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Schooling and adult cognitive skills: evidence from education reforms

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Abstract

We examine whether more schooling causally increases the cognitive skills of adults. Using education reforms in twenty-one European countries as natural experiments, we link cohorts' exposure to reforms to internationally comparable numeracy and literacy scores from Organisation for Economic Co-operation and Development–Programme for the International Assessment of Adult Competencies. An extra year of schooling increases adult skills by about 0.2–0.25 standard deviations. The gains are non-linear: completing high school and, especially, college delivers larger skill improvements than the average additional year. Across reform types, expansions of pre-primary and secondary education generate the largest increases in skills. Using information on how individuals use skills at work and at home, we provide evidence consistent with a mechanism whereby education improves access to skill-intensive jobs that sustain and further develop skills over the life cycle. Overall, raising educational attainment can deliver lasting skill gains, particularly when followed by opportunities to use and build skills in the labour market.

Keywords cognitive skills, educational policies, returns to schooling
JEL classifications H52, I21, I28

1. Introduction

Understanding the effects of education on cognitive skills is crucial. Skills improve critical outcomes, such as wages (Lindqvist and Vestman 2011; Hanushek et al. 2015) and innovation (Aghion et al. 2017). Two-thirds of the world's youth population has been estimated to lack the basic skills needed to participate effectively in modern economies

(Gust et al. 2024), and schooling is the primary tool in the hands of policymakers to bridge this gap.[1]

Despite the relevance of this relationship, prior scholarship focuses on relatively narrow contexts, age groups, and outcomes. Banks and Mazzonna (2012) and Gorman (2023) use compulsory schooling reforms in England, focusing on relatively early school leavers whose skills are measured at older ages. Older individuals are also the focus of Schneeweis et al. (2014), using similar reforms in six European countries. Several studies focus on cognitive skills of young males exploiting military tests (Cascio and Lewis 2006; Falch and Sandgren Massih 2011; Brinch and Galloway 2012; Carlsson et al. 2015). All these studies find positive impacts of an additional year of schooling on specific cognitive outcomes such as intelligence quotient, technical comprehension, or working memory.[2]

This article shows that education policies—by increasing educational attainment—can raise adults' core cognitive skills. We provide new evidence on this relationship and on the mechanisms behind it. First, leveraging reform-induced changes in educational attainment, we estimate the effect of schooling on numeracy and literacy across the working-age distribution using internationally comparable skill measures. Second, we study the returns to formal qualifications and document clear non-linearities, with larger gains around the completion of high school and, especially, college. Third, we exploit reforms targeted at different phases of education to compare their differential impacts on skills. Fourth, using unique information on how individuals use skills in everyday life, we provide evidence consistent with a mechanism whereby education improves access to skill-intensive jobs, helping sustain and magnify skill gains over the life cycle. These findings speak to education policy design, to interpretations of wage returns to schooling, and to debates on skill formation over the life cycle.

We identify the impacts of schooling on skills by exploiting educational reforms across a large number of countries. We match records on educational reforms in twenty-one European countries collected by Braga et al. (2013) to numeracy and literacy scores measured by the Organisation for Economic Co-operation and Development–Programme for the International Assessment of Adult Competencies survey. We consider six types of reform interventions aimed at increasing the level of educational attainment which are relatively homogeneous across countries. Beyond the duration of compulsory schooling, used in previous studies, we additionally exploit reforms such as tracking age and the expansion of university access or pre-primary education.[3] Our cross-country approach aligns with previous studies on wage returns to education and skills (e.g., Brunello et al. 2009; Brunello et al. 2013; Hanushek et al. 2015). Although this may limit our ability to precisely interpret our results with respect to studies considering one single institutional context (e.g., Clark and Royer 2013), our approach offers broader evidence by exploiting internationally-standardized and externally-validated cognitive skill measures.

[1] Recent evidence using Programme for the International Assessment of Adult Competencies (PIAAC) data also highlights that literacy and numeracy skills are often misaligned with job requirements across Organisation for Economic Co-operation and Development countries, underscoring the policy relevance of adult skill formation (Pérez Rodríguez et al. 2023). Relatedly, education is systematically associated with patterns of under- and over-skilling over the career (Bischof 2025).

[2] Hampf (2019) uses PIAAC data to estimate the impact of schooling on numeracy and literacy, but only on German adults aged 53–68. Liu (2018) matches educational reform data to PIAAC records to investigate the impact of educational institutions on cognitive skills. However, they examine their effects conditional on years of schooling rather than estimating the cognitive skills return to education. Other studies investigate the impact of general compared to vocational education on skills (Brunello and Rocco 2017; Ollikainen et al. 2024).

[3] The reforms we consider are: pre-primary expansion, starting age of compulsory education, duration of compulsory education, tracking age, standardized tests for career advancement, expansion of university access. See Section 2 for details.

We leverage the policy experiment embedded in educational reforms assigning different institutional settings to otherwise similar individuals based on their year of birth (Jackson et al. 2016; Lafortune et al. 2018). The main challenge in this approach is that exposed individuals are mechanically younger, potentially confounding exogenous variation in schooling and skills induced by the reforms with secular trends in education and the natural decay of cognitive ability with age. We tackle this challenge by exploiting the control group of individuals in countries that have not (yet) reformed a given educational institution. Our stacked event study design compares cognitive skills across cohorts using separate treatment and control groups for each educational reform and reform year, and then estimates average effects across the reforms considered. Outcome evolution is undistinguishable in treatment and control groups up to the reform events, supporting the validity of our design.

In our main empirical model, we exploit exogenous variation in schooling to measure its impact on skills. We use the jump in schooling induced by educational reforms as an instrument in an instrumented difference-in-differences design (DDIV) model of cognitive skills (Duflo 2001; Hudson et al. 2017; Ridley and Terrier 2025). The event-study estimates of reform impacts on schooling represent the first stage, while impacts on skills provide the reduced form. Our DDIV interpretation relies on the assumption that reforms affect skills primarily through years of schooling. The credibility of this assumption is strengthened by our selection of reforms aimed at expanding educational attainment rather than directly modifying school quality, although such reforms may still affect the educational environment indirectly. We therefore treat this as a maintained assumption. We provide suggestive evidence consistent with this interpretation by showing that, on average, the effects of different reforms on skills are proportional to their first-stage effects on schooling (Angrist et al. 2023).

One additional year of schooling increases numeracy and literacy scores in adulthood by around 0.2 standard deviations (hereafter, σ). Estimates fall within the range of previous findings on other measures of cognitive development. Both schooling and skills increase more decisively for older individuals at the time when skills are measured. Schooling boosts skills to a larger extent among individuals with medium or high socio-economic status. Our evidence aligns with several studies showing that educational interventions disproportionately improve the outcomes of relatively advantaged students (see Fe et al. 2022, and references therein).

We find that not all years of schooling are equally worthwhile in terms of cognitive skill returns. College graduation has twice the impact on skills than the average additional years of schooling. This result suggests that either college years are more productive or the signalling value of a degree in the labour market increases access to high-skill jobs, preventing human capital depreciation. Moreover, exploiting reforms targeted at different educational phases, we show that expanding preschool has the largest impact on skills (0.3σ), followed by secondary education (0.15σ). This result suggests that early and late school years may be the most productive in terms of cognitive skill development, in line with findings on the relative productivity of parental investments in human capital at different stages of childhood (Carneiro et al. 2021).

Exploring mechanisms, we show that schooling opens the door to more skill-intensive jobs. Additional education increases the probability of working in high-skill occupations and for large firms, which are found to improve workers' human capital (Arellano-Bover 2022). Moreover, exploiting PIAAC survey data on the use of skills, we find that schooling leads to a large increase in the use of numeracy, literacy and Information Technology (IT) skills skills, and in the learning opportunities available on the job. Mirroring its impacts on cognitive skills, college graduation disproportionally improves labour market outcomes and the use of skills.

We view our results as suggesting that the impacts of schooling on skills are magnified by experience in skill-intensive jobs. This interpretation fits our findings better than a sorting pattern where individuals with higher skills acquired through education self-select into skill-intensive jobs. First, schooling increases the use of skills on the job more than in everyday life. Since human capital depreciates if unused (Dinerstein et al. 2022; Hanushek et al. 2025), this result points to an important role of work experience. Second, schooling increases the opportunities to learn on the job, for example, through learning by doing or from colleagues. Learning opportunities are key for human capital *growth* (Arellano-Bover and Saltiel 2026), suggesting that the impact of schooling on skills may be partly driven by the quality of labour market experience. Finally, larger returns for medium and high socio-economic status (SES) individuals, whose education is shifted by the reforms at higher levels than low-SES individuals, could be explained by stronger impacts on the access to skill-intensive jobs.

