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The effects of schooling on cognitive skills: evidence from education expansions *

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Abstract

We quantify the causal effect of schooling on cognitive skills across 21 countries and the full distribution of working-age individuals. We exploit exogenous variation in educational attainment induced by a broad set of institutional reforms affecting different cohorts of individuals in different countries. We find a positive effect of an additional year of schooling on internationally-comparable numeracy and literacy scores. We show that the effect is substantially homogeneous by gender and socio-economic background and that it is larger for individuals completing a formal qualification rather than dropping out. Results suggest that early and late school years are the most decisive for cognitive skill development. Exploiting unique survey data on the use of skills, we find suggestive evidence that our result is mediated by access to high-skill jobs.

JEL Codes: H52, I21, I28

Keywords: Cognitive skills, Educational Policies, Returns to schooling

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1 Introduction

The effect of education on cognitive skills is a key parameter. Cognitive skills shape crucial long-run outcomes, such as wage (Lindqvist and Vestman, 2011; Hanushek et al., 2015) and innovation (Aghion et al., 2017). The health emergency triggered by COVID-19 has renewed the interest in credible and general estimates of the cognitive skills return to an additional year of schooling, which would inform the debate on policies such as school closure with a fundamental cost component (Engzell et al., 2021). More generally, 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., 2022). Schooling is the primary tool in the hand of policy makers to meet the development goal of an equitable and inclusive education for all, currently far from being reached. Relatedly, quantifying the cognitive skill return to schooling offers empirical evidence about the extent to which education serves as a fruitful investment in human capital or merely as a signaling device.

Despite the relevance of this parameter, only a few studies offer credibly causal estimates, and they focus on relatively narrow contexts, age groups, or outcomes. The estimation of the cognitive skills return to schooling faces the empirical challenge of eliminating selection bias in observed educational attainment. Several studies identify the impact of schooling by exploiting variation in educational institutions. Banks and Mazzonna (2012) and Gorman (2017) use compulsory schooling reforms in England, focusing on relatively early school leavers whose cognitive skills are measured at older ages. Schneeweis et al. (2014) use similar reforms in six other European countries, focusing on older ages. In contrast, Carlsson et al. (2015) exploit the random assignment of timing of cognitive tests to 18-year-old males in Sweden. All these studies find positive impacts of education only for specific cognitive outcomes such as technical comprehension or working memory.[1]

This paper offers causal and unprecedentedly general evidence on the long-run impact of schooling on cognitive skills. We isolate exogenous variation in schooling by matching educational reforms in 21 European countries (Braga et al., 2013) to adult numeracy and literacy scores measured in the internationally-standardised PIAAC survey published by the OECD.

[1] Other studies investigate the impact of educational type rather than attainment, estimating the effect of general rather than vocational education on earnings, cognitive, and noncognitive skills (Brunello and Rocco, 2017; Ollikainen et al., 2022).

We argue that conditional on country and cohort-specific unobservables, higher attainment induced by the educational reforms can be used to instrument schooling in the cognitive skills equation. We exploit this quasi-experimental setting to estimate the returns to an additional year of education, non-linearities in the impact of schooling, heterogeneous effects by individual characteristics and type of reforms, and to explore potential mechanisms.

We leverage the policy experiment embedded in educational reforms that assigns 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 cognitive skills induced by the reforms with secular trends in education and the natural decay of cognitive skills with age. We tackle this challenge by exploiting, for each reform, the control group of individuals in similarly-developed countries which has not (yet) reformed a given educational institution.

Our empirical strategy proceeds in two steps. First, we compare exposed and unexposed cohorts in treated and control countries in a parametric event study design (Lafortune et al., 2018; Rothstein and Schanzenbach, 2022). This design isolates the jump in schooling observed in the earliest birth cohorts exposed to a reform. To address concerns about two-way fixed effect (TWFE) estimators in the presence of treatment effect heterogeneity (De Chaisemartin and d'Haultfoeuille, 2020; Callaway and Sant'Anna, 2021; Goodman-Bacon, 2021; Sun and Abraham, 2021; Borusyak et al., 2022), we implement a stacked-by-event design that leverages the 2-by-2 difference-in-differences comparisons underlying our staggered design (e.g., Deshpande and Li, 2019; Vannutelli, 2021). We show that outcome evolution is undistinguishable across reforming and non-reforming countries up to the reform events, supporting the validity of our design.

Second, we use the jump in schooling induced by educational reforms as instrument in a DD-IV model of cognitive skills (Duflo, 2001; Hudson et al., 2017; Ridley and Terrier, 2020). This exercise relies on the exclusion restriction that educational reforms impact cognitive skills only through years of schooling. A natural concern is that an increase in school quality maybe an andditional channel. We provide indirect evidence in support of the exclusion restriction by showing that, on average, no reform impact on skills is found when there is no impact on years of schooling (Angrist et al., 2019). This evidence is in line with Brunello et al.

(2013b), showing that this exclusion restriction cannot be rejected in the case of compulsory school reforms.

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; Brunello et al., 2015). Other studies have used schooling reforms to estimate the causal impact of education on other outcomes. Exploiting compulsory schooling expansion in England, 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. Exploiting similar reforms in other European countries, Brunello et al. (2013a) show that schooling has a protective effect on the body mass index of females.

Our results bring several contributions. First, we offer substantially more general estimates of the cognitive skills return to schooling than those available. An additional year of schooling increases numeracy and literacy scores in adulthood by $0.2 - 0.25$ standard deviations (hereafter, σ). This constitutes novel evidence of the causal effect of schooling on key cognitive outcomes, forming the basis for acquiring more specific knowledge, and falls within the range of available estimates on other measures of cognitive development. Our sample of individuals aged 25-65 implies that the impact of education on cognitive skills is not limited to older ages, which have been the focus of most previous studies. Second, we estimate heterogeneous effects and show that cognitive skill returns to schooling accrue in similar magnitudes across males and females, and across individuals with different socio-economic backgrounds. Third, we investigate non-linearities in the returns to schooling and find suggestive evidence of larger-than-average effects of the completion of formal qualifications, especially high school graduation. Moreover, we find that reforms affecting preschool and secondary education exhibit the largest effects. This suggests that early and late school years may be the most decisive for cognitive skill development in adulthood, in line with recent research on the relative productivity of investments in human capital at different stages of childhood (Carneiro et al., 2021).

Finally, we explore potential mechanisms by exploiting the unique richness of the PIAAC questionnaire about the use of skills at home and on the job. We find that the impact of schooling is substantially larger on the use of skills on the job rather than at home. Schooling increases employment probability and exerts substantial positive effects on the quality of jobs conditional on working (e.g., occupation level and firm size). Mirroring the effect on cognitive skills, the impact of completing a formal qualification (high school or university graduation) on labour market outcomes is larger than proportional, especially for secondary school completion. These results suggest that access to high-skill jobs is an important channel through which education increases cognitive skills in adulthood (Gorman, 2017; Arellano-Bover, 2022). This interpretation is consistent with recent quasi-experimental evidence on skill depreciation (Dinerstein et al., 2022), and could suggest that skill depreciation comes not only from nonemployment but also from employment in less skill-intensive jobs.

A further contribution of our study is to provide causal evidence on the impact of a broad portfolio of educational reforms on the quantity and quality of human capital. We show that, on average, considered interventions increase schooling, which is likely a primary policy objective. Policies that expand access to education, such as those widening compulsory education, weakening restrictions on university access, or delaying the age of tracking, have the strongest effects on schooling, numeracy, and literacy. Other reforms have also a significant effect on school attainment, such as those expanding pre-primary education and those increasing school autonomy and accountability. We show that the reforms we consider boost educational attainment especially around high school graduation and university graduation.

2 Data and descriptive statistics

Our empirical analysis has two main ingredients. We exploit individual-level cross-country survey data including direct assessment of adult skills alongside background characteristics. We match individuals to educational reform data collected by Braga et al. (2013), including the type and time of a broad set of interventions in most European countries in 1929-2000.

The PIAAC survey

We observe internationally-standardised measures of adults' cognitive skills from the OECD Survey of Adult Skills ("PIAAC"; see OECD, 2019). Skills are measured through a validated procedure developed by groups of experts for each of the skill domains assessed. 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 mathematical information. These were assessed to provide reliable measures of competences playing a key role in processing information and providing the foundation to access and understand more specific domains of knowledge. 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 standardised to have zero mean and unit variance in the PIAAC sample.[2]

PIAAC data additionally include a background questionnaire collecting information on demographic characteristics, educational attainment, labour market experience, and the use of skills. Particularly important in our analysis are years of schooling, measuring the quantity of formal education received.[3]

PIAAC is a cross-sectional survey involving 39 countries in three cycles of data collection. Each country interviewed a random sample of the target population, formed of residents between 16 and 65 years old. We consider the 21 European countries that could be matched to educational reforms.[4] Data from Greece and Slovenia were collected in 2014, data from Hungary in 2017, and all other countries in 2011. For each individual, we obtain the year of birth by subtracting the age reported on the day of the survey.[5]

Educational reforms and reform events

We exploit a country-year level dataset on educational institutions in European countries in 1929-2000 built by Braga et al. (2013). The policy interventions considered are listed and described in Table 1, sourced from Table 2 in Braga et al. (2013). The data include 18 different types of reforms, encompassing all phases of education from preschool to higher education. Reforms are grouped in policy areas, resulting in six broader categories of interventions. Four groups of policies are targeted to the school system: policies impacting pre-primary education; policies expanding the access to education (hereafter, "expansion" reforms); policies increasing qualification requirements on teachers; policies expanding school autonomy

[2]PIAAC data provide 10 individual plausible values for cognitive scores in each domain (e.g., numeracy or literacy), derived using item response theory (Khorramdel et al., 2020). We consider a single score in each domain by averaging across all plausible values.

[3]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 EU-SILC survey).

[4]Great Britain and Northern Ireland are considered as two separate countries in our data. Records from Belgium refer to Flanders only.

[5]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., 27 in the 25-29 band).

and accountability. Two groups of policies are targeted at the university system: policies expanding university autonomy, and policies providing financial support to students. The dataset contains a variable for each reform, coded as step dummy. In our analysis, we focus on the changes in these variables over time, indicating that the corresponding institution was reformed.[6]

These institutional variables mostly capture the reforming activities of policymakers, while being unfit to measure the magnitude of the impact. Except a few variables (like the starting/ending ages for compulsory education), for all the other dimensions 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.[7] 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 achievements and subsequent wages (Checchi and van de Werfhorst, 2018). In the present case, we match PIAAC records with educational reforms based on the year of birth. We define the first exposed cohort to each reform depending on the phase of education affected (e.g., individuals born 6 years before the reform was passed for primary school interventions). As can be seen from Table 1, reforms' mapping into policy areas (column 1) does not necessarily correspond to the phase of education assessed, indicated by the target age (column 4). We report results considering both reform groupings in our empirical analysis.

To build our research design, detailed in Section 3 below, we define reform events for each country and policy intervention. Reform events are defined as the earliest observed intervention in a given country by type of reform.[8] When more than one intervention of the same type is observed in a given country, alternative choices are possible. For example,

[6]For some variables related to university access, we lack information for seven new entrants into the European community (Czech Republic, Estonia, Hungary, Latvia, Poland, Slovak Republic, Slovenia). See Braga et al. (2013) for further details on reforms and data collection.

[7]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.

[8]Most observed reforms operate in a positive direction, and we do not consider the few reform events operating in a negative direction (9% 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 4 below).

Lafortune et al. (2018) focus on the most impactful event, as measured by the strongest statistical effect on the reform target (in their context, educational spending, see their Section II.A). We believe that using the earliest intervention is most appropriate in our context as institutional reforms, differently from the court orders studied by Lafortune et al. (2018), can hardly remain unimplemented. Nonetheless, we control in all specifications for other subsequent (and previous) observed interventions to ensure we fully capture the impact of our treatments of interest (see Section 4 below).

Sample selection and descriptive statistics

In order to observe completed education, we restrict the sample to individuals aged 25 or later. In addition, we do not consider first-generation immigrants (almost 8% of the sample) since they were likely not exposed to the national education system. Finally, to avoid compositional changes across countries, we do not consider individuals born after 1986, the youngest birth cohort that we can observe across countries. These selections yield a working sample of 84,345 individuals with non-missing values of both cognitive skills and years of schooling.

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.[9] The 91% of individuals have both parents native-born, while the rest are second-generation immigrants. We define an individual with high socio-economic status as having either at least one parent with higher education or at least 200 books at home during childhood (Brunello et al., 2015). The average individual completed 12.7 years of schooling, with substantial dispersion. Lower-secondary qualifications are almost universal (95%), 64% of individuals completed high school, 34% holds at least an undergraduate degree, 24% attains a postgraduate degree.[10]

We consider 208 reform events widely spread across countries, years, and types of reforms. While Denmark is the single country with most reform events, Panel A of Figure 1 shows that all countries exhibit a significant reforming activity, with Slovak Republic and Slovenia – the least active countries – still intervening in 6 different policy areas. Considering the

[9] Schneeweis et al. (2014) focus on individuals aged 47-73, Gorman (2017) on individuals aged 48-60, and Banks and Mazzonna (2012) on individuals aged 50 or more.

[10] We define educational attainment using data on the highest qualification, available for 79,016 individuals. In addition, we ignore foreign qualifications for which the educational level is not specified (134 observations, 0.17%).

timing, Panel B and Panel C show reforms by the first affected cohort and year of passing, respectively. 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, respectively. Finally, Panel D shows reforms by type. Almost all countries expanded the length of compulsory education, increased qualifications required to teach in primary school, or widened university access, while we observe only a few reforms of the starting age of formal education and of the interest rate paid on student university loans.[11]

3 Empirical strategy

Event study design

We are interested in the causal effect of an additional year of schooling on cognitive skills in adulthood. In observational data, the identification of this parameter is complicated by selection bias, i.e., individuals who would have recorded higher numeracy and literacy scores independently from formal education are more likely to opt for additional years of schooling. At the same time, while increasing schooling may increase cognitive skills through learning, higher cognitive 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 years of schooling.

