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Microeconometric Analyses Using Large-Scale Skills Assessments

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To my parents, Christine and Werner,
my sister Magdalena and my boyfriend Johannes.
I would not have achieved this without you.

Preface

Franziska Bernadette Hampf prepared this study while she was working at the ifo Center for the Economics of Education. The study was completed in September 2019 and accepted as doctoral thesis by the Department of Economics at the University of Munich. It consists of four distinct empirical essays investigating various aspects of the link between education, productive skills, and individual labor-market success, primarily using information from large-scale skills assessments. Chapter 2 and 4 employ – among others – instrumental variable estimations, while Chapter 3 uses a fixed effects framework. Furthermore, Chapter 5 applies a difference-in-differences design to answer the underlying research question.

Keywords:	Education, Cognitive Skills, Labor Market Outcomes, Reform Evaluation, Business Cycles, College Enrollment, Large-Scale Skill Assessment, College Wage Premium, Vocational Education.
JEL-No:	E32, I20, I21, I23, I26, J24

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Referent: Prof. Ludger Wößmann
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1 Introduction

1.1 Education as a Determinant of Skill Formation and Labor-Market Success

The economic literature emphasizes the importance of marketable skills as a major determinant of individual well-being and labor-market success, as well as of the prosperity of the entire economy (Hanushek and Woessmann 2008; Acemoglu and Autor 2011). One may think of skills as a set of acquired or innate competences that contribute to the individual's *productivity*, which is rewarded on the labor market, e.g., in the form of higher wages. While innate ability – per definition – is an initial endowment that cannot be influenced, individuals can choose to invest in the accumulation of skills to increase their labor-market prospects.

In two seminal contributions, Schultz (1961) and Becker (1962) formalized these investment decisions in a theoretical framework known as the *Human Capital Theory*. Thereby, human capital corresponds to any stock of skills or characteristics that contribute to the worker's productivity. While there are numerous ways to invest in human capital, the presumably most important determinant of skill formation during the early years of an individual's life is education. Nowadays, most industrialized countries have education systems with compulsory years of schooling to ensure a minimum amount of skill endowment for all students. However, students can choose to invest in education beyond these mandatory years, depending on the costs and benefits associated with schooling investments. Potential costs of education are, for instance, tuition fees for secondary schools or universities. Furthermore, indirect costs of education, e.g., in the form of forgone earnings, need to be considered. On the other hand, education increases productive skills and thus labor-market prospects. A higher level of education is associated with higher earnings and a lower unemployment risk (see Psacharopoulos and Patrinos 2004; Heckman, Stixrud and Urzua 2006; Montenegro and Patrinos 2014; Psacharopoulos and Patrinos 2018). In addition, more years of schooling are positively related to non-pecuniary aspects, such as health behavior or life expectancy (e.g., Cutler and Lleras-Muney 2006). However, the costs and benefits related to educational investments may differ considerably across individuals, depending on various aspects, such as ability differences or parental background. Furthermore, individuals may value future benefits of their educational investment differently.[1]

Due to skill-biased technological change, global competition for high-skilled workers is steadily increasing. More and more jobs with low skill requirements – such as manual and routine tasks – have become automated and labor markets are looking for employees who can cope

[1] This means that they discount future benefits of education at a higher or lower rate.

with abstract tasks and who are capable to adjust quickly to changing work environments. This development causes a rise in returns to education, which can be observed by a widened income gap between low and high educated workers (see Katz and Autor 1999; Goldin and Katz 2007). For instance, Autor (2014) documents a substantial increase in the wage premium associated with university education and cognitive ability, which contributes significantly to the overall growth in earnings inequality in the United States. By analyzing Census data, he shows that the economic payoff to university education rose continuously between 1979 and 2012 and almost doubled during this period. Similar increases of the earnings premium are found across many advanced economies. In the light of these developments, studying the link between education and cognitive skills becomes more important than ever. A deeper understanding of the potential extent to which education can affect the accumulation of skills is essential for the development of policy implications aiming at increasing the efficiency of education programs.

This thesis consists of four empirical essays investigating various aspects of the link between education, productive skills, and individual labor-market success, primarily using information from large-scale skills assessments. Each contribution corresponds to one chapter, is self-contained, and can be read independently. In what follows, Section 1.2 provides an outline of each chapter. Section 1.3 reviews the related literature and summarizes the contribution of my thesis. All chapters of the thesis are connected by the data used to answer the specific research question – the Programme for the International Assessment of Adult Competencies (PIAAC), which will be introduced in Section 1.4. Section 1.5 discusses the microeconometric methods applied to identify the causal effect of education on skills and labor-market success. Section 1.6 resumes potential policy implications that can be derived from my thesis.

1.2 Chapter Overview

Chapter 2 provides evidence on the effect of compulsory education on labor-market relevant cognitive skills in Germany, using high-quality skill data from the large-scale PIAAC and PIAAC-L assessments. For identification, I exploit exogenous variation in the length of schooling stemming from a reform which increased compulsory years of schooling from 8 to 9 years in Germany. The introduction of the compulsory ninth grade took place at different points in time across federal states between 1946 and 1969. Due to an additional agreement of all German federal states to harmonize the start of the school year to late summer, some states introduced two short school years simultaneously to the additional ninth grade. I account for the joint incidence of the compulsory schooling reform and the short school years in an instrumental variable framework.

Results show that the German compulsory schooling reform increased schooling for basic track students by 0.97 years, while the concurrent introduction of the two short school years decreased the total amount of schooling. Difference-in-differences analyses of the reform on skills suggest a significant and strong long-run reform effect on numeracy skills. Two-stage least squares estimates exploiting variation in length of schooling due to the reform verify the reduced-form results. One more year of schooling increases numeracy skills of basic track students by about 20 percent of a standard deviation and raises the rank position in the distribution of numeracy skills by 8 to 9 percentage points, which is substantially larger than the corresponding OLS returns to schooling. Thus, my results contradict previous findings of zero skill effects of compulsory schooling in Germany.

Chapter 3, which is joint work with Marc Piopiunik and Simon Wiederhold, investigates the impact of economic conditions on college investment decisions, human capital formation, and labor-market success.[2] In particular, we assess the impact of graduating from high school in a recession on investing in college, measured through attendance, dropout, and completion. We also trace the longer-run consequences of graduating from high school in a recession on the formation of cognitive skills and labor-market outcomes. To estimate this effect, we exploit business-cycle fluctuations over a 20-year period across 28 developed countries and information from the PIAAC Survey of Adult Skills. We identify the effect of economic conditions by exploiting variation in national unemployment rates across cohorts and countries.

We find that bad economic conditions at high-school graduation positively affect college investments. An increase in the unemployment rate by 1 percentage point increases college enrollment by about 0.8 pp. The impact of macroeconomic fluctuations at high-school graduation on obtaining a college degree is of a similar magnitude as for college enrollment. In contrast, college dropout seems unaffected by the labor-market conditions at high-school graduation. Economic conditions at high-school graduation also influence subsequent human-capital formation and labor-market outcomes. A 1 pp increase in unemployment at high-school graduation raises both literacy and numeracy skills by about 1% of a standard deviation and increases monthly wages by slightly less than 1%. Heterogeneity analyses suggest that the effect of economic fluctuations on college attendance (and attainment) is stronger for individuals with higher socio-economic background, as proxied by parental education. In addition, economic conditions at high-school graduation affect cognitive skills and labor-market outcomes more strongly for women than for men. Furthermore, we provide tentative evidence that individuals who decide to attend college because of bad economic conditions at high-school graduation continue to invest in learning activities after formal schooling. We find that all outcomes are affected the most by the macroeconomic conditions at high-school graduation, whereas the effect of macroeconomic conditions in earlier or later years is typically negligible in size and not statistically significant.

Chapter 4 studies the role of cognitive skills in a comparison of university wage premia across countries. This chapter is joint work with Guido Schwerdt and Simon Wiederhold.

[2] For expositional purposes, the term *university* and *college* are used interchangeably.

Using international data from the PIAAC Survey of Adult Skills, we estimate wage returns to university education and investigate to what extent higher skills contribute to higher wages of university graduates. To explore the mechanism that drives the contribution of skills to university wage premia, we conduct a series of additional analyses. In particular, we study the extent to which wage premia are driven by selectivity into university and in how far this accounts for international differences in university wage premia. Thereafter, we conduct international difference-in-differences estimations, where we compare the skills of individuals at university entry age and post-graduation age with and without university education. We complement our cross-country analysis with detailed micro-level evidence. Making use of expanded information on university attendance in Germany, we exploit arguably exogenous variation in university proximity to identify the effect of university education on skills.

We document that university wage premia vary substantially across the 32 countries in our sample, ranging from 20 percent in Sweden to 88 percent in Singapore. When conditioning on cognitive skills, university wage premia decrease on average by about one quarter. However, the degree to which skills contribute to university wage premia varies considerably across countries. Overall, controlling for skills reduces the international country-level variance in university wage premia by 16 percent. While part of the wage premium in each country captures differential selectivity into university, its extent cannot account for the observed cross-country differences. We provide evidence from international difference-in-differences estimations that skills increase more between cohorts at university entry age and cohorts at post-graduation age for university-educated individuals than for individuals without university education. We further show that these skill premia are positively related to university wage premia across countries, suggesting that part of the international differences in wage premia is driven by variation in the extent to which university education increases productive skills. Further evidence for a positive skill-effect of university education comes from complementary micro-level evidence for Germany, where we exploit variation in the probability to enroll in university due to the distance of a high-school graduate's home town to the nearest university campus in an instrumental variable model. Our instrumental variable estimates of the skill effect of university education are large and significant, corroborating the result from the international analysis that university education increases skills.

Chapter 5 estimates the employment effects of vocational vs. general education over the life-cycle on modern labor markets in a sample of 16 countries, using information from the international PIAAC Survey of Adult Skills. In doing so, we provide evidence on the argument that vocational education facilitates the school-to-work transition but reduces later adaptability to changing work environments. For identification, we apply a difference-in-differences model that compares employment rates across education type and age. This chapter is joint work with Ludger Wößmann.

Results confirm such a trade-off over the life-cycle – an initial employment advantage of individuals with vocational compared to general education turns into a disadvantage later in life. But there is strong heterogeneity depending on the specific institutional structure of schooling and work-based training in a country. While no significant pattern is detected in the six countries without sizeable vocational systems, the declining relative age-employment pattern of individuals with vocational education is found across the ten countries with significant vocational systems, and it is strongest in countries with widely developed apprenticeship systems where industry is directly involved in education.

1.3 Related Literature & Contribution of the Thesis

Returns to investments in education based on the human capital theory have been studied since the late 1950s. Thereby, evidence on the effects of educational investments primarily focus on an individual's labor-market success, such as wages or employment. In a recent study, Psacharopoulos and Patrinos (2018) comprehensively review and present comparable estimates of the wage returns to schooling for 139 economies, showing that returns to education are generally positive with a cross-country average rate of return to education of 9 to 10 percent per year of education. While these average returns are relatively stable across time, returns to university education have increased over the last decades and exceed returns to primary and secondary education, suggesting an increasing demand for high-skilled labor. By focusing on tertiary education, Chapter 4 presents the first analysis which investigates the role of cognitive skills in explaining international differences in returns to university education.

A growing number of studies attempts to estimate causal returns to education, addressing endogeneity concerns in simple Mincerian[3] earnings regressions (see e.g., Card 1999; Heckman, Lochner and Todd 2006). Numerous studies exploit institutional aspects of the education system to estimate returns to schooling. For instance, reforms of the compulsory schooling regime are often used as quasi-experimental variation in length of schooling due to their mandatory character (e.g., Angrist and Krueger 1991; Acemoglu and Angrist 2000; Oreopoulos 2006; Pischke and von Wachter 2008; Grenet 2013). However, evidence on the returns to compulsory schooling is quite mixed, which highlights the importance of the correct empirical model specification and the institutional context of the reform (Devereux and Hart 2010; Stephens and Yang 2014; Cygan-Rehm 2018). Chapter 2 contributes to this literature by providing evidence on the effect of compulsory schooling reforms on skill formation in Germany. More precisely, the chapter provides first evidence on the long-run effect of the reform on explicitly tested, high-quality measures of labor-market relevant skills, thereby challenging previous findings by Pischke and von Wachter (2008) and Kamhöfer and Schmitz (2015), who argue that the introduction of the compulsory ninth grade in Germany was unable to raise labor-market relevant skills.

[3] The typical Mincer earnings regression is a model that explains wage income as a function of schooling and experience and is presumably one of the most widely used models in empirical economics (Mincer, 1974).

Institutional features of the education system are also frequently used to identify returns to higher levels of education. For example, Card (1993) exploits variation in university proximity in the United States in an instrumental variable analysis and finds an increase in earnings for each year of tertiary education of 10–14 percent. Angrist and Chen (2011) use the so called "GI Bill" combined with the introduction of a draft lottery for conscription to the Vietnam war in 1969 to estimate returns to tertiary education in the United States. This policy induced some cohorts of young men to obtain more education than others by providing financial and institutional support for Vietnam war veterans who attended post-secondary institutions. Randomly drafted veterans who attended university due to the GI Bill experienced earnings increases of approximately 9 percent for each additional year of education. Zimmerman (2014) applies a regression discontinuity design to estimate returns to university education for students at the margin of going to university, using rich data from the Florida State University System. Wage returns to one year at a university for these marginal students is 8.7 percent. Overall, most studies exploiting quasi-experimental variation in college education find wage returns that exceed simple OLS estimates of returns to a year of college education. Chapter 3 and 4 focus on the link between tertiary education investments, skills, and labor market success. In doing so, Chapter 3 investigates the role of economic conditions for the probability to attend college and estimates the contribution of recession-induced college education to skill formation and labor-market success. Chapter 4 presents two approaches to investigate the causal effect of college education. The findings of both analyses are consistent with the idea that university education raises labor-market relevant skills. Differences across countries in the ability of universities to raise such skills partly explain differences in university wage premia.

Returns to schooling do not only vary across different levels of education – primary, secondary, tertiary – but may also differ across different educational program types. An extensive literature analyzes the effect of vocational education on the school-to-work transition, with varying results (Shavit and Müller 1998; Ryan 2001; Zimmermann et al. 2013; Malamud and Pop-Eleches 2010). Using the International Adult Literacy Survey (IALS) of the mid-1990s, Hanushek et al. (2017*a*) extend the perspective beyond the entry phase of the labor market, showing that the relative labor-market advantage of vocational over general education decreases with age. Several recent country-specific studies that go beyond the entry phase similarly show consistent age patterns by education type (e.g., Cörvers et al. 2011; Weber 2014; Brunello and Rocco 2017; Golsteyn and Stenberg 2017). Chapter 5 confirms a strong trade-off between early advantages and later disadvantages in employment for individuals with vocational education and highlights the strong heterogeneity in trade-offs depending on the specific institutional structure of schooling and work-based training in a country.

Existing evidence on the effect of education focuses on wages or employment prospects, partly because it is the most easily observable outcome of human capital investments. In fact, in a hypothetical setting of markets under perfect competition, where the productivity of a worker is perfectly observed by the employer and rewarded by his or her marginal productivity, wages would be a good proxy for an individual's productivity.[4] However, in a real-life scenario employers cannot perfectly observe the true productivity of employees and often need to rely on some signaling value of educational attainment. Furthermore, labor market frictions may affect the extent to which skills are rewarded by means of higher wages (Hanushek et al., 2015). Thus, when seeking for a deeper understanding of the efficency of education, one may prefer a more direct measure of productivity, such as skills. However, evidence on the effect of education on skills is still rather scarce. For the US, Cascio and Lewis (2006) exploit variation in the length of schooling stemming from birthdays near the school-entry cutoff dates and find that an additional year of education positively affects AFQT scores of minorities. Carlsson et al. (2015) exploit random variation in assigned dates of military eligibility tests in Sweden and find that ten days more schooling increases crystallized intelligence test scores of men by approximately 1 percent of a standard deviation. When examining the 1947 reform of compulsory schooling in the United Kingdom, Banks and Mazzonna (2012) find a large and significant effect of the reform on men's memory and executive functioning. Similar positive effects of compulsory schooling on skills tests of older cohorts are found in other countries as well (e.g., Schneeweis, Skirbekk and Winter-Ebmer 2014).

Although many previous studies use competence measures such as IQ, there is an important distinction between IQ and (productive) skills regarding the malleability of these competence measures by means of education. Heckman, Stixrud and Urzua (2006) analyze the effect of early childhood interventions in the United States and show that these programs did not boost IQ but raised achievement test scores, schooling and social skills. This evidence is consistent with the interpretation that measures of fluid intelligence (IQ) are unaffected by education, while schooling can indeed raise performance on tests measuring learned knowledge and competences. While IQ can be affected by environmental interventions only up to age 8-10 and is rather stable afterwards (Cunha et al., 2006), achievement test scores can be affected by schooling inputs and are malleable over a much greater range of ages. Thus, Chapter 2 complements existing evidence on the skill effect of education by providing evidence on the long-run effect of schooling on explicitly tested, high-quality measures of labor-market relevant skills. Chapter 3 argues that recession-induced college education significantly increases skills as well as labor-market outcomes. Furthermore, the chapter points to potential other channels that may affect skill formation induced by macroeconomic fluctuations around the age of college decision-making. Overall, it is the first study that provides a comprehensive assessment of the impact of economic conditions at high-school graduation on both, the short-run college enrollment decisions as well as the longer-run human capital and labor-market consequences, showing that business cycle fluctuations at

[4] The *marginal productivity theory* of wages was developed i.a., by Clark, Walras, Barone, Ricardo, and Marshall. The marginal product refers to the increase in output when adding one more unit of factor of production (e.g. labor) while keeping the other factors constant. The increase in output with the addition of one unit of factors of production is known as marginal productivity. See e.g., Mazumdar (1959) or Hamermesh (2011) for a review of the theory.

sensitive points during an individual's life may be an important determinant of human capital formation.

1.4 Data

One core feature common to all chapters of this thesis is the use of new and consistent international data on labor-market relevant cognitive skills of the adult population: the *PIAAC Survey of Adult Skills*, a cross-sectional survey conducted by the Organisation of Economic Co-Operation and Development in 2012 and 2015 (OECD, 2016).[5] This survey was designed to provide internationally comparable measures of the cognitive and workplace skills possessed by adults aged 16 to 65 years. In each of the 33 participating countries, a representative sample of at least 5,000 adults participated in the PIAAC assessment.

PIAAC provides measures of cognitive skills in three domains: numeracy, literacy, and ICT (problem-solving in technology-rich environments). Each skill domain is measured on a 500-point scale. By definition of the OECD (2016), numeracy skills measure the ability to access, use, interpret, and communicate mathematical information and ideas in order to engage in and manage the mathematical demands of a range of situations in adult life. Literacy skills are defined as the ability to understand, evaluate, use, and engage with written texts to participate in society, to achieve one's goals, and to develop one's knowledge and potential. Along with information on cognitive skills, PIAAC provides information on the respondents' education, labor-market status and demographics from an internationally harmonized background questionnaire.

PIAAC made use of leading international expertise to develop valid comparisons of skills across countries and cultures. Hence, these data are superior to previous surveys in various dimensions. Many international data on individuals' competences do not include objectively measured skills alongside with information about earnings and educational background in a comparable fashion. Hence, existing literature focusing on cognitive skills often relies on self-reported measures of skills or proxies thereof, such as skill-use (see Falck, Heimisch and Wiederhold 2016). Especially when survey participants are asked to report their skills themselves, severe measurement issues may arise. Moreover, self-reported skill measures would also suffer from cross-country differences in answering behavior. Using objectively measured, internationally comparable PIAAC scores substantially reduces the problem of measurement error.

Besides the original PIAAC survey, analyses in Chapter 2 benefit from a particularity of the German PIAAC Survey: PIAAC 2012 participants as well as their household members were interviewed in three additional waves (*PIAAC-Longitudinal*, PIAAC-L) in 2014, 2015, and 2016 (GESIS, DIW and LIfBi, 2017). The additional information provided in the re-survey waves –

[5] 24 countries participated in the first round of PIAAC in 2012. In 2015, 9 more countries joined the sample.

information on the federal state of residence when the individual finished secondary education as well as the year of graduation – enables the allocation of individuals to the respective compulsory schooling regime present when the individual went to school. In addition, numeracy and literacy skills of all PIAAC-L participants were (re-)assessed in 2015, which enlarges the sample of individuals with available skill measures. Information from PIAAC-L is also used in Chapter 4 when employing an instrumental variable approach to estimate the skill effect of college education exploiting variation in college proximity in Germany. We gained exclusive access to information on the exact location of residence of PIAAC-L respondents on the municipality-level to calculate the distance between an individual's hometown and the nearest college campus.

Part of the main analysis in Chapter 2 is replicated using the Adult Cohort of the *National Education Panel Study, NEPS*, which was designed to provide a better understanding of adult education and lifelong learning in Germany. Besides an extensive background questionnaire, the data comprise information on educational and professional careers for individuals aged 30 to 73 years (as of 2017). Beside extensive information on education and working careers of individuals, the data also include skill measures in various domains, such as reading, mathematics, sciences, and ICT literacy. From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS has been carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network (Blossfeld, 2011).

Chapter 3 additionally uses information from the OECD's *Annual Business Cycle Indicators*. More precisely, annual country-specific unemployment rates are matched to the international PIAAC data based on respondents' age of high-school graduation. Countries considered in the analysis in Chapter 5 were categorized by the extent and intensity of vocationalization of the education system, based on information from OECD's *Education at a Glance*.

1.5 Empirical Methods

When studying the role of education as a determinant of skill formation and labor-market success, economists usually aim at identifying a *causal* link, that is, the extent to which a person has higher skills or higher earnings *because* of higher educational attainment (Angrist and Pischke, 2009). However, several sources of endogeneity may yield biased estimates of the schooling effect in simple Mincer-type regressions (Mincer, 1974). In most such estimations, the resulting coefficient of schooling will be positive, i.e., individuals who invest more in education have higher skills. However, no causal interpretation of the coefficient is possible, since education is not randomly assigned but rather the outcome of each individual's choice. Unobserved factors may cause some individuals to obtain higher levels of education, but these factors may – at the same time – raise skills independently or by means of other channels than schooling. A frequently given example of unobserved characteristics is innate ability. No observable variables can sufficiently control for ability differences that might lead a group of individuals to select themselves into more education. However, even in the absence of more schooling, these individuals would most likely outperform those with less years of schooling. Hence, the OLS coefficient of length of schooling would be overestimated. Indeed, Chapter 4 provides evidence of substantial skill gaps between university students and non-students already at university entry age, indicating skill-based selection into university. In contrast, discount rate bias, arising from individuals with higher discount rates choosing less education, may result in an underestimation of the true skill effect of education if individuals with high discount rates benefited most from schooling (Card, 1994). In a similar fashion, many other factors may cause an over- or underestimation of the true effect of schooling.

In the four contributions of my thesis, various empirical methods are employed to address endogeneity issues arising in simple least square estimations. In Chapter 2, an instrumental variable (IV) approach exploiting exogenous variation in length of education due to an increase in compulsory years of schooling is used to identify the causal effect of education on skills. Since the reform was implemented at different points in time across federal states, controlling for state and year fixed effects yields an identification of the causal effect of schooling in a difference-in-differences framework. The IV approach is also used in Chapter 3 to estimate the contribution of recession-induced college education to skill formation and labor-market success. More precisely, economic conditions at the time of high-school graduation are used as an instrumental variable for college attendance. In addition, Chapter 4 incorporates an IV analysis of the effect of college education on skill formation exploiting arguably exogenous variation in college attendance due to the proximity of an individual's home town to a college campus in Germany.

To estimate the impact of vocational vs. general education types on employment over the life-cycle, Chapter 5 employs a difference-in-differences approach that compares employment rates across age for individuals with general and vocational education. By doing so, the difference-in-differences coefficient captures the differential impact of general relative to vocational education on employment with each year of age. A similar approach is used in Chapter 4 to estimate the skill effect of college education. Our international difference-in-differences analysis compares the skill difference between cohorts at university entry age and cohorts at post-graduation age for university-educated individuals and individuals without university education.

In Chapter 3, the effect of economic conditions at the age of college decision-making on college enrollment, skills, and labor market outcomes is identified in a fixed effects model. By including birth-year and country fixed effects, the identification of the business cycle effect is based on differential changes in unemployment rates across birth-years and countries. Country fixed effects control for any time-invariant differences across countries, such as the quality of education systems, labor-market institutions, or government policies. These fixed effects also account for the persistent component of economic conditions within a country. Birth-year fixed effects account for shocks and characteristics common to all individuals across countries who are born in the same year. Adding these fixed effects controls very flexibly for general time trends across cohorts in our outcomes, such as a secular increase in educational attainment or rising wage levels. They also eliminate any business cycle fluctuation that are similar across countries. Furthermore, including birth-year fixed effects also controls flexibly for skill depreciation and wage changes over the life cycle. Assuming that the unemployment rate at the hypothetical college decision-making age (i.e., at high-school graduation) is uncorrelated with other variables affecting human capital investment decisions, the resulting coefficient for the business cycle effect can be interpreted causally.

1.6 Policy Implications

The ongoing skill-biased technological progress constantly changes work environments, job tasks and the skill-requirements all around the world. The global competition for high-skilled workers is steadily increasing and individuals must be able to adjust quickly to new working conditions to successfully compete on modern labor markets. In the light of these developments, studying the link between education and cognitive skills becomes more important than ever. The four contributions of my thesis intend to provide a deeper understanding of the potential extent to which education can affect the accumulation of skills, which is essential for the development of policy implications aiming at increasing the efficiency of education programs.

In this context, Chapter 2 implies that schooling expansions may be an efficient way to equip new generations of workers with marketable skills, which are crucial for long-run labor-market success. Nevertheless, the effect of reforms – such as the German compulsory schooling reform – is always dependent on the student population affected by the reform. Thus, effects on skills may vary when length of schooling is increased for students at a different age or attending other school types. Furthermore, the better the schools and teachers are prepared for the reform, the more effective is the reform. Scarcity of resources (e.g., insufficient number of teachers) and poorly designed curricula may prevent positive reform effects.

Chapter 3 provides evidence of an alternative determinant of skill formation: the economic conditions at the time of high-school graduation. We show that only economic conditions at high-school graduation, but not in previous or later years, affect the college investment decisions. This suggests that academically marginal students seem to make their college investment decisions toward the end of high school, which has important implications for the timing of policy measures to foster the transition between high school and college. Our results suggest that one way for policy-makers to increase college attendance is to target students at the end of high school, particularly during economic booms. This becomes especially important when focusing on potential longer-run effects, since we show that economic conditions at high-school graduation affect long-run skill development and labor-market success. Additionally, we show that the positive effect on college education is stronger for individuals of higher socio-economic background, which suggests that bad economic conditions at high-school graduation tend to increase educational inequality by widening the education gap between individuals from low vs. high socio-economic backgrounds.