A large body of literature has estimated wage returns to education by exploiting institutional reforms, especially interventions on compulsory schooling (e.g., Oreopoulos 2006; Brunello et al. 2009; Grenet 2013; Clark 2023).[4] We show that schooling also improves cognitive skills by exploiting cross-country measures of ability spanning the entire adulthood. Our results suggest that higher human capital is a leading explanation of the wage returns to schooling.

Deming (2025) documents that both college and cognitive skill wage premia grow over the life cycle, in contrast with canonical models of human capital investment and returns to schooling. We complement this evidence by showing that education causally increases cognitive skills, and by suggesting that the impacts of schooling on skills increase over the life cycle through access to complex jobs. Our results suggest that, besides sorting of faster learners into higher schooling and complex jobs, part of the growing life cycle returns to education can be explained by the causal impact of education on human capital.

2. Data and descriptive statistics

Our empirical analysis has two main ingredients. First, we exploit individual-level cross-country survey data including direct assessment of adult skills and background characteristics. Second, based on their country and cohort, we link these individuals to detailed information on educational reforms collected by Braga et al. (2013).

2.1 The PIAAC survey

We exploit microdata obtained from an international survey on adult skills conducted by the Organisation for Economic Co-operation and Development (OECD) (PIAAC, see OECD 2019). The first round of the survey that we exploit here was conducted between 2011 and 2014, while a second round was conducted in 2022–23. The survey is intended to measuring adults' proficiency in key information-processing skills—literacy, numeracy, and problem solving—which represent skills needed for individuals to participate in society and for economies to prosper. It also gathers information and data on how adults use their skills at home and at work.

According to Hanushek and coauthors (see, e.g., Hanushek et al. 2015) the results from international assessments of students and adults constitute good measures of human capital

[4] Studying outcomes outside of the labour market, Clark and Royer (2013) find that education has at best a small impact on health and Banks et al. (2019) find no effect on decision-making quality, while Brunello et al. (2013) show that schooling has a protective effect on the body mass index of females.

and have considerable advantages over quantity-based measures. They claim that the skills assessed by PIAAC can be interpreted as a proxy measure of human capital, the underlying assumption being that skills and educational attainment complement each other and, together, provide a better measure of human capital.

In the initial round, the survey was either computer or paper-and-pencil based, the decision relying on the interviewer. In addition to skills, the survey collects detailed information on schooling career and labour market experience.[5] We consider here literacy, defined as the ability to understand and engage with written text, and numeracy, defined as the ability to access and interpret quantitative information. Rather than defining skill levels and thresholds, cognitive skills are measured to reflect a continuum of proficiency. Throughout the analysis, we use numeracy and literacy scores standardized to have zero mean and unit variance in the PIAAC sample.[6]

2.2 Educational reforms

We exploit a dataset of educational reforms developed by Braga et al. (2013), which records major institutional changes in school systems across twenty-four European countries over the 1929–2000 period. The dataset draws on a wide range of harmonized sources—including the Eurydice database, UNESCO and OECD reports, national legislation, and input from local experts—to provide comprehensive and comparable information on institutional reforms from pre-primary to tertiary education.

For the purposes of this article, we focus on six types of reforms that are explicitly aimed at increasing the quantity of education, listed in Table 1.[7] These reforms fall into two broad policy areas:

1) Pre-primary school: we consider two types of reforms. First, pre-primary expansion, which includes fee reductions, construction of new facilities, and legal mandates to ensure universal availability of pre-primary education. Second, reforms affecting the starting age of compulsory schooling, that is, changes in the legal age of entry into formal education.
2) Education expansion: (a) reforms altering the duration of compulsory education (e.g., increasing the number of mandatory school years); (b) reforms modifying the tracking age, defined as the age at which students are sorted into different academic tracks or school types; (c) reforms introducing standardized tests for career-related decisions, such as upper-track selection; (d) the expansion of university access, including, for example, open access from vocational high schools and the geographic diffusion of tertiary institutions.

Each reform is dated precisely and encoded as step dummy depending on the number and timing of policy changes in a given country. In our analysis, we focus on the changes in these variables over time, indicating that the corresponding institution was reformed.

[5] Years of schooling data are not observed in Germany, and we impute them using the highest level of education achieved (corresponding years of schooling are drawn from the 2012 Survey on Income and Living Conditions).

[6] PIAAC data provide ten individual plausible values for cognitive scores in each domain (e.g., numeracy or literacy), derived using item response theory (Khorramdel et al. 2020). We replicate each regression for all ten plausible values and report the average coefficient across the ten regressions, adjusting standard errors following OECD (2013: Chapter 18).

[7] The reforms we do not consider have targets other than increasing the education level, or are arguably more heterogeneous across countries. These reforms pertain: teacher qualifications required at the preschool, primary, and secondary school levels; teacher and school autonomy; school evaluation; standardized tests for non-career purposes (e.g., school accountability); selectivity in university access; university autonomy; university financing.

Table 1 Educational reforms.

Area (1)	Reform (2)	Description (3)	Target age (4)
Pre-primary school	Pre-primary expansion	Fees reduction; construction of new pre-primary schools; laws obliging to make pre-primary school available to all citizens; incorporation of preschool into schooling systems	Age 3
	Starting age of compulsory education	Entry age into compulsory formal education	Age 6
Education expansion	Duration of compulsory education	Number of years of compulsory school	Age 10
	Tracking age	Age at first tracking	Age 10
	standardized tests (for career advancement)	Presence of national standardized tests for taking decisions about the school career of pupils	Age 6
	Expansion of university access	Open access from vocational high schools; geographical expansion of universities; creation of polytechnic institutions providing non-university vocational higher education	Age 15

Note: The table reports educational policies considered in our data. These are the reforms targeting the quantity of education out of the larger set of reforms collected by Braga et al. (2013). Single reforms are grouped in policy areas and reported are a brief description and the target age. The latter is used to define exposure to the reforms based on year of birth. See Section 2 and Braga et al. (2013) for details.
Source: Braga et al. (2013).

This selection of reforms is motivated by both empirical and conceptual considerations. On the one hand, these reforms generate sharp, observable discontinuities in educational attainment across cohorts, which are crucial for identification in our stacked-by-event DDIV. On the other hand, they refer to institutional changes that are well-documented, clearly defined, and relatively comparable across countries, in contrast to qualitative reforms (e.g., changes in pedagogy or school governance) that are harder to measure and more context-dependent.

While formal similarity across countries is ensured by the harmonized coding of reform types, we acknowledge that institutional capacity, political enforcement, and local administrative practices may vary. Thus, the way reforms were implemented—and their effective 'bite'—might differ across settings. However, illustrating the specific impacts on schooling of each reform we exploit is beyond the scope of our exercise. The research question we address, instead, is whether and how individuals who would have acquired lower education in the absence of an institutional reform exhibit different cognitive skill trajectories in adulthood.

Our institutional variables mostly capture the reforming activities of policymakers, while being unfit to measure the magnitude of the impact. Except a few cases (like the starting or

ending ages for compulsory education), we are only capable to identify the existence and direction of a policy change, without any assessment of the magnitude or of the effectiveness of the policy.[8] These variables have been used to study the correlation between reforming activities of governments and the distribution of educational attainments (mean and variance—Braga et al. 2013) as well as the distribution of achievement and wage (Checchi and van de Werfhorst 2018).

We match PIAAC records with educational reforms based on the year of birth. Exposed cohorts are defined based on the specific target age of each reform, borrowed from Braga et al. (2013) and reported in column 4 of Table 1 (e.g., individuals born 6 years before the reform was passed for primary school interventions).[9] We focus on the earliest observed intervention in a given country by type of reform.[10] Other studies operate alternative empirical choices, for example, Lafortune et al. (2018) focus on the most impactful event, measured by the strongest statistical effect on the reform target. We believe that using the earliest intervention is most appropriate in our context since institutional reforms, differently from the court orders studied by Lafortune et al. (2018), can hardly remain unimplemented.