We exploit the policy experiment embedded in educational reforms. Exposure to the treatment is driven by the year when reform was passed and the phase of education affected, which jointly define the first birth cohort affected by the reform. Our empirical strategy develops from the idea of comparing the outcomes of cohorts of individuals first exposed to educational reforms with those of observationally similar individuals not affected by the intervention because they attend school just before a policy kicks in. The main empirical challenge in such a comparison is that exposed individuals are mechanically younger. Simply contrasting outcomes across cohorts born before and after the reform would likely be confounded by secular trends in both years of schooling and cognitive skills. As it is well known,

[11]Figure A.3, plotting reform years by type of intervention and sorting reforms by phase of education affected, shows no systematic pattern in reform timing of different points of the educational career. Reforms affecting the secondary phase are those adopted earliest, while school autonomy reforms are the most recent policy trend.

both outcomes exhibit a steeply-increasing trend in the second half of the XX century, a fact illustrated in our sample by Figure 2. In addition, age-related skill decline may inflate the effect of schooling on skills in a simple before-after comparison.

To overcome the shortcomings of before-after comparisons, our research design complements time variation across cohorts with space variation across countries by building difference-in-differences comparisons around a reform event. We exploit the observation of a control group of similarly-developed 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. The key identifying assumption 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 in a regression framework in Section 4 below.

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 a single educational reform intervention. We are interested in the discrete jump observed in schooling and cognitive skills following the reform, parametrised by α_1 in the following model:

$$Y_{ict} = \alpha_0 + \alpha_1 \mathbb{1}(t > t_c^*) + \alpha_2 \mathbb{1}(t > t_c^*) \cdot (t - t_c^*) + \alpha_3 * (t - t_c^*) + \alpha_4 X'_{ict} + \phi_c + \lambda_t + e_{ict}, \quad (1)$$

where t is the year of birth of individual i, t_c^* is the first birth cohort affected by the reform in country c, and $\mathbb{1}(t > t_c^*)$ is an indicator equal to one if the birth cohort t is exposed to the reform. Terms involving t_c^* are coded to zero in non-reforming countries. The dependent variable Y_{ict} denotes years of schooling or cognitive ability of individual i. 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. Our identifying assumption can be indirectly tested by evaluating the hypothesis that α_3 is statistically equal to zero. The coefficient α_2, on the other hand, estimates the evolution of treatment effects after the reform. The vector X'_{ict}, added to some of our specifications to increase precision, includes background characteristics such as gender, immigration, parental education, and the number of books at home during childhood.[12]

[12]In addition, all regressions control for reforming activity variables. Specifically, we control for the time-

Several recent works have shown that, with heterogeneous treatment effects, TWFE estimators of α_1 may result in unreasonably-weighted averages of underlying treatment effects, with weights that may even be negative (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021; Callaway and Sant'Anna, 2021; Borusyak et al., 2022). With variation in treatment timing, TWFE estimation departs from the simple 2-by-2 difference-in-differences model by pooling comparisons of units initially treated in different periods, including "forbidden" comparisons of later-treated with already-treated units. 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 $(t - t_c^*)$ are contaminated by treatment effects in all other periods. Since we consider a wide range of countries and treatment periods, we can hardly rely on restrictive specifications assuming treatment effect homogeneity.

We address these concerns by building a stacked-by-event design. The intuition is that we avoid the issues with TWFE estimator by constructing a separate difference-in-difference design for each treatment wave, and by averaging across all these building blocks. In detail, we follow Deshpande and Li (2019) and Vannutelli (2021) and build separate datasets for each event year. A dataset includes individuals in countries reforming in the considered year (treatment group) and those either not passing the reform or passing it in a later period (control group).[13] This design allows us to assign a value of t_c^*, the first exposed cohort, to control individuals as well using the time of the considered event year. Finally, we stack all datasets and estimate the following specification:

$$\begin{aligned} Y_{ictw} &= \gamma_0 + \gamma_1 \mathbb{1}(t > t_c^*) \cdot T_{wc} + \gamma_2 \mathbb{1}(t > t_c^*) \cdot (t - t_c^*) \cdot T_{wc} + \gamma_3 (t - t_c^*) \cdot T_{wc} + \\ &+ \sum_l \nu_l \mathbb{1}(t = t_c^* + l) + \gamma_4 X'_{ict} + \phi_c + \lambda_t + \eta_w + e_{ictw}, \end{aligned} \tag{2}$$

varying sum of reform variables in all policy domains but the one considered, and for the continuous variables indicating the age of start of compulsory education, age of tracking, and duration of compulsory education. In addition, we control for observed reforms in the considered domain in the years preceding the birth of individuals in our sample (1929-1953). Finally, since we study the earliest intervention by country-reform, we control for the total number of reforms in the considered domain.

[13] Since we censor our nonparametric estimation at 15 years after the reform (see below), we do not include in the control group individuals in countries reforming the considered policy within 15 years from the treatment wave considered. Moreover, if the considered reform is passed by all observed countries at some point in time, we do not consider the latest treatment wave since there is no control group. Among the 18 reforms considered, this is the case only for the index of university autonomy.

where w indexes treatment waves (i.e., event years), T_{wc} is a dummy variable indicating treated units, $\mathbb{1}(t = t_c^* + l)$ indicates birth cohorts l years after reform's introduction, and η_w are treatment wave fixed effects. Note that the stacked specification includes relative event time fixed effects (ν_l), identified by considering a specific control group for each treatment wave. Coefficients γ_1, γ_2, and γ_3 have analogue interpretation to α_1, α_2, and α_3 in equation (1), respectively. Standard errors are clustered at the country-cohort level, at which treatment varies.

We compare estimates from our linear model to a non-parametric specification:

$$Y_{ictw} = \tau_0 + \sum_{l \neq 0} \tau_l \mathbb{1}(t = t_c^* + l) \cdot T_{wc} + \sum_{l} \nu_l \mathbb{1}(t = t_c^* + l) + \tau_4 X'_{ict} + \phi_c + \lambda_t + \eta_w + e_{ictw}. \quad (3)$$

Coefficients τ_l estimate the impact of educational reforms at each relative event time, where we center to zero the last period preceding the reform and exclude the corresponding indicator from estimation.[14] The joint hypothesis that $\tau_l = 0 \; \forall l < 0$ provides an indirect test of the parallel trends assumption, offering stronger evidence with respect to the hypothesis that γ_3 in (2) is equal to zero. By comparing estimates of τ_l with γ_1, γ_2, and γ_3 in (2), we show how well our linear approximation fits non-parametric estimates.[15]

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 exploit the institutionally-induced variation in schooling to estimate the skill returns to education by rescaling the (reduced-form) effect of educational reforms on numeracy and literacy by their (first-stage) impact on years of schooling.

Under additional identifying assumptions, an instrumented difference-in-differences design ("DDIV") provides causal estimates of the cognitive skills return to schooling (Hudson

[14] In our preferred specification, we consider a symmetric 15-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 15 cohorts after the intervention or later (Lafortune et al., 2018; Rothstein and Schanzenbach, 2022). Robustness to this choice is shown in Figures A.1 and A.2.

[15] We present estimates from specifications that stack data across all educational reforms considered. We simply construct a stacked-by-event dataset for each reform as described above and append all to form our estimation dataset, which includes 5,514,167 observations. All regressions include reform-by-wave fixed effects to identify our parameters of interest only from within-dataset variation.

et al., 2017).[16] These assumptions can be easily discussed by contrast with restrictions identifying a traditional IV model. Our key 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 by considering first-stage event study estimates of the effect of reforms on schooling. The exclusion restriction requires reforms to impact cognitive skills only through years of schooling. An important concern in this respect is that educational interventions may impact other inputs of the human capital production function, such as the quality of education received. We provide evidence in support of the exclusion restriction in Section (4) below by showing that, on average, no reduced-form effect on skills is detected for reforms that do not increase years of schooling. Finally, under treatment effect heterogeneity, IV estimates identify a local average treatment effect for reform compliers as long as all individuals are weakly more likely to take up additional schooling when exposed to the same reform, the monotonicity assumption. We believe that 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 an institutional reform expanding it (and for lower schooling only in the presence of a reform).

We estimate a 2SLS model of cognitive skills where schooling is instrumented by the event study specification. Our second stage equation is:

$$\begin{aligned} C_{ictw} &= \beta_0 + \beta_1 S_{ictw} + \beta_2 \mathbb{1}(t > t_c^*) \cdot (t - t_c^*) \cdot T_{wc} + \beta_3 (t - t_c^*) \cdot T_{wc} + \\ &+ \sum_l \nu_l \mathbb{1}(t = t_c^* + l) + \gamma_4 X'_{ict} + \phi_c + \lambda_t + \eta_w + u_{ict}, \end{aligned} \tag{4}$$

where C_{ict} denotes cognitive skills. The first stage is estimated by equation (2) when the dependent variable is years of schooling (S_{ict}). In other words, we instrument schooling with the jump in education following an institutional reform. Our parameter of interest is β_1, representing the impact on cognitive skills of an additional year of schooling. In what follows, we additionally discuss non-linearities by using educational qualifications as endogenous variables rather than years of schooling.

[16] See, e.g., Duflo (2001), Jackson et al. (2016), and Ridley and Terrier (2020) for empirical applications of this model.

4 Results

Effects of reforms on schooling and skills: main results

We begin our discussion of the results by presenting the event study estimates of the effects of educational reforms on years of schooling and on numeracy and literacy score.

After an educational reform is passed, we observe a significant increase in educational attainment. Estimates of γ_1 in equation (2) on years of schooling are reported in Panel A of Table 3. Column (1) presents results from uncontrolled specifications which only include a dummy variable indicating exposure to the reform (a before-after estimate). On average, individuals born after a reform kicks in opt for a half additional year of education. This large correlation likely reflects secular trends in education (see Figure 2). Event study estimates are presented in column (2), showing a causal impact of reforms of about 0.04 years of schooling. Despite being less than a tenth of the correlation in column (1), the estimated effect is strongly statistically significant. As can be seen in column (3), estimates are barely changed when controlling for individual characteristics, supporting the validity of our research design.

Undetectable impacts prior to the reform lend validity to our identifying assumption. Estimates of γ_3 in equation (2) are close to zero in magnitude and not statistically significant (see Panel A of Table 3). Non-parametric estimates suggest the same conclusion, as visualised in Panel A of Figure 3. The graph summarises results from both parametric and non-parametric models by showing estimates of coefficients τ_l from equation (3) superimposed to predictions from the linear model in equation (2). The first takeaway from the graph is that our linear model fits noisier nonparametric estimates fairly accurately. Second, coefficients at negative relative time periods, representing unexposed cohorts, revolve around zero and are mostly statistically insignificant. We cannot reject the hypothesis that all pre-reform estimates are zero ($p = 0.30$, see Table A.1, column 1).

Reform impacts on years of schooling are fairly persistent over time. This can be seen by small and statistically non-significant estimates of γ_2 in equation (2) (Panel A of Table 3). In each subsequent cohort following the introduction of a reform, its impact is predicted to decrease by 0.001 years of schooling. This is visualised by the slightly negative post-reform trend in Figure 3, resulting in a halved predicted effect 15 cohorts after the reform. Nonparametric estimates turn to a fairly precise zero at the end of the relative time window considered. This result suggests some degree of convergence in the quantity of education in the medium-long run between reforming and non-reforming countries.[17]

Observed reforms mostly impact high school or university graduation. We present in Table 4 estimates similar to Panel A of Table 3 where the dependent variables are dummies indicating attainment at different levels. Column 1 of Table 4 shows high school and university graduation increase by about 1.3 and 1.6 percentage points (hereafter, p.p.), respectively, corresponding to a 2% and a 5% increase relative to the pre-reform averages (64% and 33%, respectively).

After an educational reform is passed, we observe a significant increase in numeracy and literacy scores. Panels B and C of Table 3 show that reforms increase cognitive skills by about 0.02σ (columns 2-3). Similar to what we find for education, the magnitude is around a tenth of the before-after estimates in column (1) yet estimates are strongly statistically significant. Pre-reform effects on cognitive skills support our identifying assumption: although statistically significant, estimates of the linear pre-reform trend (γ_3 in equation 2) are very small in magnitude and we view them as precisely estimated zeroes (e.g., placebo estimate on numeracy scores in column 2, Panels B of Table 3 is 0.006σ, 3% of the estimated jump following an educational reform). This interpretation is supported by non-parametric estimates in panels B and C of Figure 3, showing that pre-reform effects are mostly not different from zero.[18] Similar to what was discussed for years of schooling, reform impacts on cognitive skills gradually fade away in younger cohorts but are fairly persistent over time, especially for literacy score. The reforms effect on the latter is less than halved 15 cohorts from a reform's introduction. Numeracy score, on the other hand, drops more evidently, but the predicted effect becomes zero only 13 cohorts after reform is kicked in.

Effects of reforms on schooling and skills by type of reform

Reforms expanding access to education are estimated to have the strongest impact on years of schooling. Since these reforms, e.g., increase the length of compulsory education or delay the time when students are separated between academic and vocational tracks, they represent

[17]See Murtin and Viarengo (2011) for a discussion of economic forces behind the convergence of years of schooling across countries.

