, overestimate the direct association of parental education with later achievement (e.g. in the United Kingdom, 0.075 for OLS versus 0.050 for IV at age 7) in all countries. Thus, we continue to interpret only the results from the more reliable IV estimation.

Interestingly, the conditional persistence (coefficient for age 5) is stronger in Germany (0.79–0.93) compared with the Netherlands (0.68) and the United Kingdom

Table 4. Proportion (per cent) of social inequality in school-age achievement explained by social inequality in preschool-age achievement (indirect channel)

Parental education	%INDIRECT			
	United Kingdom			
	Age 7		Age 11	
	OLS	IV	OLS	IV
	Metric	31.7 (2.1)	54.1 (3.9)	36.9 (2.5)
Groups				
Medium vs low	32.9 (4.0)	55.9 (6.9)	39.8 (5.1)	64.2 (8.3)
High vs low	31.1 (2.1)	53.0 (3.9)	36.6 (2.5)	59.1 (4.4)
The Netherlands				
	Age 8		Age 11	
	OLS	IV	OLS	IV
Metric	22.1 (7.6)	40.8 (14.5)	15.0 (4.8)	29.9 (10.2)
Groups				
Medium vs low	45.4 (20.4)	83.9 (37.9)	32.3 (9.6)	64.4 (20.3)
High vs low	26.8 (7.7)	49.7 (14.9)	18.1 (4.4)	36.0 (9.7)
Germany				
	Age 7		Age 9	
	OLS	IV	OLS	IV
Metric	53.4 (9.6)	64.6 (14.7)	53.9 (10.6)	83.1 (21.3)
Groups				
High vs Medium-low	52.9 (11.3)	62.7 (15.3)	49.7 (11.0)	72.5 (16.4)

Notes: Standard errors of the percentages in parentheses. % explained by the direct channel is 100 – % indirect. Parental education is measured as a metric and a group variable. OLS = naïve estimation. IV = accounting for measurement error through instrumental variable estimation. Standard errors were calculated using the Delta method.

(0.55–0.56). These findings imply that, in terms of their relative achievement and conditional on their social background, children in the United Kingdom show the highest mobility, whereas children in Germany show the lowest mobility over time. In contrast, the direct association of parental education with school-age achievement is rather similar in magnitude across all countries at age 7. At age 9–11, however, the direct effect is largest in the Netherlands and small in Germany. Note that coefficients for the direct associations fail to pass test for statistical significance in the German case, which is likely owed to the small sample size. Over and above initial achievement, parental education seems to play a role in children's achievement during primary school in the United Kingdom and the Netherlands. These findings mostly support our general hypotheses regarding the role of earlier achievement and social origin for later

achievement. However, they do not support our hypotheses about the homogeneity in achievement persistence and the comparatively stronger direct effect in the United Kingdom.

Findings are substantively the same when re-running our models using parental education dummies instead of the parental years of education (see Table 3). Here too, standard OLS models underestimate the conditional persistence while overestimating the residual association between parental education and achievement. The findings from the metric and the group-based approach are very similar in the case of the United Kingdom and Germany, which supports the linearity assumption behind the metric measurement of parental education. The categorical models reveal a different pattern for the Netherlands. When controlling for measurement error (IV), the gap between children with medium- and low-educated

parents is no longer statistically significant; a statistically significant residual effect of social origin remains only for the contrast between children with high- and low-educated parents (and the contrast between children with high- and medium-educated parents, not shown in the table). These results suggest that the direct association between parental education and school-age achievement is non-linear in the Netherlands.

The results from the first two sets of models suggest a clear conclusion: a substantial part of social inequality in school-age achievement is already determined at the beginning of the school life. However, there is evidence that social origin shapes achievement during school-age as well. Does such the additional (dis-)advantage by social origin concentrate at certain points of the distribution of early achievements? Does social origin mainly compensate for a poor start? Or does social origin mainly help children with a bright start to further excel in their school life?

Figure 2 visualizes estimation results on the statistical interactions specified in Equation 3. The visual inspection allows us to gauge how the direct social origin association (in each country and for the two points over school age) may vary as a function of achievement at age 5. Negative slopes indicate stronger compensatory effects, positive slopes stronger boosting effects, and zero-slopes constant social origin effects over the distribution of preschool-age achievement. Results from the British data suggest advantages by parental education to concentrate at the low end of the achievement distribution at age 5. Thus, parental education seems to compensate for a poor start rather than boosting further a bright start. Figure 2 indicates the opposite result for the Netherlands and Germany at ages 9–11: parental education makes a larger difference among the high performers in preschool age. However, interactions terms are statistically significant only in the United Kingdom and, therefore, it is questionable whether these findings apply beyond our samples in the Netherlands and Germany (see Supplementary Appendix Table B5). Hence, we have limited support for the prevalence of either of the two dynamics in the two non-Anglo-Saxon countries. We conclude that the direct effect of parental education is by and large homogenous among high and low performing children at age 5 in the Netherlands and, if any, in Germany. Conversely, a compensatory advantage seems to prevail over boosting in the United Kingdom.