2.3 Sample selection and descriptive statistics

We consider the twenty-one European countries participating to PIAAC with information on educational reforms. Data from Greece and Slovenia were collected in 2014, data from Hungary in 2017, and all other countries in 2011. Year of birth is obtained using reported age.[11] In order to observe completed education, we focus on individuals aged 25 or later. In addition, we do not consider first-generation immigrants (8 per cent of the sample) since they were likely not exposed to the national education systems of the country where they are observed. Finally, we do not consider individuals born after 1986, the youngest birth cohort that we observe in all countries. These selections yield a working sample of 80,066 individuals with non-missing values of cognitive skills, years of schooling, and key background variables.

Individual characteristics in the working sample are described in Table 2. Our sample includes a substantially wider span of ages with respect to previous studies (25–65), with an average of 45.[12] The birth cohorts range from 1946 to 1986. Of the sample, 92 per cent of individuals have both parents native-born, while the rest are second-generation immigrants. High-SES individuals (25 per cent) are defined as having either at least one parent with higher education or at least 200 books at home during childhood (Brunello et al. 2015). Low-

[8] The closest analogue is the measurement of employment protection by the OECD, which is based on national experts' subjective assessment of various dimensions of firing restrictions within each country (link). Each dimension is scored in a given interval (typically 0–6), and any policy change leads to a change in the specific score, which is then reweighted and aggregated with the other. In our case, national experts were consulted in order to identify 'relevant' reforms, but they were not requested to score their 'intensity' or 'impact'. We remain agnostic with respect to these dimensions and let the data speak.

[9] We show in Figure A.3 (see online supplementary material for a colour version of this figure) that our results are robust to alternative coding of the first exposed cohort. The increasing pattern of coefficients up to our main coding choice, with subsequent persistence of the estimates, suggests that younger cohorts may be only partially exposed, corroborating our choice of target age.

[10] Most observed reforms operate in a positive direction, and we do not consider the few reform events operating in a negative direction (9 per cent of the total). The concern that these reforms could alter our control group is alleviated by the fact that we include several controls to hold the institutional environment fixed and find parallel outcome trends in the pre-reform periods (see Section 3).

[11] Age is provided in 5-year bands for Austria, Germany, and Hungary. In these countries, we use the central value in each band (e.g., twenty-seven in the 25–29 band). Our main results are substantially unaffected by the exclusion of these countries from the sample (Table A.1, see online supplementary material for a colour version of this table).

[12] Schneeweis et al. (2014) focus on individuals aged 47–73, Gorman (2023) on individuals aged 48–60, and Banks and Mazzonna (2012) on individuals aged 50 or above.

Table 2 Descriptive statistics.

	N	Mean	Standard deviation	Minimum	Maximum
	(1)	(2)	(3)	(4)	(5)
Year of birth	80,066	1966.262	11.643	1946	1986
Age	80,066	45.340	11.634	25	65
Female	80,066	0.522	0.499	0	1
Both parents born in country	80,066	0.917	0.276	0	1
Mother with less than secondary education	80,066	0.583	0.493	0	1
Mother with secondary education	80,066	0.316	0.465	0	1
Mother with higher education degree	80,066	0.101	0.301	0	1
Father with less than secondary education	80,066	0.482	0.500	0	1
Father with secondary education	80,066	0.373	0.484	0	1
Father with higher education degree	80,066	0.145	0.352	0	1
≤10 books at home	80,066	0.139	0.346	0	1
11–25 books at home	80,066	0.153	0.360	0	1
26–100 books at home	80,066	0.321	0.467	0	1
101–200 books at home	80,066	0.174	0.379	0	1
201–500 books at home	80,066	0.139	0.346	0	1
>500 books at home	80,066	0.074	0.262	0	1
Low socio-economic status	80,066	0.209	0.406	0	1
Medium socio-economic status	80,066	0.538	0.499	0	1
High socio-economic status	80,066	0.253	0.435	0	1
Years of schooling	80,066	12.828	3.114	3	21
High school graduation or higher	74,995	0.654	0.476	0	1
Tertiary education or higher	74,995	0.345	0.476	0	1
University graduation or higher	74,995	0.242	0.429	0	1
Index of learning at work	51,901	1.990	0.963	−0.644	4.163
Numeracy use at home	72,744	1.937	0.907	−0.508	6.340
Numeracy use on the job	51,170	2.044	1.000	−0.090	6.050
High skilled occupation	57,761	0.316	0.465	0	1
Large firm	50,009	0.200	0.400	0	1

Note. The table reports descriptive statistics for our sample. Considered individuals are aged at least 25 in 2011, the survey year, with both parents born in the country, and with non-missing years of schooling and numeracy and literacy scores. Columns 1–5 report, respectively, the number of observations, the sample average, the standard deviation, the minimum and the maximum values observed. Statistics are conditional on non-missing observations. Labour market variables in the bottom part are observed conditional on working. See Section 2 for details.
Source: Authors' calculations.

SES individuals (21 per cent) are defined as having both parents without a high school degree and up to twenty-five books at home during childhood, and the remaining individuals are defined as mid-SES (54 per cent). The average individual completed 13 years of schooling, with substantial dispersion. While 35 per cent of individuals did not complete secondary education, 31 per cent just completed secondary education, 10 per cent hold nonacademic tertiary

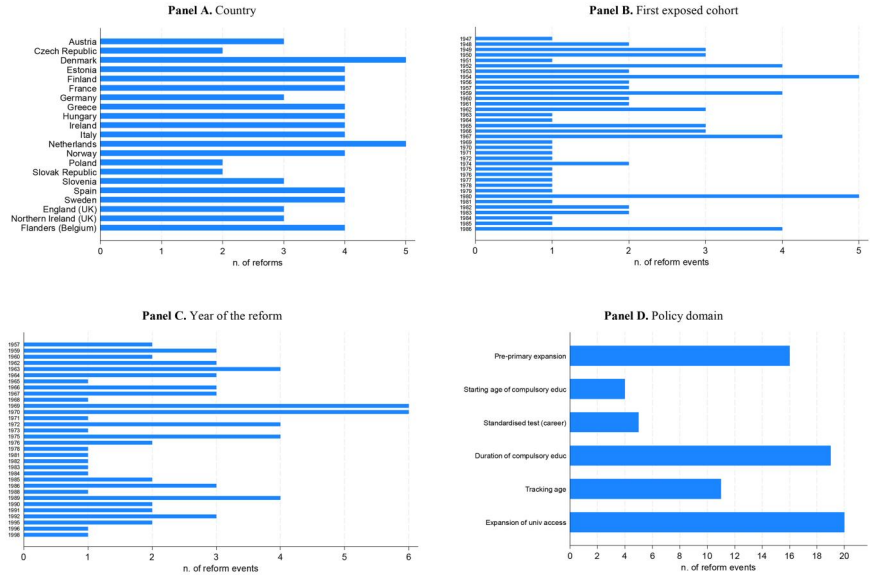

Figure 1 Reform events.

Source: Authors' calculations.

Note: The figure plots the number of reform events considered by country (a), year of birth of the first exposed cohort (b), year when the reform was passed (c), or reformed policy domain (d). We flag as a reform event the earliest intervention in a given policy domain in a given country. Considered reforms are those targeting individuals born from 1946 to 1986. Great Britain and Northern Ireland are considered two separate countries in our data. Records from Belgium refer to Flanders only. See Section 2 for details.

degrees, and 24 per cent have an academic degree.[13] Finally, conditional on working, 32 per cent of individuals work as a professional or manager ('skilled' occupation) and 20 per cent work for a firm employing 250 workers or more ('large' firm).

We consider a total of seventy-five reform events. Figure 1a shows that all countries exhibit a significant reforming activity, with Slovak Republic, Czech Republic, and Poland—the least active countries—intervening in two out of six policy domains. We observe two main waves of reforming activity, clustered around 1970 and 1990 and mostly affecting cohorts born in the late 1950s and early 1980s (Fig. 1b and c). Finally, Fig. 1d shows reforms by type. Almost all countries expanded the length of compulsory education and widened university access, while we observe only a few reforms of the starting age of formal education. The timeline of reform events by domain is visualized in Figure A.1 (see online supplementary material for a colour version of this figure).