[18]Although pre-reform estimates are jointly different from zero across all reforms (column 1 of Table A.1), they are not significant for most interventions and particularly for expansion reforms, the type of reforms with stronger impacts on skills (columns 2-7).

policies we would expect to exert the largest effects (see Table 1). Panel A of Table 5 reports estimates of the reform exposure coefficient separately for the six policy areas defined in Section 2. Columns (3) and (4) show that expansion reforms increase years of schooling by about 0.18, generating the estimate with the strongest statistical significance (F-statistics around 20). As shown in Panel A of Figure 4, pre-reform effects are close to zero and not statistically significant also when considering expansion policies only (F-test of joint significance has a p-value of 0.40, see column 3 of Table A.1). The figure also shows steeper declines in expansion reforms' impact over time compared to the results using all reforms in Panel A of Figure 3. This result stems from the combination of larger impacts of expansion reforms' introduction and null long-run effects for individuals attending schools 15 years or more from an intervention. Nonparametric estimates indeed persist around 0.15 except the long-run estimate. Interventions on pre-primary schooling and on school autonomy have an average impact of $0.13 - 0.20$ years of schooling (columns 1-2 and 5-6), while university reforms have no detectable effect on years of schooling (columns 9-12).[19] Similar patterns are found when considering educational attainment as outcome variable in Table 4. Estimates by policy area in columns (2)-(7) show that expansion, pre-primary, and school autonomy reforms exert the largest impacts. In light of these results, we additionally present estimates focusing on these three reform domains in what follows.

Reform impacts on cognitive skills by policy area broadly follow the corresponding estimates for years of schooling. Panel B of Table 5 shows that the largest estimates are recorded for expansion reforms, increasing numeracy and literacy by $0.04 - 0.05\sigma$. This result is in line with our exclusion restriction that the effect of reforms on cognitive skills operates through the impact on years of schooling, since the assumption implies no reform impacts on cognitive skills in absence of a first stage impact on schooling. While the jump in literacy after an expansion reform persists over time similarly to the impact on years of schooling (compare Panel C and Panel A of Figure 4), numeracy shows a slightly steeper convergence between reforming and non-reforming countries (Panel B). Nonparametric estimates are nonetheless positive up to 12 cohorts from an expansion reform.

[19]It may be surprising that increasing teacher qualification requirements obtain a negative sign. However, reforming educational requirements to access the teaching profession is often associated with changes in the reward policies, including teaching load, working hours, and retirement conditions, which exert different impacts on the quality of teaching (as shown for primary school teachers in Braga et al., 2020). It is therefore possible that the present reform variable captures only some of these dimensions.

IV estimates of the cognitive skill returns to schooling

The event study estimates presented above provide the first stage or reduced form of an IV model that exploits educational reforms as a source of exogenous variation in schooling. We now combine these results into the corresponding IV estimates of the effects of schooling on cognitive skills.

Instrumental variable estimates rest on the assumption that the impact of educational reforms on cognitive skills is mediated exclusively by attainment, rather than other potential channels such as school quality. The exclusion restriction is most natural when considering expansion reforms, the aim of which is mainly to increase exposure to formal education. However, we present suggestive evidence that such an assumption is credible throughout the policy interventions we observe. Figure 5 plots reduced-form event-study estimates on cognitive skills as a function of first-stage estimates on years of schooling for each of the 18 reforms we observe (a "Visual IV" plot, Angrist et al., 2019). If the exclusion restriction holds, estimated reform impacts on skills should be null when the impact on years of schooling is null. This expectation is borne out by the solid line fitting plotted estimates, which runs remarkably close to the origin for both outcomes considered (numeracy score, in Panel A, and literacy score, in Panel B). Our evidence in support of the exclusion restriction is in line with findings by Brunello et al. (2013b).[20]

Most educational reforms provide a strong first stage to estimate the cognitive skill returns to schooling. When pooling all interventions, event study estimates of the impact on years of schooling exhibit a F-statistics well below the rule-of-thumb threshold of 10 (Panel A of Table 3). At the level of single reforms, Figure 5 shows that low first stage values are likely driven by a few reforms with a negative impact on schooling (marked with light colours in Figure 5).[21] Note that our IV strategy pools variation from 18 different exogenous shocks to schooling and, as long as a single reform weakly decreases schooling for all individuals, the monotonicity assumption is not violated. However, using reforms with a negative impact on

[20] They propose a test of the exclusion restriction that compulsory school reforms impact individual outcomes only through an increase in years of schooling. Their test is based on the idea that school quantity similarly impacts numeracy and literacy, while the impact of school quality differs for the two skill measures (supported in IALS data). Under this assumption, they show that the reduced form impacts of schooling reforms on numeracy and literacy are statistically undistinguishable, supporting the exclusion restriction.

[21] These are the use of standardised tests for career advancement, the increase in qualifications required to preprimary or secondary school teachers, and the expansion of university autonomy. Studying reasons and consequences of these effects is out of the scope of this paper.

schooling likely weakens our first stage, possibly leading to inflated IV estimates and weak instrument bias. For this reason, in what follows, we discuss results obtained ignoring the four interventions with a negative first stage impact.[22]

An additional year of schooling generates positive and large returns in terms of cognitive skills in adulthood. Table 6 reports 2SLS estimates of β_1 in equation (4). Considering all reforms with a positive impact on schooling, an additional year of schooling increases literacy score by 0.22σ and numeracy score by 0.27σ (column 1), and results are remarkably similar when controlling for individual characteristics (column 2). When we focus on policy areas with largest positive impacts on schooling (expansion reforms in columns 3-4, also pre-primary and school autonomy reforms in columns 5-6), results are remarkably stable, with estimated impacts of $0.18 - 0.25\sigma$ on literacy score and of $0.19 - 24\sigma$ on numeracy score. Estimated returns to schooling on numeracy and literacy have magnitudes in the range of previous findings on other cognitive skill outcomes.[23]

IV estimates by phase of education affected

Cognitive skill returns to schooling are driven by reforms expanding preschool and secondary education. Table 7 reports 2SLS estimates of β_1 in equation (4) separately by the phase of education affected. Results show that an additional year of schooling induced by pre-school 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 1-4). Policies affecting secondary education generate large and significant impacts of around 0.2σ (column 5-6). Finally, the estimated impacts of university reforms are highly imprecise due to a weak first stage (columns 7-8). These results suggest that preschool and secondary school years are the most decisive in terms of cognitive skill development, highlighting non-linearities in the impacts of schooling on cognitive skills. The effectiveness of early years is line with the

[22]We show results using all reforms in Table A.2. Results using all reforms (columns 1-2) are inflated by the low first stage. However, when considering policy areas with strongest first stage (columns 3-6), results are similar to our preferred specifications presented below.

[23]Focusing on 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. 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 (2017) find impacts on memory in the range of $0.3 - 0.5\sigma$. However, comparability of magnitudes with previous studies is limited by the uniqueness of the age range we consider.

large economic literature on early interventions and dynamic complementarities in human capital investments (e.g., Heckman and Mosso, 2014). Moreover, if labour market plays an important role in the development of cognitive skills, as argued in the next section, our results may indicate that the skills acquired during high school, 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 most productive in terms of a child's human capital (Carneiro et al., 2021).

We find further suggestive evidence of non-linearities in returns to schooling by considering the completion of a formal qualification. Figure 6 compares the estimated return to high school and university graduation to a linear prediction using the estimated return to an additional year of schooling. The linear prediction is obtained from Table 6 by considering that high school and university graduates complete on average about 5 more years of education relative to individuals with lower attainment.[24] As shown in Panel A, estimated returns to high school on numeracy and literacy are larger than proportional, although they do not significantly differ from the linear prediction. Returns to university degree are instead slightly lower than those to an average year of schooling, although again not statistically different. The result on high school graduation prompting larger than proportional returns is confirmed when focusing on pre-primary, school autonomy, and expansion reforms (Panel B) or on expansion reforms only (Panel C).

Overall, our results suggest that expansions of preschool and secondary education disproportionally impact cognitive skills. We discuss in Section 5 how the result on high school may be mediated by access to high-skill jobs.

IV estimates by sub-group

Cognitive skill returns to schooling are mostly homogeneous across individual characteristics. Estimates by sub-group are presented in Table 8. Column (1) reports full-sample estimates from Table 6, and subsequent columns restrict the sample based on individual characteristics. Following our previous analysis, Panel A considers all reforms, Panel B focuses on pre-primary, expansion, and school autonomy reforms, and Panel C considers expansion reforms only. Estimates by gender are very similar, especially when considering expansion reforms,

[24] Estimates of cognitive skill returns to formal qualifications are presented in Table A.3.

and we can never reject they are statistically equal (columns 2-3). Cognitive skill returns to schooling are also similar by immigration background, with slightly larger impacts on second generation immigrants throughout (columns 4-5), possibly suggesting that the latter have larger benefits from intervention expanding education. Estimates are also quite homogeneous by socio-economic status (columns 6-7), although we often find larger impacts on literacy score for high-SES individuals (but not in numeracy). Finally, we generally find stronger cognitive skill returns to schooling for relatively older individuals (above 45, columns 8-9). Weaker impacts on younger generations may be expected since educational level are substantially larger in more recent cohorts (Figure 2), and additional years of schooling may have lower returns. Moreover, education may limit cognitive decay in older cohorts, a channel that does not apply to younger individuals. This result, however, may also reflect lower statistical power on younger subjects since several reforms mostly affect older cohorts, especially expansion reforms (as shown by the low first stage F-test in Panel C, column 9). When adding other groups of reforms, however, the point estimate for adults younger than 45 is very similar to the one for older individuals (Panels A and B).

5 Schooling, labour market and the use of skills

The previous section uncovered positive and robust causal impacts of schooling on cognitive skills across a wide range of countries and among compliers to different types of reforms. A potential channel through which schooling may impact numeracy and literacy is an increase in the quantity and quality of labour market participation, and especially in the access to jobs with more intense skill contents. We turn here to analyse the rich PIAAC questionnaire on the use of skills. We present estimates of the impact of schooling on the use of skills and other labour market and training outcomes in Table 9. Effects of an additional year of schooling are presented in column (1) and contrasted with the impacts of high school and university graduation in columns (2) and (3), respectively. To maximise first stage, this table considers expansion reforms only.

Individuals shifted to acquire higher levels of education by institutional reforms are more likely employed and involved in training in adulthood (column 1, Panel A of Table 9). An additional year of schooling is estimated to increase the employment likelihood by 12 p.p., and to reduce the probability of being not in employment, education, or training (NEET) by 9 p.p. The likelihood of undertaking formal or informal training in the year of the survey increases by 4 p.p. Interestingly, job-related training does not increase, possibly suggesting that more educated workers need less on-the-job training (in line with findings by Ariga and Brunello, 2006). In addition, a further year of education is estimated to increase the likelihood of working as a professional or manager by 6 p.p., and the likelihood of working in a medium or large firm (>50 employees) by 8 p.p.[25] Although they should be interpreted with caution due to data limitations, we additionally present results on earning outcomes, showing that an additional year of schooling increases earnings by about 0.8 deciles.[26]

Additional years of schooling particularly increase the use of skills at work, with substantially lower impacts on the skill content of the home activity. Panels B and C show estimates on indices of the use of skills at home and at work, respectively.[27] Larger impacts are detected on the use of skills at work rather than at home in all considered dimensions (numeracy, reading, writing, and ICT skills).[28] For example, an additional year of schooling is estimated to increase the use of numeracy at work by 0.26σ, remarkably close to the estimated impact on numeracy score, while the use of numeracy at home increases by just 0.12σ. We also find large gaps in the increase of the use of ICT skills on the job relatively to the use at home, while gaps in the use of literacy skills are less dramatic. Results also suggest that schooling increases the use of noncognitive or organisational skills on the job, such as influencing skills or task discretion.

Individuals obtaining formal qualifications experience a disproportional increase in their labour market prospects and the use of skills. This can be seen by comparing column (1) of Table 9 with columns (2) and (3), and considering that graduates from both high school and university complete nearly 5 more years of schooling than individuals with lower attainment. To ease the comparison, Figure 7 plots impacts on selected outcomes from Table 9 along with

[25] Firm size is found to be an important determinant of labour market prospects (Arellano-Bover, 2022).

[26] We do not consider continuous measures of wage since these are unavailable in 6 out of 21 countries. All countries report within-country earning deciles, which are an ordinal measure and do not ideally suit our cross-country analysis.

[27] Use-of-skill indices are provided by PIAAC through a robust standardisation effort of single questionnaire items (see section 20.5 in OECD, 2013). We standardise all indices to have zero mean and unit variance in the working sample. Indices of use of skills at work are conditional on labour market participation.

[28] The use of skills in everyday life ("at home") and on the job measures engagement with activities requiring each type of skill. For example, the use of numeracy involves, e.g., the calculation of prices, costs or budgets, the use or calculation of fractions, decimals or percentages. See Table 2.5 in OECD (2019) for details.

the linear prediction. High school graduation is found to have the largest impact on the use of skills on the job and labour market outcomes. These results mirror the patterns unveiled for cognitive skill scores in Section 4.[29]

Overall, by relating schooling expansions induced by educational reforms to detailed survey measures on the use of skills, we find suggestive evidence that our main results are partly explained by the skill content of the current job. A potential narrative for our findings is 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. This interpretation is supported by the result that schooling particularly impacts the use of skills on the job rather than at home, providing evidence against the inverse causal chain, i.e., higher cognitive skills induced by education allow high-skill individuals to sort into skill-intensive jobs. However, since we observe a cross-section where the use of skills and the level of cognitive skills are elicited simultaneously, we cannot fully rule out the latter mechanism.

6 Conclusion

This paper presented the most general evidence to date on the causal effects of education on cognitive skills. Specifically, we use a broad set of educational reforms across a large number of European countries to isolate exogenous variation in completed years of schooling for the population of working age. We link reforms to internationally-standardised measures of literacy and numeracy and find that an additional year of schooling increases cognitive skills by $0.2 - 0.25\sigma$.