The analysis of the role of cognitive skills for university wage premia in Chapter 4 sheds new light on the importance of cognitive skills in shaping differences in labor markets' returns to higher education across countries. At the same time, new research questions emerge from our findings, such as whether rising university wage premia within countries over time are partly driven by university education creating higher levels of skills, and whether wage inequality can be reduced by policy-makers by providing programs aiming at increasing skills after the end of formal education. Answering these questions holds great promise for deepening our understanding of the interplay between skills, education, and wage inequality.

Chapter 5 aims to provide a deeper understanding of the merits and limitations of different education types for employment in an increasingly globalized era by analyzing the employment effect of vocational vs. general education. Although advocates of vocational education argue that such programs facilitate the transition from school to work, we claim that a life-cycle perspective is important: while individuals who completed vocational education programs initially have better employment opportunities than individuals who completed general education programs, this pattern turns around at older ages. The estimated impact of education type on the age-employment profile is consistent with vocational education improving the transition from school to work but reducing adaptability of older workers to economic change. From an individual perspective, the results imply that people should be aware that there is a trade-off between early advantages and later disadvantages of vocational vs. general education programs over the employment life-cycle. From a policy perspective, our results suggest caution about policies that concentrate just on the current employment situation and ignore the dynamics of growing economies. For a full assessment of how different education types affect the labor-market chances of workers, policy has to set the potential advantages of vocational programs in facilitating the transition from school to work against potential disadvantages when individuals have to adjust to changing conditions later in life. In addition, the results indicate that it may be worthwhile considering the establishment of a system for lifelong learning that conveys skills facilitating their flexibility if changing labor-market conditions require occupational change.

2 The Effect of Compulsory Schooling on Skills: Evidence from a Reform in Germany[1]

2.1 Introduction

Reforms of compulsory schooling laws have been used extensively in the economic literature to study returns to education. Such reforms occurred in almost all developed countries during the 20th century in response to the labor market's increasing demand for high-skilled workers. Focusing primarily on wages, evidence suggests that returns may be substantial for the part of the student population targeted by such reforms – students at the lower end of the skill distribution who tend to leave the formal education system right after the end of compulsory schooling (e.g., Angrist and Krueger 1991; Oreopoulos 2006). These individuals are supposed to benefit from an additional year of schooling once they enter the labor market, where the role of education plays an increasingly important role. However, more recent research often fails to fully replicate previous positive findings, stirring up the discussion about the true returns to such reforms and potential reasons underlying heterogeneous findings (e.g., Devereux and Hart 2010; Stephens and Yang 2014).

This paper analyzes the extent to which the expansion of compulsory schooling improved cognitive skills in Germany. By focusing on individuals aged 53 to 68, the study provides evidence on very long-run effects of education, which is of rising relevance especially in economies facing demographic change. Using information from the *Programme of the International Assessment of Adult Competencies* (PIAAC) and the German re-survey *PIAAC-Longitudinal* (PIAAC-L), I exploit exogenous variation in the length of schooling stemming from a reform which increased compulsory years of schooling from 8 to 9 years in West Germany. The introduction of the compulsory ninth grade took place at different points in time across federal states between 1946 and 1969. Due to an additional agreement of all German federal states to harmonize the start of the school year to late summer, some states introduced two short school years simultaneously to the additional ninth grade. I account for the joint incidence of the compulsory schooling reform and short school years in the empirical application by presenting two different IV specifications. In a first application, schooling is instrumented by the compulsory schooling regime, while simultaneously controlling for the incidence of the two short school years. In an alternative IV setting, I construct a new variable that indicates hypothetical weeks of schooling, based on the compulsory schooling reform as well as the temporary introduction of two short school years.

[1] The study is part of the project "Efficiency and Equity in Education: Quasi-Experimental Evidence from School Reforms across German States (EffEE)", generously funded by the Leibniz Association under its competitive procedure. In addition, financial support by the Leibniz Competition for the research project "Acquisition and Utilization of Adult Skills" (SAW-2015-GESIS-2) is gratefully acknowledged.

Results show that the German compulsory schooling reform increased schooling for basic track students by almost one year, while the concurrent introduction of the two short school years decreased the total length of education by 0.76 years. Difference-in-differences analyses suggest a significant and strong positive long-run reform effect on numeracy skills. Effects on literacy skills cannot be estimated precisely. Two-stage least squares estimates exploiting variation in length of schooling due to the reform verify reduced-form results. One more year of schooling increases numeracy skills of basic track students by 20 percent of a standard deviation (SD) and raises the person's rank position in the distribution of numeracy skills by 8 to 9 percentage points, which is substantially larger than corresponding OLS estimates. Since skills in PIAAC are assessed many years after the end of compulsory schooling, I provide evidence that individuals affected by the reform benefit from extended schooling throughout their entire working life.

The estimated skill effect increases when the first cohort affected by the reform in each federal state is excluded from the analysis, which suggests that initial difficulties at the school-level needed to be overcome before students could fully benefit from the additional ninth grade. The results prove to be robust to the exclusion of city states and to dropping individuals, for whom the federal state of school attendance is approximated by the current state of residence. Expanding the sample to birth cohorts born between 1945 and 1970 increases the estimated numeracy skill effect. Furthermore, I show that potential attenuation bias present in previous studies may be substantial due to the approximation of the federal state of school attendance. Two potential mechanisms explain positive skill effects of education. First, students are taught skill-enhancing content during the additional year of schooling which increases cognitive skills. Second, skills may not be acquired directly during the additional year of instruction but labor-market success of affected students may have increased, which positively affects skill formation measured by PIAAC test scores. Focusing on the reform effect on school exit exam grades supports the main findings of positive numeracy and insignificant literacy skill effects and provides suggestive evidence that students affected by the reform already have higher numeracy skills at the end of secondary education.

The study contributes to two main strands of the literature. First, it provides further evidence on the effect of compulsory schooling reforms. Angrist and Krueger (1991) were the first to use such quasi-experimental variation in schooling to estimate the returns to education in the United States, finding wage gains of 7.5 percent. Oreopoulos (2006) finds wage returns to a compulsory schooling reform of 10 to 14 percent in the UK. In addition, he compares UK results with returns to compulsory schooling in other countries and finds similar results for the US (14.2 percent) and Canada (9.6 percent). Pischke and von Wachter (2008) and Kamhöfer and Schmitz (2015) find zero wage returns to compulsory schooling in Germany, not accounting for the simultaneous introduction of the two short school years in some federal states. In a recent study, Cygan-Rehm (2018) re-analyzes wage returns to compulsory schooling, accounting for the institutional background of short school years and excluding birth cohorts potentially affected by schooling distortions during World War II. Results suggest positive and significant wage effects of 6–8 percent, which contradicts previous findings. The present study provides first evidence on the long-run effect of the German compulsory schooling reform on explicitly tested, high-quality measures of labor-market relevant skills. My findings challenge previous results of Pischke and von Wachter (2008) and Kamhöfer and Schmitz (2015) who argue that the introduction of the compulsory ninth grade in Germany was unable to raise labor-market relevant skills. In fact, I show that the expansion of compulsory years of schooling raised the performance of affected students on numeracy skills tests taken almost four decades after high-school graduation.[2]

Second, this study is related to a growing literature examining the link between schooling and skills (Cascio and Lewis 2006; Carlsson et al. 2015). Only two prior studies – Schneeweis, Skirbekk and Winter-Ebmer (2014) and Kamhöfer and Schmitz (2015) – estimate skill effects to compulsory schooling in Germany with mixed results. However, the explanatory power of skill measures used in these studies is limited and the models do not account for the incidence of short school years. In contrast, PIAAC test scores provide explicitly assessed measures of labor-market relevant competencies. Furthermore, estimated skill effects to compulsory schooling in other countries, using e.g., test scores from military eligibility testing (Falch and Sandgren Massih 2011; Carlsson et al. 2015), are often restricted to a sample of male and rather young students, whereas the present study considers individuals aged 53 to 68. Identifying education policies able to raise the skill level of the population up until old age is crucial for economies undergoing demographic change, where workers tend to participate longer in the labor-market than only a few decades ago.

In what follows, Section 2.2 summarizes previous evidence on returns to compulsory schooling and skill effects of education. Section 2.3 provides background information on the German education system as well as on the two reforms affecting schooling duration. Section 2.4 introduces the data. Section 2.5 describes the empirical model used to identify the causal skill effect of schooling. Section 2.6 reports the results, including a variety of robustness checks. Section 2.7 concludes.

2.2 Literature Review

This section reviews the related literature. Section 2.2.1 summarizes studies examining the effect of compulsory schooling reforms. Section 2.2.2 outlines existing evidence on the skill effects of education.

[2] I shy away from estimating wage returns to compulsory schooling. Individuals considered in my analysis are aged between 53 and 68 years, hence many of them may already be retired or are close to retirement, with possibly reduced working hours. In fact, more than one third of former basic track students in the preferred regression sample are already retired or in early retirement. Only 27 percent are still full-time employed.

2.2.1 Effects of Compulsory Schooling Reforms

International Evidence

Reforms of compulsory schooling occurred in almost all developed countries during the twentieth century. A vast amount of research exists, exploiting variation in the length of schooling induced by these reforms to estimate returns to education. Angrist and Krueger (1991) were the first to use compulsory schooling laws to estimate the returns to schooling in the United States. They exploit variation in length of schooling due to the fact that children born in the beginning of the year start school at an older age and can therefore drop out after completing less schooling than children born at the end of the year.[3] Using quarter of birth as an instrument for education, they find wage returns to compulsory schooling of 7.5 percent for men, which is hardly different from simple OLS estimates. According to the authors, this suggests that there is little bias in conventional estimates. Acemoglu and Angrist (2000) use variation in child labor restrictions and compulsory schooling laws over time across US states and find significantly positive wage returns, comparable to evidence provided in Angrist and Krueger (1991).[4]

Harmon and Walker (1995) exploit exogeneous variation in schooling induced by raising the minimum school-leaving age in the United Kingdom from 14 to 15 in 1947, and from 15 to 16 in 1972. This change in compulsory length of schooling was particularly influential because of the high share of students who dropped out of school as soon as they reached the minimum dropout age. IV results on wage returns to compulsory schooling of 15 percent clearly exceed corresponding OLS estimates of 6 percent. Oreopoulos (2006) exploits the same compulsory schooling reform and finds wage gains of 10 to 14 percent, arguing that the estimated Local Average Treatment Effects (LATE) can be interpreted as average treatment effects in the United Kingdom due to the high share of students affected by the reform. In addition, he compares UK results with returns to compulsory schooling in other countries, finding similar results for the US (14.2 percent) and slightly smaller returns in Canada (9.6 percent).[5]

Still, evidence on the wage effect of compulsory schooling is mixed. Thereby, estimated returns to schooling do not only vary across different countries. Often, even results within a country cannot be replicated or are highly dependent on the underlying data, sample restrictions, and empirical strategy. Stephens and Yang (2014) argue that the key assumption of the identification strategy – the common trends assumption – is unlikely to hold in the context of changes of compulsory schooling regimes in the United States. More precisely,

[3] Hence, Angrist and Krueger (1991) do not identify the schooling effect from a *change* in compulsory schooling regulations, but from a particularity of it. Students in the US were allowed to drop out of school as soon as they reach the dropout age, without necessarily completing the school grade.

[4] Further studies using changes in compulsory schooling laws in the United States focus on, e.g., the effect of education on mortality (Lleras-Muney 2005, 2006), on crime (Lochner and Moretti, 2004), or on wage returns to skills using PIAAC test scores (Hanushek et al., 2015).

[5] Exploiting only the change in compulsory schooling in 1972, Buscha and Dickson (2012) also find a positive reform effect on wages, measured 40 years after the reform.

the identification of the reform effect relies on the assumption that all other developments across states during the period of compulsory schooling reforms are uncorrelated with the law changes, educational improvements, and the outcomes of interest. Thus, previous estimates may be driven by a variety of factors that had disproportionate effects across regions rather than by variation within states over time as is typically thought to identify these models. The authors show that previously found positive wage returns to compulsory schooling in the US become insignificant and partly even wrong-signed once allowing birth cohort effects to vary across regions, suggesting that effects are entirely driven by differential regional developments.

Based on earlier work by Harmon and Walker (1995) and Oreopoulos (2006), Devereux and Hart (2010) were unable to replicate positive wage returns in the UK, using the very same data set as Oreopoulos (2006) as well as a complementary data set with superior wage information. Their results suggest average wage returns to compulsory schooling of 3 percent, without any effect for women, and a 4-7 percent wage increase for men. More recently, Dolton and Sandi (2017) re-analyze the effect of compulsory schooling on wages in the United Kingdom and provide evidence for a positive rate of return for men of 6 percent, exploiting not only Britain's changes in compulsory schooling years but also the 1962 Education Act, that modified the actual school leaving dates based on month of birth. The authors highlight the importance of equation specification when estimating returns to compulsory schooling by showing that previous estimates are highly sensitive to the functional form chosen for identification.

Various studies exploit changes in compulsory years of schooling to estimate wage returns to education in other countries. In doing so, positive returns are found in Norway and Sweden, although they are not directly comparable to other studies because the increase in compulsory years of schooling was embedded in broader reforms of the education system (Aakvik, Salvanes and Vaage 2010; Meghir and Palme 2005). Focusing on France and the Netherlands, evidence that uses variation in the length of compulsory schooling suggests zero returns to education (Oosterbeek and Webbink 2007; Grenet 2013).

The Effect of the Compulsory Schooling Reform in Germany

In an analysis of the German compulsory schooling reform, Pischke and von Wachter (2008) (henceforth PW) find zero wage effects for students who were compelled to stay in school for 9 instead of 8 years.[6] In another study, exploiting variation in schooling from another reform that temporarily shortened the length of a school year due to a change in the start of school years from spring to late summer, Pischke (2007) reports zero wage and employment effects either, albeit grade repetition in primary school increased and students were less likely to attend higher secondary school tracks. This contradicts other studies in the context of

[6] Several other studies examine the effect of the German compulsory schooling reform on other outcomes, see e.g., Piopiunik (2014) for intergenerational transmission of education, Kemptner, Jürges and Reinhold (2011) for health effects, and Siedler (2010) for political interest, voting turnout and democratic values.

Germany investigating returns to schooling using different instruments such as the presence of World War II, parental education or schooling infrastructure (Ichino and Winter-Ebmer 1999, 2004; Becker and Siebern-Thomas 2001). However, differences in estimated returns may stem from the fact that results in each of the mentioned studies need to be considered as LATE, i.e., they measure the marginal effect only on the part of the population that is affected by the instrument. Since different instruments have different complier groups, LATE estimations may very likely differ from each other, depending on the empirical setting (Imbens and Angrist, 1994).

Several reasons may underlie heterogeneous estimates of returns to schooling exploiting changes in compulsory schooling. Variation in returns across countries is quite reasonable due to differences in national education systems, characteristics of the reform, or the population share affected by the reform. According to PW, one potential explanation for zero returns to compulsory schooling in Germany – while evidence suggests up to 15 percent in other countries – is the fact that labor-market relevant skills are learned earlier in Germany than elsewhere. Hence, students do not acquire significantly higher skills during the additional ninth grade that would increase wages due to higher productivity. However, the authors are unable to test their hypothesis directly. In a replication study, Kamhöfer and Schmitz (2015) confirm previous results of zero wage returns to compulsory schooling in Germany by replicating PW's study with data from the German Socio-Economic Panel (SOEP). In addition to the estimation of wage return, the authors test PW's hypothesis of missing skill accumulation during the additional ninth grade as a potential reason for zero returns. The estimated reform effect on skill measures available in the SOEP data suggests zero effects, which strengthens PW's hypothesis. However, competencies tested in SOEP only comprise a rather broad basic ability measure, which presumably cannot fully capture labor-market relevant cognitive skills. More precisely, skills are proxied by a simple word fluency score that is assessed by an ultra-short intelligence test in which respondents have to name as many animals as possible within 90 seconds.

In a recent replication study, Cygan-Rehm (2018) re-estimates the effect of compulsory schooling in Germany on earnings and finds contradicting results. Using the same data as PW, her estimates suggest wage returns to schooling of 6-8 percent. More specifically, point estimates of the reform effect on hourly wages are only slightly larger than in PW, however, the interpretation differs sharply as these are statistically significant and robust across different specifications. Such controversial evidence on returns to education within the same country evaluating the identical reform seems puzzling and highlights the importance of institutional details, econometric specifications and estimation techniques employed. Cygan-Rehm (2018) explains the differential findings with three minor modifications. First, while PW study cohorts born between 1930 and 1960, she restricts the sample to individuals born 1945-1960, thus excluding cohorts who might have been affected by schooling distortions during World War II. Second, she excludes the cohort of the year of the introduction of the compulsory ninth grade. Due to cutoff regulations for primary school enrollment, individuals in these years were only partly affected. However, those affected and not affected cannot be identified in the data. Third – and most importantly – she accounts for the concurrent introduction of two compressed school years, which negatively affected length of schooling in some federal states during the reform period. Her findings of positive wage effects are confirmed in a complementary analysis using German social security records. In the empirical application, I follow the structure proposed by Cygan-Rehm (2018) in large parts, while discussing each step in detail and providing additional evidence on the robustness of my results to various changes of the sample or the integration of the institutional context.[7] By doing so, I provide evidence of the long-run effect of the German compulsory schooling reform on explicitly tested, high-quality measures of labor-market relevant skills.

2.2.2 The Effect of Education on Skills

The ongoing skill-biased technological change constantly increases the demand for high-skilled labor. Thus, most of the reforms of compulsory schooling and other educational reforms during the 20th century in developed countries aimed at increasing the success of young individuals by equipping them with appropriate skills demanded on the labor-market. However, evidence on the effect of education on skills is still scarce.

Cascio and Lewis (2006) exploit variation in the length of schooling stemming from birthdays near the school-entry cutoff dates on AFQT scores in the United States. An additional year of education positively affects test scores of black students by more than 30 percent of a SD, an effect size equivalent to about one-third of the black-white test score gap. Falch and Sandgren Massih (2011) use longitudinal data to estimate the effect of an additional year of schooling on the IQ difference between age 10 and 20 in Sweden. OLS estimates, controlling for selection into noncompulsory schooling, suggest an IQ increase of approximately 20 percent of a SD for one additional year of education. Carlsson et al. (2015) exploit random variation in assigned dates of military eligibility tests in Sweden and find that ten days more schooling increases crystallized intelligence test scores of men by approximately 1 percent of a SD, whereas zero effects are estimated for fluid intelligence test scores.[8] Brinch and Galloway (2012) estimate the effect of a compulsory schooling reform in Norway on IQ scores of male individuals aged 19. The reform increased compulsory schooling from 7 to 9 years during the 1960s. IV estimates suggest a 3.7-point increase in IQ scores per year of schooling, which is less than the estimated association between IQ scores and schooling in simple OLS regressions (5.0 points).

[7] Kemptner, Jürges and Reinhold (2011) also discuss the potential underestimation of the reform effect due to the concurrent introduction of the two short school years in some federal states. In their analysis of the effect of schooling on health outcomes and health-related behavior, they perform a robustness check by re-coding the endogenous schooling-variable taking into account the actual time spent in school instead of highest grade completed. Results suggest that the estimates are not very sensitive to the inclusion of short school years. However, they lose a substantial share of observations.

[8] According to a theory published in 1971 by the psychologist Raymond Cattell, general intelligence can be split into fluid intelligence and crystallized intelligence (Cattell, 1971). Fluid intelligence is often measured by test instruments commonly known as IQ tests.

One difficulty in identifying the effect of education on labor-market relevant skills is the limited availability of appropriate skill measures. Although many previous studies use measures of crystallized intelligence such as IQ, there is an important distinction between IQ and achievement or skills tests. Some scholars argue that education has little effect on IQ scores, others claim that IQ scores are indeed malleable and hence can be affected by education. Heckman, Stixrud and Urzua (2006) show that early childhood programs, such as Headstart and the Perry Preschool Program, did not boost IQ but raised achievement test scores, schooling and social skills. This evidence is consistent with the interpretation that measures of fluid intelligence – such as IQ – are unaffected by education, while schooling can indeed raise performance on tests measuring learned knowledge and competences. According to Cunha et al. (2006), IQ can be affected by environmental interventions up to age 8-10, but is rather stable thereafter. Instead, achievement test scores are affected by IQ, schooling inputs, and non-cognitive skills, and are malleable over a much greater range of ages than IQ. Put differently, the skill effect of schooling is measured best via achievement or skills test scores because these are the competence dimensions that can be affected by education and will also be rewarded on the labor market.

Focusing on information about older cohorts in the US Health and Retirement Survey, Glymour et al. (2008) show that increases in compulsory schooling lead to improvements in performance on memory tests conducted many decades after school completion. Exploiting the 1947 reform of compulsory schooling in the United Kingdom, Banks and Mazzonna (2012) estimate the effect of education on older-age cognitive abilities, applying a regression discontinuity design. They find a large and significant effect of the reform on men's memory and executive functioning, using simple cognitive tests from the Longitudinal Survey on Aging.

Evidence on the skill effect of education in Germany is very limited. The present study is probably most related to Schneeweis, Skirbekk and Winter-Ebmer (2014), who estimate the effect of secondary education on cognitive skills towards the end of working age. For identification, the authors exploit exogenous variation in length of education from compulsory schooling reforms across six European countries, including Germany. They find a positive impact of secondary schooling on memory scores and a protective effect on cognitive decline regarding word fluency measures provided in the Survey of Health, Aging and Retirement in Europe (SHARE). Following the empirical strategy proposed by PW, they do not account for the simultaneous presence of short school years in some federal states. Furthermore, skill dimensions tested in SHARE depict various domains of cognitive functioning, such as memory, fluency, numeracy, and orientation-to-date. I complement findings in Schneeweis, Skirbekk and Winter-Ebmer (2014) by providing evidence on the long-run effect of schooling on explicitly tested, high-quality measures of labor-market relevant skills, while accounting for the institutional context of the reform.

2.3 Institutional Background

2.3.1 The German Education System

In Germany, children usually start primary school at the age of six. After four years of primary school, students transit to secondary education. The country is characterized by an early tracking system, where children attend one out of three different secondary school types depending largely on their performance in primary school, on parental choice, and to a smaller extent on the primary teacher's recommendation (Dustmann, 2004). The two city states Bremen and Hamburg were the only states that tracked students in grade seven during the observational time.[9] The three potential tracks differ in length, the academic content of the curriculum, and the degree obtained after successful graduation. The academic track (*Gymnasium*) is the intentionally most demanding track. The degree awarded after grade 13 is a university entry qualification (*Hochschulreife, Abitur*). Most students who graduate from academic track will enroll in university afterwards.[10] However, they can also apply for an apprenticeship. When attending intermediate track schools (*Realschulen*), students receive their final degree after grade 10 (*Mittlere Reife, Mittlerer Schulabschluss*), which allows them to either continue education on an academic track school or start an apprenticeship, which the majority of intermediate track students does. Basic track schools (*Hauptschulen*) constitute the least demanding track, aimed at preparing students for an apprenticeship. Before the reform, students were required to stay in basic track until the end of eighth grade. After the reform, compulsory schooling ended after grade 9. After the successful completion of the final grade, including an exit exam, students receive a basic school leaving certificate (*Qualifizierter Hauptschulabschluss, Erster Schulabschluss*). However, students may also finish formal education after the end of compulsory schooling without the qualifications for the certificate. Basic track student usually apply for an apprenticeship after school. Figure 2.1, Panel A, shows Micro Census information about the relevance of each track by birth cohorts. Within the group of people born between 1945 and 1949, 58 percent attended the basic track, while only 18 (23) percent attended intermediate (academic) track schools.[11] Over time, the fraction of basic track students dropped, while intermediate as well as academic track attendance increased.

Due to strict educational decentralization, each of the 16 federal states of Germany is autonomous with respect to education policy (*Bildungsföderalismus*). Since its establishment in 1949, the federal states organize their collaboration via the Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany (*Kultusministerkonferenz*). Within this council, resolutions regarding changes in education

[9] The tracking system became less strict in more recent years such that some schools offer more than one track and switching between tracks is facilitated. However, changes in track attendance are still rare and multi-track schools (*Gesamtschulen)* did not exist during our observational period.

[10] After grade 12, students can receive a field-specific university entry qualification (*fachgebundene Hochschulreife, Fachabitur*), which enables them to enter Universities of Applied Sciences.

[11] Track attendance is measured based on highest secondary school degree achieved.

policy have to be passed unanimously. Once a resolution is passed in the council, each federal state is responsible for its implementation (Hepp, 2011).

2.3.2 The Compulsory Schooling Reform

In the early post World War II period, basic track students used to leave formal education after grade 8 in all ten federal states of the Federal Republic of Germany (West Germany). The compulsory ninth grade was introduced in various years across states, which creates ample between-state variation in the timing of the reform. Table 2.1 lists the year of the introduction of a compulsory ninth grade for each federal state as well as birth cohorts first affected by the reform (information based on Leschinsky and Roeder 1980; Petzold 1981; Piopiunik 2014; Cygan-Rehm 2018). Schleswig-Holstein and Saarland – where a ninth grade was already wide-spread before the beginning of the war – reintroduced a compulsory ninth grade shortly after the war in 1947 and in 1958, respectively.[12] Beside these two states, only the city states Hamburg and Bremen introduced an additional year for basic track students before the end of the 1950s. In 1962, Niedersachsen introduced the compulsory ninth grade. However, it was only mandatory in schools with clearly differentiated grades, which caused a delayed introduction especially in rural areas where bigger *Mittelpunktschulen* were established only gradually.[13] In a similar fashion, Hessen set the legal framework for the reform of compulsory schooling as early as 1960, which could not be implemented before 1967. When the prime ministers of all states agreed upon the compulsory ninth grade nationwide at the *Kultusministerkonferenz* in 1964 (Hamburg Accord), also Nordrhein-Westphalen, Rheinland-Pfalz, and Baden-Württemberg introduced the compulsory ninth grade. After the Bavarian reform in 1969, all students in West Germany were required to stay in school for nine years.[14]

[12] The year refers to the first year when basic track students graduated under the new compulsory schooling regime of 9 years (Table 2.1, Column 1).

[13] Due to the insufficient size of some schools, students could not be taught in differentiated grades. This was predominantly problematic in rural areas. Hence, rural school reforms across Germany, beginning in the late 1950s and 1960s (*Landschulreformen*), merged small basic track schools in each village of a region to a centralized bigger basic track school (*Mittelpunktschule*). This facilitated the creation of single-grade classrooms, where teachers could teach the curriculum of one specific grade instead of teaching several grades in one classroom at the same time (Leschinsky and Roeder, 1980). Although this may raise concerns about diverging trends across states prior to the compulsory schooling reform, I cannot directly account for these concurrent developments due to missing information about the urbanization degree of the location of the school, that the individuals attended. However, I prove results to be robust to excluding the two city states of Hamburg and Bremen, who may have had single-grade classrooms over the entire sample period.