3. Empirical strategy

We are interested in the causal effect of additional schooling on cognitive skills in adulthood. The identification of this parameter is challenged by selection bias, that is, individuals with higher potential numeracy and literacy commonly opt for higher schooling. At the same

[13] We define educational attainment using data on the highest qualification, available for 74,995 individuals. We ignore foreign qualifications for which the educational level is not specified (0.17 per cent of observations).

time, while additional schooling may increase skills through learning, higher skills may increase schooling through a lower cost of effort, generating reverse causality. We tackle these challenges by exploiting educational reforms as exogenous shocks to schooling. We start by presenting the event study design used to isolate such exogenous variation. We then illustrate our main empirical model, exploiting the event study design to instrument schooling in the cognitive skill equations.

3.1 Event study design

We exploit the policy experiment embedded in educational reforms. Exposure to the treatment depends on the year when a reform is passed and the phase of education affected, jointly defining the first exposed cohort. We start from the idea of comparing the outcomes of the youngest cohort exposed to a given reform with those of slightly older individuals who are not exposed only because they attend school just before the policy kicks in. However, simply comparing adjacent cohorts around a reform would likely be confounded by secular trends and age effects in both schooling and skills. Both outcomes are well-known to exhibit a steeply-increasing trend in the second half of the XX century, a fact illustrated in our sample by Figure A.2 (see online supplementary material for a colour version of this figure). In addition, age-related skill decline may inflate the effect of schooling on skills in a simple before-after comparison.

Our research design complements cross-cohort variation with cross-country variation by building difference-in-differences comparisons around a reform event. We define a control group of countries which have not (yet) undertaken a given educational reform and compare the change in outcomes between exposed and unexposed cohorts in an event study design. While our research design leverages variation at the country-cohort level, the unit of analysis is an individual affected by the reforms—that is, individuals who are pushed by policy interventions to acquire more schooling. The causal effects we estimate, therefore, can be interpreted as the average effect of the fraction of a schooling year generated by standard government activity.

We adopt the approach by Lafortune et al. (2018) and Rothstein and Schanzenbach (2022) and consider a parametric event study model. To fix ideas, consider one single educational reform intervention. We are interested in the discrete jump observed in schooling and skills following the reform, parametrized by α_1 in the following model:

$$S_i = \alpha_0 + \alpha_1 \mathbb{1}(\mathrm{t} > \mathrm{t}^*_{\mathrm{c(i)}}) + \alpha_2 \mathbb{1}(\mathrm{t} > \mathrm{t}^*_{\mathrm{c(i)}}) \cdot (\mathrm{t} - \mathrm{t}^*_{\mathrm{c(i)}}) + \alpha_3 * (\mathrm{t} - \mathrm{t}^*_{\mathrm{c(i)}}) + \mathrm{X}'_{\mathrm{i}}\gamma + \phi_{\mathrm{c(i)}} + \lambda_{\mathrm{t(i)}} + \mathrm{e_i}, \quad (1)$$

where t denotes the year of birth, t^*_c is the first birth cohort exposed to the reform in country c, and $\mathbb{1}(t > t^*_c)$ indicates exposure. The outcome S_i denotes years of schooling of individual i, and we additionally estimate event studies of numeracy and literacy scores. In this formulation, the coefficient α_3 estimates whether outcomes of reforming and non-reforming countries are on divergent paths before the policy is implemented, and the coefficient α_2 estimates the evolution of treatment effects after the reform. The vector X'_i, added to some of our specifications to increase precision, includes gender, age, parental immigration, education of both parents, and the number of books at home during childhood. Dummies for countries and cohorts are denoted by ϕ_c and λ_t, respectively. Standard errors are clustered at the country-cohort level, at which treatment varies (Arellano-Bover 2022).[14]

[14] When clustering at the country level through wild cluster bootstrap (Roodman et al. 2019), the coefficients of interest in our main specification remain statistically significant despite this conservative choice (P = .026) for the reforms impact on schooling in column 2, Panel A of Table 3.

The key identifying assumption in our event study design is that, absent the reform, treatment and control countries would have experienced the same change in outcomes across cohorts. We provide evidence in support of this assumption by estimating a non-parametric specification:

$$S_i = \tau_0 + \sum_{l \neq 0} \tau_l 1(t = t^*_{c(i)} + l) + X_i'\delta + \phi_{c(i)} + \lambda_{t(i)} + r_i. \quad (2)$$

Coefficients τ_l estimate the impact of educational reforms at each relative event time, where the youngest cohort unexposed to the reform ($l = 0$) forms the reference group.[15] The joint hypothesis that $\tau_l = 0 \forall l < 0$ provides an indirect test of the parallel trends assumption.

We implement our event study in a stacked-by-event design to address the concerns on TWFE models.[16] In short, we avoid the issues with the TWFE estimator by constructing a separate difference-in-difference comparison for each treatment wave, that is, for each event year, and by averaging across all these building blocks (see, e.g., Deshpande and Li 2019). Specifically, we build a separate dataset for each treatment wave including countries reforming in the considered year (treatment group) and countries either not passing the reform or passing it after more than 15 years (control group). We then stack all treatment waves to form our estimation dataset. In this procedure, individuals in never-reforming countries serve as controls multiple times, once per treatment wave. By defining separate treatment and control groups for each treatment wave, this design allows us to add relative time dummies to the controls specified in equation (1). Finally, our results are obtained by additionally stacking data on the six educational reforms considered, yielding 2,034,899 observations. All regressions include reform-by-wave dummies to identify our parameters of interest only from within-dataset variation. This implies that our design only compares treated and control units specifically defined for each reform event depending on countries and cohorts affected.[17] Nevertheless, we include reforming activity variables in all regressions to isolate variation driven by the reform of interest. Specifically, we control for: (1) the time-varying sum of reform dummies in all policy domains but the one considered, to capture reform intensity outside the domain of interest; (2) the continuous variables indicating the age of start of compulsory education, age of tracking, and duration of compulsory education, to account for country-cohort specific differences in these institutional settings; (3) the observed reforms in the domain of interest in birth cohorts predating our sample (1929–53); (4) the total number of reforms in the considered domain, to capture reform intensity in the domain of interest.

[15] We consider a symmetric fifteen-cohort window around a reform event. Observations from earlier or later relative time periods are binned so that, e.g., the coefficient τ_{15} estimates reform impacts fifteen cohorts after the intervention or later (Lafortune et al. 2018). Robustness to this choice is shown in Figure A.4 (see online supplementary material for a colour version of this figure).

[16] With variation in treatment timing, TWFE estimators compare units initially treated in different periods, including 'forbidden' comparisons of later-treated with already-treated units, resulting in unreasonably weighted averages of underlying treatment effects (De Chaisemartin and d'Haultfoeuille 2020; Callaway and Sant'Anna 2021; Goodman-Bacon 2021; Borusyak et al. 2024). Particularly relevant for our framework, Sun and Abraham (2021) show that, unless treatment effects are homogeneous across units first treated in different periods, event study estimates at a given relative time $(t - t^*_{c(i)})$ are contaminated by treatment effects in all other periods.

[17] Although reform events may overlap, that is, the same country may intervene in more than one policy domain starting from the same birth cohort, overlapping reforms contribute differently to our estimation in the stacked-by-event design. Treatment and control groups of reforming and not (yet) reforming countries, indeed, vary across policy domains.

3.2 Instrumenting years of schooling

The event study design discussed above allows us to estimate the causal effect of educational reforms on schooling and cognitive skills. In the next step, we estimate the impacts of schooling on skills by rescaling the (reduced-form) effects of reforms on skills by the (first-stage) impacts of reforms on schooling.