Our results imply that schooling impacts the development of cognitive skills in adulthood. Although the external validity of the impacts of XX-century educational reforms to the current context should be carefully debated, our findings offer at least suggestive evidence of large and persistent human capital costs in policies like school closures as long as remote learning is an imperfect substitute for in-person education. Moreover, we find suggestive evidence that the impact of schooling on cognitive outcomes is mediated by the skill content

[29] Our results complement the analysis by Gorman (2017) with unique evidence on the use of skills. Her findings that compulsory schooling decreases the likelihood of undertaking a routine occupation in an individual's first job are in line with our conclusions.

of jobs, highlighting the benefits of promoting employment in skill-intensive sectors. If the level of cognitive skills in the population is an interesting outcome *per se* – perhaps because of their benefits on non-monetary outcomes such as health and voluntary work – our results imply that policymakers should monitor and promote the development of skills in the non-working population, which may be at a higher risk of cognitive decay.

Our findings may help to disentangle the determinants of the impact of education on labour market outcomes such as earnings, which is crucial to assess the validity of different economic models. 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% and 60% of the wage returns to schooling. Following Carlsson et al. (2015), we draw from the literature an average estimate of the wage return to an additional year of schooling of 8%, and an average impact of an additional standard deviation of cognitive skills on earnings of 10-20%. Applying the latter estimates to the 0.25σ impact of schooling on cognitive skills in our preferred specifications, the gain in numeracy and literacy alone could be responsible for a 2.5-5% wage return to an additional year of schooling. Therefore, our estimates imply that an important part of the wage return to schooling can be attributed to the quality of human capital, in line with the conclusions by Carlsson et al. (2015). The residual impact of education on earnings could be driven by other factors such as improved networks or signaling of individual ability to employers. We find suggestive evidence of larger-than-average impacts of schooling on cognitive skills from completion of formal qualification, potentially implying that signaling to employers plays a role in shaping labour market experience and, in turn, the accumulation of cognitive skills.

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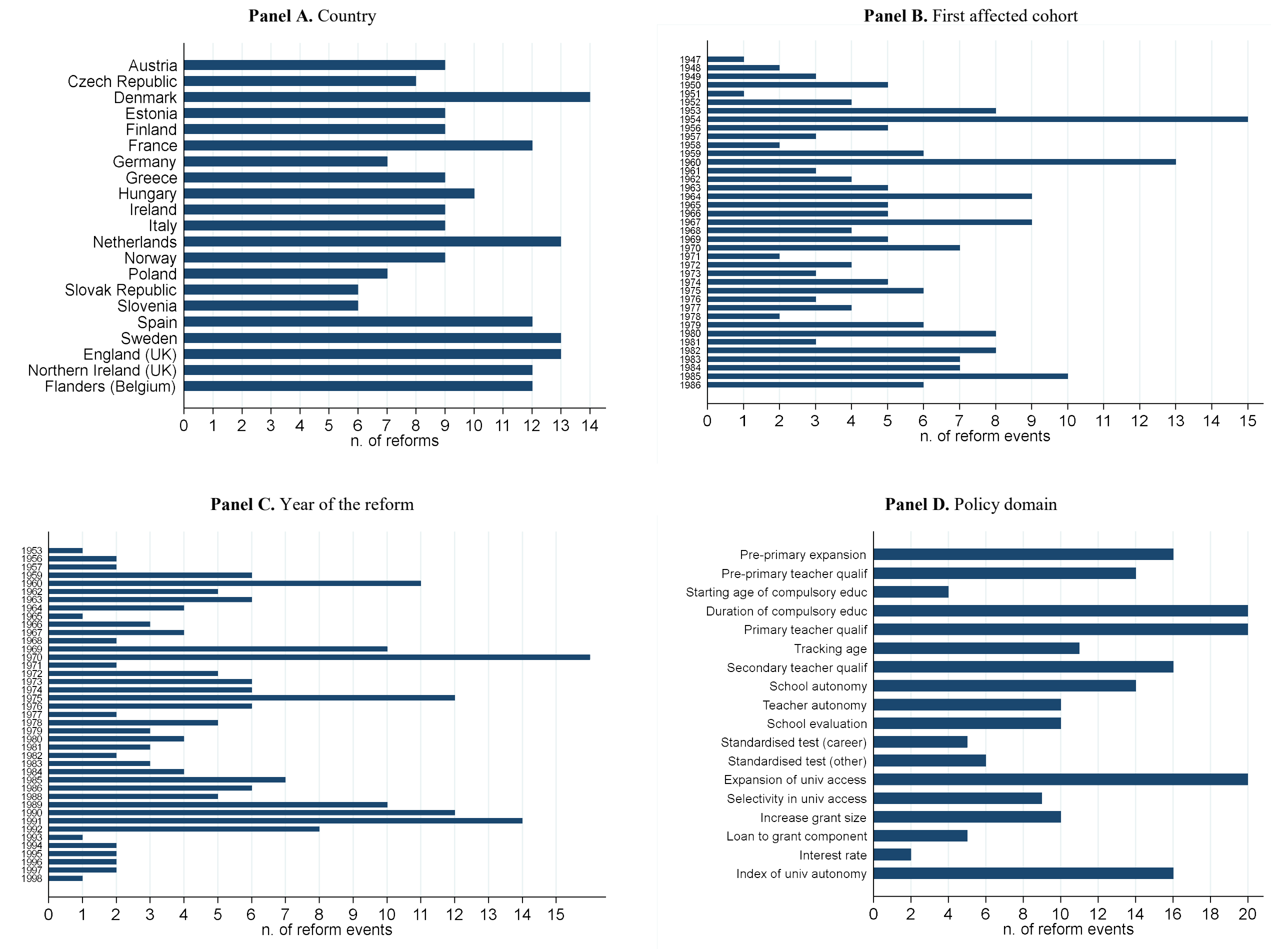
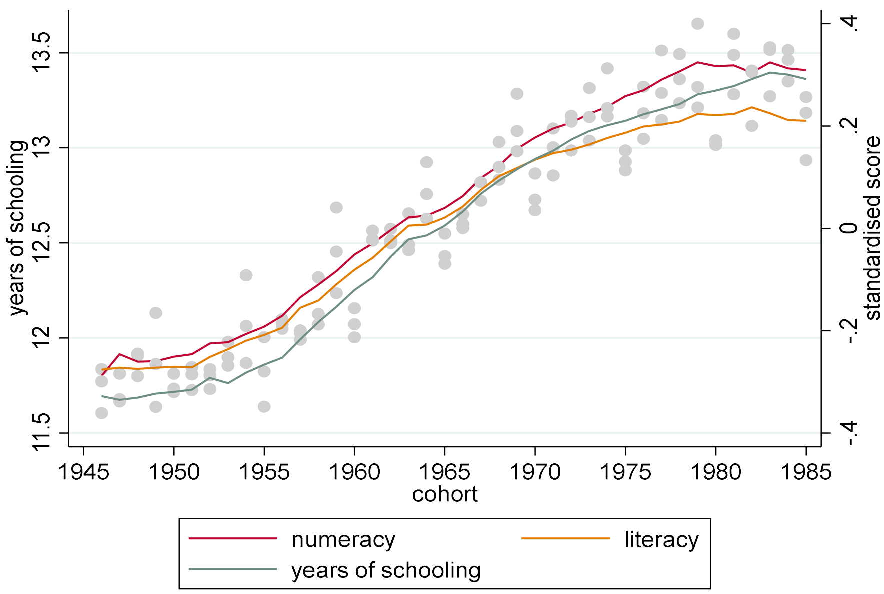
Figure 1: Reform events by country



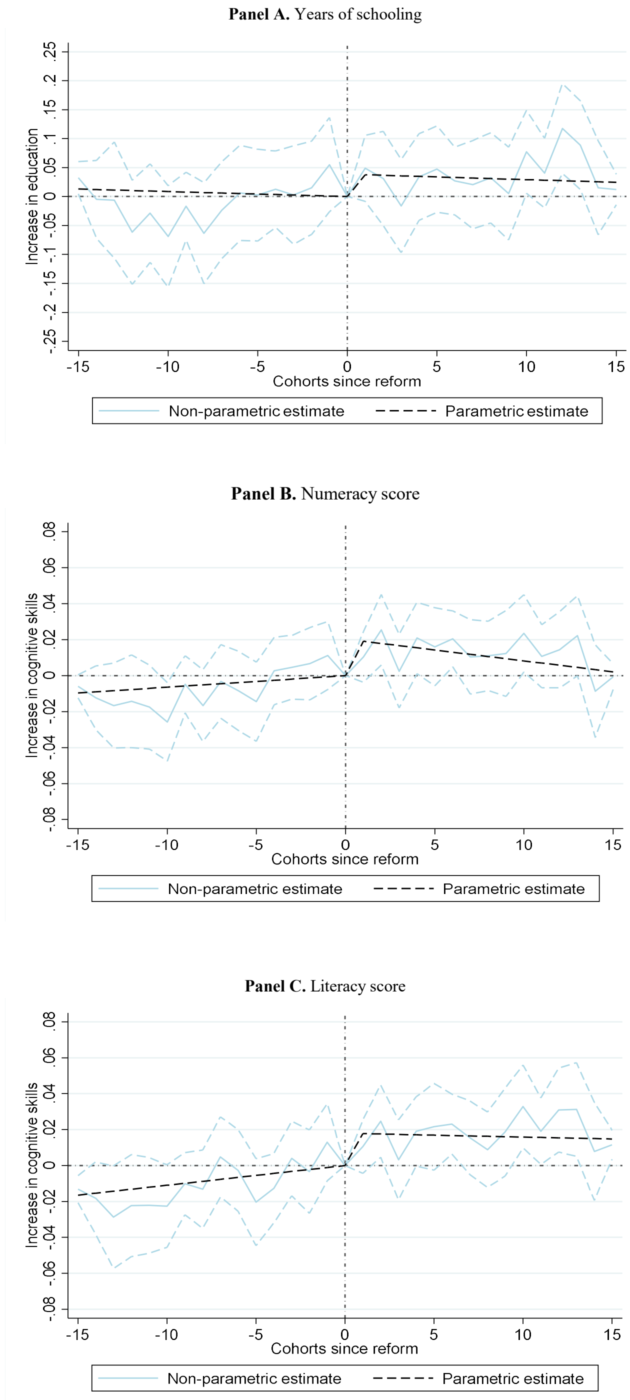
Note. The figure plots the number of reform events considered by country (Panel A), year of birth of the first-affected cohort (Panel B), year when the reform was passed (Panel C), or reformed policy domain (Panel D). We flag as reform event the earliest intervention in a given policy area in a given country. Considered reforms are those affecting individuals born from 1947 to 1986. See Section 2 for details.

Figure 2: Secular trends



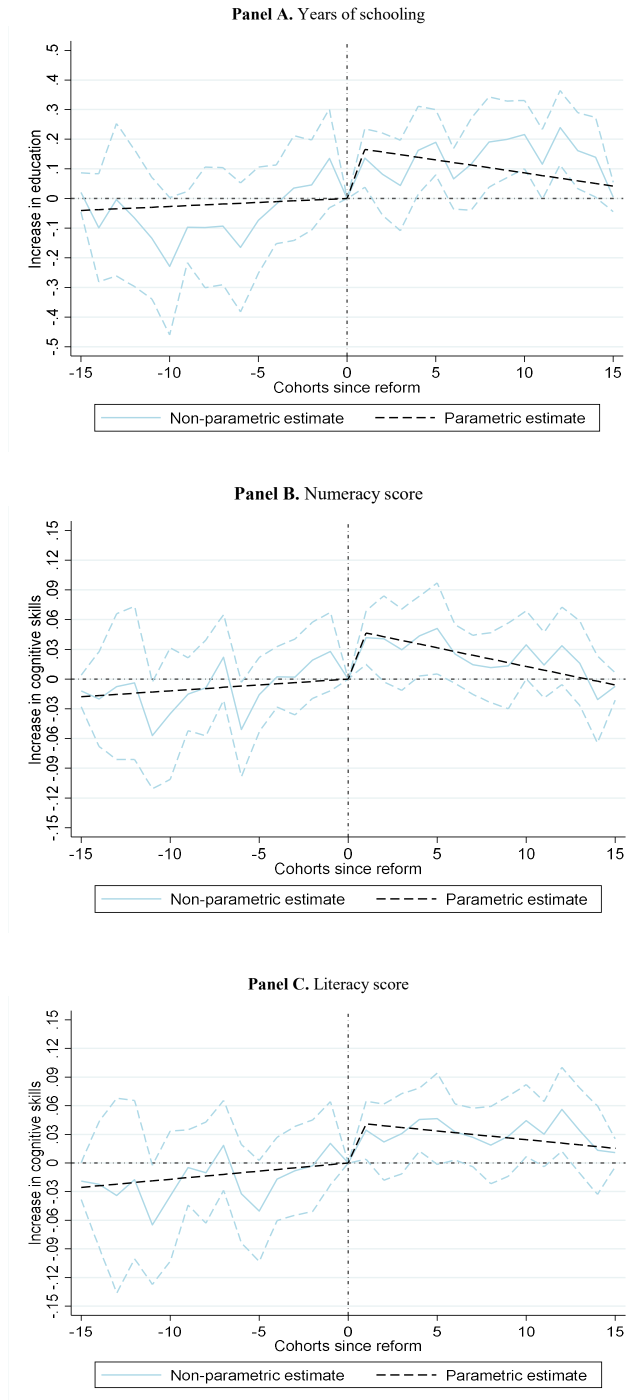
Note. The figure plots numeracy and literacy scores, and years of schooling, as a function of year of birth. Grey dots represent observed datapoints, while solid lines report 5-year moving averages of each variable. Years of schooling are derived by PIAAC from the highest level of education attained. Cognitive skills are directly measured by testing surveyed adults. Numeracy and literacy scores are obtained by averaging the 10 plausible values provided, and by standardising variables to have unit mean and zero variance in the sample. See Sections 3 and 4 for details.