[14] Note that applied reform years differ across studies. Reform years used in this study are based on Leschinsky and Roeder (1980) and Petzold (1981), which are identical to reform years used in Piopiunik (2014) and Cygan-Rehm (2018). Reform years in PW and Kamhöfer and Schmitz (2015) differ for Schleswig-Holstein (1941 vs. 1932), Hamburg (1934 vs. 1931), Bremen (1943 vs. 1944) and Saarland (1949 vs. 1943). Due to the detected inconsistencies, Cygan-Rehm (2018) reviewed the original state laws and official statistics on actual ninth grade attendance from Federal Statistical Office, which confirmed reform information used in her replication study. Since cohorts born before 1945 are excluded from the regression sample, unclear information about exact reform years for Schleswig-Holstein, Hamburg and Bremen is irrelevant in the present study. Furthermore, the

The reform impulse of the immediate after-war period came from the fact that many children lacked essential knowledge and preparation for their looming labor-market entry. They suffered from comprised schooling time as well as quality caused by teaching disruptions, teacher shortages, and destruction due to bombing and evacuations. Nevertheless, the most important argument of reform advocates in the early 1950s was the fear of high youth unemployment rates. At that time, big graduating cohorts were about to flood a labor market that did not offer sufficient apprenticeships and job positions (Leschinsky and Roeder, 1980). The introduction of a compulsory ninth grade was supposed to prevent students from early unemployment and to disencumber the labor market in the short-run. With the beginning of the 1960s, this argument became more and more obsolete and lost ground against the ubiquitously increasing demand for high-skilled workers, the effort to improve students' physical and psychological readiness for the labor market and the maturity of their occupational choice (Petzold, 1981).

The introduction of the additional year of education for all students in the basic track was not meant to spread the required content of the basic track curriculum more widely. Rather, the ninth grade was supposed to strengthen students' basic competencies by emphasizing specific areas of learning with a clear focus on labor markets and the working environment. The specific accentuation of content was developed to reach a more individualized and naturalistic teaching environment. Nevertheless, basic school subjects – such as mathematics, German language, biology, and geography – were scheduled with regular teaching hours per week equal to previous grades' curricula. As Baumert (1979) and Leschinsky and Roeder (1980) point out, the ninth grade subjects' syllabus showed a clear effort of educational policy to raise the quality of education in basic track schools and its attractiveness by diminishing the gap between basic and intermediate track.

2.3.3 Short School Years

In Germany and most other countries of the northern hemisphere, the school starting date is in late summer after several weeks of vacation. This was not common practice in West Germany until the late 1960s. Apart from Bavaria, all states started the school year in spring.[15] Back then, politicians felt that it was more sensible to start the school year after summer vacation as in other parts of Europe, and they wanted to achieve uniformity in this policy across states (Pischke, 2007). The transition to the new schooling season took place between April 1966 and July 1967.

earliest birth cohort observed in Saarland is born in 1949. Hence, the inconsistencies in applied reform years are irrelevant in my analysis.

[15] The school starting date was not regulated and harmonized across states during the German Empire and the Weimar Republic. The National Socialist Party declared the school starting date to be August 1 in 1941, harmonized across all federal states. After 1945, all states (except Bavaria) changed the date back to Easter (April 1).

As described extensively in Pischke (2007), there was plenty of variation across states in the organization of the transition period. Students in Bavaria were unaffected by the reform because the start of the school year was already set to late summer. Four other states decided to reach the new school starting date via two consecutive short school years, while introducing the compulsory ninth grade at the same time: Nordrhein-Westphalen, Hessen, Rheinland-Pfalz, and Baden-Württemberg. Thereby, the period between April 1966 to summer 1967 was split into two compressed school years, from April 1, 1966 to November 30, 1966 and from December 1, 1966 to July 31, 1967. The content of each grade's curriculum, however, remained unchanged. Schleswig-Holstein, Bremen and Saarland – who had already introduced the compulsory ninth grade by then – also transitioned to the new school starting date by introducing two short school years. Niedersachsen introduced short school years as well, but gave additional instruction time in subsequent years for some types of schools. Instead, Hamburg transitioned to the new school start via a single long school year, with graduating classes in each track finishing at the end of March.

Overall, students fully affected by the short school years lost a total of two thirds of a regular year (24 instead of 37 weeks in each year). In contrast to the compulsory schooling reform, students across all tracks were affected by the reform. Focusing on basic track, students affected by the compulsory schooling reform, who nominally had completed 9 years of schooling, actually graduated after eight years and four months. Hence, ignoring this fact in the empirical application would potentially lead to an underestimation of the true effect of the introduction of a compulsory ninth grade (Cygan-Rehm, 2018).

2.4 Data

Until recently, the lack of valid skill information limited the potential to analyze the link between education and labor-market relevant skills in many countries. This changed substantially with the release of the PIAAC Survey of Adult Skills in 2012. This section introduces the data and skill measures and describes the sample used for identifying the skill effect of compulsory schooling.

2.4.1 The PIAAC Data

PIAAC was designed to provide representative measures of cognitive skills possessed by adults aged 16 to 65 years in 24 participating countries and was first conducted in 2011/2012 (OECD, 2016).[16] The present analysis benefits from a particularity of the German PIAAC Survey: PIAAC 2012 survey participants (*anchor persons*) as well as their household members were interviewed in three additional waves (*PIAAC-L*): 2014, 2015, and 2016 (GESIS, DIW and LIfBi, 2017). In the remainder of the paper, I will refer to the PIAAC data, implicitly meaning the combined PIAAC and PIAAC-L data.

[16] In 2015, 9 more countries took part in PIAAC.

PIAAC provides measures of cognitive skills in three domains: literacy, numeracy, and ICT (problem-solving in technology-rich environments). I will focus on the first two skill domains for various reasons. First, ICT skills were only tested in 2012. Numeracy and literacy skills, instead, were re-tested in 2015. Second, in contrast to numeracy and literacy skills testing, ICT skills needed to be assessed in a computer-based test environment. PIAAC survey participants were allowed to refuse to take the test, which is likely to be correlated with the ICT competence level. Third, individuals without a minimum basic knowledge of computer usage were not tested (i.e., individuals who failed a basic test and hence were also tested paper-based in the other two skill domains). Since the PIAAC respondents considered in my empirical analysis are rather old (aged 53 to 68), I might face non-random non-response with respect to the ICT skill domain (see Falck, Heimisch and Wiederhold 2016).

Each skill domain is measured on a 500-point scale (Zabal, Martin and Rammstedt, 2017).[17] Along with information on cognitive skills, PIAAC provides information on the respondents' education, labor market status and demographics from an extensive background questionnaire. The allocation of observations into pre- and post-reform cohorts is possible because of information on the federal state of residence when the individual finished secondary education as well as the year of graduation. In addition, the data contains some wage information. However, I will not focus on direct labor market outcomes due to various reasons. First and foremost, individuals in my preferred sample are 53 to 68 years old.[18] Many persons may already be retired or are close to retirement, with possibly reduced working hours. In fact, more than one third of former basic track students are already retired or in early retirement. Only 27 percent state to be still full-time employed. Second, the wage measure in PIAAC is rather imprecise compared to data used in previous studies and suffers from measurement error. PIAAC respondents in PIAAC-L wave 2014, 2015, and 2016 were only asked to state their monthly wage, which is less accurate than hourly wage and hours worked need to be accounted for. Thus, this study focuses on skill effects of compulsory schooling and refers to wage effects in Cygan-Rehm (2018), who analyzes the Qualification and Career Survey (QaC) as well as social security records with precise wage information for a big subsample of the German population.[19]

2.4.2 Skills Test Scores in PIAAC

PIAAC anchor persons were tested by means of PIAAC test instruments in 2012 as well as in 2015. Other household members were only tested in 2015 with a slightly shorter test using NEPS test instruments.[20] Around 30 percent of anchor persons also received the NEPS test

[17] Throughout, I use the first of overall ten plausible values of the PIAAC scores.

[18] See Section 2.4.3 for a description of the final regression sample.

[19] The QaC is a repeated cross section of employed workers of German nationality in the age group 15 to 65 (each wave samples about 25000 workers). The survey is conducted by the Institute for Employment Research (IAB) and the Federal Institute for Vocational Education and Training (BIBB).

[20] These are tests designed for the National Education Panel Study (NEPS), which provides data on educational and professional careers as well as on competence acquisition across adult life.

in addition to the PIAAC test in 2015, which is an essential asset for the re-scaling exercise of NEPS skills test scores of household members, as illustrated below. PIAAC anchor persons, who took both skills tests, are referred to as *both-takers* in the remainder of the paper.

As described in broader detail in Zabal, Martin and Rammstedt (2017), PIAAC and NEPS skills tests incorporated in wave 2015 were designed to depict specific competence domains which are defined very homogeneously across test regimes. Literacy in PIAAC is conceived as "understanding, evaluating, using and engaging with written texts to participate in society, to achieve one's goals, and to develop one's knowledge and potential" (Jones et al., 2009). According to the definition in Gehrer et al. (2013), reading competence in NEPS testing focuses on text comprehension and text handling in everyday-type situations, i.e., the ability to read and comprehend different types of texts widely, irrespective of prior knowledge. PIAAC numeracy skills represent "the ability to access, use, interpret, and communicate mathematical information and ideas, in order to engage in and manage the mathematical demands of a range of situations in adult life" (Gal et al., 2009). NEPS mathematical competence displays the ability to flexibly apply mathematical knowledge in real world situations requiring mathematical problem solving (Weinert et al., 2011).[21]

Anchor persons in PIAAC may have up to three different skill measures: from PIAAC tests in 2012, from PIAAC tests in 2015, and from NEPS tests in 2015 (only both-takers).[22] Household members surveyed in PIAAC only have one skill value, measured by means of NEPS test instruments in 2015. To use the largest possible amount and variation of skill information, I expand the original PIAAC sample to allow individuals to appear multiple times in the data, depending on the available skill information. Accordingly, observations will be weighted such that each individual receives the same weight, which is split equally across in-sample appearances (i.e., observations) of the individual.[23]

To receive comparable skill measures across the two different test regimes, PIAAC and NEPS, scores of the latter test were adjusted. For the re-scaling exercise of NEPS test scores, I use the full sample of individuals and utilize skill information of both-takers. 1571 (1561) individuals

[21] The two types of tests differ slightly in the format and mode of assessment. While most tasks in NEPS are multiple choice questions – except of few short open entry items in mathematics – PIAAC items include primarily open response items and only very few closed-format items such as multiple-choice. Furthermore, PIAAC tests were computer-based with an optional paper-based mode and not time-restricted. NEPS tests instead were paper-based throughout and time-restricted (Zabal, Martin and Rammstedt, 2017).

[22] PIAAC participants with skill values in 2012, who could not be re-surveyed or tested in follow-up waves, are excluded from the sample due to missing information about federal state of school attendance and missing information about year of high school graduation (survey questions in wave 2014). Furthermore, PIAAC does not provide re-scaled 2012 test scores for this group of individuals. Thus, I cannot include these observations in my analysis, despite potential concerns about endogenous attrition. In fact, non-resurveyed PIAAC 2012 participants were slightly more likely to have attended basic track and have slightly lower numeracy and literacy skills (as measured in 2012).

[23] Results do not change quantitatively when using the original sample and average skill scores within each skill domain, but are less precise.

in the full sample of PIAAC-L have a non-missing PIAAC numeracy (literacy) skill score as well as a non-missing NEPS numeracy (literacy) skill score, tested at the very same time.[24] The correlation between the two skill measures is 0.67 for numeracy and 0.68 for literacy. We re-scale NEPS test scores by means of the following equation:

$$T^{NEPSnew} = \frac{(T^{NEPS} - \overline{T^{NEPS}_{bothtaker}})}{SD^{NEPS}_{bothtaker}} * SD^{PIAAC}_{bothtaker} + \overline{T^{PIAAC}_{bothtaker}} \tag{2.1}$$

In a first step, NEPS scores are standardized using the mean ($\overline{T^{NEPS}_{bothtaker}}$) and standard deviation ($SD^{NEPS}_{bothtaker}$) of NEPS scores of both-takers. Thereafter, resulting values are multiplied by the SD of PIAAC scores of both-takers ($SD^{PIAAC}_{bothtaker}$), before adding their mean PIAAC-score ($\overline{T^{PIAAC}_{bothtaker}}$). Thus, the re-scaling exercise facilitates a uniform interpretation of skill values on the original PIAAC 500-point scale.

2.4.3 Sample

Several restrictions are put on the sample to cleanly estimate the effect of schooling on cognitive skills. While focusing on a rather restricted sample may limit the generalizability of my findings, it should not affect the internal validity of the resulting estimates, since the restrictions are based on variables that are likely to be unaffected by reform. As Table A2.1 shows, the initial sample amounts to 16133 observations. The sample is restricted to West German federal states, which drops approximately 24 percent of observations. Following Cygan-Rehm (2018), I only consider individuals born between 1945 and 1960. Individuals born during World War II may have suffered from wartime shocks or disrupted instruction time. Furthermore, some of these early cohorts experienced temporary extensions of compulsory schooling before the war, which cannot be identified in the data (see Cygan-Rehm 2018). Another reason for the time restriction is the limited number of observations of such early birth cohorts in PIAAC. Applying these time restrictions leaves me with 3560 observations.[25]

The distribution of skill scores is shown in Figure 2.2. Panel A displays the distribution of numeracy and literacy skills for the final regression sample. The mean skills test score is 273 points for numeracy and 270 for literacy, whereas numeracy skills are spread significantly broader (SD of 48 points vs. 40 points for literacy). When focusing on each test regime separately (Panel B), one observes that the distribution of numeracy skills are almost identical, with a slightly higher mean for PIAAC 2012 (277 points vs. 274 and 272 points for PIAAC 2015 and NEPS in PIAAC 2015, respectively). A similar picture arises when considering literacy skills, with even less differences in mean skill values across test regimes. Overall, the final regression sample consists of 625 (625) observations for numeracy (literacy) skills in PIAAC 2012 as well as

[24] For the distribution of NEPS and PIAAC z-standardized test scores of both-takers see Figure A2.1.

[25] Individuals born in 1945 are not anchor persons but household members. The earliest birth cohort observed among anchor persons is 1946. Results do not change when excluding observations born in 1945.

in PIAAC 2015, and 524 (536) observations for NEPS test-takers in PIAAC 2015. In the empirical analysis, z-standardized values of the re-scaled skill measures will be used.

Table 2.2 shows sample means for all variables of interest. Years of schooling display years spent in primary and secondary education, derived by the difference between the school graduation year and the year of birth, subtracting the school starting age of six. Some studies using e.g., German Micro Census data (PW, Kemptner, Jürges and Reinhold 2011), do not have information on the year of school graduation and thus need to impute length of schooling based on the highest secondary school degree and the compulsory schooling regime. Hence, the schooling information provided in PIAAC is likely to be superior to previous studies.[26] Across all tracks, individuals attend school for around 10.4 years. Focusing only on basic track students, the number shrinks to 8.8 years.

Track attendance is defined by the highest secondary school degree obtained. The basic track takes up the biggest share in PIAAC (43 percent). Compared to official statistics from the German Micro Census (Figure 2.1), track shares in PIAAC seem to be quite representative of the overall population for the birth cohorts of interest (1945–1960).

2.5 Empirical Framework

In a simple regression of cognitive skills on length of schooling and on a set of control variables, the resulting coefficient of length of schooling is expected to be positive, i.e., individuals who invest more in education have higher skills. However, no causal interpretation of the coefficient is possible, since education is not randomly assigned but rather the outcome of each individual's schooling choice. Unobserved factors may cause some individuals to obtain higher levels of education, but these factors may – at the same time – raise skills independently or by means of other channels than school. A frequently given example of unobserved characteristics is innate ability. No observable variables can sufficiently control for ability differences, that might lead a group of individuals to select themselves into more education. However, even in the absence of more schooling, these individuals would most likely outperform those with less years of schooling. Hence, the OLS coefficient of length of schooling would be overestimated (positive selection bias). In contrast, discount rate bias, arising from individuals with higher discount rates choosing less education in an optimization model, may results in an underestimation of the true skill effect of education if this group of individuals is also less likely to invest in skill accumulation (Card, 1994). According to Becker's model of human capital investment (Becker, 1962), individuals invest in education until the marginal return to an additional year of schooling equals the marginal discount rate. Hence, individuals with less education may either have relatively low returns (i.e., low-ability students) or high discount rates. High discount rates are associated with individuals from

[26] Due to some implausibly high and low reported schooling years, the variable is trimmed at the 1st and 99th percentile within track.

poorer families or with a stronger distaste for education (Card, 1994). In a similar fashion, many other factors may cause an over- or underestimation of the true effect of schooling.

Two alternative mechanisms may underlie positive skill effects of compulsory schooling. On the one hand, students learn relevant content during the additional year of schooling which directly increases cognitive skills. On the other hand, the additional ninth grade may not affect numeracy and literacy skills directly, but raises the labor-market success of affected students, which positively affects skill formation measured by PIAAC test scores. For instance, individuals affected by the reform might get a better job that offers more opportunities to promote skills. Furthermore, evidence suggests a negative relationship between work interruptions and skills. Edin and Gustavsson (2008) investigate the link between skill depreciation, work interruptions and subsequent wages, using information from the International Adult Literacy Survey in Sweden. They find that a full-year of non-employment is associated with a 5-percentile decrease in skills. Hence, skills of individuals affected by the compulsory schooling reform may decline more slowly due to an overall higher employment probability. Since skills in PIAAC are measured many years after labor-market entry, the present study is unable to fully disentangle the two potential channels.[27]

Changes in compulsory years of schooling are a frequently used quasi-experimental setting in economic studies, since such reforms mostly target a rather comparable group of people – low-skilled students who likely leave school after the end of compulsory education – and impose an exogenous increase in length of schooling for this specific group. I estimate the impact of compulsory schooling on cognitive skills exploiting the reform described above, which introduced a mandatory ninth grade for basic track students in West Germany. Exploiting variation in the timing of the reform across states in a two-stage least squares framework, I estimate the following outcome equation:

$$Skills_i = \beta_0 + \beta_1 \widehat{Schooling}_i + \lambda_{state} + \delta_{cohort} + \chi_i' \beta_3 + \epsilon_i \tag{2.2}$$

where $Skills_i$ are z-standardized PIAAC test scores of individual *i* and $Schooling_i$ is the length of primary and secondary education in years. Time spent in post-secondary or tertiary education is not included the variable. The corresponding first-stage equation,

$$Schooling_i = \alpha_0 + \alpha_1 CSR + \alpha_2 SSY + \gamma_{state} + \mu_{cohort} + \chi_i' \alpha_3 + \varepsilon_i \tag{2.3}$$

regresses years of schooling on an indicator for the compulsory schooling regime (CSR), which is either 8 or 9 years. By including state and birth cohort fixed effects, λ_{state} and δ_{cohort}, the reform effect is identified within a difference-in-differences setting. While the compulsory

[27] Furthermore, PIAAC test scores only comprise two competence domains. It may be the case that students benefit even more from education in terms of higher skills, which cannot be fully captured by numeracy and literacy test scores.

schooling reform increased the time students spend in school, the introduction of the compressed school years in 1966/67 reduced instruction time. This concurrent development is accounted for by including a dummy that equals 1 if the individual went to school during 1966 and 1967 in one of the federal states that introduced the shortened school years, SSY.[28] All specifications control for differences across gender and test regimes. A small fraction (less than 10 percent) of individuals has missing information for the federal state of school attendance. For those observations, the variable is approximated by the federal state of residence at the time of the PIAAC survey. A dummy identifying these individuals is included throughout all specifications.[29]

In an attempt to address the concurrent incidence of the compulsory schooling reform and the introduction of the two short school years more thoroughly, I complement the analysis by an alternative IV approach. Therein, the endogenous schooling variable is instrumented by the hypothetical total amount of weeks spent in school. This variable varies by federal state of school attendance, track, and birth cohort. For instance, an individual who was born in 1950 and attended basic track in Nordrhein-Westphalen was supposed to stay in school for 296 weeks – eight years of schooling (37 weeks) without any compressed school years. When born in 1955, it took 307 weeks to complete the same school track in Nordrhein-Westphalen due to the compulsory ninth grade as well as the exposure to the short school years (7 years with 37 weeks each, 2 years with 24 weeks each). However, this same person would have stayed in school for 333 weeks in Bavaria. Hence, this variable captures both institutional peculiarities simultaneously.

Instrumental variable methods deliver local average treatment effects (LATE), presenting the treatment effect only for the group of people affected by the reform (Imbens and Angrist, 1994). In the present study, the complier population consists of students who attend basic track schools and would have left school right after the end of compulsory schooling. Hence, estimated effects may differ compared to other estimates on skill effects of education, using e.g., groups of students at different ages or in a higher school track (see Card 1999). Effects may also differ when using short school years as main instrument: students in all tracks and grades during this time were affected by the reform, which can explain different skill effects in corresponding estimations. Due to the same reason, estimated coefficients are hard to interpret in an IV setting where both instruments are used simultaneously. This is one reason

[28] Theoretically, students who entered school only in the second year of the short school years or those who graduated after one year were only partially affected. Most of these students, however, were not affected at all because federal states tried to prevent student entering primary school in December 1966, and graduating cohorts were often given more time to graduate. For more detailed information see Pischke (2007). Estimated coefficients do not change significantly when the indicator for short school years is generated the way presented here or in line with Pischke (2007), where the respective variable could obtain three different values depending on the intensity of exposure to the short school years.

[29] Robustness specifications in Section 2.6.3 show that results are robust to excluding observations with approximated information on federal state of school attendance.

for restricting the preferred sample to basic track students instead of considering the entire student sample across all tracks.[30]

The validity of an instrument is defined by its relevance and exogeneity. With respect to compulsory schooling reforms, the relevance of the instrument can be directly tested (see section 2.6.1) and is considered indisputable due to its mandatory character. However, the exclusion restriction cannot be tested directly. The identification of the skill effect of compulsory schooling relies on the assumption that – conditional on covariates – a change in compulsory schooling regulation is uncorrelated to cognitive skill development, except through its effect on education. Put differently, all other changes that occur across federal states during this period, are uncorrelated with the law change, educational improvements, and skill outcomes. The common trends assumption is violated as soon as unobserved factors have disproportionate effects on birth cohorts across regions. One such factor may be, e.g., school quality improvements. Stephens and Yang (2014) show for the United States that this is indeed the case: when re-analyzing wage returns to compulsory schooling, they allow birth year fixed effects to vary across the four US Census regions (West, Midwest, South, Northeast). They find that positive significant effects of compulsory schooling in previous studies become insignificant and even wrong-signed, once cohort effects are allowed to vary across the four regions.

While it is meaningful to assume differential developments across these groups of US states – e.g., due to the improvement of school quality in Southern states (Card and Krueger, 1992), a rational split of German federal states into regions is not straightforward. From today's perspective, a reasonable division would be an East-West split of federal states. However, since compulsory schooling reforms took place in West German federal states only and prior to the reunification, East Germany is excluded from the analysis. A north-south split of states based on their geographical location would be possible but not reasonable because of missing economic or political proximity of the states within one region. In addition, the inclusion of region-specific fixed effects reduces part of the identifying variation across states. This statistical concern is substantial due to the limited number of federal states in West Germany (10 federal states). Thus, allowing cohort effects to vary between northern and southern regions would yield a loss of precision without reducing concerns regarding the violation of the common trends assumption. Nevertheless, adding region-specific fixed effects yields qualitatively similar and statistically non-distinguishable results from the preferred specification.[31] Alternatively, the common trends assumption is relaxed in some specifications by adding region-specific linear trends. However, due to the relatively small sample, this seems to be an overly demanding specification.

[30] With respect to the relatively small sample size, restricting the sample to individuals directly affected by the reform – basic track students – also increases the precision of estimated reform effects. The focus on basic track students is only feasible if the reform did not affect track attendance directly. This will be addressed in section 2.6.

[31] The statistical difference between estimated coefficients is tested using seemingly unrelated estimations.

To account for within-group dependence, I follow the conventional approach and assume errors to be correlated among individuals from the same state and birth cohort. According to Abadie et al. (2017), clustering standard errors within state by year of birth cells is justified because treatment assignment is perfectly correlated within these cells. In regressions with basic track students only, standard errors are adjusted for 100 clusters.[32]

2.6 Results

This section summarizes findings on the effect of the compulsory schooling reform in Germany. Section 2.6.1 reports the estimated reform effect on length of schooling. Section 2.6.2 depicts reduced-form and IV results on the reform effect on skills. Various robustness checks are presented in section 2.6.3. Appendix A2.1 summarizes the results of a replication of the analysis using an alternative data set (the Adult Cohort of the National Education Panel Study, NEPS).

2.6.1 Reform Effects on Educational Attainment

Despite the mandatory character of the reform, a uniform jump across all federal states in the length of schooling from 8 to 9 years at the time of the reform is not reasonable to assume. Some states introduced a (voluntary) ninth grade on a regional level even before the reform. In other regions, the introduction of the additional grade was not realizable immediately across regions due to capacity constraints in rural areas. In addition, short school years reduced the time students spent in school. Figure 2.3 displays average years of education several years before and after the reform, for individuals across different secondary school tracks. Basic track students, who went to school prior to the compulsory schooling reform, report on average 8.4 years of education. After the reform, the average length of schooling increases to almost 9 years. This rather clear jump in years of schooling is only observable for basic track students, while no substantial changes are observed for students attending intermediate or academic track schools, who were not affected by the compulsory schooling reform.[33]

Table 2.3 reports first-stage estimation results of the effect of the compulsory schooling reform on years of primary and secondary education for the sample of basic track students. All specifications include dummies for gender, birth cohort, federal state, test regime, and a dummy indicating whether the federal state of school attendance needed to be approximated

[32] Standard errors increase slightly when clustering at the federal state level (10 clusters), which would be the most conservative way.

[33] Please note that the average length of schooling within each cell is rather noisy because of the small number of individuals within each track-year cell. For instance, the number of individuals in each cell of Panel A (basic track) varies between 4 and 18; between 3 and 15 in Panel B; between 2 and 11 in Panel C. The number of individuals is the relevant dimension when analyzing reform effects on length of schooling, since one individual is observed up to three times in the data due to the multiple skills tests available (see Section 2.4), but length of schooling is equal across all observations for each individual.

by the federal state at the time of the interview (see Section 2.4). Panel A, Column 1, shows the estimated reform effect of a simple OLS regression using only the change in the compulsory schooling regime while neglecting the potential simultaneous presence of short school years. The compulsory schooling reform led to an increase of 0.71 years of education for basic track students. When taking reduced instruction time due to the short school years into account (Column 2), the effect of compulsory schooling on educational attainment increases to 0.97 years, while being affected by the short school years reduces time spent in school by 0.76 years. Hence, length of schooling increased by a quarter of a year for individuals under the new compulsory schooling regime who were also exposed to short school years.