Summary: To What Extent Is Social Inequality in School-Age Achievement Generated Before and During Primary Schooling?

Based on the model specified by Equation 2, Table 4 summarizes the relative importance of the direct (*C* in

Figure 1) and indirect channels (*AB* in Figure 1) relating social origin (parental education) to school-age achievement. Table 4 shows the percentage of the social inequality in school age that is explained by the amount of social inequality accumulated by age 5 (indirect channel *AB*). Note that the percentage explained by the direct channel is simply 100 minus the percentage explained by the indirect channel. Numbers are shown separately for each country, time-point over primary schooling, and measurement approach to parental education (metric versus group-based).

Our IV estimates let us conclude that the lion's share of the social gaps in achievement observed in school-age is determined before children enter school life in Germany and the United Kingdom. As the IV estimates suggest, unequal achievement at age 5 explains from 53 to 64 per cent of the overall social gaps observed in school age in the United Kingdom (a minimum of 53 per cent at age 7 comparing children with high- and low-educated parents—a fraction of $1 - 0.298/0.634 = 0.53$ —and a maximum of 64 at age 11 comparing children with medium- and low-educated parents—a fraction of $1 - 0.088/0.247 = 0.64$). These shares are even higher in Germany, where preschool age inequality is responsible for 63 to 83 per cent of school-age gaps. Yet, social inequality in preschool age is not the whole story. From 17 to 47 per cent of the association between social origin and achievement in school age seems attributable to mechanisms that operate after children entered school life in the United Kingdom and Germany.

We observe a partly different scenario in the Netherlands. Based on the IV estimates from the metric approach, one would conclude that language differences observed in kindergarten age explain the minority of social inequalities in school age (around 41 and 30 per cent at ages 8 and 11, respectively). However, when resorting to the comparison between parental education groups some interesting differences emerge. Language differences observed in kindergarten age explain around 64–84 per cent of later gaps between children with medium- and low-educated parents. These results are consistent with the findings from the United Kingdom and Germany. The scenario is very different when looking at the high-low gap, which seems driven by language inequality observed in preschool age to a lower extent (36–50 per cent). However, we need to bear in mind that our estimation may still exaggerate the importance of the direct channel.

Finally, we want to highlight that standard OLS models that assume no error in achievement measures at age 5 produce a quite biased picture. As Table 4 demonstrates, naïve OLS models severely underestimate the

relative importance of the indirect channel in comparison to IV estimation. Taking an average among all countries, timepoints, and approaches to the measurement of parental education, we conclude that the proportion of social inequality in school-age achievement explained by social inequality in preschool age is 68 per cent higher when we correct for measurement error. If measurement error responsible for RTM was neglected, one would mistakenly conclude that from half to the great majority of social inequality in school achievement was generated during the school period in all countries.

Conclusions

We analyzed parental education gaps in language skills among primary-school students in Germany, the Netherlands, and the United Kingdom, and estimated the extent to which those gaps can be explained by skill gaps settled before children enter school. We found that social inequality in language achievement was well-established even before children had entered formal schooling. Those early gaps were larger in the United Kingdom and Germany compared with the Netherlands. Moreover, these social gaps remained stable over primary schooling in the United Kingdom and Germany but enlarged in the Netherlands. But, do early gaps explain why children of low-educated parents fall behind their peers in school-age? We found evidence for substantial persistency of social inequality in achievement hierarchies over time. Social inequality in achievement settled before school exposure explains a major share of social inequality in achievement among school-age children: around 55–65 per cent in the United Kingdom and even up to 65–80 per cent in Germany and the Netherlands (for the latter this holds only when looking at the gap between children with medium- and low-educated parents). Using the United Kingdom as a benchmark, those findings add new evidence on European countries to the previous analyses on the United States, Canada, and Australia by Bradbury *et al.* (2015). Our results suggest that persistency in social inequality in achievement is by no means lower, and indeed partly even higher, in European countries that differ substantially from the Anglophones in terms of their educational and institutional contexts. Altogether, there is mounting evidence that social-origin gaps in school achievement are explained by inequality mechanisms operating in the years before school life. Nonetheless, a non-negligible proportion of social gaps in school age may be attributable to social background continuing to shape children's achievement over schooling (around 20–35 per cent in Germany and the Netherlands, 35–45

per cent in the United Kingdom). Finally, we tested for the prevalence of either boosting and compensation mechanisms. We did not find sufficient evidence that would support the prevalence of either of those mechanisms in Germany and the Netherlands. However, for the United Kingdom, we found some evidence for the prevalence of the compensatory role of social origin because the social advantage in school age concentrated at the lower end of the achievement distribution in preschool age.