Figure 3: Event study graphs: all reforms



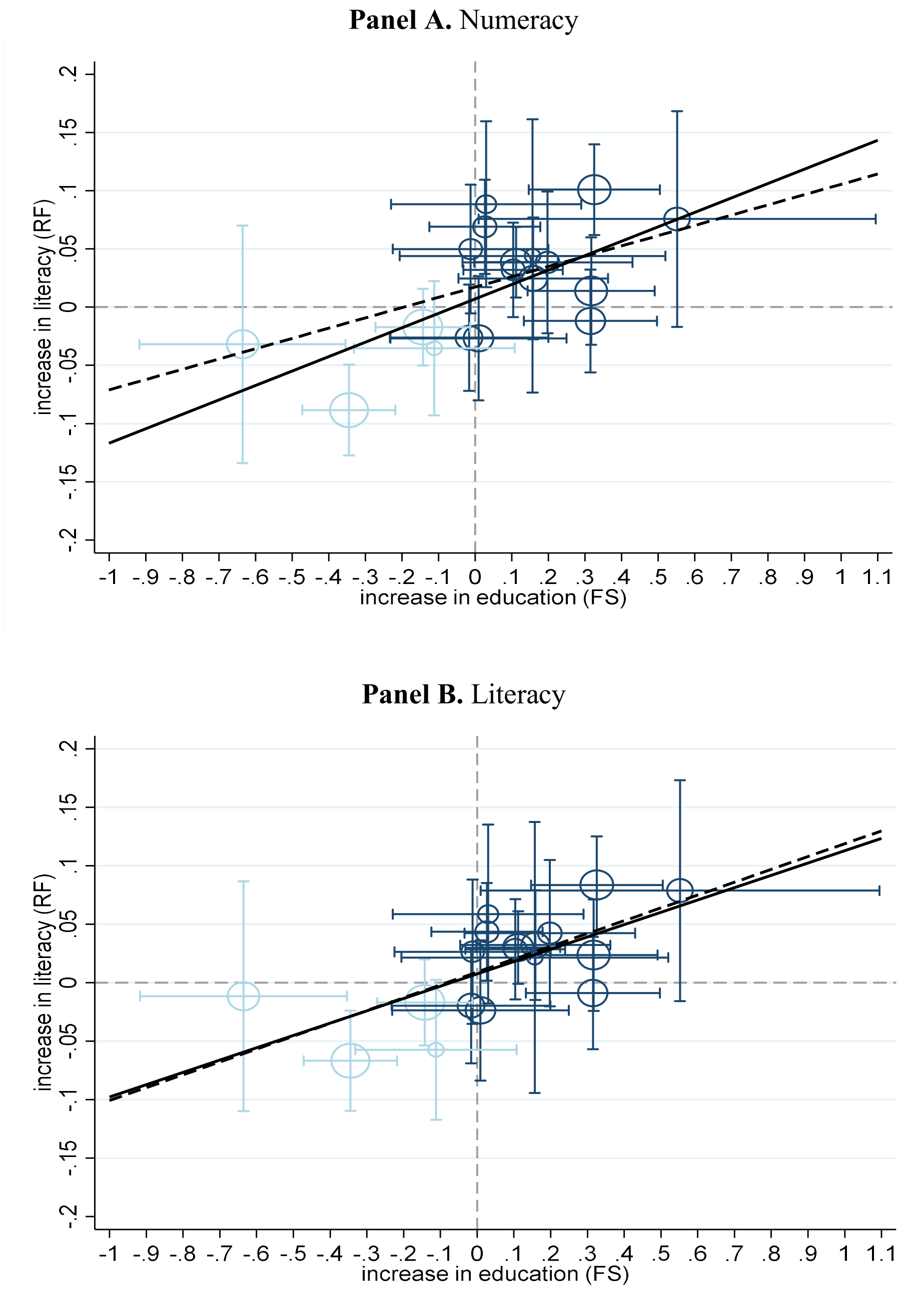
Note. The figure plots event study graphs for years of schooling (Panel A), numeracy score (Panel B), and literacy score (Panel C). Blue solid lines report relative-time-specific coefficients from equation (3), and dashed blue line plot the 95% confidence interval. Black dashed lines report estimates of predicted impacts from the parametric model in equation (2). Estimation is performed in a dataset stacking each treatment waves for all reforms, where for each wave individuals in non-reforming countries or in those reforming 15 years later or more are used as control. Relative time is normalised to zero in the latest period before a reform event, and the corresponding dummy variable is excluded from equation (3) forming 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. See Section 2 for details on reform events and Section 3 for details on estimation.

Figure 4: Event study graphs: expansion reforms



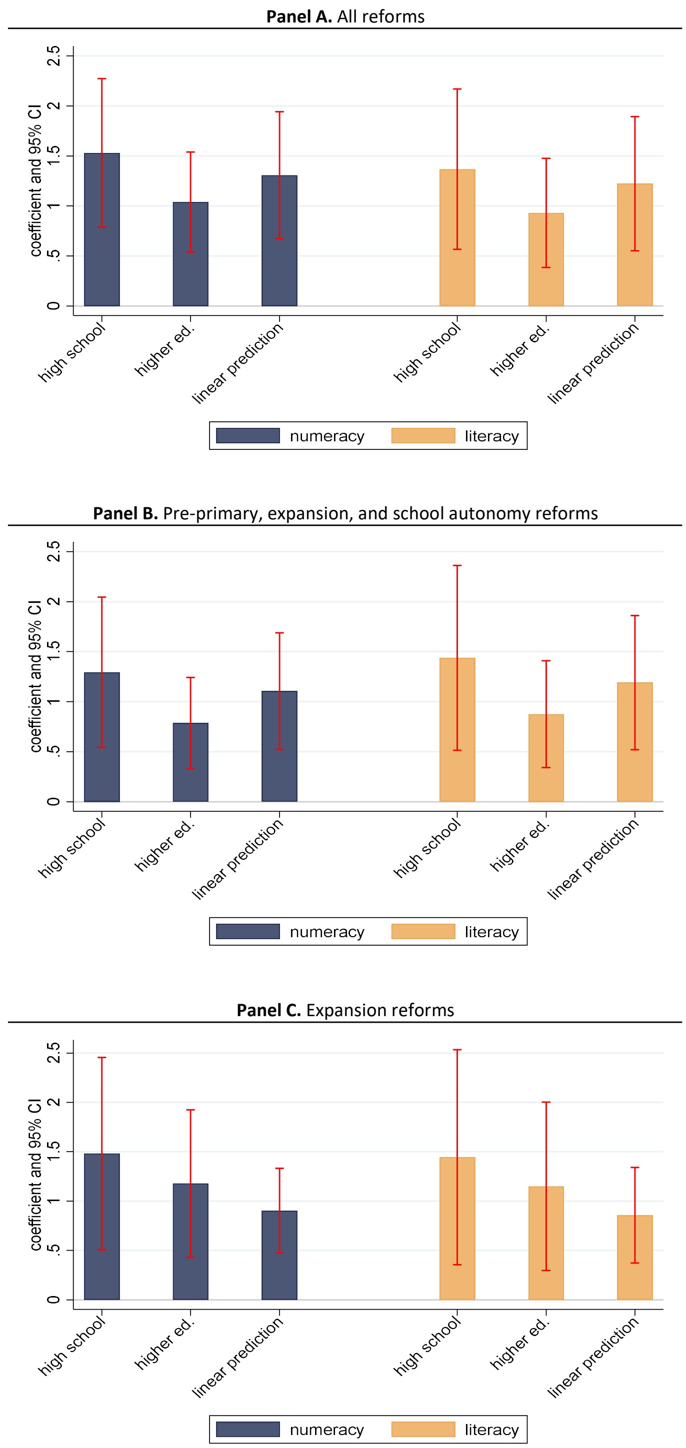
Note. The figure plots event study graphs for years of schooling (Panel A), numeracy score (Panel B), and literacy score (Panel C). Blue solid lines report relative-time-specific coefficients from equation (3), and dashed blue line plot the 95% confidence interval. Black dashed lines report estimates of predicted impacts from the parametric model in equation (2). Estimation is performed in a dataset stacking each treatment waves for expansion reforms only, where for each wave individuals in non-reforming countries or in those reforming 15 years later or more are used as control. Relative time is normalised to zero in the latest period before a reform event, and the corresponding dummy variable is excluded from equation (3) forming 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. See Section 2 for details on reform events and Section 3 for details on estimation.

Figure 5: Reform Visual IV for the cognitive skill returns to schooling



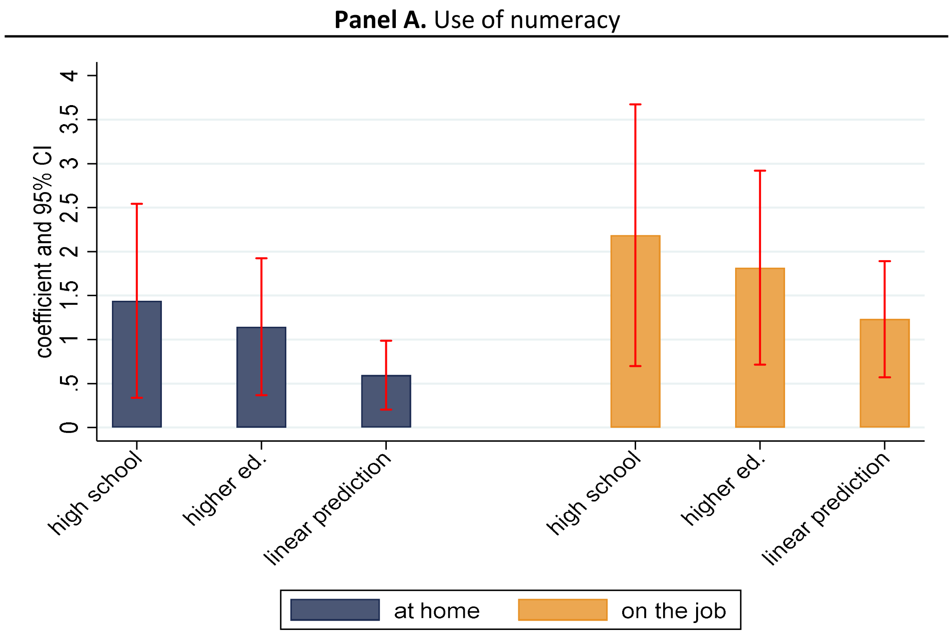
Note. The figure plots event-study estimates of reform impacts on cognitive skills (reduced form) as function of the corresponding impacts on years of schooling (first stage) by reformed policy domain. Markers represent separate estimates of reform exposure coefficients from equation (2) for each of the 18 interventions described in Section 2, weighted by the n. of observations. Light markers highlight reforms with a negative first stage, which are not used in IV estimation. Horizontal and vertical spikes delimit the 95% confidence interval of the first-stage and reduced-form estimates, respectively. Solid or dashed lines show the best fit through the 18 estimates, or through the 14 reforms with positive first stage, respectively. Panels A and B consider numeracy and literacy scores on the vertical axis, respectively, both standardised to have zero mean and unit variance in the working sample. See Section 3 and Section 4 for details.

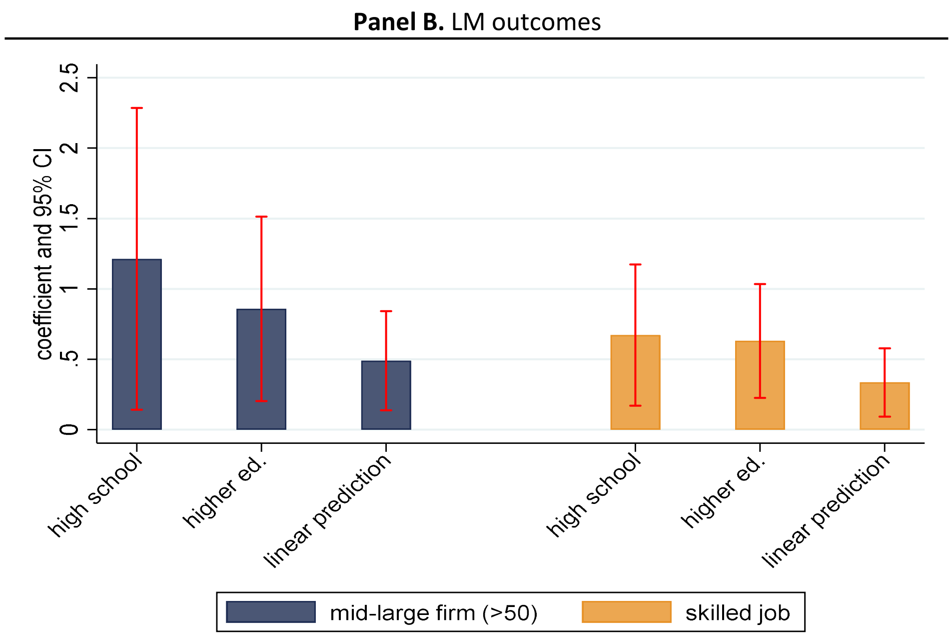
Figure 6: Non-linear impacts of schooling: cognitive skills



Note. The figure plots IV estimates of the cognitive skills return to schooling. It compares the estimated return to high school and university graduation (Table A.2) to the linear prediction obtained by using the estimated return to an additional year of schooling (Table 7). The latter is obtained by considering that individuals with and without graduation have similar gaps of almost five years of schooling for both high school and university. Panel A considers all reforms, Panel B restricts to pre-primary and school autonomy reforms, and Panel C considers expansion reforms only,. See Section 3 and Section 4 for details.