The specification in Column 3 relaxes the common trends assumption by accounting for potential differential developments across regions or states. When including state-specific linear trends in birth cohorts, coefficients of the reform effect are slightly reduced. Even in this demanding specification with respect to the comparably small sample size, the effect of the compulsory schooling reform remains statistically significant and large. Basic track students, who were affected by the reform, attended school for 0.74 more years. Being exposed to short school years reduces schooling by slightly more than half a year.

Specifications in Panel B substitute the reform indicators by hypothetical weeks of schooling, which unifies the compulsory schooling reform as well as short school years in one single variable. A one-week increase of the hypothetical schooling variable is associated with an increase in years of schooling of 0.03. Put differently, a one-year increase (37 weeks) in hypothetical length of schooling is associated with an almost one-year increase in reported years of schooling. When accounting for state-specific linear trends, the coefficient decreases slightly to 0.02 (0.74 years).

An independent analysis of reform effects for the group of basic track student is only feasible if the compulsory schooling reform did not affect the probability to attend a specific secondary school track. Table A2.2 reports regression results of the compulsory schooling reform on a dummy indicating the secondary school track. Insignificant coefficients across all secondary school tracks and model specifications suggest that the reform did not affect the extensive margin of track attendance, i.e., students were not more or less likely to attend basic track schools due to the reform. Thus, conditioning the sample on basic track students seems legitimized and yields causal estimates.[34] Restricting the preferred estimation sample to basic track students increases the power for identifying the causal reform effect due to focusing on a relatively homogeneous group of students, who were all affected by the reform. When considering students across all three secondary school tracks instead, results look qualitatively similar but weaker in size as well as in statistical significance (Table A2.3). For instance, Panel A, Column 3, reports a coefficient of 0.56 years when being affected by the compulsory schooling

[34] Using information from the QaC, PW find that students affected by the reform are slightly less likely to attend basic track. However, the effects are small, not significant, and are essentially zero for the much larger sample in the Micro Census. In contrast, Cygan-Rehm (2018) reports a substantial shift of students from basic to intermediate track due to the reform.

reform. This is similar to the multiplication of the share of basic track students (43 percent) and the reform coefficient for the basic track sample, 0.74.

Table A2.4 reports first-stage estimation results of the reform on years of schooling, using samples of different tracks. In a placebo test setting, the compulsory schooling reform should not affect length of schooling for students attending other tracks. However, students from all tracks should be affected by the introduction of short school years. This hypothesis is partly confirmed in the data – neither intermediate track nor academic track students report more years of schooling in response to the compulsory schooling reform and schooling decreased slightly for individuals affected by short school years (even though not precisely estimated).[35] Hypothetical weeks of schooling – which also account for the incidence of short school years across all tracks – do not significantly affect length of schooling for intermediate and academic track students. This indicates, that most of the effect of hypothetical weeks of schooling on realized length of schooling is driven by variation due to the compulsory schooling reform. Measurement error in the variable indicating exposure to short school years is presumably substantial due to the numerous adjustments of instruction time across as well as within federal states during the short school years, such as extending the school year for graduating cohorts or the postponement of school entry cohorts, which is unobserved in the data.

Overall, the introduction of the compulsory compulsory ninth grade – partly accompanied by the introduction of two short school years – significantly affected the length of schooling for basic track students. Whether students benefited from the reform in terms of higher marketable skills will be examined in the following section.

2.6.2 Reform Effects on Skills

Reduced-Form Estimation Results

Given the strong evidence on the effect of the German compulsory schooling reform on schooling, I exploit exogenous variation in length of schooling due to the reforms to estimate the effect of education on the development of labor-market relevant cognitive skills. Table 2.4 reports reduced-form effects of the reform on numeracy skills. Again, all specifications control for gender differences in skills and include dummies for each birth cohort and federal state of school attendance. Furthermore, indicators for each test regime account for level differences between test instruments and survey waves.[36]

[35] The results are qualitatively the same when estimated in specifications with state-specific linear trends (not shown).

[36] The number of observations is almost cut by half compared to the first-stage observations. Overall, the sample consists of 746 observations with numeracy skill information and 741 observations with literacy skills. However, all 1487 observations are based on 364 individuals. Of these, 306 individuals have between one and three numeracy skills measures, and 301 individuals literacy skill measures. Due to the adjusted weights for each observation, first-stage results are almost identical, no matter whether the sample of 1487 observations (with either numeracy or literacy skill measure), of 746 observations (numeracy) or 741 observations (literacy) is considered. For example, an individual X has two numeracy and two literacy skill measures. Thus, X appears four

The introduction of an additional ninth grade led to an increase in numeracy skills of affected basic track students by 0.20 SD, which amounts to approximately 9 PIAAC points (Panel A, Column 1). When including state-specific linear trends in Column 2, the point estimate of the reform effect is quantitatively the same but cannot be estimated precisely. Although standard errors increase due to the demanding specification, estimates suggests that the positive reform effect on numeracy skills is not driven by differential developments across states. Compressed instruction time due to the temporary introduction of short school years decreases numeracy skills. In Column 2, the negative effect is even larger than the positive skill effect of the compulsory schooling reform and more precisely measured.

Test scores are limited informative if point differences cannot be interpreted economically. Thus, focusing on an individual's test performance *relative* to other individuals – e.g., via the position in the overall distribution of skills – provides additional insights of the reform effect from an economic perspective. Column 3 and 4 of Table 2.4 show that the compulsory schooling reform also increased the percentile rank in the numeracy skill distribution. Being affected by the additional ninth grade raises the position in the numeracy skill distribution by 8.5 percentage points. Being affected by the short school years decreases the rank. When accounting for state-specific linear trends, the positive rank effect of the compulsory schooling reform is entirely offset by the negative effect of short school years for students in the new schooling regime who were exposed to the short school years.

A positive reform effect on numeracy skills is confirmed when the reform indicator and the short school years dummy are substituted by hypothetical weeks of schooling in Panel B. An increase of one week raises numeracy skills by 0.005 SD. Put differently, one hypothetical year more schooling increases numeracy skills by 0.19 SD. When including state-specific linear trends in Column 2, the estimated skill effect increases slightly to 0.007 SD, which corresponds to 0.26 SD higher skills for one more hypothetical year of schooling. Similarly, the percentile rank within the numeracy skill distribution increases by 7.4 percentage points (Column 3).

Table 2.5 shows reduced-form results for literacy skills. Neither the compulsory schooling reform nor the introduction of short school years significantly affected literacy skills. Although the coefficient size of estimated reform effects on literacy skills are positive and large, standard errors exceed the point estimates throughout all specifications.

Two-Stage Least Squares Estimation Results

Two-stage least squares estimations in Table 2.6 exploit exogenous variation in length of schooling due to the introduction of the compulsory ninth grade, simultaneously controlling for the incidence of short school years.[37] Panel A reports first-stage results, which are almost

times in the sample of 1487 observations. the weight given to each of these observations is 1/4 of the weight of individual X (which is the same for each individual). When considering the sample with numeracy skills only (746 observations), only two observations belong to individual X. Hence, each observation is given the weight 1/2.

[37] An alternative way to account for the introduction of the two short school years would be to add the variable to the set of instruments. Due to the potentially high measurement error in the indicator for short school years,

identically to those shown in Table 2.3. Estimated coefficients differ only marginally due to a reduction in the sample from 364 individuals to 306 individuals because 58 individuals do not have any numeracy skill information. Panel B shows second-stage results for the effect of compulsory schooling on numeracy skills. The instrument F statistics exceed the conventional threshold of 10 for strong instruments throughout all specifications, despite a sharp drop when including state-specific linear trends in Column 2 and 4, due to the reduction in precision.

The estimated reform effect on numeracy skills in Column 1 is very similar to the effect based on reduced-form estimations.[38] Students who attended school for one more year due to the compulsory schooling reform have 0.21 SD higher numeracy skills, measured by means of PIAAC tests around four decades after the end of compulsory schooling. When adding state-specific linear trends, the point estimate even increases, but cannot be estimated precisely. The percentile rank position of students affected by the reform increases by almost 9 percentage points (Column 3).

IV estimates of the effect of compulsory schooling on numeracy skills exceed simple OLS estimates for the sample of students from all tracks, which suggest 0.12 SD higher numeracy skills and and 3.8 percentage points higher rank for each additional year of schooling (Table A2.5). Hence, simple OLS regressions seem to be biased downwards. However, effects estimated by instrumental variable methods are interpreted as local average treatment effects, which means that the effects refer to the group of students targeted by the reform. This group may differ from the average secondary school student. The group of compliers in the present setting, i.e., basic track students, seem to benefit more than the average student from an additional year of schooling. This is reasonable due to relatively few years these students have spent in formal education when compulsory schooling ends. Furthermore, these students are likely to have relatively high discount rates, which led them to choose the basic secondary school track in the first place.

The skill effect of compulsory schooling does not change when applying the alternative instrumental variable approach using hypothetical weeks of schooling in Table 2.7. One additional year of schooling raises numeracy skills of individuals by 0.20 SD and their rank position by 8 percentage points. When relaxing the common trends assumption in Column 4, results suggest a rank increase of 12 percentage points, which is significantly estimated on a 10-percent level. This suggests that the positive numeracy skill effect is not driven by differential developments across federal states. On the contrary, the reform did not affect literacy skills (Table A2.6, A2.7).

The findings presented above do not support PW's hypothesis of zero wage returns to compulsory schooling due to the lack of skills acquired in ninth grade of basic track schools in

I choose to include it as control variable. When using both institutional changes as instruments, results are qualitatively the same. However, the first-stage F statistic is lower compared to the F statistic in specifications with one instrument only.

[38] This is not surprising due to a first-stage reform effect of almost one year.

Germany. Taking the institutional particularities of the simultaneous introduction of short school years into account, I find significant positive effects of the introduction of a compulsory ninth grade on numeracy skills, which are still measurable approximately four decades after school completion. One potential interpretation of my results may be an emphasis on mathematical content in the curriculum of the additional ninth grade. To improve students' competencies and hence readiness for the approaching labor market entry, teachers may have focused on improving the students ability to use and process mathematical information in an everyday working environment. PW's hypothesis is reasonable with respect to literacy competencies because basic reading skills required for a successful labor-market entry may be learned earlier than in grade nine. Unfortunately, my data does not allow to test this hypothesis directly.

2.6.3 Robustness Analyses

To prove that the estimated IV results of the skill effect of compulsory schooling are not sensitive to varying samples, this section presents a set of robustness checks. For better illustration, I focus on numeracy skill effects. However, robustness checks using numeracy percentile ranks yield quantitatively and qualitatively very similar results.

Children in Germany tend to start school at the age of six. However, if they are born late in the year, they are likely to enter primary school only in the year when they turn seven. In my preferred specification, individuals born late in the year of the first cohort affected by the reform (see Table 2.1), are treated as being fully affected by the reform, although only part of them actually were affected. Since I do not have precise information on the month of birth in PIAAC, it is insightful to exclude the first cohort affected by the new compulsory schooling regime to address this source of measurement error. As Column 1 of Table 2.8 shows, estimated first-stage effects of the compulsory schooling reform on length of schooling are robust to the exclusion of the first cohort affected by the reform in each federal state (Panel A). However, the estimated skill effect of the reform increases to 0.32 SD, suggesting measurement error in the assignment of the compulsory schooling regime indicator for the first cohort affected by the reform. A potential alternative interpretation for the higher skill effects when excluding the first affected cohort in each state may be initial difficulties for teachers teaching the new curricula and adapting teaching methods. Such phase-in of positive reform effects is well imaginable in case of learning effects of teachers as well as gradual resource expansion, such as the hiring of new teachers.[39]

The two city states Bremen and Hamburg used to track students only in grade seven. Hence, students were given more time to decide which track they wanted to attend, which may result in a different set of students attending basic track schools. In addition, these states may differ from others in their development of the education system over time, which raises concerns

[39] Corresponding results for the alternative IV approach using hypothetical weeks of schooling is shown in Table A2.8.

about the validity of the common trends assumption. However, excluding the two city states does not change the estimated effect on numeracy skills (Column 2). Students affected by the reform have 0.20 SD higher numeracy skills.[40] When expanding the sample by ten years to consider individuals born between 1945 and 1970, the estimated first-stage coefficient of the reform effect on length of schooling is slightly reduced (Column 3). However, the IV estimate of the skill effect increases to 0.25 SD higher numeracy skills.

Substantial attenuation bias may be present in estimated reform effects in PW using the QaC and imputed years of education. In their analysis of wage returns to the German compulsory schooling reform, the estimated increase in length of schooling for the sample of basic track students only amounts to 0.29 years. According to the authors, this hints to a sizable attenuation bias due to classical measurement problems. Such measurement error is caused, among others, because PW only observe the current federal state of residence instead of the state of school attendance. Hence, migration may lead to measurement error in the instrumental variable. However, even non-classical measurement error may arise in case of non-random migration of individuals across federal states. The present study provides evidence on the size of the bias due to migration. Table 2.9, Panel A, shows first-stage reform effects when using the individual's current state of residence for the identification of the compulsory schooling regime (Column 1). Results suggest a sizable attenuation bias due to measurement error. The schooling effect of the additional ninth grade shrinks to almost half of the initial size with a very low F statistic. Attenuation bias due to inaccurate measurement of the respective federal state for identification of the reform status seems to be present for short school years as well.[41] Even though it is important to keep the presence of measurement error in mind, this does not necessarily lead to a biased IV estimation of skill effects, since the discussed measurement error refers to the instrument. Hence, IV estimates are still unbiased as long as the measurement error is not systematically correlated with the reform and skills (Pischke and von Wachter, 2008). However, IV estimates of the reform effect on skills suggest the opposite – the coefficient becomes insignificant and shrinks to 0.06 SD. Thus, severe attenuation bias may result in an underestimation of the true reform effect on skills.

In the preferred sample of interest, information about the federal state of school attendance is missing for a small fraction of basic track students.[42] Thus, the reform status for these observations is identified based on the federal state at the time of the interview. Excluding these individuals decreases the estimated reform effect on schooling slightly (Column 2). Students affected by the reform stay in school for 0.90 years longer than students under the old compulsory schooling regime. The estimated skill effect of 0.20 SD higher numeracy skills

[40] Please note that the two city states do not contribute to the identification of the reform effect because I only observe individuals after the reform in these two states.

[41] Specifications using hypothetical weeks of schooling suffer from similar measurement error – the coefficient of the reform effect on length of schooling is reduced by 37 percent when identifying the treatment status by federal state of residence at the time of the survey (not shown).

[42] This is the case for 18 basic track students (65 observations).

is quantitatively identical to the estimate using the preferred sample specification. However, it cannot be measured precisely due to the loss of 9 percent of the sample.

As emphasized in section 2.5, one may think of two potential mechanisms underlying positive skill effects of education. First, students might be taught skill-enhancing content during the additional year of schooling which increases cognitive skills. Second, skills may not be acquired directly during the additional year of instruction but labor-market success of students in the new schooling regime may have increased, which positively affects skill formation measured by PIAAC test scores. I cannot disentangle the two mechanisms directly due to the fact that skills in PIAAC are measured many years after labor-market entry. However, some individuals in PIAAC report the grade they have received in their final school exit examination in Math and German. Table A2.9 shows IV estimates of the effect of compulsory schooling on exit exam grades. German school grades range from 1 (very good) to 6 (insufficient). Hence, a negative coefficient is interpreted as grade improvement. Results suggest that the reform improved slightly students' performance in Mathematics, while German exit exam grades are unaffected by the reform. Due to the large number of missing grade information and potentially large measurement error, exam grade effects cannot be estimated precisely.[43] Hence they should be seen as rather suggestive evidence that part of the skill effect of the compulsory schooling reform is already generated in school.

2.7 Conclusion

The study analyzes skill effects of a compulsory schooling reform in West Germany between 1946 and 1969. Findings suggest that the introduction of a mandatory ninth grade significantly increased the length of schooling for basic track students. This additional education led to higher numeracy skills that are still measureable around four decades after the reform. Thus, results challenge previous evidence on returns to education exploiting the same reform while neglecting important institutional features. The hypothesis of PW and Kamhöfer and Schmitz (2015), who argue that the introduction of the compulsory ninth grade in German basic track schools was unable to raise labor-market relevant skills, cannot be supported by the results presented here. Thus, I contribute to the discussion of the effectiveness of compulsory schooling reforms with respect to labor-market relevant skills in a country characterized by a particular education system with early tracking and a unique apprenticeship system. Beyond that, the analysis provides new evidence on the causal link between education and skill formation.

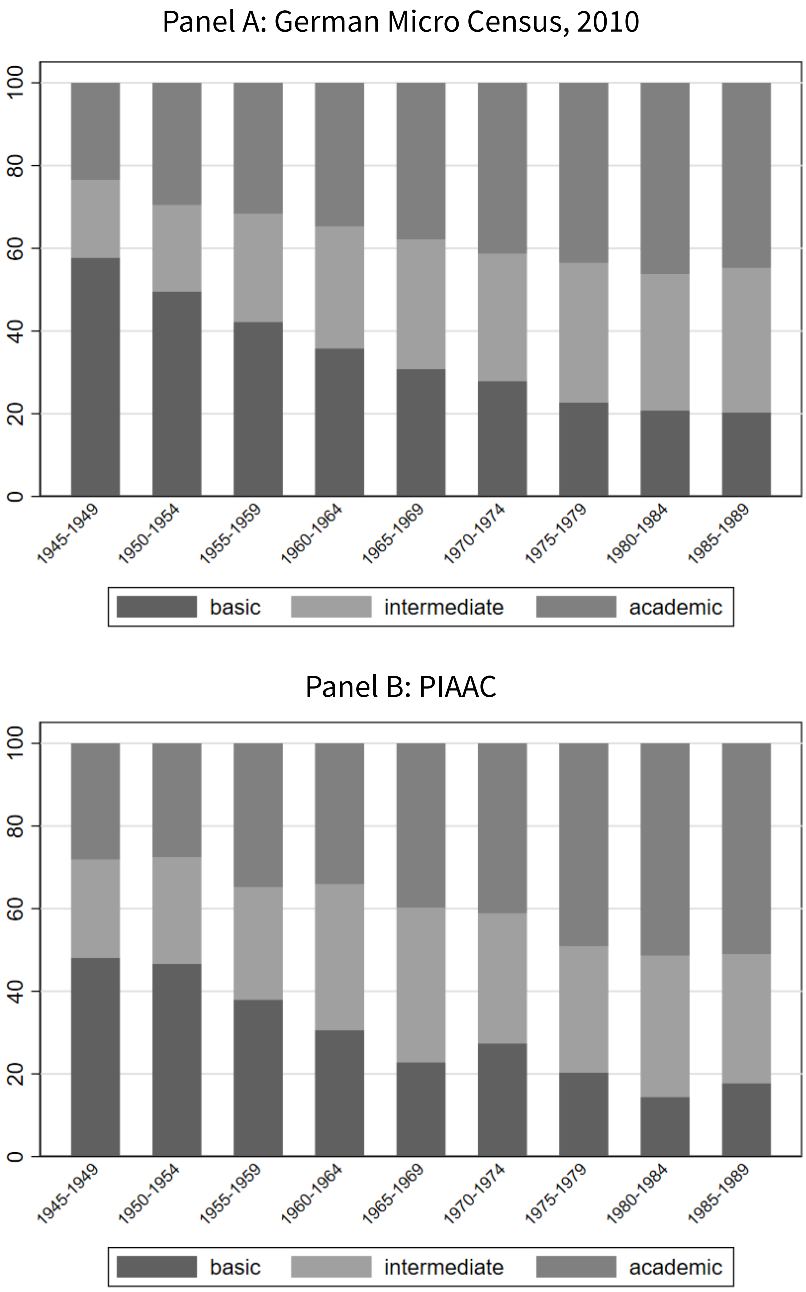
While the IV estimate on the skill effect of compulsory schooling presents a local average treatment effect for a specific subgroup of students, the complier population of the German reform constitutes an interesting sub-population. The reform was targeted at basic track

[43] I assume large measurement error due to the fact that PIAAC respondents report school exit exam grades many years after graduation. Thus, they may not remember grades accurately.

students in Germany. While compliers in other countries are students who would drop out of school after the end of compulsory schooling, German basic track students were entirely affected by the reform, because the school track ended after those compulsory schooling years and transitions to higher secondary school track was very rare. As Figure 2.1 shows, the share of students affected by the reform was notably large because a majority of students in secondary schools attended basic track at the time of the compulsory schooling reforms. In the context of ongoing technological change and the associated increase in labor markets' demand for high-skilled workers, this study implies that schooling expansions may be an efficient way to equip new generations of workers with marketable skills, which are essential for long-run labor-market success. Nevertheless, the effect of such reforms is always dependent on the student population affected by the reform. Thus, effects on skills may vary when length of schooling is increased for students attending higher school tracks. Furthermore, the better the schools and teachers are prepared for the reform, the more effective is the reform. Scarcity of resources (e.g., insufficient number of teachers) and poorly designed curricula may prevent positive reform effects.

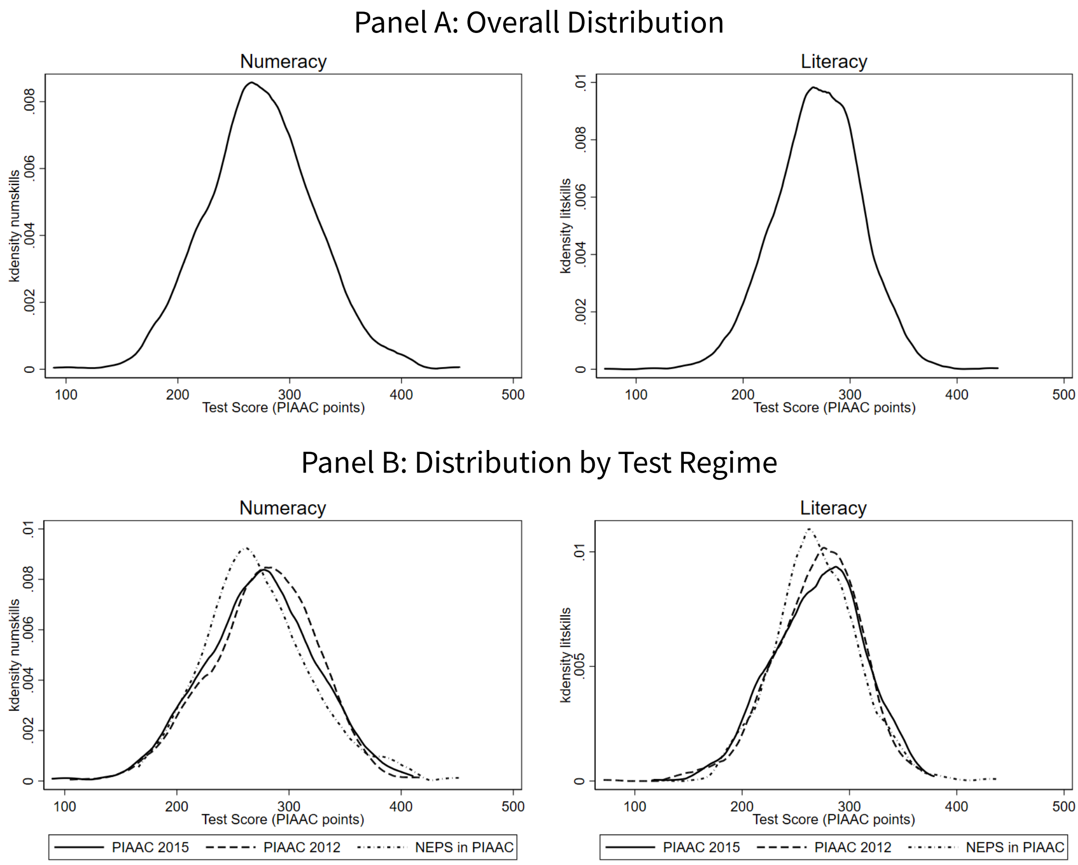
Figures and Tables

Figure 2.1 : Share of Students Across Secondary School Tracks



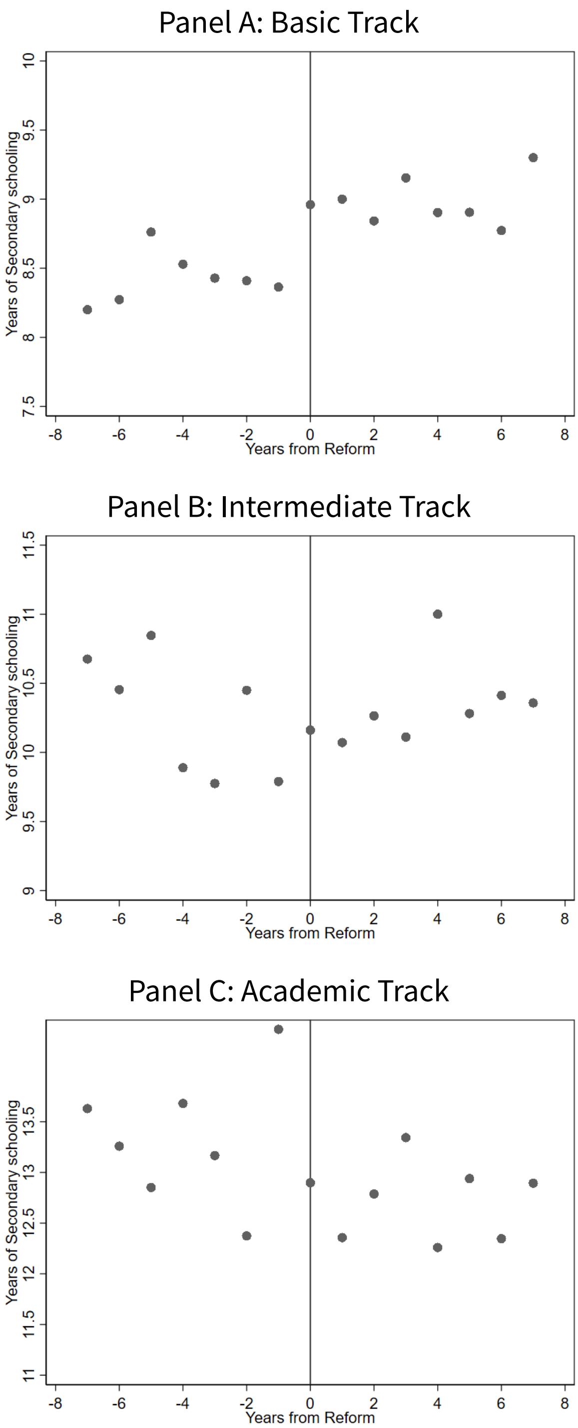
Notes: Share of students in each secondary school track by 5-year age cohorts, based on highest school degree achieved. Sample: birth cohorts born between 1945 and 1989. *Data source:* German Micro Census 2010, PIAAC.