Under additional identifying assumptions, an instrumented DDIV provides causal estimates of the cognitive skills return to schooling (Hudson et al. 2017). The parallel trends assumption, on which the event study design hinges, replaces the stronger independence assumption commonly imposed on instrumental variables. The relevance condition requires reforms to significantly shift educational attainment, an assumption testable through first-stage event study estimates. The monotonicity assumption requires that all individuals are weakly more likely to take up additional schooling when exposed to the same reform. Violations of this assumption are unlikely, since they would require the existence of a non-negligible group of individuals opting for higher schooling only in the absence of a given reform. Most importantly, the exclusion restriction requires the reforms to affect cognitive skills only through additional schooling. A standard concern in this literature, however, is that reforms aimed at increasing educational attainment may also affect the educational environment—for example through congestion, capacity constraints, changes in peer composition, or related institutional adjustments (Pekkarinen et al. 2009; Bianchi 2020). If so, a strict quantity-only interpretation of the exclusion restriction may not hold, as reforms could affect skills through channels other than years of schooling alone. We therefore interpret our instrumental variables estimates as capturing the effect of reform-induced schooling in the educational setting generated by the reforms. This concern is reduced, though not eliminated, by our focus on reforms whose primary objective is to expand educational attainment rather than directly modify school quality. In Section 4, we provide suggestive evidence that reforms with larger first-stage effects on schooling also tend to generate proportionally larger reduced-form effects on skills.[18] Relatedly, Brunello et al. (2013) provide evidence consistent with this exclusion restriction in the context of compulsory-schooling reforms.

We estimate a 2SLS model of cognitive skills where schooling is instrumented by its discrete jump after reforms' introduction. Our second stage equation is:

$$C_i = \beta_0 + \beta_1 \widehat{S}_i + \beta_2 1(t > t^*_{c(i)}) \cdot (t - t^*_{c(i)}) + \beta_3 (t - t^*_{c(i)}) + \eta X_i' + \phi_{c(i)} + \lambda_{t(i)} + u_i, \tag{3}$$

where C_i denotes cognitive skills, either numeracy or literacy. The first stage is estimated by equation (1) when the dependent variable is schooling, and $\widehat{S}_i$ denotes predicted years of schooling from the first stage. The coefficient β_1 represents the impact on cognitive skills of an additional year of schooling. We additionally investigate non-linearities by using attainment dummies as endogenous variables rather than years of schooling.

4. Results

We begin by presenting event study estimates isolating the causal effects of educational reforms on schooling and cognitive skills. We then present the cognitive skills returns to

[18] As an additional robustness check motivated by the school-quality concern, we re-estimated the main specifications excluding compulsory-schooling reforms. The estimated effects on numeracy and literacy remain similar in sign and magnitude to the baseline, with some loss of precision and a weaker first stage, as expected. Detailed results are available from the authors upon request.

schooling, estimated by using reform impacts on schooling as the first stage of a 2SLS model. The section ends by discussing heterogeneous effects by subgroup and phase of education.

4.1 Event study estimates

The educational reforms we consider, on average, lead to a significant increase in schooling. Panel A of Table 3 reports estimates of α_1 in equation (1) when the dependent variable is years of schooling. Reforms are estimated to increase schooling, on average, by 0.16 years (column 1). Estimates are barely changed when controlling for socio-economic background (column 2), and remain similar when focusing on expansion reforms only (columns 3 and 4). Tests of the statistical significance of the exposure dummy indicate that the increase in schooling following educational reforms can serve as a strong first stage for our analysis (F-statistics in the range 15–25).

Undetectable impacts on schooling prior to the reforms lend validity to our identifying assumption. Figure 2a plots non-parametric estimates of τ_l from equation (2) superimposed to predictions from the linear model in equation (1). First, our linear model fits noisier non-parametric estimates fairly accurately. Second, coefficients at negative relative time periods, representing unexposed cohorts, revolve around zero and are not statistically significant. We cannot reject the hypothesis that all pre-reform estimates are zero ($P = .4149$).

Reforms have positive reduced-form impacts on numeracy and literacy. Individuals exposed to educational reforms have, on average, 0.04σ higher numeracy score and 0.03σ higher literacy score (column 1, Panel B of Table 3). Similarly to estimated impacts on schooling, these results are robust to the inclusion of additional controls (column 2), and remain similar when focusing on expansion reforms only (columns 3 and 4).

Reforms leading to larger increases in schooling lead to larger increases in cognitive skills. This can be seen in Fig. 3, plotting reduced-form estimates as a function of first-stage estimates separately for each of the six reforms we consider (a 'Visual IV' plot, Angrist et al. 2023). A linear approximation, shown by the solid line, fits our estimates fairly well and runs remarkably close to the origin for both numeracy and literacy (Fig. 3a and b, respectively). Although these patterns are consistent with reforms affecting skills primarily through schooling, they do not by themselves rule out concurrent changes in the educational environment; we therefore view them as suggestive evidence rather than as a direct test of the exclusion restriction.[19]

4.2 Cognitive skill returns to schooling

Schooling generates large cognitive skill gains in adulthood. Panel C of Table 3 reports 2SLS estimates of β_1 in equation (3). An additional year of schooling increases numeracy score by $0.18 - 0.26\sigma$ and literacy score by $.18 - 0.24\sigma$. We find similar results across different reform areas (all reforms in columns 1 and 2; expansion reforms only in columns 3 and 4) and with or without controlling for socio-economic background (columns 1 and 3 or columns 2 and 4, respectively). Estimated impacts of schooling on numeracy and literacy are in the range of previous findings on other population targets and cognitive skill outcomes.[20]

[19] The only reform with negative reduced form and first stage estimates is the introduction of standardized tests for career advancement. Since these tests are meant to guide student's educational choice and tracking, potentially increasing the match between students and the instruction level, they could be expected to increase overall schooling (Braga et al. 2013). However, they could also decrease education if they induce teaching to the test or discourage students from pursuing further education. Patterns in Fig. 3 are robust to the exclusion of this reform.

[20] Among 18-year-old males, Carlsson et al. (2015) find a 0.21σ and 0.14σ impact of an additional year of schooling on synonyms and technical comprehension test scores, respectively. Other study on teenagers find a $0.20 - 0.25\sigma$ effect on intelligence quotient (Falch and Sandgren Massih 2011), or a $0.30 - 0.35\sigma$ effect

Table 3 Main results.

	All reforms		Expansion reforms	
	(1)	(2)	(3)	(4)
Dependent variable				
	Panel A. First stage			
Years of schooling	0.158***	0.169***	0.163***	0.187***
	(0.034)	(0.033)	(0.041)	(0.039)
***F*-statistics**	21.08	25.69	15.54	23.34
	Panel B. Reduced form			
Numeracy	0.040***	0.041***	0.034***	0.034***
	(0.009)	(0.008)	(0.011)	(0.010)
Literacy	0.032***	0.041***	0.029**	0.036***
	(0.010)	(0.009)	(0.012)	(0.011)
	Panel C. IV			
Numeracy	0.256***	0.242***	0.208***	0.185***
	(0.065)	(0.056)	(0.068)	(0.057)
Literacy	0.203***	0.240***	0.177**	0.192***
	(0.065)	(0.062)	(0.073)	(0.063)
N	2,034,899	2,034,899	1,324,983	1,324,983
***N* (individuals)**	80,066	80,066	80,066	80,066
Year, country, reform-by-wave fixed effects (Fes)	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y
Control variables		Y		Y

Note: The table shows estimates of the impacts of educational reforms on schooling (Panel A) and skills (Panel B), and of the cognitive skills returns to schooling (Panel C). Panels A and B report estimates of α_1 in equation (1); Panel C reports estimates of the year of schooling coefficient β_1 in equation (3). Columns 1 and 2 consider all reforms, while columns 3 and 4 focus on expansion reforms only. Estimated specifications in columns 1 and 3 include country, year of birth, and reform-by-wave dummies. In addition, reform activity controls are added as detailed at the end of Section 3. Columns 2 and 4 further control for gender, parental immigration, number of books at home, father's and mother's educational attainment. Estimation is performed in a dataset stacking each treatment wave for all reforms, where for each wave, individuals in non-reforming countries or in those reforming 15 years later or more are used as controls. Reported *F*-statistics test the hypothesis of α_1 being equal to zero. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for the description of reforms and reform events, Section 3 for identification and estimation. Results are discussed in Section 4.

* $P < .1$. ** $P < .05$. *** $P < .01$.

Source: Authors' calculations.

on numeracy and literacy tests (Cascio and Lewis 2006). Studying individuals at older ages, Schneeweis et al. (2014) estimate a 0.14σ and 0.16σ effect on immediate and delayed memory, respectively, while Banks and Mazzonna (2012) and Gorman (2023) find impacts on memory of $0.3 - 0.5\sigma$. Focusing on Germany, Hampf (2019) find a 0.2σ impact on numeracy, but no significant effects on literacy.