Figure 7: Non-linear impacts of schooling: use of skills and labour market





Note. The figure plots IV estimates of the labour market return to schooling. It compares the estimated return to high school and university graduation to the linear predictions obtained by using the estimated return to an additional year of schooling. The latter is obtained by considering that individuals with and without graduation have similar gaps of almost five years of schooling for both high school and university. Estimates are drawn from Table 9. Panel A plots estimates of the use of numeracy (standardised indexes are provided in PIAAC data). Panel B plots estimates of selected labour market outcomes. See Section 5 for details.

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 pre-school into schooling systems	Age 3
Education expansion	Starting age of compulsory education	Entry age into compulsory formal education	Age 6
	Duration of compulsory education	Number of years of compulsory school	Age 10
	Tracking age	Age at first tracking	Age 10
	Standardised 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
School teacher qualification	Pre-primary school teacher qualification	Increase educational requirement to be employed as a pre-primary, primary, or secondary school teacher	Age 3
	Primary school teacher qualification		Age 6
	Secondary school teacher qualification		Age 10
School autonomy and accountability	Standardised tests for other purposes	Presence of national standardized test for other purposes (e.g. measuring performance of schools)	Age 6
	School evaluation	Creation of structures for the steering and evaluation of its education system; carrying out of independent external inspections and evaluations; legislations strengthening the importance of school evaluation; measurement of school performance through the testing of samples of students	Age 6
	School autonomy	Reforms increasing autonomy in school management and decision-making processes	Age 6
	Teacher autonomy	Increase degree of autonomy for teacher in primary and secondary education	Age 6
University autonomy and selectivity	Selectivity in university access	Introduction of admission tests; introduction of national exam for entry to higher education; entrance to higher education based on candidates' marks at exit of secondary school	Age 15
	Index of university autonomy	Autonomy at tertiary level in the following dimensions: budget, recruitment, organization, logistic, courses organization, self-evaluation and development plans	Age 15
University financial support	Increase grant size	Increase financial support at tertiary level through grant	Age 15
	Loan-to-grant component	Dimension of the loan component to the grant component for financial support at tertiary level	Age 15
	Interest rate	Interest rate charged to loans for tertiary education	Age 15

Note. The table reports educational policies considered in our data, sourced from Table 2 in 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.

Table 2: Descriptive statistics

	N	Mean	SD	Min	Max
	(1)	(2)	(3)	(4)	(5)
Year of birth	84,345	1966.2470	11.6443	1946	1986
Age	84,345	45.33	11.64	25	65
Female	84,344	0.5227	0.4995	0	1
Both parents born in country	83,962	0.9137	0.2808	0	1
Mother with less than secondary education	82,248	0.5836	0.4930	0	1
Mother with secondary education	82,248	0.3162	0.4650	0	1
Mother with higher education degree	82,248	0.1003	0.3004	0	1
Father with less than secondary education	81,320	0.4823	0.4997	0	1
Father with secondary education	81,320	0.3736	0.4838	0	1
Father with higher education degree	81,320	0.1440	0.3511	0	1
<= 10 books at home	83,797	0.1421	0.3491	0	1
11-25 books at home	83,797	0.1546	0.3616	0	1
26-100 books at home	83,797	0.3209	0.4668	0	1
101-200 books at home	83,797	0.1725	0.3779	0	1
201-500 books at home	83,797	0.1368	0.3436	0	1
>500 books at home	83,797	0.0731	0.2602	0	1
High socioeconomic status	83,797	0.2494	0.4327	0	1
Years of schooling	84,345	12.75	3.13	3	21
Lower secondary graduation or higher	78,882	0.9508	0.2163	0	1
High school graduation or higher	78,882	0.6441	0.4788	0	1
Uncompleted university	78,882	0.1411	0.3482	0	1
University graduation or higher	78,882	0.3375	0.4729	0	1
Postgraduate education or higher	78,882	0.2355	0.4243	0	1

Note. The table reports descriptive statistics in our sample. Considered individuals are those aged at least 25 in 2011, the survey year, born in country, and with non-missing years of schooling and numeracy and literacy scores. Columns (1) to (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. See Section 2 for details.

Table 3: Event study estimates

	Before-after estimates	Event study estimates	
	(1)	(2)	(3)
		Panel A. Years of schooling ("first stage")	
Post reform	0.4904***	0.0386**	0.0498***
	(0.0507)	(0.0195)	(0.0184)
Trend		-0.0009	-0.0007
		(0.0007)	(0.0007)
Post reform X cohorts elapsed		-0.0001	-0.0016
		(0.0019)	(0.0018)
F-statistics: post reform		3.91	7.30
		Panel B. Numeracy ("reduced form")	
Post reform	0.1870***	0.0203***	0.0228***
	(0.0144)	(0.0049)	(0.0047)
Trend		0.0006***	0.0008***
		(0.0002)	(0.0002)
Post reform X cohorts elapsed		-0.0019***	-0.0023***
		(0.0005)	(0.0005)
F-statistics: post reform		17.45	23.40
		Panel C. Literacy ("reduced form")	
Post reform	0.2253***	0.0180***	0.0235***
	(0.0167)	(0.0053)	(0.0052)
Trend		0.0011***	0.0013***
		(0.0002)	(0.0002)
Post reform X cohorts elapsed		-0.0013**	-0.0021***
		(0.0006)	(0.0006)
F-statistics: post reform		11.55	20.74
N	5,514,167	5,514,167	5,237,769
N (individuals)	84345	84345	80066
Year, country, reform-by-wave FEs		Y	Y
Relative time FEs		Y	Y
Reforming activity controls		Y	Y
Control variables			Y

Note. The table shows estimates from parametric event study models in equation (2). Column (1) reports estimates from uncontrolled specifications including only the exposure dummy based on year of birth. Column (2) add country, year of birth, and reform-by-wave dummies. In addition, reform activity controls are included: the time-varying sum of reforms in other policy domains than the one considered, reforms in the same domain in previous years (1929-1953), age of start and end of compulsory education, age of tracking, duration of compulsory education, and the total n. of reforms in country. Control variables added in coulmn (3) are gender, a cubic polynomial in age, parental immigration, n. of books at home, father's and mother's educational attainment. Estimation is performed in a dataset stacking each treatment waves for all reforms, where for each wave individuals in non-reforming countries or in those reforming 15 years later or more are used as control. Reported F-statistics test the hypothesis of the exposure dummy being equal to zero. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 3 and Section 4 for details. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Reform impacts on attainment dummies

	All	Pre-primary	Expansion	Teaching	School autonomy	University autonomy	Financial support
	(1)	(2)	(3)	(5)	(4)	(6)	(7)
Lower secondary graduation or higher	-0.0033*	-0.0069	0.0093**	-0.0042	0.0001	0.0047	-0.0082
	(0.0019)	(0.0049)	(0.0040)	(0.0033)	(0.0056)	(0.0049)	(0.0057)
High school graduation or higher	0.0126***	0.0232***	0.0197***	0.0076	0.0202**	-0.0033	0.0027
	(0.0025)	(0.0088)	(0.0054)	(0.0057)	(0.0090)	(0.0073)	(0.0100)
Uncompleted university	-0.0049**	-0.0113	-0.0097***	0.0087**	-0.0254***	0.0059	-0.0063
	(0.0019)	(0.0071)	(0.0036)	(0.0041)	(0.0085)	(0.0048)	(0.0059)
University graduation or higher	0.0161***	0.0462***	0.0235***	0.0009	0.0386***	-0.0098	0.0115
	(0.0027)	(0.0106)	(0.0054)	(0.0057)	(0.0103)	(0.0077)	(0.0107)
Postgraduate education or higher	0.0071***	0.0315***	0.0072*	-0.0103**	0.0256***	-0.0006	0.0174*
	(0.0021)	(0.0084)	(0.0042)	(0.0049)	(0.0082)	(0.0054)	(0.0089)
N	4,918,957	639,701	1,255,016	980,676	1,276,676	292,202	474,686
N (individuals)	74995	74995	74995	74995	74995	56059	52847
Year, country, reform-by-wave FEs	Y	Y	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y	Y

Note. The table shows event study estimates of educational reforms impact on attainment dummies. Reported are estimates of the reform exposure dummy from parametric event study models in equation (2). Specification and estimation follow column (3) of Table 3. Column (1) considers all reform interventions, column (2) restricts the sample to pre-primary school reforms, column (3) to expansionary reforms, column (4) to teacher qualification reforms, column (5) and (6) to school and university autonomy reforms, respectively, and column (7) to reforms of financial support to higher-education students. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for details on reform events and Section 3 for details on estimation. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Reform impacts by policy area

	Pre-primary		Expansion		Teaching		School autonomy		University autonomy		Financial support	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
						Panel A. "First stage"						
Year of schooling	0.1501**	0.1556**	0.1744***	0.1799***	-0.1642***	-0.1268***	0.1318*	0.2055***	0.0652	0.0390	0.0003	-0.0007
	(0.0696)	(0.0670)	(0.0405)	(0.0382)	(0.0373)	(0.0350)	(0.0707)	(0.0654)	(0.0587)	(0.0547)	(0.0767)	(0.0722)
F-statistics (post reform)	4.65	5.39	18.59	22.20	19.38	13.13	3.48	9.89	1.24	0.51	0.00	0.00
						Panel B. "Reduced form"						
Numeracy	0.0349**	0.0438***	0.0503***	0.0449***	-0.0157*	-0.0108	-0.0192	-0.0047	0.0046	0.0015	0.0392**	0.0290*
	(0.0154)	(0.0150)	(0.0110)	(0.0099)	(0.0091)	(0.0083)	(0.0152)	(0.0161)	(0.0163)	(0.0142)	(0.0179)	(0.0157)
Literacy	0.0179	0.0390**	0.0427***	0.0433***	-0.0191*	-0.0145	-0.0157	0.0017	-0.0062	-0.0087	0.0310	0.0160
	(0.0157)	(0.0160)	(0.0114)	(0.0107)	(0.0101)	(0.0091)	(0.0167)	(0.0176)	(0.0178)	(0.0158)	(0.0213)	(0.0172)
N	757,828	709,916	1,387,581	1,324,983	1,126,435	1,071,035	1,421,180	1,363,130	315,126	292,839	506,017	475,866
N (individuals)	84345	80066	84345	80066	84345	80066	84345	80066	59526	56181	56148	52965
Year, country, reform-by-wave FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control variables		Y		Y		Y		Y		Y		Y

Note. The table shows event study estimates of educational reforms impact by policy domain. Reported are estimates of the reform exposure dummy from parametric event study models in equation (2). Specification and estimation follow columns (2)-(3) of Table 3. Columns (1)-(2) restrict the sample to pre-primary school reforms, columns (3)-(4) to expansionary reforms, columns (5)-(6) to teacher qualification reforms, columns (7)-(8) and (9)-(10) to school and university autonomy reforms, respectively, and columns (11)-(12) to reforms of financial support to higher-education students. Reported F-statistics test the hypothesis of the exposure dummy being equal to zero. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for details on reform events and Section 3 for details on estimation.*** p<0.01, ** p<0.05, * p<0.1

Table 6: Cognitive skill returns to schooling

	Return to an additional year of schooling					
	All reforms		Expansion reforms		Pre-primary, expansion, and school autonomy reforms	
	(1)	(2)	(3)	(4)	(5)	(6)
Numeracy	0.2783***	0.2734***	0.2304***	0.1889***	0.2429***	0.2316***
	(0.0679)	(0.0677)	(0.0500)	(0.0458)	(0.0660)	(0.0622)
Literacy	0.2161***	0.2558***	0.1963***	0.1791***	0.2128***	0.2491***
	(0.0669)	(0.0717)	(0.0540)	(0.0516)	(0.0698)	(0.0715)
First-stage F-test	19.87	22.18	29.33	33.23	17.02	21.77
N	4,259,535	4,045,161	1,090,790	1,043,145	3,269,798	3,116,191
N (individuals)	84345	80066	84345	80066	84345	80066
Year, country, reform-by-wave FEs	Y	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y	Y
Control variables		Y		Y		Y

Note. The table shows 2SLS estimates of the cognitive skill return to an additional year of schooling. Reported are estimates of the year of schooling coefficient in equation (4), instrumented by the reform exposure dummy. The sample is restricted to the 14 educational interventions with a positive impact on schooling. Estimated specifications follow columns (2)-(3) of Table 3. Columns (3)-(4) consider expansion reforms only, columns (5)-(6) adds pre-primary and school autonomy reforms. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for details on reform events and Section 3 for details on estimation. *** p<0.01, ** p<0.05, * p<0.1

Table 7: Cognitive skill returns to schooling by phase of education

	Return to an additional year of schooling							
	Pre-school reforms		Primary school reforms		Secondary school reforms		University reforms	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Numeracy	0.3099***	0.3401***	0.0167	0.0692	0.1895***	0.1816***	0.6502	0.9300
	(0.0842)	(0.1298)	(0.0939)	(0.0737)	(0.0596)	(0.0550)	(0.4629)	(1.0889)
Literacy	0.2569***	0.2903**	-0.0206	0.0763	0.1919***	0.1942***	0.3849	0.5111
	(0.0763)	(0.1200)	(0.1012)	(0.0787)	(0.0655)	(0.0618)	(0.3293)	(0.6747)
First-stage F-test	12.73	8.522	5.227	10.52	22.15	28	1.078	0.414
N	468,604	435,618	1,949,267	1,864,451	688,531	660,523	1,223,402	1,151,327
N (individuals)	84345	80066	84345	80066	84345	80066	1.078	0.414
Year, country, reform-by-wave 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