Figure 2.2 : Distribution of PIAAC Skill Scores



Notes: Distribution of numeracy and literacy skills after re-scaling exercise. Sample: individuals born between 1945-1960 in West Germany. Panel A: overall distribution of skills. Observations for numeracy (literacy): 1,774 (1,786). Panel B: Distribution of skills, by test regime. Observations for numeracy (literacy): 625 (625) in PIAAC 2012; 625 (625) in PIAAC 2015; 524 (536) in NEPS test in PIAAC 2015. *Data source:* PIAAC.

Figure 2.3 : Average Years of Schooling, by Secondary School Track



Notes: Average years in secondary school in years before and after the compulsory schooling reform. Sample: individuals born between 1945-1960 in West Germany; basic track students (Panel A), intermediate track students (Panel B), academic track students (Panel C). *Data source:* PIAAC.

Table 2.1 : Introduction of a Compulsory Ninth Grade in German Basic Track Schools

Federal State	First year basic track students graduated after 9th grade	First birth cohort with compulsory 9th grade	Short school years
Hamburg	1946	1931	no
Schleswig-Holstein	1947	1932	yes
Saarland	1958	1943	yes
Bremen	1959	1944	yes
Niedersachsen	1962	1947	no
Nordrhein-Westphalen	1967	1953	yes
Hessen	1967	1953	yes
Rheinland-Pfalz	1967	1953	yes
Baden-Württemberg	1967	1953	yes
Bayern	1969	1955	no

Notes: The table reports the year of the introduction of the compulsory ninth grade in basic track schools (Column 1), and the first birth cohort affected by the reform (Column 2), for each federal state in West Germany. Column 3 reports whether that federal state introduced two short school years between april 1966 and july 1967. *Data source:* Information on compulsory schooling reforms based on Cygan-Rehm (2018), Piopiunik (2014), Leschinsky and Roeder (1980), and Petzold (1981). Information on short school years based on Pischke (2007) and Cygan-Rehm (2018).

Table 2.2 : Descriptive Statistics

	all tracks	basic track
Years of schooling (primary & secondary)	10.40	8.79
Numeracy skills	274.0	253.8
Literacy skills	269.9	249.8
Female	.51	.50
Year of birth	1954	1953
Basic track	.43	
Intermediate track	.29	
Academic track	.28	
Individuals	850	364
Observations	3553	1487

Notes: Mean values of outcome and control variables, for the entire sample (Column 1) and for sample of basic track students (Column 2). Years of schooling refer to years in primary and secondary school. Numeracy and literacy skills are measured on a 500-points scale. Basic/intermediate/academic track indicators equal 1 if the individual's highest secondary school degree is a basic/intermediate/academic degree, zero otherwise. Number of individuals differs from number of observations because individuals may appear up to three times in the sample due to multiple skills testing (see Section 2.4). *Data source:* PIAAC.

Table 2.3 : Reform Effect on Length of Schooling

	Dependent variable: length of schooling		
	(1)	(2)	(3)
Panel A: compulsory schooling reform & short school years			
Compulsory Schooling Reform	.714***	.969***	.736***
	(.182)	*(.164)*	*(.207)*
Short School Year		−.762***	−.582**
		(.184)	*(.245)*
Panel B: hypothetical weeks of schooling			
Weeks of schooling		.027***	.021***
		(.004)	*(.006)*
State-specific linear trend	No	No	Yes
Individuals	364	364	364
Observations	1487	1487	1487

Notes: Ordinary least squares estimation. Dependent variable: length of schooling (years in primary and secondary school). Sample: individuals born between 1945-1960 in West Germany, who attended basic track schools. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 3 includes state-specific linear trends. Robust standard errors clustered at state x year of birth cells (100 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table 2.4 : Reform Effect on Numeracy Skills – Reduced-Form Analysis

	Numeracy Skills		Numeracy Rank	
	(1)	(2)	(3)	(4)
Panel A: effect of compulsory schooling reform & short school years				
Compulsory Schooling Reform	.201*	.205	.085**	.074
	(.118)	*(.163)*	*(.040)*	*(.056)*
Short School Year	–.134	–.246**	–.049	–.088**
	(.094)	*(.100)*	*(.032)*	*(.034)*
Panel B: effect of hypothetical weeks of schooling				
Weeks of schooling	.005**	.007*	.002**	.002*
	(.003)	*(.004)*	*(.001)*	*(.001)*
State-specific linear trend	No	Yes	No	Yes
Individuals	306	306	306	306
Observations	746	746	746	746

Notes: Ordinary least squares estimation. Dependent variable: z-standardized numeracy skills (Column 1, 2) and percentile rank (Column 3, 4). Sample: individuals born between 1945-1960 in West Germany, who attended basic track. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 2 and 4 include state-specific linear trends. Robust standard errors clustered at state x year of birth cells (100 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table 2.5 : Reform Effect on Literacy Skills – Reduced-Form Analysis

	Literacy Skills		Literacy Rank	
	(1)	(2)	(3)	(4)
Panel A: effect of compulsory schooling reform & short school years				
Compulsory Schooling Reform	.156	.241	.046	.052
	(.176)	*(.253)*	*(.057)*	*(.077)*
Short School Year	.112	.029	.029	.001
	(.137)	*(.139)*	*(.046)*	*(.046)*
Panel B: effect of hypothetical weeks of schooling				
Weeks of schooling	.002	.004	.001	.001
	(.004)	*(.006)*	*(.001)*	*(.002)*
State-specific linear trend	No	Yes	No	Yes
Individuals	301	301	301	301
Observations	741	741	741	741

Notes: Ordinary least squares estimation. Dependent variable: z-standardized literacy skills (Column 1, 2) and percentile rank (Column 3, 4). Sample: individuals born between 1945-1960 in West Germany, who attended basic track. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 2 and 4 include state-specific linear trends. Robust standard errors clustered at state x year of birth cells (100 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table 2.6 : Effect of Compulsory Schooling on Numeracy Skills – 2SLS Analysis I

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
Panel A: First-Stage				
Compulsory Schooling Reform	.969***	.738***	.969***	.738***
	(.166)	*(.210)*	*(.166)*	*(.210)*
Short School Year	−.763***	−.575**	−.763***	−.575**
	(.187)	*(.247)*	*(.187)*	*(.247)*
Dependent variable:	Numeracy Skills		Numeracy Rank	
	(1)	(2)	(3)	(4)
Panel B: Second-Stage				
Years of schooling	.207*	.277	.088**	.101
	(.119)	*(.224)*	*(.041)*	*(.079)*
State-specific linear trend	No	Yes	No	Yes
Individuals	306	306	306	306
Instrument F statistic	34.22	12.35	34.22	12.35
Observations	746	746	746	746

Notes: Two-stage least squares estimation. Dependent variable Panel A: length of schooling (years in primary and secondary school). Dependent variable Panel B: z-standardized numeracy skills (Column 1, 2) and percentile rank (Column 3, 4). Sample: individuals born between 1945-1960 in West Germany, who attended basic track. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 2 and 4 include state-specific linear trends. Robust standard errors clustered at state x year of birth cells (100 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table 2.7 : Effect of Compulsory Schooling on Numeracy Skills – 2SLS Analysis II

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
Panel A: First-Stage				
Weeks of schooling	.027***	.021***	.027***	.021***
	(.004)	*(.006)*	*(.004)*	*(.006)*
Dependent variable:	Numeracy Skills		Numeracy Rank	
	(1)	(2)	(3)	(4)
Panel B: Second-Stage				
Years of schooling	.197**	.325	.080**	.117*
	(.096)	*(.199)*	*(.034)*	*(.071)*
State-specific linear trend	No	Yes	No	Yes
Individuals	306	306	306	306
Instrument F statistic	41.13	12.46	41.13	12.46
Observations	746	746	746	746

Notes: Two-stage least squares estimation. Dependent variable Panel A: length of schooling (years in primary and secondary school). Dependent variable Panel B: z-standardized numeracy skills (Column 1, 2) and percentile rank (Column 3, 4). Sample: individuals born between 1945-1960 in West Germany, who attended basic track. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 2 and 4 include state-specific linear trends. Robust standard errors clustered at state x year of birth cells (100 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table 2.8 : Effect of Compulsory Schooling on Numeracy Skills I (Various Samples)

	Excl. 1st cohort	Excl. city states	1945 – 1970
Panel A: First-Stage (Dependent Variable: Years of Schooling)			
Compulsory Schooling Reform	.940***	.967***	.899***
	(.215)	*(.167)*	*(.161)*
Short School Year	−.792***	−.765***	−.940***
	(.195)	*(.187)*	*(.163)*
Panel B: Second-Stage (Dependent Variable: Numeracy Skills)			
Years of schooling	.321***	.208*	.251*
	(.123)	*(.120)*	*(.132)*
Instrument F statistic	19.16	33.33	31.26
Observations	694	734	1199

Notes: Two-stage least squares estimation. Dependent variable Panel A: length of schooling (years in primary and secondary school). Dependent variable Panel B: z-standardized literacy skills. Sample: individuals born between 1945-1960 in West Germany, who attended basic track. Column 1 excludes first cohort affected by the reform; Column 2 excludes city states; Column 3 extends sample to cohorts born between 1945 and 1970. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Robust standard errors clustered at state x year of birth cells. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table 2.9 : Effect of Compulsory Schooling on Numeracy Skills (Samples Based on State Identifiers)

	State of Residence Today	No Approx. State
Panel A: First-Stage (Dependent Variable: Years of Schooling)		
Compulsory Schooling Reform	.525**	.898***
	(.250)	*(.169)*
Short School Year	−.630***	−.748***
	(.220)	*(.169)*
Panel B: Second-Stage (Dependent Variable: Numeracy Skills)		
Years of schooling	.056	.201
	(.301)	*(.135)*
Instrument F statistic	4.41	28.26
Observations	638	681

Notes: Two-stage least squares estimation. Dependent variable Panel A: length of schooling (years in primary and secondary school). Dependent variable Panel B: z-standardized literacy skills. Sample: individuals born between 1945-1960 in West Germany, who attended basic track. Column 1 uses federal state of residence today for identification of the compulsory schooling regime; Column 2 excludes observations, for which federal state of school attendance is approximated by federal state of residence today. All specifications include a gender control, year of birth FE, federal state of school attendance FE, and dummies for each test regime. Robust standard errors clustered at state x year of birth cells. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Appendix

Appendix A2.1 Replication – Effect of the Compulsory Schooling Reform on Length of Schooling Using NEPS

In this section, I replicate the analysis of the effect of the German compulsory schooling reform on length of schooling using data from the *National Educational Panel Study (NEPS): Starting Cohort Adults*.[1] In what follows, I will introduce the data before summarizing the results on the reform effect on years of schooling. I will conclude with a brief discussion why a replication analysis of the reform effect on skill formation by means of currently available skill information in NEPS is not possible.

Data

NEPS was designed to provide a better understanding of adult education and lifelong learning in Germany. Besides an extensive background questionnaire, the data comprises information on educational and professional careers for individuals aged 30 to 73 years (as of 2017).[2] I impose identical restrictions to the NEPS sample as to PIAAC – individuals born between 1945 and 1960 in West Germany – to guarantee a high degree of comparability between data sets. This leaves me with a sample of 3233 observations across all tracks and 1099 observations in basic track, which is substantially larger than the preferred PIAAC sample.[3]

Table A2.10 reports sample means of outcome and control variables. NEPS provides information about the educational career of individuals in the form of spell data comprising each educational episode separately (e.g., two schooling episodes: primary education and secondary education). Thus, each schooling episode is reported on a monthly basis, including the start and end of each school episode. Here, the sum of exact months spent in school is used to compute years of schooling (divided by 12 and rounded to full years). Across all tracks,

[1] National Educational Panel Study (NEPS): Starting Cohort 6 – Adults (Adult Education and Lifelong Learning), doi:10.5157/NEPS:SC6:3.0.1. The NEPS data collection is part of the Framework Programme for the Promotion of Empirical Educational Research, funded by the German Federal Ministry of Education and Research and supported by the Federal States.

[2] From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network (Blossfeld, 2011). The field time of the NEPS Adult Survey already started in 2007, prior to the foundation of the National Educational Panel Study. The adult survey 2007/08 was conducted by the Institute for Employment Research (IAB) under the name of “Working and Learning in a Changing World” (ALWA). After that, the data collection of the adult survey continued under the umbrella of the NEPS. Until the end of 2018, nine waves of the panel were released in a Scientific Use File.

[3] For the analysis using NEPS data, the number of observations equals the number of individuals because I focus on first-stage estimation results (the expansion of the PIAAC sample was justified by the availability of multiple skill measures per individual).

individuals attend school for almost 10.4 years, which is identical to the average length of schooling in PIAAC. Basic track students leave school on average after 8.6 years. 37 percent of individuals in NEPS attended basic track, which is substantially smaller than the share of basic track students in PIAAC. Figure A2.3 illustrates the share of students in each secondary schools track across 5-year birth cohorts from 1945 to 1989. The general pattern of development of track shares in NEPS is comparable to official statistics from the German Micro Census (Panel A). However, individuals in NEPS are less likely to attend basic track during the observational period. While 44 percent of individuals born between 1945 and 1949 attended basic track, the share decreases to less than 26 percent in 1955–1959. In contrast, the share of basic track students in the German Micro Census, which is a representative sample of the German population, dropped from 58 percent to 42 percent during the same period.

Reform Effects on Educational Attainment

Figure A2.4 displays average years of schooling before and after the introduction of the additional ninth grade. On average across the seven years prior to the reform, basic track students report less than 8.3 years of education. After the reform, length of schooling increased to 8.9 years, with a visible jump in the first year of the introduction of the compulsory ninth grade. Such a pattern is only observable for the sample of basic track students, while length of schooling did not change for students in intermediate or academic track.

Table A2.11 reports coefficients of the reform effect on educational attainment. Following the empirical strategy outlined in Section 2.5, all specifications include dummies for gender, birth cohort, federal state, and a dummy indicating whether the federal state of school attendance is approximated by the federal state of residence at the time of the survey.[4] Furthermore, all regressions include dummies for each NEPS subsample. The NEPS Adult Cohort consists of four subgroups: ALWA sample (respondents were initially surveyed for the ALWA survey and later transferred into NEPS, birth cohorts 1956–1986); W1 Refreshment sample (appended observations to the original ALWA sample in NEPS wave 1, birth cohorts 1956–1986); W1 Augmentation sample (extension of included birth cohorts in NEPS wave 1, birth cohorts 1944–1955); W3 Refreshment sample (appended observations to NEPS sample in wave 3, birth cohorts 1944–1986).

Results reported in Panel A, Column 1, suggest that the reform led to 0.44 years more schooling, when neglecting the potential exposure to the short school years. When accounting for this institutional particularity, the reform effect increases to 0.53 years (Column 2). Being affected by the short school years decreases length of schooling by 0.24 years. When allowing for differential developments across states in Column 3, the effect of the compulsory schooling reform decreases slightly and the effect of the introduction of the short school years is essentially zero and insignificant. A similar picture arises when substituting the reform indicator by

[4] This is the case for 57 observations in the basic track sample (5.5 percent).

hypothetical weeks of schooling in Panel B. One more hypothetical week of schooling is associated with 0.013 more years of schooling. Put differently, a hypothetical one year increase (37 weeks) leads to a 0.48 year increase in actual schooling. Accounting for state-specific linear trends decreases the coefficient to 0.3 years for one additional hypothetical year of schooling.

Compared to the first-stage estimation results using PIAAC data (Table 2.3), the reform effect on length of schooling is much smaller when replicated with NEPS. One potential reason underlying the differential findings is attenuation bias due to measurement error in the variable indication length of schooling in NEPS. The two data sets differ quite substantially with respect to gathering information about the individual's educational career. PIAAC asks respondents about their highest secondary school degree as well as the year of graduation – information that respondents can (rather easily) remember, even many years after graduation. In contrast, NEPS participants are required to recall their entire life when surveyed for the first time. They need to report the start and end (exact to the month) of each episode (spell) in their educational career. Reporting the entire life history is probably much more demanding and hence may be more prone to measurement error.[5]

Table A2.12 reports regression results on the reform effect on the probability to attend a specific secondary school track. Students affected by the reform were not less likely to attend basic track schools, which contradicts findings in Cygan-Rehm (2018) that the reform led substantially more students to choose the higher track. In contrast, evidence from the analysis of NEPS suggests that students were less likely to attend academic track after the reform. In specifications using state-specific linear trends (Column 2), this effect is significant on the 10 percent level. Basic and intermediate track schools seem to become more popular.

Although the coefficient on basic track attendance is not significant, concerns may arise that an independent analysis of the reform effect for the sample of basic track students may not yield causal estimates if the reform changed the student composition. Thus, Table A2.13 reports reform effects for the sample of students across all tracks. In my preferred specification, the compulsory schooling reform raised length of schooling by 0.24 years. This corresponds to 0.65 years, when dividing the coefficient by the share of basic track students (37 percent).

The effect of the German compulsory schooling reform on length of schooling increases to 0.64 years when excluding individuals with approximated indicators of federal state of school attendance.[6] As mentioned in Section 2.3.2, applied reform years differ slightly across studies. When using reform years based on PW and Kamhöfer and Schmitz (2015), the reform status of 28 basic track students changes in the present study and estimated reform effects on length

[5] Another reasonable explanation for the lower reform effect may be the approximation of years of schooling by dividing months of schooling by 12 and rounding to integer numbers. However, estimating to reform effect on months of schooling yields qualitatively similar results.

[6] The coefficient increases to 0.59 years in the specification controlling for state-specific linear trends (not shown).

of schooling decrease to 0.43 years (0.33 years with state-specific linear trend). This suggests substantial inaccuracy in the treatment assignment of previous studies.

Reform Effects on Skills

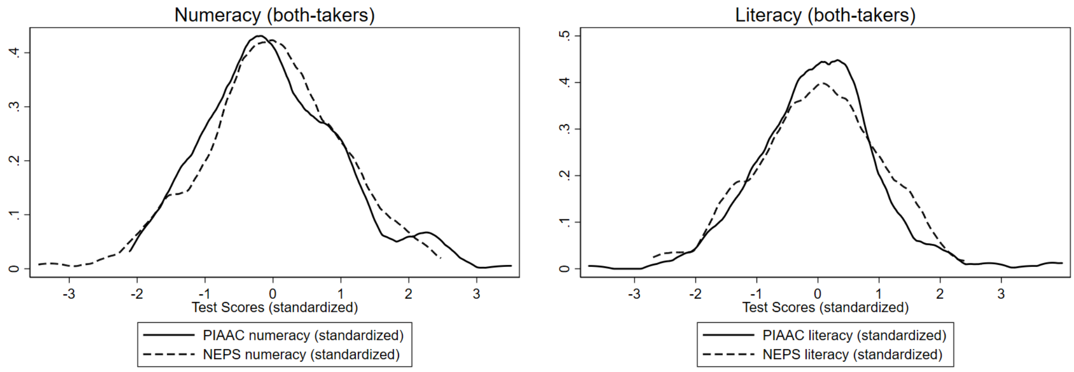
Beside extensive information on education and working careers of individuals in the NEPS adult cohort, the data also comprises skill measures in various domains, such as reading, mathematics, sciences, and ICT literacy. Unfortunately, I cannot use this information to replicate the analysis of the effect of the German compulsory schooling reform on skill formation due to various reasons. First and foremost, not all individuals in NEPS were tested in the same wave across all skill domains. Reading and mathematics – which best represent the PIAAC skill domains of numeracy and literacy skills (see skills tests of household members in PIAAC) – were tested only in wave 3 and wave 9. Reading skills were additionally tested in wave 5, but only for individuals of the subsample “W3 Refreshment sample”. The most recently released wave 9 was the first, in which a small fraction of individuals from all subsamples took the reading and mathematics test (NEPS, 2018).

Second, exogenous variation in length of schooling stemming from the increase in compulsory years of schooling is only based on two subsamples of NEPS (W1 Augmentation and W3 Refreshment). The ALWA sample as well as the W1 Refreshment sample include individuals born earliest in 1956. However, the youngest birth cohort in Germany under the old compulsory schooling regime of eight years were students born in Bavaria in 1954 (see Table 2.1). The subsample “W1 Augmentation sample” extended the NEPS sample by including birth cohorts born between 1944 and 1955. Thus, only the two subsamples W1 Augmentation and W3 Refreshment contribute to the identifying variation in length of schooling exploited in the IV analysis of the reform effect on skills.

Third, a comprehensive documentation of skill measures and their comparability across waves is not yet released, which limits the potential of using NEPS skill measures across waves in the empirical application. Based on these limitations, only skill measures available in wave 9 could be used for an analysis of the skill effect of the compulsory schooling reform in Germany. This limits the sample size drastically. Table A2.14 reports corresponding IV results on numeracy (mathematics) and literacy (reading) skills, assessed in wave 9. Panel A reports first-stage coefficients of the reform effect on length of schooling, for the adjusted sample of individuals with available skill information in each domain. The reform had a strong effect on years of schooling for basic track students affected by the reform and F statistics above 20 suggest a strong instrument. However, 2SLS results in Panel B suggest no significant skill effect, which is most likely due to high standard errors in this relatively small sample.

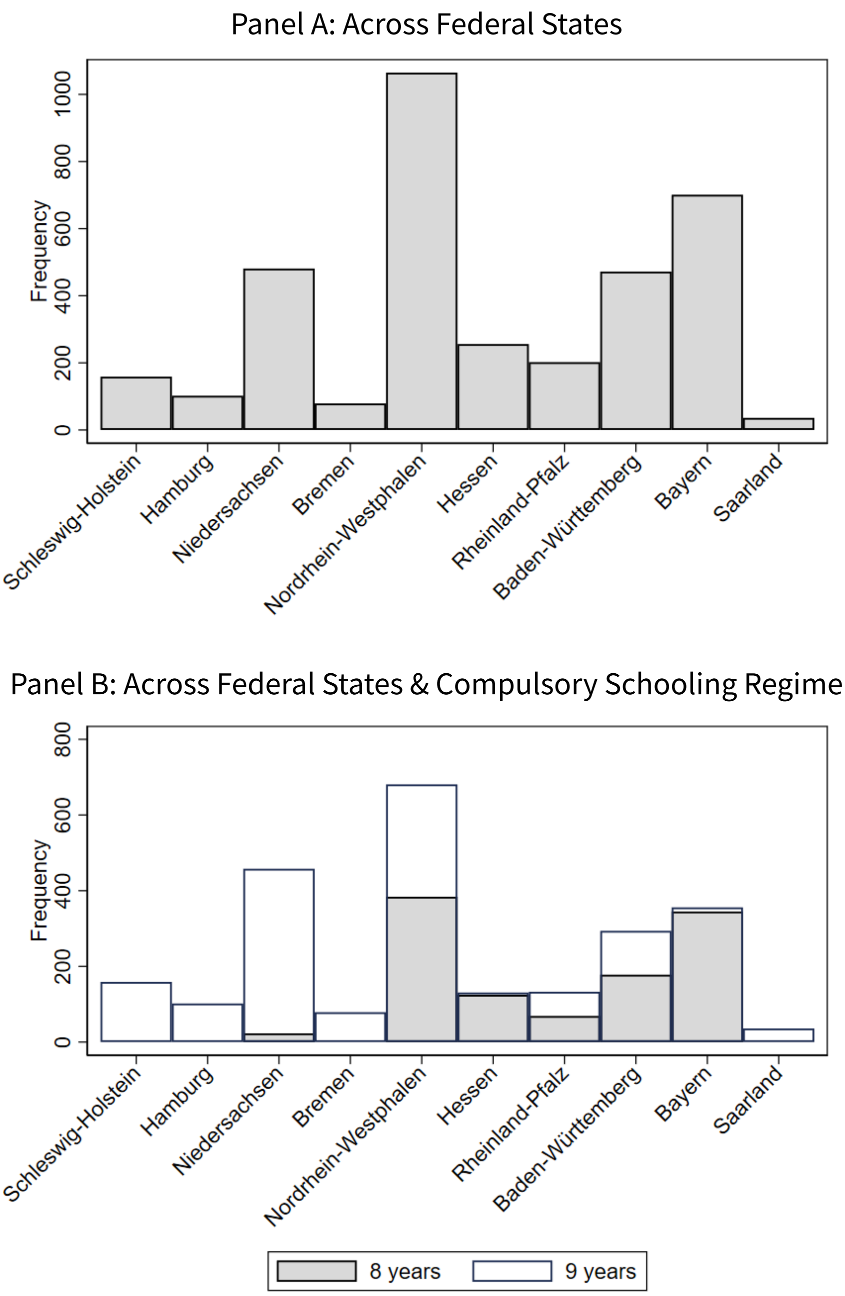
Appendix A2.2 Appendix Figures and Tables

Figure A2.1 : Distribution of PIAAC & NEPS Standardized Test Scores of Both-Takers in PIAAC 2015



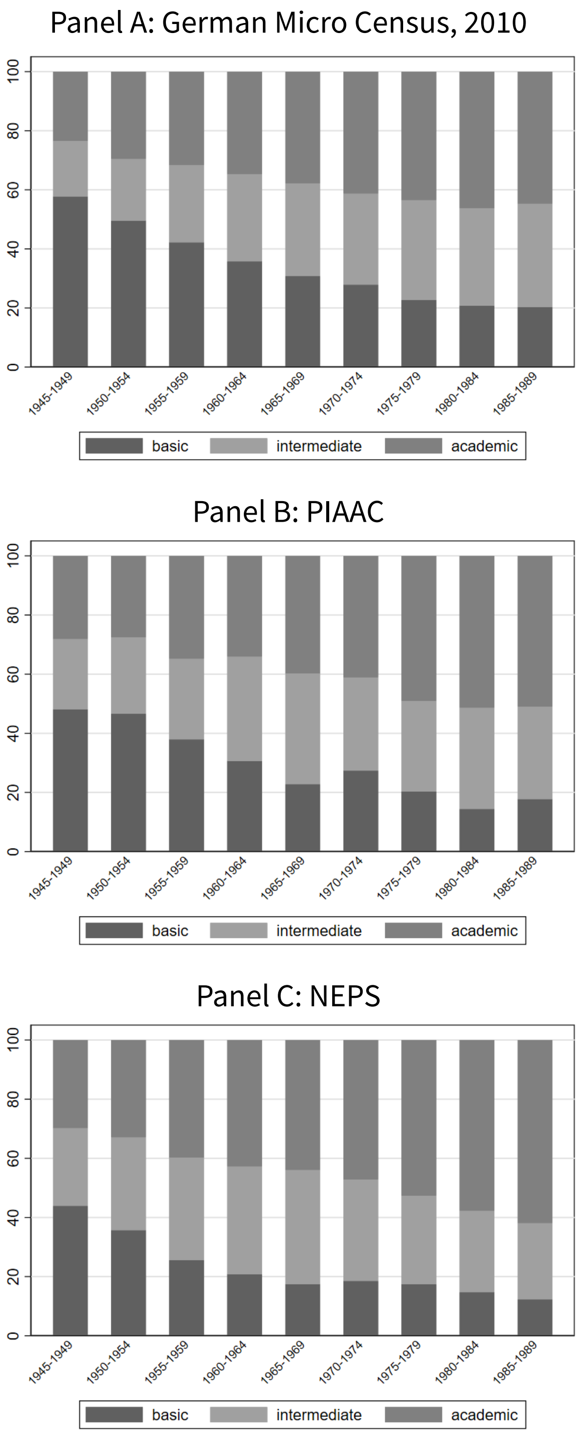
Notes: Distribution of PIAAC and NEPS numeracy and literacy skills (standardized on sample of both-takers for each skill domains). Sample: individuals born between 1945-1960 in West Germany, who were tested by means of PIAAC as well as NEPS test instruments in PIAAC 2015. *Data source:* PIAAC.