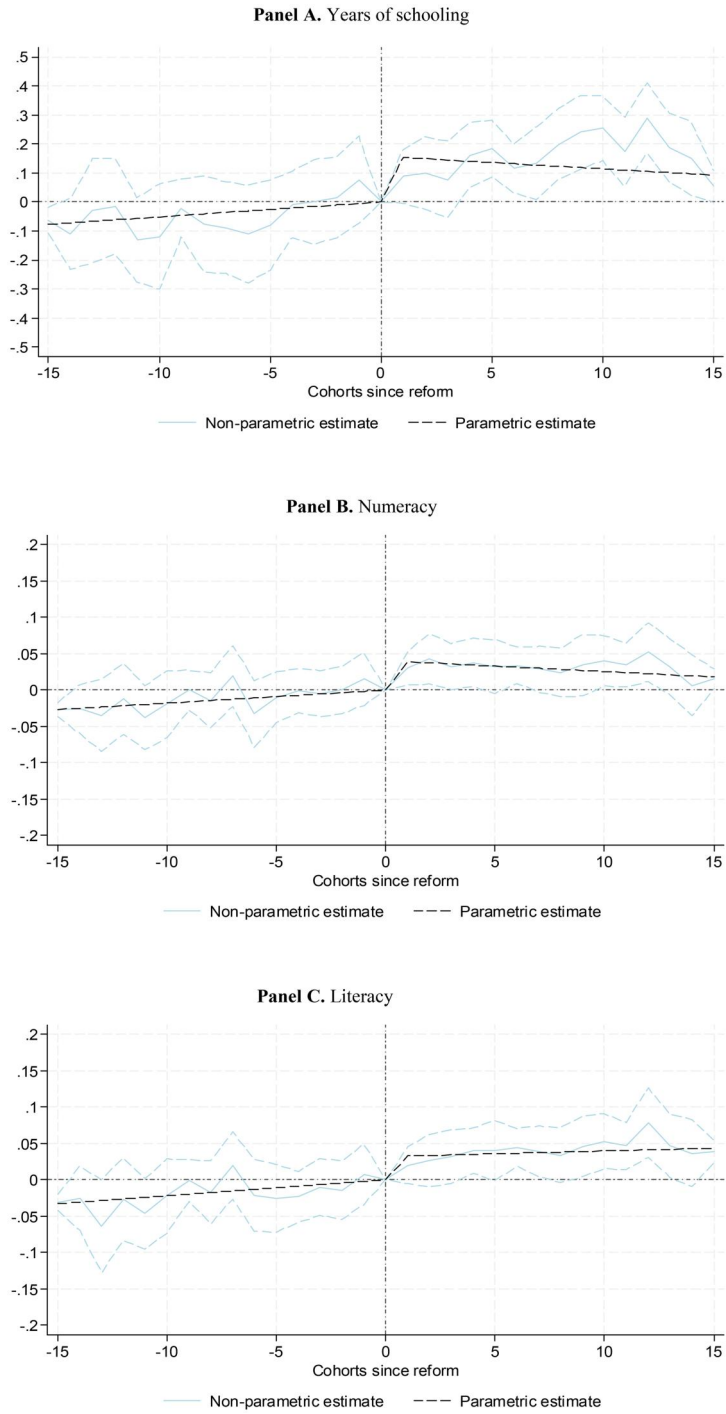

Figure 2 Event study graphs.
Source: Authors' calculations.

Note: The figure plots event study graphs for years of schooling (a), numeracy score (b), and literacy score (c). Blue solid lines report relative-time-specific coefficients from equation (2), dashed blue line plots the 95 per

Figure 2 Continued.
cent confidence interval. Black dashed lines report estimates of predicted impacts from the parametric model in equation (1). Data are stacked across each treatment wave for all reforms. In each wave, individuals in countries that either did not reform or reformed at least 15 years later are used as controls. Relative time equals zero in the latest period before a reform event, and this period forms the comparison group. Relative time is censored to a (−15, 15) window, with extreme points binning all observations observed at earlier or later relative times. The test for joint significance of pre-reform impacts on schooling in panel (a) yields $P = .4149$. Pretreatment coefficients are tested for relative event periods (−14, −1) since the binned coefficient at l = −15 conflates observations from earlier cohorts, inflating statistical significance. See Section 2 for the description of reforms and reform events, and Section 3 for identification and estimation. Results are discussed in Section 4. The figure outline resembles Figure 3 in Lafortune et al. (2018).

The impacts of schooling on skills are non-linear, with college graduation leading to a disproportional increase in numeracy and literacy. Figure 4a compares the estimated returns to high school, tertiary, or college degrees completion to a linear prediction obtained from the estimated return to one additional year of schooling. The linear prediction multiplies estimates in column 2, Panel C of Table 3 by five, the additional schooling years completed, on average, by high school and college graduates compared to non-graduates. Estimates of cognitive skill returns to formal qualifications are obtained by substituting graduation dummies to years of schooling in our 2SLS model (Table A.2, see online supplementary material for a colour version of this table). For both numeracy and literacy, the impact of a college degree is twice as large as the linear prediction, although the difference between the two estimates is not statistically significant. High school completion also exhibits higher than proportional impacts, albeit to a smaller extent, while a generic tertiary degree has similar impacts to the average of five additional schooling years. College graduation is likely the most consequential step for subsequent career development, granting access to high-skill jobs. We explore the mediating role of labour market experience in Section 5.

Educational reforms increase schooling and skills more pronouncedly for younger, non-native, and medium-high SES individuals. Separate estimates by subgroups are presented in Table 4, where column 1 reports full-sample coefficients from column 2 of Table 3. Reforms increase both schooling and skills to a higher extent for non-natives, resulting in similar IV estimates (columns 2 and 3). Moreover, reform impacts on skills are larger for older cohorts (aged above 45, columns 4 and 5, Panel B), possibly suggesting that education limits the age-related cognitive decay. We find stronger cognitive skill returns to schooling among individuals with medium or high SES, while relatively disadvantaged individuals exhibit smaller returns and not statistically different from zero (columns 6–8). This result aligns with heterogeneous effects of primary school spending on theory-of-mind ability (Fe et al. 2022), and of compulsory schooling reforms on earnings (Barcellos et al. 2021), both found to be larger among relatively advantaged students. Since educational reforms have stronger positive impacts on the college graduation of medium or high SES relative to low-SES individuals (Table A.3, see online supplementary material for a colour version of this table), this result aligns with our findings of non-linear impacts of schooling on skills.

Cognitive skill returns to schooling are driven by reforms expanding preschool and secondary education. An additional year of schooling induced by preschool reforms has large effects on numeracy and literacy scores (around 0.3σ), while primary school reforms have a small and not statistically significant impact (columns 2 and 3, Panel C of Table 5). Policies targeted to secondary education generate statistically significant estimates, 0.13σ for numeracy