Note. The table shows 2SLS estimates of the cognitive skill return to an additional year of schooling by phase of education affected. Reported are estimates from specifications following Table 7. The sample is restricted to the 14 educational interventions with a positive impact on schooling. Columns (1)-(2) consider reforms affecting preschool, columns (3)-(4) consider reforms affecting primary education, columns (5)-(6) consider reforms affecting primary education, columns (7)-(8) consider reforms affecting higher education. Reforms are mapped into these groups following the last column of Table 1. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for details on reform events and Section 3 for details on estimation. *** p<0.01, ** p<0.05, * p<0.1

Table 8: Heterogeneous returns to schooling

	Return to an additional year of schooling								
	All	Gender		Both parents born in country		Socio-economic status		Age	
		Male	Female	No	Yes	Medium or Low	High	>45	<=45
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
				Panel A. All reforms					
Numeracy	0.2734***	0.3267***	0.2473***	0.3521***	0.2669***	0.2946***	0.2984***	0.4175***	0.3043***
	(0.0677)	(0.1022)	(0.0626)	(0.1279)	(0.0718)	(0.1015)	(0.0585)	(0.1259)	(0.1115)
Literacy	0.2558***	0.2419***	0.2719***	0.3034**	0.2496***	0.2805***	0.3205***	0.4775***	0.2879***
	(0.0717)	(0.0913)	(0.0726)	(0.1271)	(0.0759)	(0.1088)	(0.0610)	(0.1512)	(0.1041)
First-stage F-test	22.18	13.73	19.95	6.725	20.20	11.46	30.68	10.24	10.31
N	3,797,277	1,926,450	2,118,711	346,282	3,698,879	3,063,319	981,842	2,032,342	2,012,819
N (individuals)	74995	38237	41829	6663	73403	59793	20273	40247	39819
				Panel B. Pre-primary, expansion, and school autonomy reforms					
Numeracy	0.2316***	0.3120***	0.1879***	0.2959***	0.2283***	0.2416***	0.2517***	0.3493***	0.3729*
	(0.0622)	(0.1008)	(0.0556)	(0.1093)	(0.0673)	(0.0918)	(0.0587)	(0.1020)	(0.2200)
Literacy	0.2491***	0.2729***	0.2422***	0.2558**	0.2499***	0.2584**	0.3246***	0.3636***	0.5025*
	(0.0715)	(0.0966)	(0.0705)	(0.1079)	(0.0778)	(0.1076)	(0.0641)	(0.1152)	(0.2569)
First-stage F-test	21.77	12.75	19.28	6.570	19.75	10.81	29.11	13.16	4.169
N	3,116,191	1,479,563	1,636,628	271,417	2,844,774	2,351,614	764,577	1,555,244	1,560,947
N (individuals)	80066	38237	41829	6663	73403	59793	20273	40247	39819
				Panel C. Expansion reforms					
Numeracy	0.1889***	0.1725***	0.1998***	0.2586***	0.1856***	0.2021***	0.2890***	0.2908***	-0.2440
	(0.0458)	(0.0611)	(0.0507)	(0.0857)	(0.0485)	(0.0574)	(0.0884)	(0.0735)	(0.5673)
Literacy	0.1791***	0.1543**	0.1985***	0.2216**	0.1782***	0.1882***	0.3363***	0.3115***	-0.2988
	(0.0516)	(0.0616)	(0.0601)	(0.0912)	(0.0550)	(0.0634)	(0.0998)	(0.0867)	(0.6414)
First-stage F-test	33.23	22.63	24.93	10.91	29.85	24.60	12.71	23.15	0.560
N	1,043,145	491,969	551,176	92,153	950,992	799,650	243,495	528,170	514,975
N (individuals)	80066	38237	41829	6663	73403	59793	20273	40247	39819
Year, country, reform-by-wave FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y	Y	Y	Y

Note. The table shows 2SLS estimates of the cognitive skill return to an additional year of schooling separately for subgroups based on indiivdual characteristics. The sample is restricted to the 14 educational interventions with a positive impact on schooling. Specification and estimation follow columns (1)-(2) in Panel B of Table 7 (estimates are reported in column 1). The sample is restricted to expansion reforms only in Panel A, and includes also pre-primary and school autonomy reforms in Panel B. Estimation is restricted to male or female (columns 2 and 3, respctively), native individuals or those borne elsewhere (columns 4 and 5, respectively), or individuals with low or medium or with high socioeconomic status (SES) (columns 6 and 7, respectively), individuals aged more than 45 or younger (columns 8 and 9, respectively). 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 details on reform events and Section 3 for details on estimation. *** p<0.01, ** p<0.05, * p<0.1

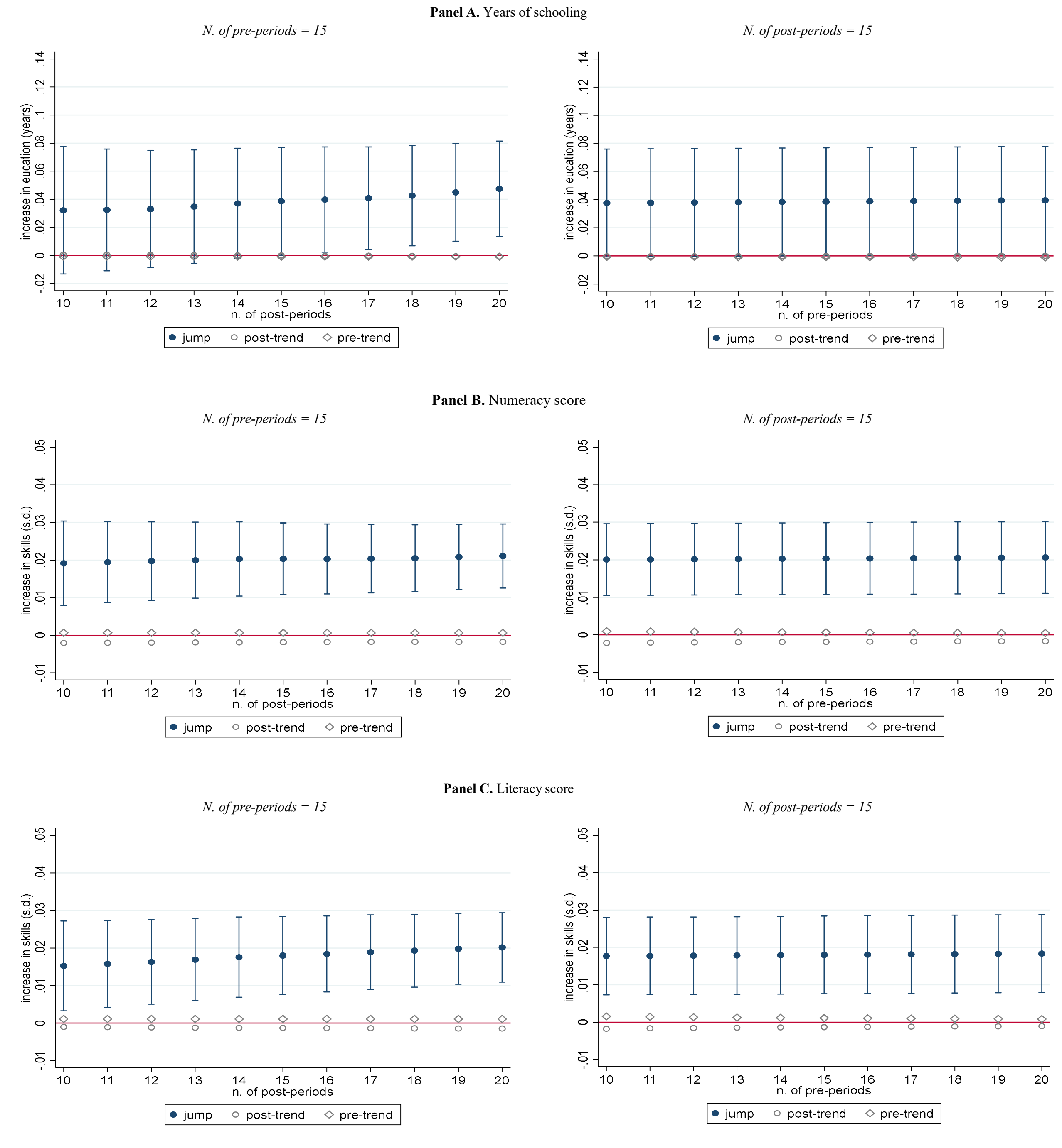
Table 9: Schooling and the use of skills

	Expansion reforms				
	Returns to an additional year of schooling	Returns to high school graduation	Returns to university graduation	N. of obs.	N. of individuals
	(1)	(2)	(3)	(4)	(5)
Panel A. Employment and training outcomes					
NEET	-0.0938***	-0.7873**	-0.6274**	1,042,459	80019
	(0.0297)	(0.3665)	(0.2680)		
Employed	0.1197***	0.9828**	0.7817**	1,043,145	80066
	(0.0353)	(0.4251)	(0.3053)		
Unemployed	-0.0156	-0.2027	-0.1612*	1,043,145	80066
	(0.0103)	(0.1244)	(0.0964)		
Training in the past 12 months	0.0428**	0.3236	0.2575	1,042,948	80058
	(0.0208)	(0.2389)	(0.1797)		
On-the-job training in the past 12 months	0.0096	0.0863	0.0687	1,043,041	80059
	(0.0111)	(0.1263)	(0.1003)		
Occupation as manager or professional	0.0594***	0.6544***	0.5767***	742,831	57761
	(0.0220)	(0.2536)	(0.1871)		
Medium-size firm (>50)	0.0843***	1.1531**	0.7339**	640,733	50009
	(0.0319)	(0.5606)	(0.2906)		
Large firm (>250)	0.0319	0.5685	0.3618	640,733	50009
	(0.0247)	(0.3903)	(0.2213)		
Monthly earning decile	0.8224***	8.5371***	7.1885***	656,420	52022
	(0.2038)	(2.5243)	(1.8774)		
Panel B. Use of skills at home					
Numeracy	0.1245***	1.4396**	1.1450***	1,205,380	72744
	(0.0419)	(0.5626)	(0.3969)		
Reading	0.1807***	2.3118***	1.8587***	1,029,345	79087
	(0.0416)	(0.6878)	(0.4569)		
Writing	0.1652***	1.8272***	1.4414***	911,368	71079
	(0.0505)	(0.6673)	(0.4592)		
ICT	0.1923**	2.3558*	1.4518**	775,853	62299
	(0.0889)	(1.3892)	(0.6398)		
Panel C. Use of skills at work					
Numeracy	0.2576***	2.1862***	1.8166***	643,823	51170
	(0.0705)	(0.7592)	(0.5629)		
Reading	0.2407***	2.4670***	1.9474***	739,491	58976
	(0.0556)	(0.8054)	(0.5197)		
Writing	0.2182***	1.9925***	1.6197***	854,772	53930
	(0.0692)	(0.6971)	(0.5100)		
ICT	0.3883**	54.9084	1.6048**	522,059	43787
	(0.1864)	(534.6791)	(0.7430)		
Influencing	0.1638***	1.7689**	1.3356***	699,449	56693
	(0.0565)	(0.7039)	(0.4738)		
Planning	0.1116**	1.2567**	0.9318**	712,159	57325
	(0.0509)	(0.6371)	(0.4345)		
Task discretion	0.1348**	1.7292**	1.4581**	720,800	56810
	(0.0615)	(0.7544)	(0.5977)		
Year, country, reform-by-wave FEs	Y	Y	Y		
Relative time FEs	Y	Y	Y		
Reforming activity controls	Y	Y	Y		
Control variables	Y	Y	Y		

Note. The table shows IV estimates of the effect of an additional year of schooling (column 1), of high school graduation (column 2), or of university graduation (column 3) on labour market outcome and the use of skills. The sample is restricted to expansion reforms. Column (1) reports coefficients on the reform exposure dummy from specifications similar to column (4) of Table 7. Similar specifications are employed in columns (2) and (3), where the endogenous variables are graduation outcomes rather than years of schooling. Outcome variables considered in Panel A are labour market outcomes, where variables other than employment outcomes are conditional on working at the time of the survey. Panels B and C consider indexes of the use of skills at home and on the job, respectively. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 5 for details. *** p<0.01, ** p<0.05, * p<0.1

Appendix

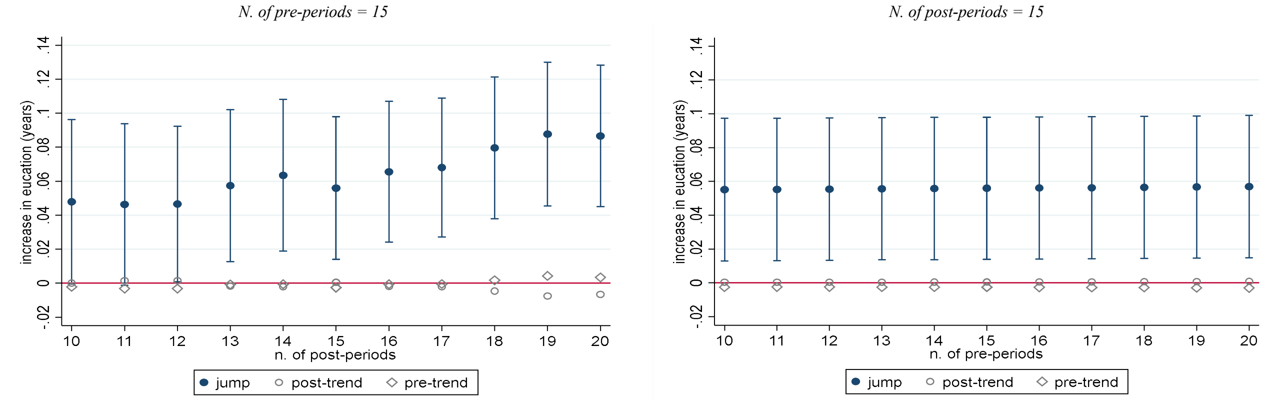
Figure A.1: Robustness to the choice of relative time window