Figure A2.2 : Distribution of Observations Across Federal States



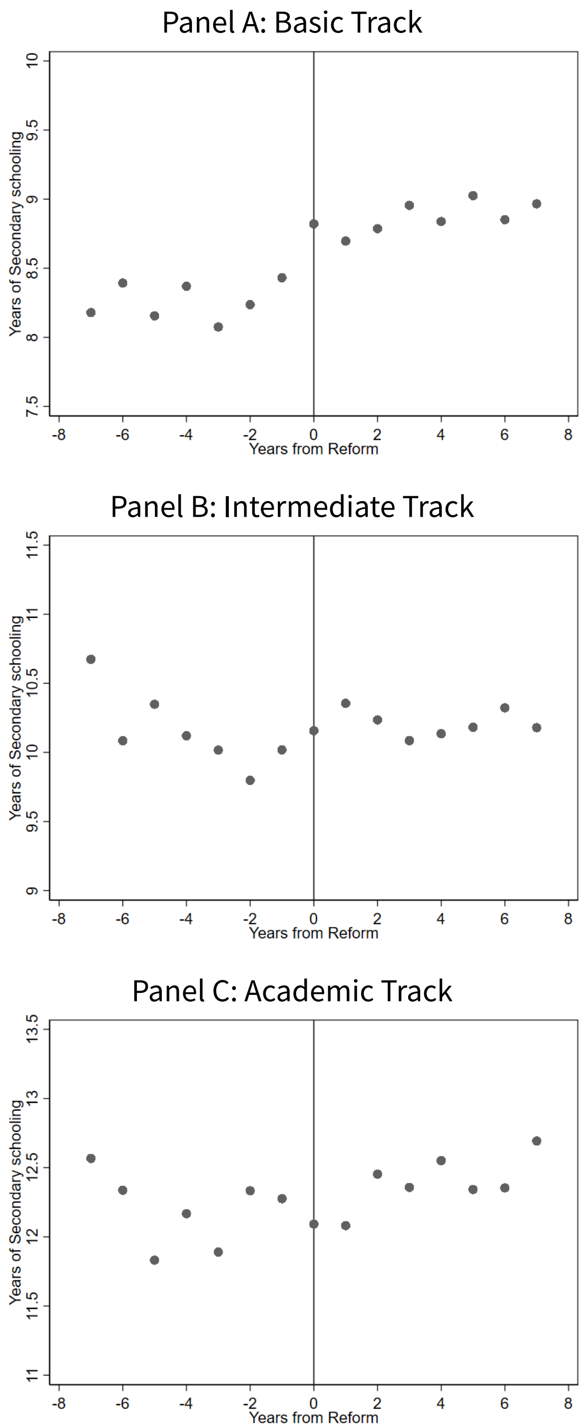
Notes: Panel A: distribution of observations across federal states. Panel B: distribution of observations in each compulsory schooling regime across federal states. Sample: individuals born between 1945-1960 in West Germany. *Data source:* PIAAC.

Figure A2.3 : Share of Students Across Secondary School Tracks – NEPS Analysis



Notes: Share of students in each secondary school track by 5-year age cohorts, based on highest school degree achieved. Sample: birth cohorts born between 1945 and 1989. *Data source:* German Micro Census 2010, PIAAC, NEPS.

Figure A2.4 : Average Years of Schooling, by Secondary School Track – NEPS Analysis



Notes: Average years in secondary school in years before and after the compulsory schooling reform. Sample: individuals born between 1945-1960 in West Germany; basic track students (Panel A), intermediate track students (Panel B), academic track students (Panel C). *Data source:* NEPS.

Table A2.1 : Dropped Observations due to Sample Restrictions

	Observations	Dropped observations	Share of original sample
initial data:	16133		
West Germany only:	13555	2578	84.0
born 1945 - 1960:	3553	10002	22.0

Notes: Number of observations, number of dropped observations and share of initial sample lost due to imposed sample restrictions.
Data source: PIAAC.

Table A2.2 : Reform Effect on Track Attendance

	(1)	(2)
Panel A: Dependent variable: individual attends basic track		
Compulsory Schooling Reform	.016	.022
	(.079)	*(.106)*
Panel B: Dependent variable: individual attends intermediate track		
Compulsory Schooling Reform	.000	.067
	(.059)	*(.092)*
Panel C: Dependent variable: individual attends academic track		
Compulsory Schooling Reform	−.016	−.089
	(.068)	*(.092)*
State-specific linear trend	No	Yes
Observations	3553	3553

Notes: Ordinary least squares estimation. Dependent variable: Dummy which equals 1 if the individual attended basic track (Panel A), intermediate track (Panel B), or academic track (Panel C). Sample: individuals born between 1945-1960 in West Germany. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Robust standard errors clustered at state x year of birth cells (142 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table A2.3 : Reform Effect on Length of Schooling, All Tracks

	Dependent variable: length of schooling		
	(1)	(2)	(3)
Panel A: compulsory schooling reform & short school years			
Compulsory Schooling Reform	.234	.284	.556***
	(.183)	*(.183)*	*(.212)*
Short School Year		−.316**	−.237
		(.147)	*(.168)*
Panel B: hypothetical weeks of schooling			
Weeks of schooling		.019***	.020***
		(.004)	*(.004)*
State-specific linear trend	No	No	Yes
Individuals	852	852	852
Observations	3553	3553	3553

Notes: Ordinary least squares estimation. Dependent variable: length of schooling (years in primary and secondary school). Sample: individuals born between 1945-1960 in West Germany. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, a track indicator and an indicator whether the federal state of school attendance is approximated by state of residence today. Robust standard errors clustered at state x year of birth cells (142 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table A2.4 : Reform Effect on Length of Schooling, by Secondary School Track

	Dependent variable: length of schooling		
	Basic track sample	Intermediate track sample	Academic track sample
Panel A: compulsory schooling reform & short school years			
Compulsory Schooling Reform	.969***	.114	−.367
	(.164)	*(.294)*	*(.535)*
Short School Year	−.762***	−.154	.160
	(.184)	*(.337)*	*(.630)*
Panel B: hypothetical weeks of schooling			
Weeks of schooling	.027***	.005	−.007
	(.004)	*(.013)*	*(.025)*
Individuals	364	245	243
Observations	1487	1023	1043

Notes: Ordinary least squares estimation. Dependent variable: length of schooling (years in primary and secondary school). Sample: individuals born between 1945-1960 in West Germany, by track, as indicated in column header. All specifications control for the exposure to short school years, include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Robust standard errors clustered at state x year of birth cells. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table A2.5 : The Relationship Between Length of Schooling and Skills

	Numeracy	Numeracy	Numeracy Rank	Numeracy Rank
Years of schooling	.118***	.115***	.038***	.038***
	(.012)	*(.012)*	*(.004)*	*(.004)*
Observations	1770	1770	1770	1770
	Literacy	Literacy	Literacy Rank	Literacy Rank
Years of schooling	.139***	.136***	.045***	.044***
	(.013)	*(.013)*	*(.004)*	*(.004)*
State-specific linear trend	No	Yes	No	Yes
Observations	1783	1783	1783	1783

Notes: Ordinary least squares estimation. Dependent variable: z-standardized numeracy and literacy skills (Column 1, 2) and percentile rank (Column 3, 4), as indicated in the column header. Sample: individuals born between 1945-1960 in West Germany. All specifications control for the exposure to short school years, include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 2 and 4 include state-specific linear trends. Robust standard errors clustered at state x year of birth cells. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table A2.6 : Effect of Compulsory Schooling on Literacy Skills – 2SLS Analysis I

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
Panel A: First-Stage				
Compulsory Schooling Reform	.961***	.720***	.961***	.720***
	(.165)	*(.207)*	*(.165)*	*(.207)*
Short School Year	–.762***	–.591**	–.762***	–.591**
	(.185)	*(.250)*	*(.185)*	*(.250)*
Dependent variable:	Literacy Skills		Literacy Rank	
	(1)	(2)	(3)	(4)
Panel B: Second-Stage				
Years of schooling	.163	.335	.048	.072
	(.178)	*(.365)*	*(.058)*	*(.107)*
State-specific linear trend	No	Yes	No	Yes
Individuals	301	301	301	301
Instrument F statistic	34.03	12.09	34.03	12.09
Observations	741	741	741	741

Notes: Two-stage least squares estimation. Dependent variable Panel A: length of schooling (years in primary and secondary school). Dependent variable Panel B: z-standardized literacy skills (Column 1, 2) and percentile rank (Column 3, 4). Sample: individuals born between 1945-1960 in West Germany, who attended basic track. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 2 and 4 include state-specific linear trends. Robust standard errors clustered at state x year of birth cells (100 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table A2.7 : Effect of Compulsory Schooling on Literacy Skills – 2SLS Analysis II

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
Panel A: First-Stage				
Weeks of schooling	.027***	.020***	.027***	.020***
	(.004)	*(.006)*	*(.004)*	*(.006)*
Dependent variable:	Literacy Skills		Literacy Rank	
	(1)	(2)	(3)	(4)
Panel B: Second-Stage				
Years of schooling	.062	.209	.020	.048
	(.142)	*(.279)*	*(.047)*	*(.084)*
State-specific linear trend	No	Yes	No	Yes
Individuals	301	301	301	301
Instrument F statistic	41.08	12.35	41.08	12.35
Observations	741	741	741	741

Notes: Two-stage least squares estimation. Dependent variable Panel A: length of schooling (years in primary and secondary school). Dependent variable Panel B: z-standardized literacy skills (Column 1, 2) and percentile rank (Column 3, 4). Sample: individuals born between 1945-1960 in West Germany, who attended basic track. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 2 and 4 include state-specific linear trends. Robust standard errors clustered at state x year of birth cells (100 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table A2.8 : Effect of Compulsory Schooling on Numeracy Skills II (Various Samples)

	Excl. 1st cohort	Excl. city states	1945 – 1970
Panel A: First-Stage (Dependent Variable: Years of Schooling)			
Weeks of schooling	.027***	.027***	.029***
	(.005)	*(.004)*	*(.004)*
Panel B: Second-Stage (Dependent Variable: Numeracy Skills)			
Years of schooling	.271***	.198**	.166*
	(.096)	*(.096)*	*(.091)*
Instrument F statistic	30.78	40.95	45.63
Observations	694	734	1199

Notes: Two-stage least squares estimation. Dependent variable Panel A: length of schooling (years in primary and secondary school). Dependent variable Panel B: z-standardized literacy skills. Sample: individuals born between 1945-1960 in West Germany, who attended basic track. Column 1 excludes first cohort affected by the reform; Column 2 excludes city states; Column 3 extends sample to cohorts born between 1945 and 1970. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Robust standard errors clustered at state x year of birth cells. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table A2.9 : Effect of Compulsory Schooling on Exit Exam Grades

	Math	German
Panel A: instrumented by compulsory schooling reform		
Years of schooling	−.101	.086
	(.188)	*(.230)*
Instrument F statistic	28.03	23.05
Panel B: instrumented by hypothetical weeks of schooling		
Years of schooling	−.204	.017
	(.179)	*(.208)*
Instrument F statistic	32.93	27.53
Observations	722	703

Notes: Two-stage least squares estimation. Dependent variables: School exit exam grades in Math (Column 1) and German (Column 2). Grades vary from 1 (best grade) to 5 (worst grade); thus, negative coefficients are interpreted as performance increase. Sample: individuals born between 1945-1960 in West Germany, who attended basic track. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Robust standard errors clustered at state x year of birth cells. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table A2.10 : Descriptive Statistics – NEPS Analysis

	all tracks	basic track
Years of schooling (primary & secondary)	10.38	8.60
Female	.47	.45
Year of birth	1953	1952
Basic track	.37	
Intermediate track	.26	
Academic track	.37	
Observations	3233	1099

Notes: Mean values of outcome and control variables, for the entire sample (Column 1) and for sample of basic track students (Column 2). Years of schooling refer to years in primary and secondary school. Basic/intermediate/academic track indicators equal 1 if the individual's highest secondary school degree is a basic/intermediate/academic degree, zero otherwise. *Data source:* NEPS.

Table A2.11 : Reform Effect on Length of Schooling – NEPS Analysis

	Dependent variable: length of schooling		
	(1)	(2)	(3)
Panel A: compulsory schooling reform & short school years			
Compulsory Schooling Reform	.444***	.533***	.446***
	(.100)	*(.110)*	*(.101)*
Short School Year		–.241**	.067
		(.117)	*(.115)*
Panel B: hypothetical weeks of schooling			
Weeks of schooling		.013***	.008**
		(.003)	*(.003)*
State-specific linear trend	No	No	Yes
Observations	1099	1099	1099

Notes: Ordinary least squares estimation. Dependent variable: length of schooling (years in primary and secondary school). Sample: individuals born between 1945-1960 in West Germany, who attended basic track schools. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 3 includes state-specific linear trends. Robust standard errors clustered at state x year of birth cells (146 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* NEPS.

Table A2.12 : Reform Effect on Track Attendance – NEPS Analysis

	(1)	(2)
Panel A: Dependent variable: individual attends basic track		
Compulsory Schooling Reform	.026	.042
	(.043)	*(.053)*
Panel B: Dependent variable: individual attends intermediate track		
Compulsory Schooling Reform	.042	.059*
	(.030)	*(.033)*
Panel C: Dependent variable: individual attends academic track		
Compulsory Schooling Reform	−.068	−.101**
	(.042)	*(.044)*
State-specific linear trend	No	Yes
Observations	3233	3233

Notes: Ordinary least squares estimation. Dependent variable: Dummy which equals 1 if the individual attended basic track (Panel A), intermediate track (Panel B), or academic track (Panel C). Sample: individuals born between 1945-1960 in West Germany. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Robust standard errors clustered at state x year of birth cells (159 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* NEPS.

Table A2.13 : Reform Effect on Length of Schooling, All Tracks – NEPS Analysis

	Dependent variable: length of schooling		
	(1)	(2)	(3)
Panel A: compulsory schooling reform & short school years			
Compulsory Schooling Reform	.219**	.242**	.227
	(.093)	*(.102)*	*(.147)*
Short School Year		−.108	−.033
		(.104)	*(.107)*
Panel B: hypothetical weeks of schooling			
Weeks of schooling		.016***	.015***
		(.002)	*(.003)*
State-specific linear trend	No	No	Yes
Observations	3233	3233	3233

Notes: Ordinary least squares estimation. Dependent variable: length of schooling (years in primary and secondary school). Sample: individuals born between 1945-1960 in West Germany. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, a track indicator and an indicator whether the federal state of school attendance is approximated by state of residence today. Robust standard errors clustered at state x year of birth cells (159 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* NEPS.

Table A2.14 : Effect of Compulsory Schooling on Numeracy Skills – NEPS Analysis

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
Panel A: First-Stage				
Compulsory Schooling Reform	.683***	.749***	.581***	.592***
	(.148)	*(.148)*	*(.123)*	*(.133)*
Short School Year	−.246*	−.237**	−.277**	−.115
	(.128)	*(.111)*	*(.130)*	*(.138)*
Dependent variable:	Numeracy Skills		Literacy Skills	
	(1)	(2)	(3)	(4)
Panel B: Second-Stage				
Years of schooling	.049	.170	−.104	.002
	(.342)	*(.375)*	*(.194)*	*(.234)*
State-specific linear trend	No	Yes	No	Yes
Instrument F statistic	21.23	25.58	22.42	19.74
Observations	353	353	553	553

Notes: Two-stage least squares estimation. Dependent variable Panel A: length of schooling (years in primary and secondary school). Dependent variable Panel B: z-standardized numeracy skills (Column 1, 2) and literacy skills (Column 3, 4). Sample: individuals born between 1945-1960 in West Germany, who attended basic track. All specifications include a gender control, year of birth FE, federal state of school attendance FE, dummies for each test regime, and an indicator whether the federal state of school attendance is approximated by state of residence today. Column 2 and 4 include state-specific linear trends. Robust standard errors clustered at state x year of birth cells (100 clusters). Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* NEPS.

3 International Evidence on the Impact of Graduating from High School in a Recession: College Investments, Skill Formation, and Labor-Market Outcomes[1]

3.1 Introduction

Human capital investment decisions have been studied extensively. Besides discount rates and credit constraints, individuals' expectations about the returns to education play an important role. Investment in schooling is affected by *perceived* monetary returns (Jensen, 2010) and by changes in the returns (Abramitzky and Lavy, 2014). Perceived monetary returns, often elicited in surveys through subjective earnings expectations, also affect the decision to enroll in college (e.g., Nguyen 2008; Stinebrickner and Stinebrickner 2012; Attanasio and Kaufmann 2014; Kaufmann 2014; Schweri and Hartog 2017).[2]

Subjective expectations are shaped by economic events individuals experience during their lifetime. There is ample evidence that personal experiences of economic conditions affect individuals' willingness to take financial risks (Malmendier and Nagel 2011; Malmendier, Tate and Yan 2011; Aizenman and Noy 2015), consumption behavior (Malmendier and Steiny 2017; Malmendier and Shen 2018), employment decisions (De Mello, Waisman and Zilberman 2014; Nagler, Piopiunik and West forthcoming), as well as preferences for redistribution and political parties (Giuliano and Spilimbergo 2014; Roth and Wohlfahrt 2018). In this paper, we investigate the impact of economic conditions on human capital investment decisions and subsequent labor-market outcomes.

We exploit business cycle conditions at high-school graduation as a source of exogenous variation in the labor-market options of potential college students. In particular, we study the impact of graduating from high school in boom versus bust years on college enrollment, dropout, and completion. Because the business cycle conditions at high-school graduation are exogenous to these outcomes, our reduced-form results reflect causal effects. Our estimations exploit business cycle fluctuations over a 20-year period across 28 developed countries. Besides investigating individuals' short-run college decisions, we also trace the longer-run consequences of graduating from high school during a recession on cognitive skill formation and labor-market outcomes. Hence, this is the first paper that provides a comprehensive assessment of the impact of economic conditions at high-school graduation on both the

[1] This chapter was coauthored by Marc Piopiunik, ifo Institute, and Simon Wiederhold, Catholic University Eichstaett-Ingolstatt.
[2] Subjective earnings expectations also influence the choice of college major (Arcidiacono, Hotz and Kang 2012; Wiswall and Zafar 2015).

short-run college enrollment decisions and the longer-run human capital and labor-market consequences.

We use data from the Programme for the International Assessment of Adult Competencies (PIAAC), administered by the OECD. PIAAC is a cross-sectional survey, covering representative samples of adults in developed countries. We focus on persons aged 25-39 years, most of whom have already finished their formal education and entered the labor market. PIAAC provides internationally comparable assessments of adults' literacy and numeracy skills. Furthermore, the rich background questionnaire contains information on individuals' educational background, including college enrollment, degree, and dropout, as well as labor-market outcomes such as wages, employment, and training participation. Information on parental background allows us to investigate whether the impact of economic conditions at high-school graduation varies with parents' education.

We identify the effect of economic conditions at high-school graduation by exploiting variation in national unemployment rates across countries over time. By including fixed effects for countries and birth years, the estimates are based on economic shocks that are specific to a country and birth cohort. In particular, country fixed effects control for any differences between countries that are similar across cohorts. For example, they account for cross-country differences in the likelihood to attend or complete college, for differences in numeracy and literacy skills, and for different wage levels. Birth year fixed effects control for any differences between cohorts that are similar across countries. While these fixed effects eliminate common unemployment patterns across countries, they also control flexibly for any time trends in our outcomes, such as the secular increase in college attainment. We thus effectively compare individuals of the same age and within the same country who faced different economic conditions when graduating from high school. Since the data set is cross-sectional, the cohort fixed effects implicitly also control flexibly for skill depreciation over the life cycle. We also account for the fact that unemployment rates are serially correlated by controlling for economic conditions in the years before and after high-school graduation.

We find that bad economic conditions at high-school graduation *positively* affect college investments. An increase in the unemployment rate by 1 percentage point (pp) increases college enrollment by about 0.8 pp, an increase by 1.6% from the international mean. Another way to illustrate the magnitude of this effect is to use the difference between the lowest and highest unemployment rate during our observation period, which amounts to 6 pp on average across the 28 countries in our sample. Such an increase in the unemployment rate would raise college enrollment by 4.6 pp, or 9%. Economic fluctuations at high-school graduation have a similar impact on successfully *completing* college. Consistent with this result, we find that college dropout is unaffected by the labor market conditions at high-school graduation.

The *positive* effects of *bad* economic conditions at high-school graduation on college attainment also carry over to longer-run human-capital formation and labor-market success. A 1 pp increase in unemployment at high-school graduation raises both literacy and numeracy skills by about 1% of a standard deviation and increases monthly wages by slightly less than 1%. While both effects are modest in magnitude, they are statistically significant at least at the 5% level. We also find evidence that individuals who graduate from high school during worse economic conditions continue to invest more in human capital after the end of formal education. More precisely, an increase in the unemployment rate by 1 pp increases the probability of participating in further training activities by 0.4 pp (0.7%).

Heterogeneity analyses suggest that the effect of economic fluctuations on college attendance (and attainment) is stronger for individuals with higher socio-economic background, as proxied by parental education. One potential explanation is that the negative income effect during recessions is likely more pronounced for disadvantaged families, limiting their ability to pay for college.[3] Our finding is consistent with existing evidence from the United States that college enrollment of low-income households is less countercyclical than that of high-income households (e.g., Christian 2007; Méndez and Sepúlveda 2012). This implies that recessions at high-school graduation increase educational inequality. Furthermore, we find that economic conditions at high-school graduation affect cognitive skills and labor-market outcomes more strongly for women than for men.

While it seems likely that the positive (reduced-form) effects on skill formation and labor market outcomes are driven by increased college education, there may also be other mechanisms at work, for example, increased investments in learning (independent from college attendance) or changes in occupational choice (e.g., toward jobs that use and reinforce skills by providing a more challenging work environment or a lower risk to become unemployed). To provide tentative evidence that the recession-induced college investment is an important mechanism explaining the impact on skill formation and labor-market outcomes, we estimate an instrumental-variable model that instruments college enrollment with the unemployment rate at high-school graduation. This model relies on the (arguably strong) assumption that, conditional on the covariates, economic conditions at high-school graduation are orthogonal to any factors influencing skills and labor-market outcomes other than college enrollment. Our results suggest that college education, which is induced by bad economic conditions at high-school graduation, has sizeable positive effects on cognitive skills, wages, and training participation. Effect sizes are larger than the corresponding OLS estimates, suggesting that compliers – those individuals who attend college only *because* of bad economic conditions at high-school graduation – have higher returns to college education than the average individual. This is in line with previous evidence on the returns to college (e.g., Card 1993).

Existing evidence suggests that economic decisions are affected not only by current economic conditions, but also by conditions experienced earlier in life (Malmendier and Nagel 2011; Rao 2016). This is an important observation since unemployment rates are correlated across years. Despite this fact, existing studies investigating the effect of economic conditions on college

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[3] Part-time jobs that help financing college are also scarcer during bad economic times, further aggravating the negative income effect for individuals from disadvantaged families (Kane 1994; Dellas and Sakellaris 2003).

enrollment almost exclusively focus on the economic condition at high-school graduation. This leaves open the question whether high-school graduates decide to enroll in college due to the *current* economic condition, or whether their decisions were (also) affected by *previous* economic conditions (which may be correlated with the current condition). We show that only economic conditions at high-school graduation, but not in previous or later years, affect the college investment decisions. Knowing the age at which economic conditions are most relevant for college enrollment decisions is important for policy-makers when designing policies to foster the transition between secondary and tertiary education. Our results suggest that one way for policy-makers to increase college attendance is to target students at the end of high school, particularly during economic booms.

Our paper is related to existing studies that investigate the effects of economic conditions at various points during an individual's life, most importantly at high-school graduation.[4] The countercyclical college enrollment pattern we find is in line with previous evidence. Betts and McFarland (1995) find a countercyclical pattern of community college enrollment in the United States between the late 1960s and mid-1980s; Dellas and Sakellaris (2003) confirm this pattern for U.S. college enrollment in the same period. Clark (2011) and Sievertsen (2016) find that higher unemployment rates at high-school graduation increases enrollment in post-secondary education (college and other post-secondary programs) in England and Denmark, respectively. Using data on 28 European countries, Ayllon and Nollenberger (2016) investigate whether the high unemployment rates during the Great Recession increased college enrollment and led to transitions from the labor market back to education. Similar to our results, they also find countercyclical higher education decisions.[5]

These studies, like our study, estimate reduced-form effects of economic conditions on college enrollment by comparing individuals who graduated from high school in good vs. bad economic times. A positive effect of bad economic conditions on college enrollment suggests that a nontrivial fraction of high-school graduates consists of academically marginal students, that is, individuals who attend college *because* of the bad economic conditions at high-school graduation. Since these marginal students are likely of lower ability than individuals who attend college independent of economic conditions, attending college – the only outcome in the previous studies – does not necessarily imply that these marginal students will also successfully complete college or benefit from it on the labor market.[6] This paper fills this gap by investigating the impact of economic conditions at high-school graduation also on college completion, subsequent formation of skills, and later labor-market outcomes.

[4] Other studies have investigated the effects on educational decisions and labor-market outcomes of economic conditions at birth (Rao, 2016) and of contemporaneous economic conditions (Méndez and Sepúlveda 2012; Johnson 2013; Barr and Turner 2015; Long 2015; Alessandrini, Kosempel and Stengos 2015). None of the existing studies have investigated the effects on cognitive skills.

[5] Adamopoulou and Tanzi (2017) investigate the impact of the Great Recession on college dropout in Italy.

[6] Note that the zero (reduced-form) effect on college dropout suggests that the probability of successfully completing college is not significantly different between these marginal students and the average college student.