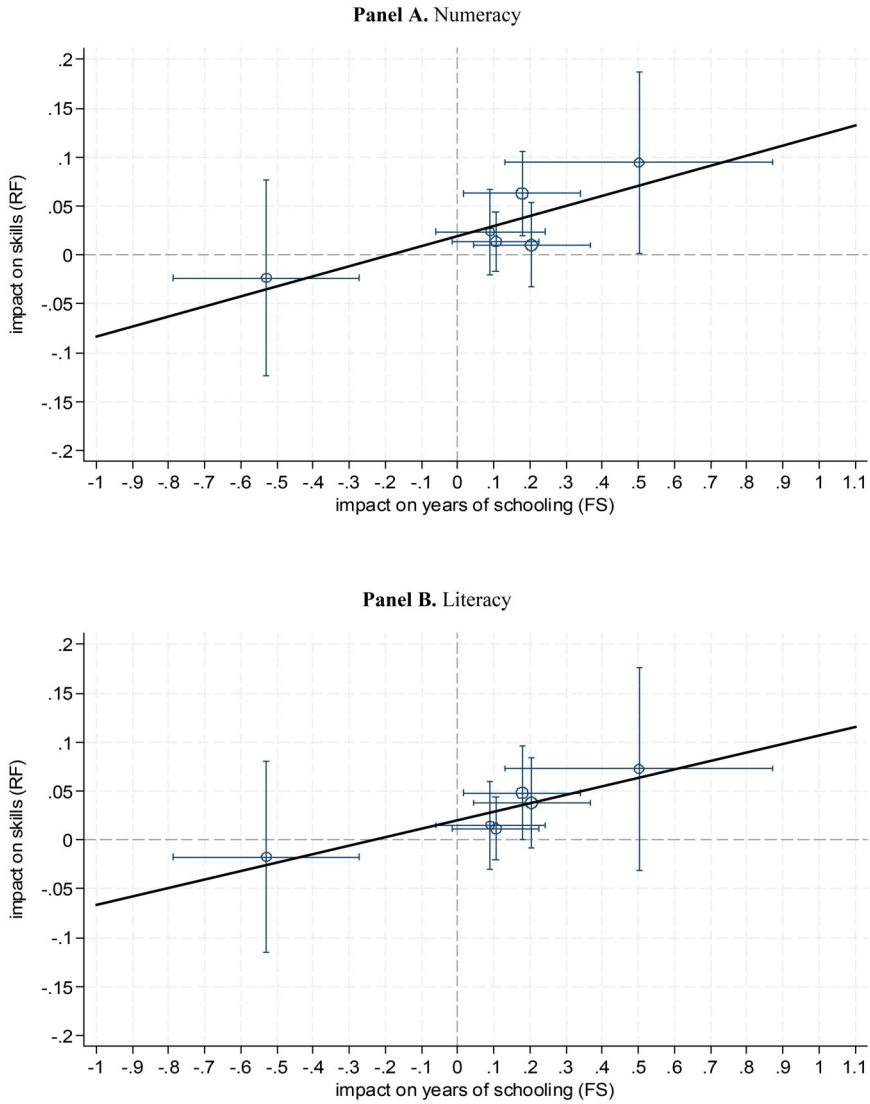

Figure 3 Visual IV.

Source: Authors' calculations.

Note: The figure plots event-study estimates of reform impacts on cognitive skills (reduced form) as function of the corresponding impacts on years of schooling (first stage) by reformed policy domain. (a) Numeracy. (b) Literacy. Markers represent separate estimates of reform exposure coefficients from equation (1) for each of the six interventions listed in Table 1, weighted by the number of observations. Horizontal and vertical spikes delimit the 95 per cent confidence interval of the first-stage and reduced-form estimates, respectively. Solid lines show the best fit through the six estimates. Panels (a) and (b) consider numeracy and literacy scores on the vertical axis, respectively, both standardized to have zero mean and unit variance in the working sample. See Section 2 for the description of reforms and reform events, Section 3 for identification and estimation. Results are discussed in Section 4.

and 0.19σ for literacy (column 4). Finally, university reforms have substantial but imprecisely estimated impacts due to a relatively weak first stage (column 5). These results suggest that preschool and secondary school years may be the most decisive in terms of cognitive skill

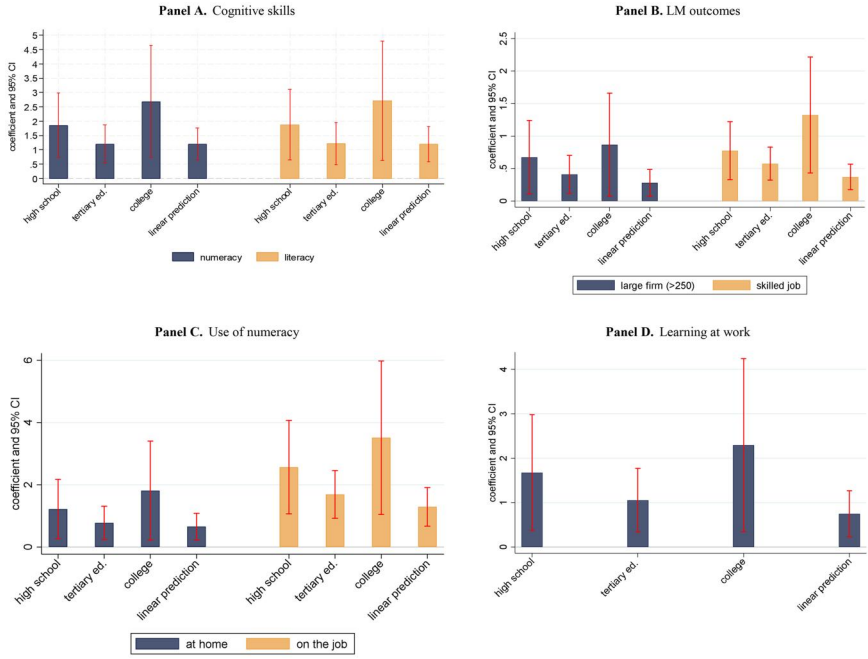

Figure 4 Non-linear impacts of schooling.
Source: Authors' calculations.

Note: The figure plots IV estimates of the cognitive skills return to schooling. (a) Cognitive skills, (b) LM outcomes, (c) Use of numeracy, and (d) Learning at work. Panel (a) compares the estimated return to high school, tertiary, and college degree completion (Table A.2, see online supplementary material for a colour version of this table) to the linear prediction obtained by using the estimated return to an additional year of schooling (Panel C of Table 3). The linear prediction is obtained by considering that individuals with and without graduation have similar gaps of 5 years of schooling for all the degrees considered, on average. Red lines mark 95 per cent confidence intervals. Panels (b)–(d) plot analogous comparisons for labour market and use of skill outcomes drawing from estimates in Table A.4 (see online supplementary material for a colour version of this table). See Section 2 for the description of reforms and reform events, Section 3 for identification and estimation. Results are discussed in Section 4.

development. The effectiveness of early years aligns with the large economic literature on early interventions and dynamic complementarities in human capital investments (e.g., Heckman and Mosso 2014). Moreover, high school education, enabling individuals to shift away from elementary, low-skilled occupations, may prove crucial for later cognitive development. Early and late childhood are also the periods where household income is more strongly associated with child's future human capital (Carneiro et al. 2021).

5. Schooling, labour market, and the use of skills

The previous section uncovered positive and robust causal impacts of schooling on cognitive skills. One potential channel through which schooling may impact numeracy and literacy is by increasing the quantity and quality of labour market participation. We exploit the rich PIAAC questionnaire on labour market experience and the use of skills to study a series of potential mediating outcomes.

Additional schooling increases job quality. We estimate 2SLS models as in equation (3) where the dependent variable is a labour market outcome, and observation is conditional on

Table 4 Heterogeneity analysis.

	All	Both parents born in country		Age		Socio-economic status		
		No	Yes	>45	≤45	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable								
	Panel A. First stage							
Years of schooling	0.169***	0.263***	0.159***	0.170***	0.046	0.126**	0.130***	0.176***
	(0.033)	(0.067)	(0.033)	(0.037)	(0.029)	(0.050)	(0.031)	(0.039)
***F*-statistics**	25.69	15.41	22.53	21.60	2.508	6.503	17.44	20.66
	Panel B. Reduced form							
Numeracy	0.041***	0.065***	0.038***	0.057***	0.019*	0.019	0.041***	0.047***
	(0.008)	(0.021)	(0.008)	(0.012)	(0.010)	(0.017)	(0.010)	(0.012)
Literacy	0.041***	0.059**	0.039***	0.057***	0.019**	0.024	0.039***	0.052***
	(0.009)	(0.024)	(0.009)	(0.014)	(0.009)	(0.018)	(0.011)	(0.013)
	Panel C. IV							
Numeracy	0.242***	0.246***	0.241***	0.337***	0.414	0.148	0.312***	0.268***
	(0.056)	(0.079)	(0.062)	(0.082)	(0.322)	(0.133)	(0.087)	(0.072)
Literacy	0.240***	0.224***	0.243***	0.337***	0.416	0.189	0.304***	0.297***
	(0.062)	(0.085)	(0.069)	(0.092)	(0.294)	(0.151)	(0.089)	(0.077)
N	2,034,899	182,800	1,852,099	1,011,033	1,023,866	403,038	1,134,192	497,669
***N* (individuals)**	80,066	6,663	73,403	40,247	39,819	16,703	43,090	20,273

(continued)

Table 4 (continued)

	All	Both parents born in country		Age		Socio-economic status		
		No	Yes	>45	≤45	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year, country, reform-by-wave fixed effects (FEs)	Y	Y	Y	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y	Y	Y

Note: The table shows separate estimates by subgroups. Specification and estimation follow column 2 of Table 3, with full-sample replicated in column 1. Considered groups are: individuals with or without immigrant origin (columns 2 and 3, respectively); individuals aged more than 45 or younger (columns 4 and 5, respectively); individuals with low, medium, or high socio-economic status (SES, columns 6–8). SES is defined as high if at least one parent attained higher education or there were more than 200 books at home during childhood. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for the description of reforms and reform events, Section 3 for identification and estimation. Results are discussed in Section 4.