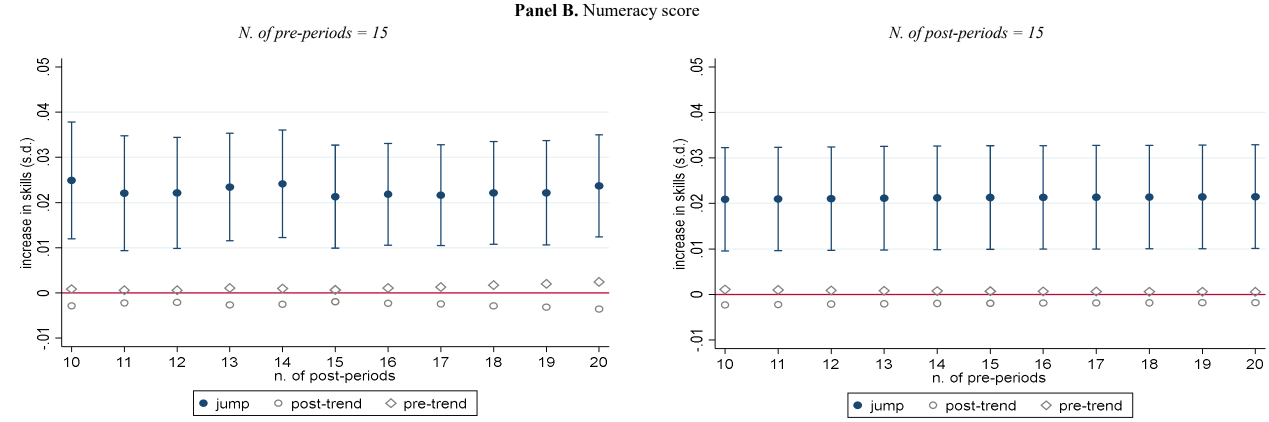
Note. The figure plots event study estimates of reforms' impact as function of the relative-time window considered. Estimates are from specifications similar to column (2) of Table 3. Panels A, B, and C consider years of schooling, standardised numeracy score, and standardised literacy score as dependent variables, respectively. In each panel, left-hand graphs show estimates obtained when considering 15 pre-periods as a function of the n. of post-periods considered. Right-hand graphs show estimates obtained when considering 15 post-periods as a function of the n. of pre-periods considered. In all graphs, extreme points bin all observations observed at earlier or later relative times. Blue markers and 95% confidence intervals report estimates of the reform exposure dummy. Grey circles and diamonds report estiamtes of linear trends post and pre-reforms, respectively. See Section 2 for details on reform events and Section 3 for details on estimation.

Figure A.2: Robustness to the choice of relative time window (hold composition fixed)

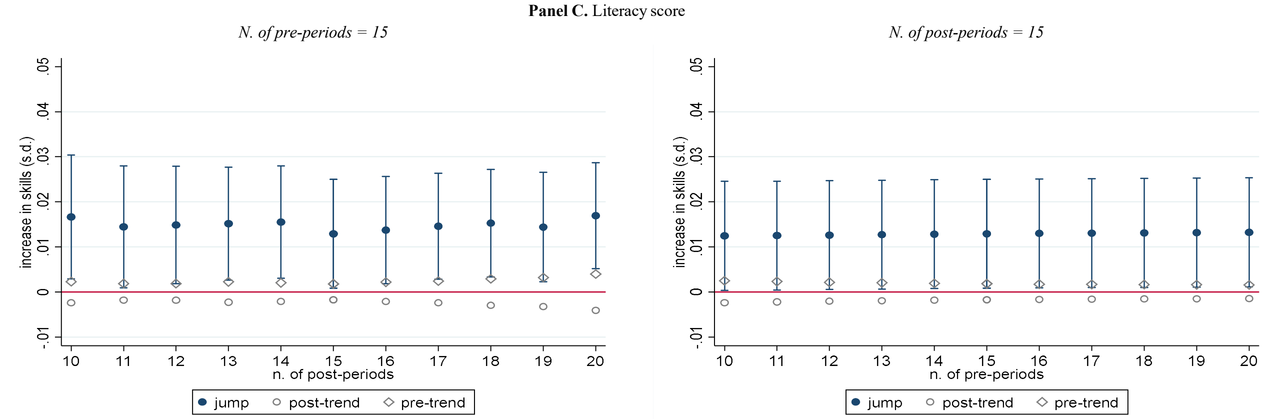
Panel A. Years of schooling



Panel B. Numeracy score

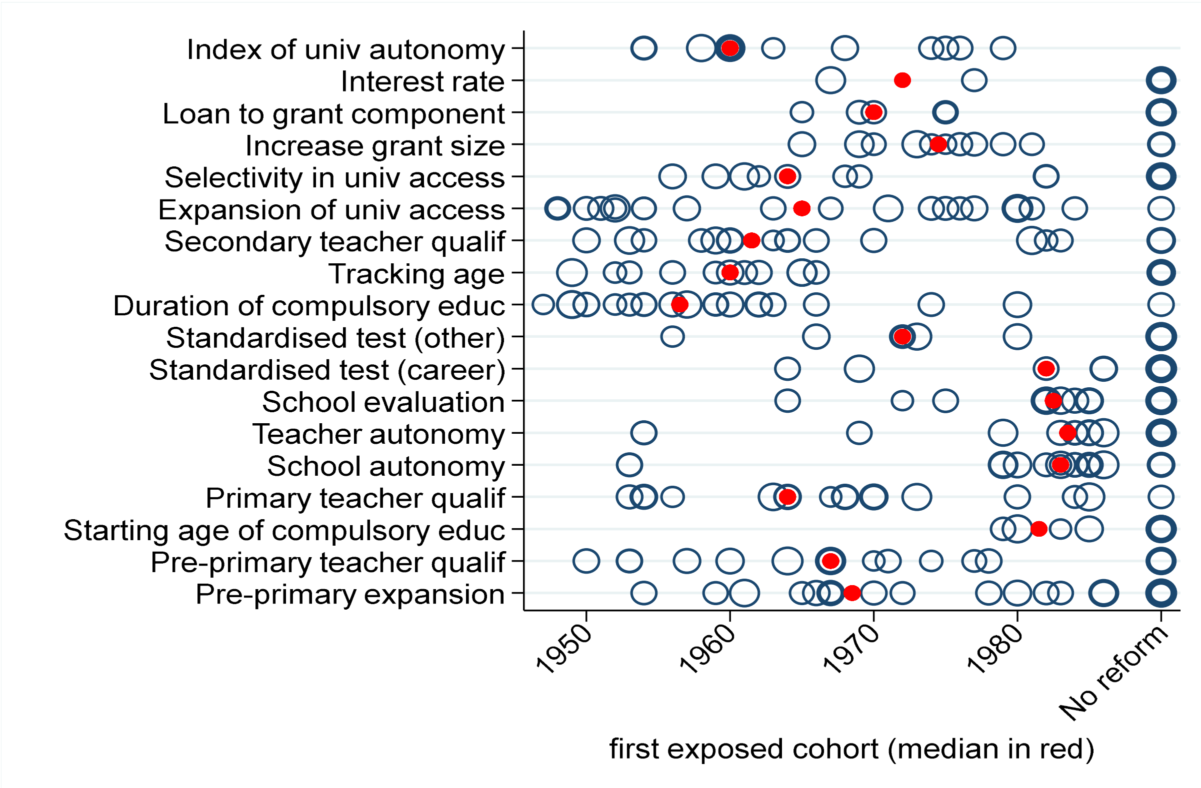


Panel C. Literacy score



Note. The figure plots event study estimates of reforms' impact as function of the relative-time window considered. The sample includes only treatement waves that are observed throughout the time window considered. Estimates are from specifications similar to column (2) of Table 3. Panels A, B, and C consider years of schooling, standardised numeracy score, and standardised literacy score as dependent variables, respectively. In each panel, left-hand graphs show estimates obtained when considering 15 pre-periods as a function of the n. of post-periods considered. Right-hand graphs show estimates obtained when considering 15 post-periods as a function of the n. of pre-periods considered. In all graphs, extreme points bin all observations observed at earlier or later relative times. Blue markers and 95% confidence intervals report estimates of the reform exposure dummy. Grey circles and diamonds report estiamtes of linear trends post and pre-reforms, respectively. See Section 2 for details on reform events and Section 3 for details on estimation.

Figure A.3: Reform timeline



Note. The figure plots first exposed cohorts by country and reform. Markers are weighted by the number of observations. Red markers plot the median earliest affected cohort among reforming countries. Non-reforming countries are plotted at the right hand of the horizontal axis. Reforms are sorted in descending order of phase of education affected. See Section 2 for details.

Table A.1: Joint significance of pre-reform coefficients

	All	Pre-primary	Expansion	Teaching	School autonomy	University autonomy	Student financing
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Year of schooling	0.2960	0.1393	0.3995	0.2797	0.1951	0.0441	0.0055
Numeracy	0.0132	0.0000	0.3125	0.1128	0.3582	0.6486	0.2852
Literacy	0.0000	0.0000	0.1397	0.0233	0.2101	0.7808	0.2209
N	5,514,167	757,828	1,387,581	1,126,435	1,421,180	315,126	506,017
N (individuals)	84345	84345	84345	84345	84345	59526	56148
Year, country, reform-by-wave FEs	Y	Y	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y	Y	Y

Note. The table shows p-values of F-statistics testing the hypothesis of pre-reform coefficients in equation (3) being jointly equal to zero. See Table 3 for estimation and specifcation details. Column (1) consider all reform interventions, column (2) restricts the sample to pre-primary school reforms, column (3) to expansionary reforms, column (4) to teacher qualification reforms, column (5) and (6) to school and university autonomy reforms, respectively, and column (7) to reforms of financial support to higher-education students. See Section 2 for details on educational reforms, and Section 3 for estimation details.

Table A.2: Cognitive skill returns to schooling: all reforms

	Return to an additional year of schooling					
	All reforms		Expansion reforms		Pre-primary, expansion, and school autonomy reforms	
	(1)	(2)	(3)	(4)	(5)	(6)
Numeracy	0.5270**	0.4586***	0.2884***	0.2496***	0.2708***	0.2615***
	(0.2548)	(0.1754)	(0.0712)	(0.0639)	(0.0826)	(0.0745)
Literacy	0.4664*	0.4727**	0.2446***	0.2405***	0.2411***	0.2863***
	(0.2487)	(0.1920)	(0.0722)	(0.0698)	(0.0861)	(0.0856)
First-stage F-test	3.910	7.297	18.59	22.20	12.32	17.30
N	5,514,167	5,237,769	1,387,581	1,324,983	3,566,589	3,398,029
N (individuals)	84345	80066	84345	80066	84345	80066
Year, country, reform-by-wave FEs	Y	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y	Y
Control variables		Y		Y		Y

Note. The table shows 2SLS estimates of the cognitive skill return to an additional year of schooling. Reported are estimates of the year of schooling coefficient in equation (4), instrumented by the reform exposure dummy. Estimated specifications follow columns (2)-(3) of Table 3.Columns (1)-(2) consider all reforms, columns (3)-(4) consider expansion reform only, columns (5)-(6) adds pre-primary and school autonomy reforms. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for details on reform events and Section 3 for details on estimation. *** p<0.01, ** p<0.05, * p<0.1

Table A.3: Cognitive skill returns to formal qualifications

	All reforms		Expansion reforms		Pre-primary, expansion, and school autonomy reforms	
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A. Returns to high school graduation					
Numeracy	1.5850***	1.5303***	1.8280***	1.4813***	1.3968***	1.2936***
	(0.3944)	(0.3791)	(0.5074)	(0.4969)	(0.4526)	(0.3831)
Literacy	1.1222***	1.3677***	1.6342***	1.4451***	1.1774**	1.4383***
	(0.3964)	(0.4094)	(0.5349)	(0.5553)	(0.4894)	(0.4716)
First-stage F-test	18.76	22.18	12.86	13.18	12.29	17.95
	Panel B. Returns to university graduation					
Numeracy	1.2504***	1.0399***	1.6418***	1.1783***	0.9391***	0.7870***
	(0.3040)	(0.2547)	(0.4417)	(0.3806)	(0.2906)	(0.2328)
Literacy	0.8853***	0.9294***	1.4677***	1.1494***	0.7917**	0.8751***
	(0.3153)	(0.2783)	(0.4751)	(0.4356)	(0.3158)	(0.2725)
First-stage F-test	31.79	39.68	17.06	21.26	29.20	43.27
N	3,992,549	3,797,277	1,037,233	993,412	3,047,423	2,909,789
N (individuals)	78882	74995	78882	74995	78882	74995
Year, country, reform-by-wave FEs	Y	Y	Y	Y	Y	Y
Relative time FEs	Y	Y	Y	Y	Y	Y
Reforming activity controls	Y	Y	Y	Y	Y	Y
Control variables		Y		Y		Y

Note. The table shows 2SLS estimates of the cognitive skill return to high school (Panel A) or university (Panel B) completion. Estimates are from specifications similar to Table 7, where the endogenous variable of interest is the attainment of formal educational qualification. Columns (3)-(4) consider expansion reforms only, columns (5)-(6) include also pre-primary and school autonomy reforms. The dependent variable is standardised numeracy or literacy score. Standard errors clustered on the 762 country-year cells and reported in parentheses. See Section 2 for details on reform events and Section 3 for details on estimation. *** p<0.01, ** p<0.05, * p<0.1

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