Our work is also related to studies investigating the labor-market consequences of economic conditions at the time of college graduation. These studies consistently find substantial *negative* wage effects of graduating from college during a recession that can persist for several decades (Kahn 2010; Oreopoulos, von Wachter and Heisz 2012; Altonji, Kahn and Speer 2016; Liu, Salvanes and Sørensen 2016; Schwandt and von Wachter 2019). In contrast, our findings indicate that bad economic conditions can also have long-run *positive* labor-market effects if they prevail at another point during an individual's lifetime.

Section 3.2 describes the international PIAAC data and business cycle information, and derives the hypothetical age of college decision-making for each country. Section 3.3 presents the empirical strategy. Section 3.4 provides the main results, robustness checks, and the instrumental variable results. Section 3.5 concludes.

3.2 Data

3.2.1 PIAAC Data

We use cross-sectional data from the Programme for the International Assessment of Adult Competencies (PIAAC), administered by the Organisation for Economic Co-operation and Development (OECD, 2016). PIAAC was designed to provide internationally comparable measures of the literacy and numeracy skills of adults aged 16 to 65 years.[7] In each of the participating countries, a representative sample of at least 5,000 adults participated in the PIAAC assessment.[8] An extensive background questionnaire contains detailed information on respondents' demographic characteristics, education, and labor-market outcomes.

Our empirical analysis is based on 28 countries, all of which are OECD members. From the 33 PIAAC countries, we exclude Indonesia and the Russian Federation because the data are not representative of the entire population (in the Russian data, the population of the Moscow municipal area is not included; in Indonesia, only Jakarta took part in PIAAC). Furthermore, we exclude Cyprus, Lithuania, and Singapore due to missing information on unemployment rates for a substantial part of our analysis period.

[7] PIAAC also assessed skills in the domain *problem solving in technology-rich environments*, reflecting ICT skills. However, several countries (Cyprus, France, Indonesia, Italy, and Spain) did not assess ICT skills. Furthermore, in the countries that tested ICT skills, not all respondents took part in the assessment, raising concerns about sample selectivity. Reasons for not participating in the test were a lack of any computer experience, failing a short initial ICT test, and opting out of the ICT skills assessment (see Falck, Heimisch and Wiederhold 2016 for details). Therefore, we focus on the two main skill domains, literacy and numeracy, which were assessed in all participating countries and of all respondents. See OECD (2013) for a more encompassing description of the tested skill domains.

[8] 24 countries participated in the first round of PIAAC in 2011/12 and 9 countries participated in the second round in 2014/15.

To assess the impact of economic conditions at high-school graduation on college decisions, we consider three distinct outcomes: attending college, obtaining a college degree, and dropping out of college. The binary indicator *college degree* equals 1 if a respondent's highest level of formal education is ISCED level 5 or 6; it equals 0 otherwise. The binary indicator for *college dropout* equals 1 if respondents report to have started, but not completed tertiary education; 0 otherwise.[9] The binary indicator for *college attendance* equals 1 if the respondent is currently enrolled in college, holds a college degree, or has dropped out of college; 0 otherwise.[10] In addition to investigating business cycle effects on college investment and cognitive skills, we also consider several labor-market outcomes. Our primary outcome is the logarithm of gross monthly earnings (incl. bonuses) for wage and salary workers as well as for self-employed.[11] We measure investments in human capital after the end of formal schooling by using an indicator variable which equals 1 if the respondent has participated in training activities in the year before the survey; 0 otherwise.[12]

3.2.2 Business Cycle Information

We measure economic conditions using national unemployment rates.[13] Data come from the OECD's Annual Business Cycle Indicators. As explained in detail below, our focus is on the unemployment rate at high-school graduation, but we also consider the unemployment rates several years before and after an individual's graduation. To do so, we match annual country-specific unemployment rates to respondents depending on their year of birth. Appendix Figure A3.1 shows the development of the national unemployment rates for all 28 countries in our sample from 1990–2009.[14] First, we observe that mean unemployment rates differ

[9] Note, however, that this information is not available for individuals who were enrolled in formal education at the time of the PIAAC interview. Furthermore, the United States does not provide any information about uncompleted educational qualifications in the PIAAC Public Use File; consequently, the United States is excluded from all college dropout analyses.

[10] In the United States, *college attendance* indicates either being currently enrolled in college or holding a college degree, as we cannot identify individuals who dropped out of college in the past (see above).

[11] The PIAAC Public Use File reports monthly wages for Austria, Canada, Germany, Sweden, and the United States only as a worker's decile rank in the country-specific wage distribution. For Germany, we obtained the Scientific Use File, which contains continuous wage information. For the remaining countries, we follow Hanushek et al. (2015) in assigning the decile median of monthly wages to each survey participant belonging to the respective decile of the country-specific wage distribution. Using wages in coarse categories in some countries is unlikely to affect our results because Hanushek et al. (2015) show that using decile medians instead of continuous wages has no substantive impact on their returns-to-skills estimates. Moreover, in each country, we trim the bottom and top 1% of the wage distribution to limit the influence of outliers. In auxiliary analysis, we also use gross hourly wages, for which we make an analogous adjustment for missing continuous wage information.

[12] PIAAC asks respondents for the following types of training: on-the-job training, seminars/workshops, private lessons, as well as open/distance education.

[13] Unemployment rates have been extensively used as indicators of economic (labor-market) conditions in the literature of business cycle effects; see, for instance, Genda, Kondo and Ohta (2010), Kahn (2010), Kondo (2012), Maclean (2014), and Oreopoulos, von Wachter and Heisz (2012).

[14] This period covers the high-school graduation years of the individuals in our sample. See the description of sample restrictions in Section 3.2.4.

substantially across countries, from 5.3% in Japan to 24.2% in Spain. Second, and more importantly for our identification, business cycles across countries are synchronized only to some extent. The correlation of annual unemployment rates over the period 1990 and 2009 between any of our country pairs range from -0.85 (Japan–Netherlands) to 0.96 (Australia–Canada). In terms of volatility, for example, the difference between the highest and lowest unemployment rate during our sample period ranges from 2.2 percentage points in Austria to 13.6 percentage points in Estonia. Due to the inclusion of fixed effects for countries and year of birth (see Section 3), we identify the impact of economic conditions at high-school graduation by using differential unemployment rate fluctuations between countries after netting out unemployment changes that are common to all countries.

3.2.3 Hypothetical Age at College-Decision Making

An important aspect when estimating the effect of economic conditions on tertiary education investments is the timing of the decision to enroll in college vis-à-vis to enter the labor market. We hypothesize, and provide evidence, that college-enrollment decisions are mainly made in the period around high-school graduation. However, PIAAC does not provide information about the actual year, or age, when respondents finished high school.[15] We thus calculate the *hypothetical* age at high-school graduation, defined as the official school enrollment age in a country plus the number of years required to complete upper secondary education in that country, as reported by official statistics. We use the number of years to complete ISCED 3, which is the final stage of secondary education in most OECD countries.[16]

Using the *hypothetical* age at high-school graduation has the advantage that it is exogenous to economic conditions. In contrast, individuals' *actual* graduation age may well be affected by the labor-market conditions at the end of high school (see, e.g., Kahn 2010; Rao 2016). In particular, students who want to directly enter the labor market without attending college may postpone their high-school graduation when economic conditions are unfavorable. Similarly, students may try to speed up high-school graduation (or even leave high school without a degree) during good economic times.

The hypothetical age at high-school graduation according to our definition varies between 18 and 19 years across countries in our sample. OECD data on actual enrollment by age are consistent with these high-school graduation ages (Figure 3.1). The share of students attending high school in the year before our hypothetical high-school graduation age is normalized to 100 in each country (point -1 on the x-axis). Panel A shows that about 70% of those students have finished (or left) high school when they have reached the hypothetical high-school graduation age. In the years after, the share of students finishing high school further increases,

[15] Respondents report only the year of finishing the highest education level, which is typically vocational training or college education.

[16] Our primary source of information is OECD (1999), which allows the mapping of ISCED levels to national education levels. Additional information on the mapping of educational programs to ISCED levels come from the UNSECO Institute for Statistics, see http://uis.unesco.org/en/isced-mappings.

reducing the average high-school enrollment rate to just 3.4% two years after the hypothetical graduation age. Notably, each country in our sample exhibits this pattern, although there is some variation in the exact enrollment shares at different years around the hypothetical high-school graduation age (Panel B).

It is worth noting that a complete drop in school enrollment (from 100% to 0%) at the hypothetical age at high-school completion cannot be expected for various reasons. For example, some students do not start primary school at the official school-entry age, but enroll one year later. Furthermore, (repeated) grade retention increases the age at high-school graduation, with the likelihood of grade retention varying across countries.[17] Furthermore, students may also skip a grade and graduate from school earlier (which occurs much less frequently than grade retention). Hence, consistent with Figure 3.1, our derived hypothetical high-school graduation age can be regarded as a lower bound of the actual age at high-school graduation.[18]

Some countries draft citizens into compulsory military service, typically taking place after high-school graduation. Therefore, the relevant timing of college decision-making varies across countries not only because of differences in the official high-school graduation age, but also because of differences in the existence and length of compulsory military service. In all countries with compulsory military service with the exception of Israel, conscription is limited to men, which introduces variation in the timing of college decision-making within countries across gender. Figure 3.2 depicts the hypothetical (gender-specific) age at college decision-making which we use in the empirical analysis, calculated as the hypothetical age at high-school graduation plus the length of compulsory military service.[19] Australia, Canada, Ireland, Japan, New Zealand, the United Kingdom, and the United States did not have compulsory military service during our period of interest. In some countries, only some (male) cohorts were obliged to complete compulsory military service: in our period of observation, compulsory conscription was abolished in the Netherlands (in 1991), Belgium (1992), Czech Republic (1992), France (1996), Spain (2001), Slovenia (2003), and Italy (2004).[20] Consequently, for males in these countries the hypothetical age at college decision-making differs across cohorts. Compulsory military service was limited to approximately one year in most countries. The

[17] According to the PISA 2009 student questionnaire, grade repetition is rare in Finland, the Slovak Republic, Slovenia, Sweden, and the United Kingdom. However, more than 20% of students repeat at least one grade in primary or secondary school in Germany, Chile, and the Netherlands. In Belgium, almost 35% of students repeat a grade at least once during their school career (Ikeda and García, 2014).

[18] Information on age at high-school graduation is available in PIAAC only for the subgroup of respondents whose *highest* education level is upper secondary education (48% of our sample). Since this group is likely negatively selected with respect to ability, we refrain from using this information as a proxy for the (average) actual high-school graduation age in a country. Still, the mean and median age at high-school completion in this subgroup is in line with our estimated high-school graduation age in each country.

[19] Information on compulsory military service for each country comes from the CIA factbook – see https://www.cia.gov/library/publications/the-world-factbook/fields/333.html – as well as from national data sources.

[20] Thus, the average of the hypothetical age at college decision-making in Figure 3.2 is not an integer value for these countries.

only countries in which compulsory service exceeded one year are Israel, South Korea, and Norway.[21] Due to these considerations, the hypothetical age at college decision-making varies between 18 and 21 years.

The economic condition around hypothetical college decision-making, our explanatory variable of interest, is constructed as follows: Each individual is assigned the national annual unemployment rate of the year in which the individual reached the hypothetical age of college decision-making (see Figure 3.2).[22] Three of the 28 countries in our sample (Canada, New Zealand, and the United States) do no report the exact age of respondents, but provide age only in 5-year intervals.[23] These individuals are assigned the average annual unemployment rate across the 5 years that correspond to the respective 5-year age interval. This introduces some measurement error in the economic conditions at high-school graduation. We show that our results are robust to simultaneously excluding the three countries without exact age information (Section 3.4.5). For simplicity, we use the terms "age/year of hypothetical college decision-making" and "age/year of hypothetical high-school graduation" synonymously.

3.2.4 Sample

We restrict the sample to individuals aged 25 to 39 years. We apply the minimum age restriction since the vast majority of individuals should have completed their education decision by age 25, allowing us to observe individuals in the labor market. The maximum age restriction is implemented to retain as many countries in our sample as possible. Former communist countries – Czech Republic, Estonia, Poland, Slovak Republic, and Slovenia – did not report valid unemployment rates before the fall of the Iron Curtain in 1989. Thus, we would lose observations for these countries due to missing information about economic conditions around the time of high-school graduation. Furthermore, education decisions during the communist regime were presumably less free than those in other countries, depending rather on political attitudes and connections than on market incentives (see, e.g., Hanushek, Piopiunik and Wiederhold 2018). Another reason for including only younger individuals in the baseline sample is to have a close link between potential college attendance and later-life outcomes (such as cognitive skills and wages).[24]

[21] We round the length of military service, expressed in years, to the next integer number (that is, 1 or 2 years). Furthermore, compulsory military service in some countries depends on religion, educational attainment, or other factors that we cannot account for. We are also unable to account for the fact that some individuals were exempt from military service due to health issues. For these reasons, our measure of the length of the compulsory military service is just an approximation of the actual length of the draft.

[22] PIAAC contains the age of the respondent, but not year of birth. We calculate year of birth by subtracting the age of the respondent at the time of the survey from the year in which the survey is conducted. This creates some measurement error since neither the exact day of the interview nor the exact birthday of the respondent are known. Since this measurement error is supposed to be classical in nature, our estimates of business cycle effects are likely to be attenuated.

[23] Neither does the Austrian PIAAC Public Use File contain the exact age of respondents. However, we obtained access to the Austrian Scientific Use File from the national data center, which provides the exact age.

[24] In Appendix Table A3.13, we show that results are similar when also including older individuals up to age 59.

Furthermore, we exclude individuals who have achieved their highest educational degree in a different country (excluding less than 2% of the sample).[25] Finally, we balance our sample such that unemployment rates could be assigned to all individuals five years prior to six years after the hypothetical age at college decision-making, because we want to investigate whether economic conditions at high-school graduation are more (or less) relevant for higher education decisions than the conditions in previous or later years (see Section 3.3 for details).[26] The resulting final estimation sample comprises 51,241 individuals from 28 countries.

Appendix Table A3.1 reports the means of all outcome variables and the unemployment rate at the hypothetical year of college decision-making, separately by country and for the pooled sample.[27] On average, 51% of individuals are currently enrolled in college or have previously attended college. While the share of college attendees exceeds two-thirds in Canada and South Korea, it is only one-quarter in Turkey. The share of persons who have obtained a college degree amounts to 42%, showing a similar cross-country pattern as college attendance. College dropout also differs considerably across countries. More than one-quarter of students who ever attended a college dropped out in Chile and Italy, while less than every tenth college student dropped out in Japan, Korea, and Greece. This pattern is comparable to official statistics provided by the OECD, which show that Italy (Japan) has the highest (lowest) proportion of students who enter a tertiary program but leave before completing a degree (see OECD 2008, Chart A4.1).

Table A3.1 also reports average literacy and numeracy skills in a country, which are measured on a 500-point scale.[28] Individuals in Japan perform highest in literacy (309 PIAAC points) and rank second in numeracy, with only slightly lower average numeracy scores than Finland (301). Average performance is worst in Turkey for literacy (232) and in Chile for numeracy (220). The average unemployment rate in the hypothetical year of college decision-making ranges from 3.6% in South Korea to 17.7% in the Slovak Republic, illustrating the wide variation in economic conditions across the countries in our sample.

[25] Due to this sample restriction, we also exclude individuals who were exposed to the economic conditions in the test country, but obtained a college degree abroad. While we would have liked to keep these observations in the sample, it is not feasible to identify them as PIAAC reports only in which country the highest educational degree was obtained.

[26] This drops slightly less than 7% of our sample. The reason for the loss of observations is the fact that unemployment rates are not available prior to 1996 in Slovenia, 1994 in the Slovak Republic, 1990 in the Czech Republic and Poland, and 1989 in Estonia.

[27] The sample size for Canada is substantially larger than for any other country surveyed in PIAAC because Canada oversampled to obtain regionally representative adult skills. Sampling weights provided by PIAAC are used in the empirical estimation to account for sample size differences across countries (see Section 3.3 for more information about weights).

[28] In the econometric analysis, we standardize skills to have mean 0 and standard deviation 1 within each country. For illustration, one standard deviation in numeracy skills corresponds to one out of five proficiency levels in PIAAC, which is roughly twice the skill difference between PIAAC respondents with lower and upper secondary education (see also Hanushek et al. 2017*b*).

Table A3.2 reports the means of all control variables. 50% of the individuals are female and 13% are first-generation migrants, that is, persons who were born in a country other than the PIAAC test country. The mean age in post-communist countries is slightly lower than in other countries due to missing unemployment information for the oldest birth cohorts in the sample. We condition on family background characteristics that were determined before individuals reached the hypothetical age at high-school graduation. This includes mothers' and fathers' education level and the number of books at home, a common proxy for socio-economic status. Parents' highest level of education is reported in three categories: low (ISCED 1, 2, and 3C), which means that lower secondary education is the highest education level; intermediate (ISCED 3 and 4), which corresponds to upper secondary and post-secondary non-tertiary education; and high (ISCED 5 and 6), referring to a college degree. Books at home is reported in six categories, ranging from 10 books or less in the lowest category to more than 500 books in the highest category.[29]

3.3 Empirical Strategy

Using the cross-sectional PIAAC data, we investigate the impact of economic conditions at high-school graduation on college investment decisions and later-life outcomes. We start our empirical analysis by estimating the following regression:

$$Y_{ict} = \beta_0 + \beta_1 UER_{ct} + \boldsymbol{X_{ict}\beta_2} + \mu_c + \delta_{birthyear} + \varepsilon_{ict} \tag{3.1}$$

where Y_{ict} denotes the outcome of interest of individual i in country c (hypothetically) making the college-decision in year t. In the college investment models, Y_{ict} is an indicator variable representing whether an individual has (a) ever been enrolled in college, (b) obtained a college degree, or (c) dropped out of college. In the skill models, Y_{ict} denotes measured cognitive skills in literacy and numeracy. When looking at labor-market outcomes, Y_{ict} denotes log monthly earnings and participation in training activities, respectively.

Our coefficient of interest, β_1, measures the effect of the national unemployment rate at the hypothetical college decision-making age t. $\boldsymbol{X_{ict}}$ is a vector of individual-level covariates, including gender and migrant status, as well as mother's and father's education level (3 categories) and number of books at home when the individual was 15 years old (6 categories),

[29] As is common in large-scale surveys such as PIAAC, a small share of respondents with available information on outcome variables and labor-market information has missing values for some background characteristics. Since we consider various control variables and since a portion of these variables is missing for some individuals, dropping all observations with any missing value would result in a substantial sample reduction. We therefore imputed values for missing control variables (migrant status, mother's and father's education, and number of books at home) by using the country means of each variable. To ensure that imputed data are not driving our results, all our regressions include an indicator for each variable with missing data that equals 1 for imputed values and 0 otherwise.

reflecting individuals' socio-economic background. All specifications include country fixed effects, μ_c, and year-of-birth fixed effects, $\delta_{birthyear}$.[30] The error term, ε_{ict}, is clustered at the country * year-of-birth level, that is, the level at which the treatment variable varies. We employ the sample weights provided in PIAAC, adjusted such that each country * year-of-birth cell receives the same weight.[31]

The coefficient β_1 is identified based on differential changes in unemployment rates across birth years and countries. We isolate this variation by including fixed effects for countries and birth years. Country fixed effects control for any time-invariant differences across countries (e.g., quality of education systems, labor-market institutions, or government policies). Hence, they absorb, for example, cross-country differences in college attendance or completion rates, cognitive skills, and wage levels. These fixed effects also account for the persistent component of economic conditions within a country. Year-of-birth fixed effects account for shocks and characteristics common to all individuals across countries who are born in the same year. Adding these fixed effects controls very flexibly for general time trends across cohorts in our outcomes, such as a secular increase in educational attainment or rising wage levels. They also eliminate any business cycle fluctuations that are similar across countries. Including year-of-birth fixed effects also controls flexibly for skill depreciation and wage changes over the life cycle.[32]

Interpreting β_1 in Equation 3.1 as a causal effect requires that the unemployment rate at the hypothetical college decision-making age is not correlated with other factors affecting human capital investment decisions. Since unemployment rates are correlated across years (Figure A3.1), one may particularly be worried that β_1 might pick up the effects of economic conditions in years before or after high-school graduation. For instance, Rao (2016) shows that human-capital investment decisions are shaped by economic events people experience early in life, suggesting that β_1 may reflect the influence of economic conditions (long) *before* high-school graduation. Moreover, the estimated β_1 may in principle also capture the impact of economic conditions after high-school graduation. First, people who entered the labor market after high school may lose their jobs during a recessive period and re-enter formal education in response (see e.g., Barr and Turner 2015; Ayllon and Nollenberger 2016). Second, concerning our cognitive skills and labor-market outcomes, there is ample evidence that economic conditions at college graduation affect subsequent labor-market outcomes such as earnings and wages (e.g., Kahn 2010; Oreopoulos, von Wachter and Heisz 2012; Altonji,

[30] Note that year of birth is not perfectly collinear with the decision year t because t varies by gender in some countries (see Section 3.2).

[31] Three countries do not report the exact age of respondents in the PIAAC Public Use File: Canada, New Zealand, and the United States. Hence, in these countries we use 5-year age groups and each 5-year cohort receives five time the weight of a single-year cohort.

[32] Since we use cross-sectional data and there is no variation in the hypothetical age at high-school graduation within a country across cohorts, controlling for year-of-birth fixed effects implies that we simultaneously control for the hypothetical age at high-school graduation. This means that we cannot distinguish between age effects and cohort effects.

Arcidiacono and Maurel 2016; Liu, Salvanes and Sørensen 2016; Schwandt and von Wachter 2019). To account for the persistence of economic conditions, we also estimate models that augment Equation 3.1 by including the unemployment rates in several periods before and after the hypothetical college decision-making age.[33]

In our preferred specification, we use the average unemployment rate during several years around the hypothetical college decision-making age (from *t-1* until *t+2*). One reason to extend the time period of potentially relevant economic conditions beyond the (single) hypothetical college decision-making year is that PIAAC does not report the exact year when individuals completed high-school. The actual graduation age may differ from the hypothetical age if students enrolled in primary school earlier or later than the official school starting age or if they repeated or skipped a grade. Furthermore, in countries with compulsory military service, not all eligible candidates actually serve in the military. There is also variation in the length of service with respect to, for instance, religion and state of health, which is unobservable to us. Besides military service, there are additional reasons why the decision to enroll in college deviates from the official high-school graduation year. According to the National Postsecondary Student Aid Study, more than one-third of undergraduate students in 1992–93 as well as in 2011–12 in the United States have waited one year or more after high-school graduation to enroll in college. The observed college enrollment time gap may also be caused by sickness, marriage, or pregnancy (Bozick and DeLuca, 2005). Lin (2019) further highlights that many U.S. universities increasingly promote the so-called "gap year". Due to these factors, our preferred specification uses the average unemployment rate over four successive years, covering the period from one year before the hypothetical college decision-making age up to two years afterwards to measure the labor-market conditions that potentially affect individuals' human-capital investment decisions. Hence, our preferred specification reads as follows:

$$\begin{aligned} Y_{ict} = \alpha_0 &+ \alpha_1 UER_{c,before} + \alpha_2 UER_{c,affected} + \alpha_3 UER_{c,after} \\ &+ \boldsymbol{X_{ict}\alpha_4} + \mu_c + \delta_{birthyear} + \epsilon_{ict} \end{aligned} \tag{3.2}$$

Here, $UER_{c,before}$, $UER_{c,affected}$, and $UER_{c,after}$ represent, respectively, the average national unemployment rates from five to two years prior to the age at hypothetical college decision-making, from one year prior to two years after the age at hypothetical college decision-making, and from three years to six years after the age at hypothetical college decision-making.[34] The coefficient of interest is α_2, reflecting the impact of the business cycle at the most sensitive

[33] Note that reverse causality is unlikely to threaten identification of our model because human-capital investment decisions of individuals at high-school graduation age – potentially affecting future economic conditions by altering the stock of human capital and distribution of skill of the workforce (see, e.g., Romer 1990; Hanushek and Woessmann 2015) – do not influence the unemployment rate that school leavers currently face.

[34] In a robustness analysis, we control for yearly unemployment rates up to ten years prior to the official high-school graduation age (Section 3.4.5). To avoid losing more birth cohorts, the main specification considers only the average unemployment rate across the four years preceding the affected cohort.

period of college decision-making.[35] In the remainder of the paper, we will refer to the average unemployment rate in this period as the unemployment rate of the "affected cohort".

3.4 Results

This section summarizes the results of the business cycle effects on college investment decisions (Section 3.4.1), cognitive skill formation (Section 3.4.2), and labor-market success (Section 3.4.3). Section 3.4.4 explores the heterogeneity of business cycle effects across gender and the socio-economic background. A set of robustness analyses is summarized in Section 3.4.5 and Section 3.4.6 presents instrumental-variable estimations of the contribution of college education to the formation of cognitive skills and labor-market success for individuals experiencing bad economic conditions at the hypothetical age of college decision-making.

3.4.1 The Impact of Economic Conditions on College Investment

First, we investigate the effect of labor-market conditions at high-school graduation on investments in college education. Figure 3.3 shows business cycle effects on college enrollment, college degree, and college dropout at various points over an individual's early life. With t we denote the year when an individual hypothetically decides on college education, that is, the hypothetical high-school graduation age plus length of compulsory military service (see Section 3.2 for details). Each dot represents a coefficient that stems from a separate regression of the respective outcome on the unemployment rate in the period indicated on the horizontal axis and a full set of covariates.[36] Panel A shows that the impact of the unemployment rate on college enrollment is positive and strongest in the hypothetical year of college decision-making, t. An increase in the national unemployment rate of 10 percentage points (pp) is associated with a 6.4 pp higher enrollment probability. Relative to the mean college enrollment rate in our sample (51%), this corresponds to a 12.5% higher enrollment probability.

Even though the effect of the unemployment rate in year t is strongest, also unemployment rates in previous and subsequent years seem to matter for college investment decisions. As discussed in detail in Section 3.3, this likely reflects the serial correlation of unemployment rates as well as the fact that individuals have actually finished high school in years around the hypothetical college decision-making year t. Hence, the right-hand graphs of Figure 3.3 show results using the unemployment rates averaged over four-year cohorts around the hypothetical college decision year ("affected cohorts"), of the pre-period ("before"), and of the post-period ("after") (see description in Section 3.3). Panel A shows that the unemployment rate faced by

[35] Due to the serious multicollinearity of unemployment rates over time within countries, which results in a co-movement of regressors, we refrain from estimating the effect of unemployment rates between (*t-5*) to (*t+6*) in one single regression. See also Dellas and Koubi (2003).
[36] Appendix Table A3.3 reports the corresponding regression results.

affected cohorts has a significantly positive impact on college enrollment, while the economic conditions in the pre-period and post-period do not appear to matter for college enrollment.[37] The respective coefficients in the pre-period and post-period also differ significantly from the coefficient on the affected cohort, corroborating the result that economic conditions around high-school graduation are most relevant for college investment decisions.