Our study contributes to educational inequality research by drawing scholars' attention to the importance of those mechanisms of (dis-)advantage which generate unequal outcomes in school life but operate long before children enter the classroom. Evidence from a variety of countries demonstrates that social gaps in learning expand long before school and remain by and large stable over schooling (Cheadle, 2008; Bradbury *et al.*, 2015; von Hippel and Hamrock, 2019; Skopek and Passaretta, 2021). This very evidence brings to the fore an intuition, namely that inequality mechanisms operating in the early years are formative for unequal outcomes in school. Our article substantiated such intuition and showed that most of social inequality in school-age achievement is generated before school starts.

Our results also deliver complementary evidence to seasonal comparison research that pointed towards the compensatory role of schooling (Downey, von Hippel and Broh, 2004), although recent research suggest schooling does not exacerbate nor compensate for SES inequality in achievement (von Hippel and Hamrock, 2019; Passaretta and Skopek, 2021). While we were not in the position of disentangling the role of school and non-school factors, the fact that a large proportion of the social gaps observable in school is rooted the preschool period contrasts with the critical view of schooling as the main locus of the social reproduction of inequality (Downey and Condron, 2016; Passaretta and Skopek, 2021). On the methodological side, we showed that naïve estimation methods assuming error-free measures of early achievement systematically underestimate the dynamics of achievement inequality. In fact, had we relied on standard OLS, our conclusions would have been opposite, namely, that most of social inequality in achievement found in school is produced by mechanisms operating during school age.

Findings from our study can inform social policy. First, promoting more equal opportunities before children go to school seems to be an effective way of reducing social inequalities in school-age achievement. In this regard, our findings connect to a large body of research arguing that early educational programs can yield

substantial benefits, particularly for disadvantaged children (see van Huizen and Plantenga, 2018; Kulic *et al.*, 2019). Hence, whether directed towards the expansion of early education and care or the direct support within the family environment (e.g. Doyle, 2020), early interventions have the potential to reduce not only social inequality in early educational achievement but also at later educational stages. The cross-country comparison suggests that preschool-age interventions, while generally more effective than school-age interventions everywhere, would be particularly effective in Germany, where preschool-age gaps explain most of the social inequality in achievement later.

Our study has limitations that invite further research. First, our study could not tap into the various micro-level mechanisms underlying the association between social origin and achievement. Other research may try to isolate those mechanisms while considering that early-life mechanisms are especially consequential for later achievement inequality. Second and related to the first point, our evidence is of associational nature and limited in identifying the causal relationship between earlier and later achievement as well as the causal effect of social origin on achievement. However, our nuanced description can provide orientation to studies that attempt to isolate more specific cause-and-effect relationships. Third, we investigated achievement gaps by parental education. Although being one of the most important factors determining the own social origin and family SES (Duncan, Magnuson and Votruba-Drzal, 2015), there are other crucial dimensions at play such as parental occupational status, prestige, family income, or wealth. Further research could attempt to aggregate those various factors of SES into composite measures or try to estimate their potential separate roles. Finally, although all tests used were related to the domain of language, the exact tests partly differed over time in the United Kingdom and the Netherlands. This caveat might explain some of the cross-country differences in findings but is unlikely to explain the general similarities we have detected. Further research may also try to replicate our analyses for different intellectual domains, such as numeracy and maths.

Supplementary Data

Supplementary data are available at ESR online.

Notes

- 1 We follow the competence development of children age 5 in 2005/2006 in the United Kingdom, 2007/

2008 in the Netherlands, and 2010/2011 in Germany.

- 2 The sampling design of the MCS differs slightly from NEPS and COOL. The MCS has an age-based design while NEPS and COOL have a school-based design. However, kindergarten is almost universal in Germany and mandatory by age 5 in the Netherlands, thus making children sampled in kindergarten representative of the population of children at the national level.
- 3 COOL also includes test scores on reading skills (ages 8 and 11). We obtain similar results when using data on reading instead of vocabulary scores.
- 4 Strengths and limitations of the relative approach have been discussed extensively in other studies (see, e.g. Skopek and Passaretta, 2021).
- 5 An error-in-variables (EIV) approach may be an alternative to correct for measurement error. Unfortunately, the lack of precise information on test reliability for all tests and countries rendered an EIV approach infeasible in the context of our study.

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