* $P < .1$. ** $P < .05$. *** $P < .01$.

Source: Authors' calculations.

Table 5 Cognitive skill returns to schooling by phase of education.

	All	Preschool reforms	Primary school reforms	Secondary school reforms	University reforms
	(1)	(2)	(3)	(4)	(5)
Dependent variable					
	Panel A. First stage				
Years of schooling	0.169***	0.242***	−0.301***	0.265***	0.147**
	(0.033)	(0.080)	(0.092)	(0.048)	(0.059)
***F*-statistics**	25.69	9.153	10.72	30.28	6.298
	Panel B. Reduced form				
Numeracy	0.041***	0.083***	0.003	0.033**	0.021
	(0.008)	(0.022)	(0.031)	(0.015)	(0.014)
Literacy	0.041***	0.075***	0.015	0.051***	0.017
	(0.009)	(0.025)	(0.037)	(0.015)	(0.015)
	Panel C. IV				
Numeracy	0.242***	0.341***	−0.010	0.126**	0.142
	(0.056)	(0.125)	(0.104)	(0.054)	(0.102)
Literacy	0.240***	0.310**	−0.048	0.193***	0.113
	(0.062)	(0.124)	(0.127)	(0.060)	(0.108)
N	2,034,899	435,618	556,136	660,523	382,622
***N* (individuals)**	80,066	80,066	80,066	80,066	78,142
Year, country, reform-by-wave fixed effects (FEs)	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y

Note: The table shows estimated cognitive skill returns to an additional year of schooling by phase of education affected. Specification and estimation follow column 2, Panel C of Table 3, with full-sample replicated in column 1. Reforms affecting preschool, primary education, secondary education, and higher education are separately considered in columns 2, 3, 4, and 5, respectively. Reforms are mapped into these groups following column 4 of Table 1. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for the description of reforms and reform events, Section 3 for identification and estimation. Results are discussed in Section 4.

* $P < .1$. ** $P < .05$. *** $P < .01$.

Source: Authors' calculations.

participation. Results from this exercise are summarized in Fig. 4b–d. The rightmost blue bar in Fig. 4b indicates that one additional year of schooling leads to a 6 percentage point higher probability of working for a large firm. Employment in large firms is associated with higher skill growth, suggesting better learning opportunities than in small firms (Arellano-Bover 2022). The rightmost yellow bar indicates that one additional year of schooling leads to a 7 percentage point higher probability of working in a high-skill occupation (professional or manager).[21]

[21] Underlying estimates are reported in Table A.4 (see online supplementary material for a colour version of this table) along with additional outcomes, displaying similar patterns. We do not consider wages since

Additional schooling increases the skill intensity of an individual's job, and the opportunity to acquire new skills. Figure 4c indicates that one additional year of schooling leads to 0.26σ (rightmost yellow bar) higher use of numeracy on the job, against a substantially lower effect on the use of numeracy at home (rightmost blue bar, 0.13σ). Additional schooling also increases a summary index of learning opportunities on the job (Fig. 4d).[22] Hence, higher schooling opens the door to occupations with more intense use of complex skills.

Mirroring our main results on cognitive skills, college graduation disproportionally improves job quality and the use of skill. Similarly to Fig. 4a, Fig. 4b–d contrast the impacts of completing a high school, tertiary, or college degree in columns 2–4 of Table A.4 (see online supplementary material for a colour version of this table) with linear predictions obtained from column 1. College graduation has more than double the effect of five additional average years of schooling on the probability of working in high-skill occupations and for a large firm, on the use of numeracy, and on the summary index of learning on the job. Results suggest that college degree may have a large positive impact on cognitive skills in adulthood by enabling access to higher-quality jobs.

Overall, the impacts of schooling and formal qualification on the skill intensity of the current job closely mimic their impacts on cognitive skills. Our findings suggest that individuals acquiring higher levels of education access more complex and qualified jobs, which challenge them in their day-to-day working life and foster the continuous development of cognitive skills. Several results support this 'dynamic' interpretation of our findings against a 'static' causal chain, where individuals with higher cognitive skills produced by education sort into skill-intensive jobs. First, since the use of skills prevent their age-related decay (Hanushek et al. 2025), larger schooling impacts on the use of skills on the job rather than at home point to an important role of the labour market experience. Second, the positive impact of schooling on learning opportunities at the firm, which are found to improve human capital (Arellano-Bover and Saltiel 2026), suggest that schooling improves also skills *growth*. Third, we find larger reduced-form impacts of schooling on skills for older cohorts (see Table 4), suggesting that the cognitive skill advantage provided by schooling is not fixed from career beginning but widens with experience. In line with our conclusions, Gorman (2023) finds that compulsory schooling decreases the likelihood of undertaking a routine occupation. Our results complement their analysis with unique evidence on the use of skills.

6. Conclusion

This article offers unique evidence on the relationship between schooling and cognitive skills. We use a broad set of educational reforms across twenty-one European countries to isolate exogenous variation in schooling for the population of working age. We find that an additional year of schooling increases cognitive skills by $0.20 - 0.25\sigma$. Beyond this traditionally studied parameter, we show that the impacts of schooling on skills are non-linear and that they are likely magnified by the skill intensity of labour market experience.

these are unavailable in six out of twenty-one countries. Although within-country earning deciles are available, such ordinal measures do not suit our cross-country analysis.

[22] The use of skills in everyday life ('at home') and on the (current or last) job measures engagement with activities requiring each type of skill. For example, the use of numeracy involves, for example, the use or calculation of fractions, decimals, or percentages (see Table 2.5 in OECD 2019). 'Learning at work' summarizes the intensity of on-the-job learning by doing, from other colleagues, or through new products or services. Both types of indices are obtained through a cross-country analysis of comparability and reliability of single questionnaire items (see Section 20.5 in OECD 2013). We standardize all indices to have zero mean and unit variance in the working sample.

Our results offer key insights for policymakers investing in education. First, policies that expand attainment can generate sizeable and lasting gains in core skills, not only in formal credentials. Second, we show that reforms expanding preschool and secondary education are more effective than those targeting other phases of education. Together with recent evidence on sensitive stages of human-capital development (Carneiro et al. 2021), this pattern points to educational phases where policy interventions may be particularly consequential. Third, the role of post-school work experience highlights the benefits of promoting employment in skill-intensive sectors and encouraging firms to provide workplace learning opportunities (Arellano-Bover and Saltiel 2026). More generally, because skill use and learning at work track the skill gains from schooling, education policies may be most effective when complemented by labour-market conditions that foster skill-intensive tasks and workplace learning. Finally, our results also suggest that, to prevent cognitive decay, policy should monitor and promote skill development not only among non-workers (Dinerstein et al. 2022) but also among workers in low-skill occupations.

More broadly, our findings can help to disentangle the determinants of the wage returns to schooling, which are crucial to assess the validity of different economic models (Clark and Martorell 2014). While a direct assessment of wage effects is precluded to us by data limitations, a back-of-the-envelope calculation implies that cognitive skills could explain between 30 per cent and 60 per cent of the wage returns to schooling. Following Carlsson et al. (2015), we draw two parameters from the literature: a 8 per cent wage return to an additional year of schooling, and a 10–20 per cent wage return to 1σ higher skills. Applying to the latter our estimates of 0.25σ skill return to schooling, gains in numeracy and literacy imply a 2.5–5 per cent wage return to an additional year of schooling. The residual impact of education on earnings could be driven by other factors such as improved networks or signalling of individual ability to employers. The disproportional impacts of high school and college degrees on labour market outcomes that we presented may suggest that, as long as employers use qualifications as a signal conditional on candidates' human capital, qualifications can lead to higher skills over and beyond the direct human capital effect of schooling through the access to skill-intensive jobs.

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Supplementary material

Supplementary material is available on the *Oxford Economic Papers Journal* online. The material includes data, replication files, and the Supplementary Appendix.

Conflicts of interest

None declared.

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