Table 3.1 shows our baseline regression results. Columns 1–3 of Panel A correspond to the right-hand graph in Panel A of Figure 3.3, separately including average unemployment rates in each of the periods. In Column 4, the three average unemployment rates are jointly included. A 10 pp increase in the average unemployment rate during the sensitive period of college decision-making leads to a 7.7 pp (or 15.1%) increase in college enrollment. Another way to interpret effect magnitudes is to use the maximum spread in unemployment rates (that is, difference between the highest and lowest unemployment rate) in a country. An increase in unemployment similar to the international mean in the country-specific unemployment spread (6 pp) would raise college enrollment by 4.6 pp or 9%. While this effect is rather modest in size, it is similar to the findings by Dellas and Sakellaris (2003) for the United States, and is even larger than the effect across 28 European countries by Ayllon and Nollenberger (2016).

While we observe that more students enroll in college when labor-market conditions at the end of high school worsen, this does not necessarily imply that the affected (academically marginal) students also finish college successfully.[38] For example, if the skill requirements in college are too high, academically marginal students may eventually drop out without obtaining a degree. Furthermore, marginal students may react more strongly to changing economic conditions during studies, dropping out of college to enter the labor market as soon as employment opportunities become better. However, we find very similar business cycle effects on college completion as on college enrollment (see Panel B of Figure 3.3). In fact, the estimated coefficient in the baseline specification in Column 4 in Panel B of Table 3.1 indicates that a 10 pp increase in the average unemployment rate for the affected cohort leads to a 7.7 pp increase in college completion, which translates to a 18.3% increase from the international mean (42%). A higher unemployment rate around high-school graduation is not associated with a higher probability of dropping out of college (Panel C of Figure 3.3 and Table 3.1, respectively).[39]

[37] As in the left-hand graphs of Figure 3.3, the coefficient estimates come from separate regressions. However, results are very similar when including all three unemployment rates simultaneously (Table 3.1).

[38] PIAAC provides only very coarse measures of field of study. Thus, we are not able to test whether field-of-study choices vary across the business cycle. For business cycle effects on field of study, see Blom, Cadena and Keys (2015), Altonji, Kahn and Speer (2016), and Liu, Sun and Winters (2017).

[39] The negative coefficient on the post-period unemployment rate (that is, average unemployment three to six years after the hypothetical high-school graduation) in the drop-out estimation can potentially be explained by the lower opportunity costs of completing college when labor-market conditions are bad. Consistent with our finding, Adamopoulou and Tanzi (2017) show that the probability of dropping out of college decreased during the Great Recession in Italy. However, once we condition on the unemployment rates of the pre-period and affected cohorts, the coefficient on post-period unemployment becomes statistically insignificant, albeit retaining a negative sign (Column 4 in Panel C of Table 3.1).

Overall, labor-market conditions at the time of high-school graduation significantly affect subsequent human-capital investment decisions. When unemployment rates are high, high-school graduates are more likely to enroll in college and to obtain a college degree. In contrast, college dropout seems unaffected by the labor-market conditions at high-school graduation.[40]

3.4.2 The Impact of Economic Conditions on Cognitive Skills

Next, going beyond the short-run effects on college investment decisions, we investigate whether economic conditions at high-school graduation also affect longer-run human capital formation, as measured by internationally comparable cognitive skills. Skills are assessed when individuals in our sample are between 25 and 39 years old, which is several years after finishing college for most. Figure 3.4, constructed analogously to Figure 3.3, presents the business cycle effects on literacy skills (Panel A) and numeracy skills (Panel B). We find that bad economic conditions at high-school graduation lead to significantly higher cognitive skills around five to 20 years later. Moreover, we again observe that only the unemployment rates around high-school graduation significantly affects skills, pointing to college enrollment as a potential mechanism of the skill increase (see Section 3.4.6).

Table 3.2 reports the regression results. As in Table 3.1, average unemployment rates before, at, and after high-school graduation first enter separately (corresponding to the coefficients shown in the right-hand graphs in Figure 3.4) and then simultaneously. In our preferred specification in Column 4, an increase in the unemployment rate of 10 pp at high-school graduation raises literacy skills by 0.09 SD (Panel A). The impact on numeracy skills is very similar; here, a 10 pp increase in the unemployment rate leads to an increase in numeracy skills by 0.08 SD (Panel B). These results suggest that labor-market conditions at high-school graduation do not only affect immediate college investment decisions, but have lasting impacts on the formation of cognitive skills.

3.4.3 The Impact of Economic Conditions on Labor-Market Outcomes

Most individuals in our sample of 25–to–39 year olds (87%) have already completed their formal education. This allows us to assess the impact of economic conditions at high-school graduation on two important labor-market outcomes: monthly wages and participation in training activities. In the wage analysis, we exclude all individuals who do not report a wage or who are still enrolled in any type of formal education.[41] When looking at training participation,

[40] Since business cycle effects on college dropout are close to zero and statistically insignificant, we do not present heterogeneity results or robustness checks for this outcome in Sections 3.4.4 and 3.4.5.

[41] Turkey reports monthly wages only in deciles and we were not able to retrieve decile means for the Turkish wage distribution. We thus exclude Turkey from the wage analysis. Additionally, wage information is missing for some respondents who report to be employed (7.5%). Figure A3.2 shows that the unemployment rate five years before up to six years after high-school graduation are unrelated to the composition of the wage sample, suggesting no issue of sample selectivity with respect to economic conditions.

persons currently not active in the labor market remain in the sample, because we have information on training activities on-the-job and off-the-job.

Panel A of Figure 3.5 reveals that individuals who faced worse economic conditions at the end of high school earn higher wages several years later. While unemployment rates before and after high-school graduation are unrelated to monthly earnings, a 10 pp higher unemployment rate during the sensitive period of college decision-making increases monthly wages by about 8%. The effect size is similar when additionally controlling for the labor-market conditions before and after the sensitive period (Column 4 of Table 3.3, Panel A).

In a further analysis, we divide the positive earnings effect into a labor-supply effect and a "productivity" effect (Table A3.4). An increase in the unemployment rate at high-school graduation by 10 pp increases the probability of working full-time (that is, at least 30 hours per week) by 4 pp (Panel A) and raises weekly working time by about 1.2 hours (Panel B).[42] However, among full-time employees,[43] we do not find robust evidence that economic conditions raise hourly wages (Panel C).[44] This suggests that adverse conditions at high-school graduation mainly affect monthly wages due to an increase in labor supply, while there is no strong evidence that hourly wages, a measure of productivity, are also affected.

Our finding that bad economic conditions at high-school graduation positively affect long-run labor-market success complements previous studies examining the business cycle effects at the time of college graduation (e.g., Kahn 2010; Oreopoulos, von Wachter and Heisz 2012; Altonji, Kahn and Speer 2016). These studies suggest a significant and persistent negative effect of entering the labor-market during recessionary periods. The important difference between these studies and our study is the critical period under investigation. While individuals at the end of college may have a hard time avoiding entering a recessionary labor market (e.g., by enrolling in graduate school, see Bedard and Herman 2008; Johnson 2013), individuals finishing high school can easily avoid entering a recessionary labor market by enrolling in college. Our study is the first that provides evidence on positive recession effects at high-school graduation on later labor-market outcomes.

Economic conditions at the time of high-school graduation not only affect monthly wages and the probability of working full-time, but also impact the likelihood of participating in training activities . Panel B of Figure 3.5 provides graphical evidence, while Panel B of Table 3.3 presents the corresponding regression results. In our preferred specification in Column 4 of Table 3.3, a 10 pp increase in the unemployment rate at high-school graduation leads to a 3.7 pp increase in training participation. This effect size translates to a 7% increase from

[42] We do not find any impact of the unemployment rate at high-school graduation on being employed at all (not shown).

[43] This analysis excludes self-employed because they do not report hourly wages in PIAAC.

[44] Ignoring labor-market conditions before and after the sensitive period (Column 2 in Table A3.4, Panel C), a 10 pp increase in the unemployment rate increases hourly wages of full-time employed workers by 4.6% (statistically significant at the 10% level).

the international mean (52%). The probability to be engaged in training is unaffected by the economic conditions before or after high-school graduation. Our results suggest that bad economic conditions at high-school graduation not only influence human capital formation due to their (immediate) effect on college education, but also due an increased propensity to participate in learning activities during the labor-market phase.

The business cycle effects on labor-market outcomes may of course be driven by college education, as is well known that college graduates earn more and are more likely to participate in on-the-job training and other lifelong learning activities. We explore this possibility in Section 3.4.6.

3.4.4 Heterogeneity by Gender and Parental Education

The impact of economic conditions at high-school graduation on college investment decisions, skill formation, and labor-market outcomes may differ across socio-economic groups. We now examine effect heterogeneity with respect to individuals' gender and socio-economic status (SES), proxied by the education level of parents.[45] For each of our six main outcomes – college enrollment, college degree, literacy skills, numeracy skills, monthly wages, and training participation – we augment our baseline specification (Equation 3.2) by an interaction of the unemployment rate at high-school graduation with the respective subsample indicator. All heterogeneity results are reported in Table 3.4.[46]

Economic conditions at high-school graduation affect the college investment decisions of men and women similarly (Column 1 of Table 3.4, Panels A and B). If anything, women tend to react slightly stronger to adverse labor-market conditions at the end of high school, but the interaction term is not statistically significant at conventional levels. However, we find heterogeneity in business cycle effects with respect to socio-economic background (Column 2). Experiencing a 10 pp higher unemployment rate at the time of high-school graduation increases both college enrollment and completion of low-SES individuals by 7 pp, and even by 11 pp for high-SES individuals.

The finding that economic decisions at high-school graduation are more relevant for college investment decisions of high-SES individuals are in line with ability-to-pay considerations (Dellas and Sakellaris 2003; Christian 2007). Low-SES parents are more likely to lose their jobs during economic downturns than their high-SES counterparts because low-skilled jobs are typically more severely hit by recessions. The associated income effect should lead to a more procyclical enrollment pattern (that is, higher college enrollment during boom periods) for individuals with low-educated parents. Thus, the lower (family) income during recessions likely

[45] We define high-SES as having at least one parent with a college degree (low-SES: neither parent has a college degree). Based on this definition, 32% of individuals in our sample are high-SES.
[46] Results are qualitatively similar if we additionally interact the unemployment rates of the pre- and post-high-school graduation periods with the female and high parental education indicator, respectively (results not shown).

prevents some individuals – especially those with lower-educated parents – from attending college due to liquidity constraints. Furthermore, our results are consistent with Dellas and Sakellaris (2003), who investigate the cyclicality of college enrollment in the United States from 1968 to 1988. While they observe no gender differences, there are significant differences across ethnic groups, with college enrollment of Blacks not being affected by labor-market condition. Similarly, Christian (2007) studies business cycle effects on U.S. college enrollment from 1968 to 2000. He finds a procyclical college enrollment pattern among people in households expected to have lower incomes, that is, where liquidity constraints are most likely binding.

When investigating heterogeneous business cycle effects on cognitive skills, we find substantial gender differences (Panels C and D of Table 3.4). The impact on both literacy and numeracy skills is roughly twice as large for females as compared to males. These findings suggest that women benefit more from recessions during such sensitive years in terms of skill formation, for instance, because of gender-differences in recession-induced field-of-study choices at college or differences in occupational choices later on the labor market. While cognitive skills of individuals with high-SES parents also seem to benefit slightly more from bad economic conditions at high-school graduation than their low-SES counterparts, interaction effects are not statistically significant.

The stronger business cycle effects on both literacy and numeracy skills for women also translate to higher success in the labor market (Panels E and F of Table 3.4). The effect of economic conditions at the end of high-school on both wages and training participation is more than twice as large for females as for males. There is no strong evidence that business cycle effects on wages differ by socio-economic background; however, high-SES individuals increase their participation in adult learning activities in response to recessionary periods at high-school graduation significantly more than low-SES individuals.

In sum, our heterogeneity analysis suggests that bad economic conditions at high-school graduation increase the education gap between individuals from low versus high socio-economic backgrounds. This is true for college enrollment, successfully completing college, and participating in training after formal schooling. The results for cognitive skills and monthly wages point in the same direction, but are less precisely estimated.

3.4.5 Robustness Analysis

In this section, we show that our results are robust to changes in the empirical specification and are not driven by specific countries or birth cohorts.

Changes in the Empirical Specification and Definitions of the Sensitive Period

Table A3.5 shows that our results are not affected by the individual-level control variables. The coefficients on the unemployment rate at high-school graduation on all main outcomes change very little going from the specifications without controls for individual background (odd columns) to those with background controls (even columns). These findings are reassuring that our estimates are not biased due to unobserved characteristics. Furthermore, Figure A3.3 shows how control variables are related to the unemployment rate from five years before up to six years after high-school graduation. None of the control variables shows a pattern similar to our outcome variables, and coefficients are mostly insignificant. A joint F-test of a regression of the unemployment rate in a given period on all background control variables reveals that controls are never jointly significant at a level of 5% or better.[47]

Furthermore, we estimate business cycle effects when unemployment rates are solely based on the age at official high-school graduation, that is, ignoring compulsory military service. While results are qualitatively similar, the estimated business cycle effects on all main outcome variables are somewhat reduced, consistent with higher measurement error (Table A3.7).[48] These results support our baseline definition of affected cohorts that accounts for compulsory military service periods, which reduces measurement error, increasing coefficients without affecting standard errors.[49]

Our preferred specification controls for the national economic conditions prior to and after the age at high-school graduation. By doing so, we account for both the serial correlation of unemployment rates and the common finding that economic decisions and beliefs are affected by conditions experienced early in life (Malmendier and Nagel 2011; Rao 2016). While we restrict our preferred model to incorporate just six years prior to high-school graduation, Table A3.8 also adds the unemployment rate seven to ten years before high-school graduation. Despite controlling for economic conditions in so many pre-years, the impact of the unemployment rate experienced in the sensitive period from one year prior up to two years after high-school graduation remains sizeable for all outcomes.[50] While coefficients become somewhat smaller for college investment, skills, and wages, the business cycle effect on training participation even increases. Strikingly, economic conditions in all considered years before high-school graduation are never significantly related to the respective outcome.[51] This

[47] Only the female composition seems to be related to unemployment rate during the years around high-school graduation. To check whether the change in the gender composition of the sample affects our findings, we add to our baseline specification gender-specific cohort fixed effects. This does not affect our results (see Table A3.6).

[48] In the estimations that do not adjust for compulsory military service, coefficients are between 8% (literacy skills) and 19% (training) smaller than in our main specification. The coefficient in the wage regression is even reduced by 40% and is much less precisely estimated.

[49] Heterogeneity analyses using this adjusted specification of hypothetical college-decision age show that women are affected by unemployment rates earlier than men, and that individuals in countries with compulsory military service are affected later than those in countries without conscription (results not shown). These findings further imply that a meaningful definition of the age at college decision-making should account for (gender-specific) compulsory military service.

[50] In this analysis, we lose 6% of observations because some countries do not report unemployment rates for early years. Results are qualitatively similar when we include unemployment rates of all years until birth (not shown).

[51] Exceptions are the unemployment rates in *t-5* (*t-6*), which are significantly negatively (positively) correlated with wages. Importantly, the economic conditions in these years are unrelated to all outcomes except for wages.

finding supports our interpretation that individuals adjust their college investment decisions due to the economic conditions at *high-school graduation*, rather than considering conditions in earlier periods.

Moreover, Table A3.9 shows that our results are robust to including country-specific linear trends in birth years. These trends allow the cohort effect to evolve differently across countries, absorbing cross-country differences in secular trends in college enrollment and, more generally, economic progress. However, at the same time, the country-specific linear trends may already capture part of the effect we are interested in, since the impact of economic conditions at high-school graduation is now identified only from deviations of the business cycle from the long-run trend. Accordingly, while the pattern of results is similar as in the baseline model, effects are less precisely estimated.

Excluding Country Groups and Birth Cohorts
The estimated countercyclical pattern of college investments and later-life outcomes is also robust to changes in the sample. Table A3.10 excludes all countries without exact age information, that is, Canada, New Zealand, and the United States. The effects on college investments, skill formation, and training participation hardly change. We even observe somewhat stronger effects on wages, which may be due to the reduction in measurement error due to the fact that wages in Canada and the United States are not reported continuously, but were approximated by the decile means (see Section 3.2). One may also conjecture that former communist countries may be special cases, because of their status as transition economies or because college decision-making may still be different than in other developed countries due to their communist heritage. When excluding the five former communist countries from our sample – the Czech Republic, Estonia, Poland, Slovak Republic, Slovenia – business cycle effects on college investments and wages are unchanged, while effects on skills and adult learning participation become considerably stronger than in the full sample.

Finally, we exclude each country or age group separately.[52] Results, shown in Tables A3.11 and A3.12, indicate that none of our main results is driven by specific countries or age groups. However, wage effects are substantially lower once we exclude Chile, which suggests that wages have a particularly strong countercyclical pattern in this country. One potential explanation for this result is that among our sample countries Chile has by far the highest returns to cognitive skills (Hanushek et al., 2017*b*). This is another indication that wage effects of bad economic conditions at high-school graduation may arise due to increased skills.[53]

Hence, we consider these results as statistical artefacts, as some statistically significant effects are likely to occur just by chance when considering 6*9=54 pre-period coefficients (as we do in Table A3.8).

[52] Since we cannot exclude single age-groups for Canada, New Zealand, and the United States due to missing precise age information, we always drop the respective five-year age cohort in these countries.

[53] Results are also robust to excluding first-generation immigrants from the analysis (not shown).

3.4.6 Instrumental Variable Approach

In this section, we provide suggestive evidence concerning the mechanisms that underlie the effect of economic conditions at high-school graduation on skill formation and labor-market outcomes. In particular, we explore the role of recession-induced college investment in explaining the reduced-form impact on skill formation and labor-market success. To do so, we estimate an instrumental-variable (IV) model, instrumenting college enrollment with the unemployment rate at high-school graduation.[54] This exercise relies on the assumption that, conditional on the covariates, business cycle conditions at the time of high-school graduation are orthogonal to all factors (other than college enrollment) influencing later-life outcomes. Although it seems unlikely that the positive (reduced-form) effects on skill formation and labor market outcomes are entirely driven by college education, our results provide tentative evidence on the importance of recession-induced college education for long-run human capital formation and labor-market success.

Results of the two-stage least squares estimations of the effects of college education, in terms of both cognitive skills and labor-market success, are shown in Panels B and C of Table 3.5. Panel A provides the OLS results for comparison. In the OLS estimations, college enrollment is associated with 0.62 SD higher literacy and numeracy skills, 30% higher wages, and a 22 pp higher participation in adult learning activities. The corresponding IV results suggest substantially larger positive effects of college enrollment on later-life outcomes (Panel B), even when conditioning on labor-market conditions in the periods before and after high-school graduation (Panel C).[55] Controlling for the average unemployment rate after high-school graduation is particularly important in the IV specification. As a growing literature shows that labor-market conditions at the time of labor-market entry have large and persistent negative effects on career outcomes, one may suspect that the exclusion restriction is violated without this control due to the serial correlation of unemployment rates. However, reassuringly, results are little affected.

The fact that the IV coefficients are larger than the OLS coefficients can potentially be explained by the fact that the complier population whose effect is identified in the IV model has higher returns to college education than the average individual. Our complier population consists of marginal students, that is, individuals who would not have attended college in good economic times at high-school graduation. Previous evidence by Card (1993) shows that the returns to college education are higher for marginal students than for an average student, since marginal students require higher expected returns to be induced to attend college than infra-marginal students (Kaufmann, 2014).

[54] Results are very similar when instrumenting college degree instead.

[55] First-stage F-statistics show that the instrument is strong, with values of around 25 in the samples with information on skills or training, and values of 10–13 in the (much smaller) sample of persons with wages. Thus, weak-instrument bias (Staiger and Stock 1997; Stock, Wright and Yogo 2002) is unlikely to be a problem in our context.

3.5 Conclusion

We investigate the effects of economic conditions at high-school graduation by exploiting variation in national unemployment rates across countries and birth cohorts. We find that economic conditions at high-school graduation affect college investments. An increase in the unemployment rate increases college enrollment as well as college completion.

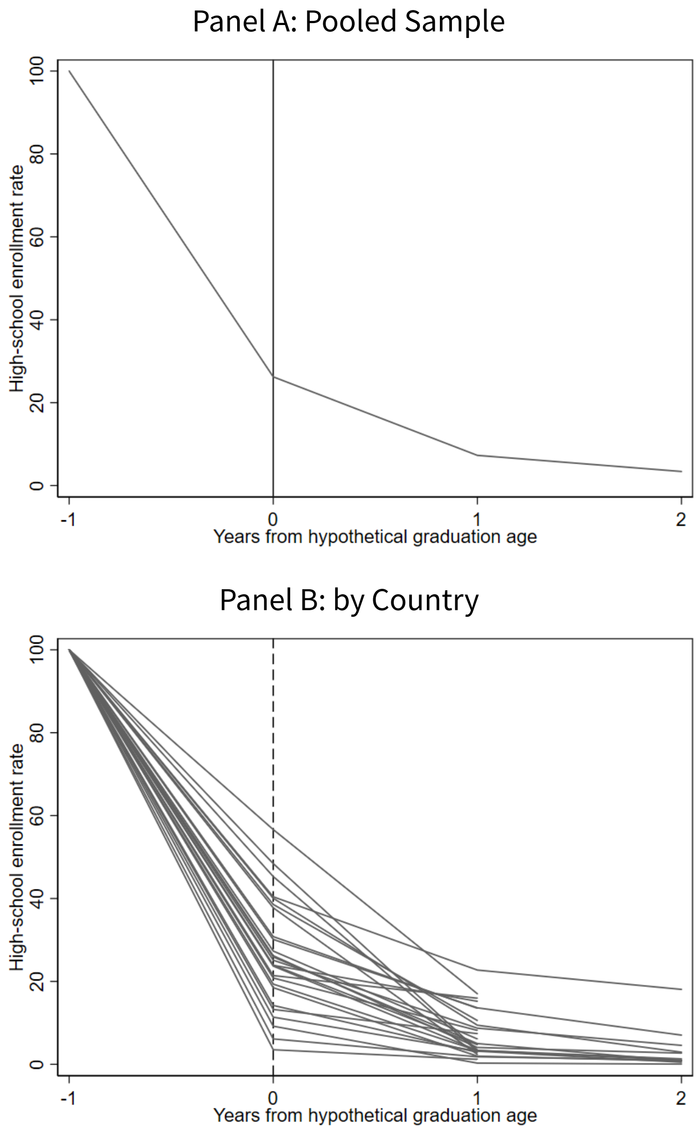
We are the first who show that economic conditions at high-school graduation also affect longer-run outcomes. Our findings suggest a positive effect of bad economic conditions at high-school graduation on literacy and numeracy skills as well as on wages and training participation. Instrumental-variable estimates provide tentative evidence that college education is one important mechanism through which these bad economic conditions affect cognitive skills and labor-market outcomes.

Importantly, all outcomes are affected the most by the economic conditions at high-school graduation, whereas the economic conditions in earlier or later years typically have negligible effects. This suggests that academically marginal students, who are the focus of many policy programs aiming to increase the attractiveness of college education, seem to make their college investment decisions toward the end of high school. This has important implications for the timing of policy measures to foster the transition between high school and college.

Finally, we also find that the positive effect of bad economic conditions at high-school graduation on college education is stronger for individuals of higher socio-economic background. This finding suggests that bad economic conditions at high-school graduation tend to increase educational inequality by widening the education gap between individuals from low versus high socio-economic backgrounds.

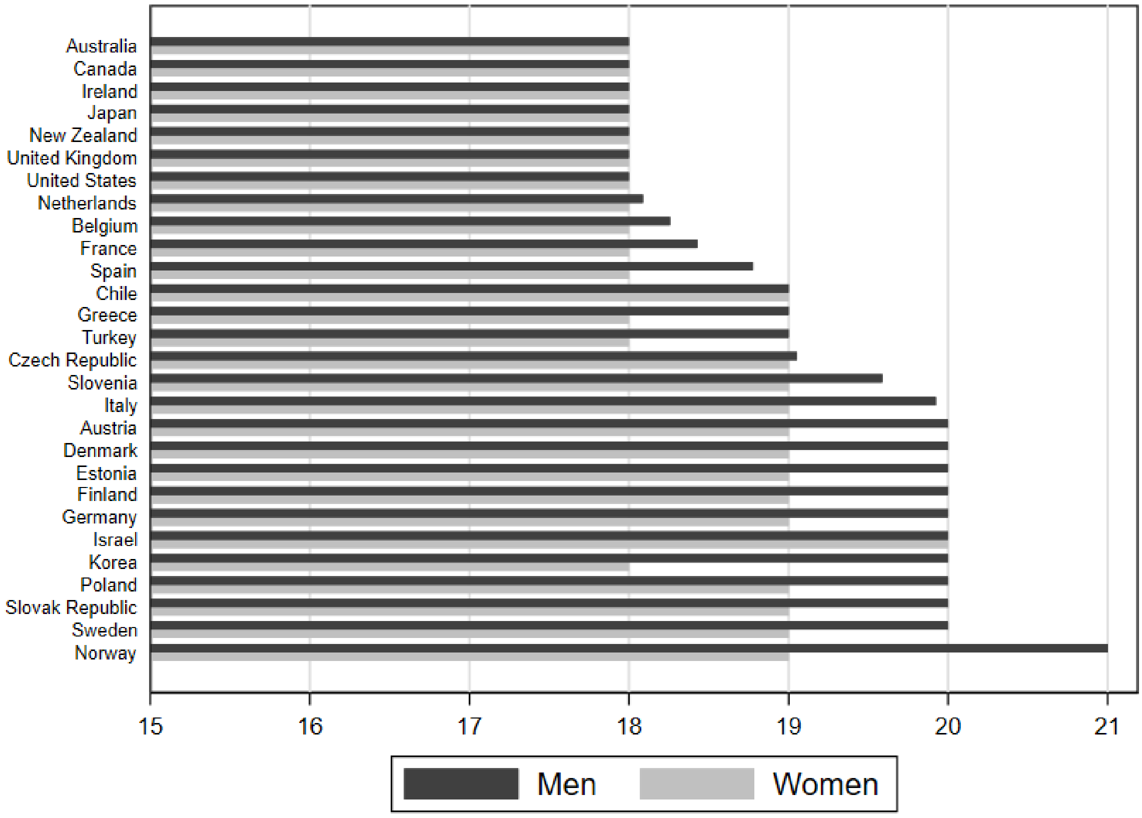
Figures and Tables

Figure 3.1 : High-School Enrollment Rate, by Distance to Graduation Age



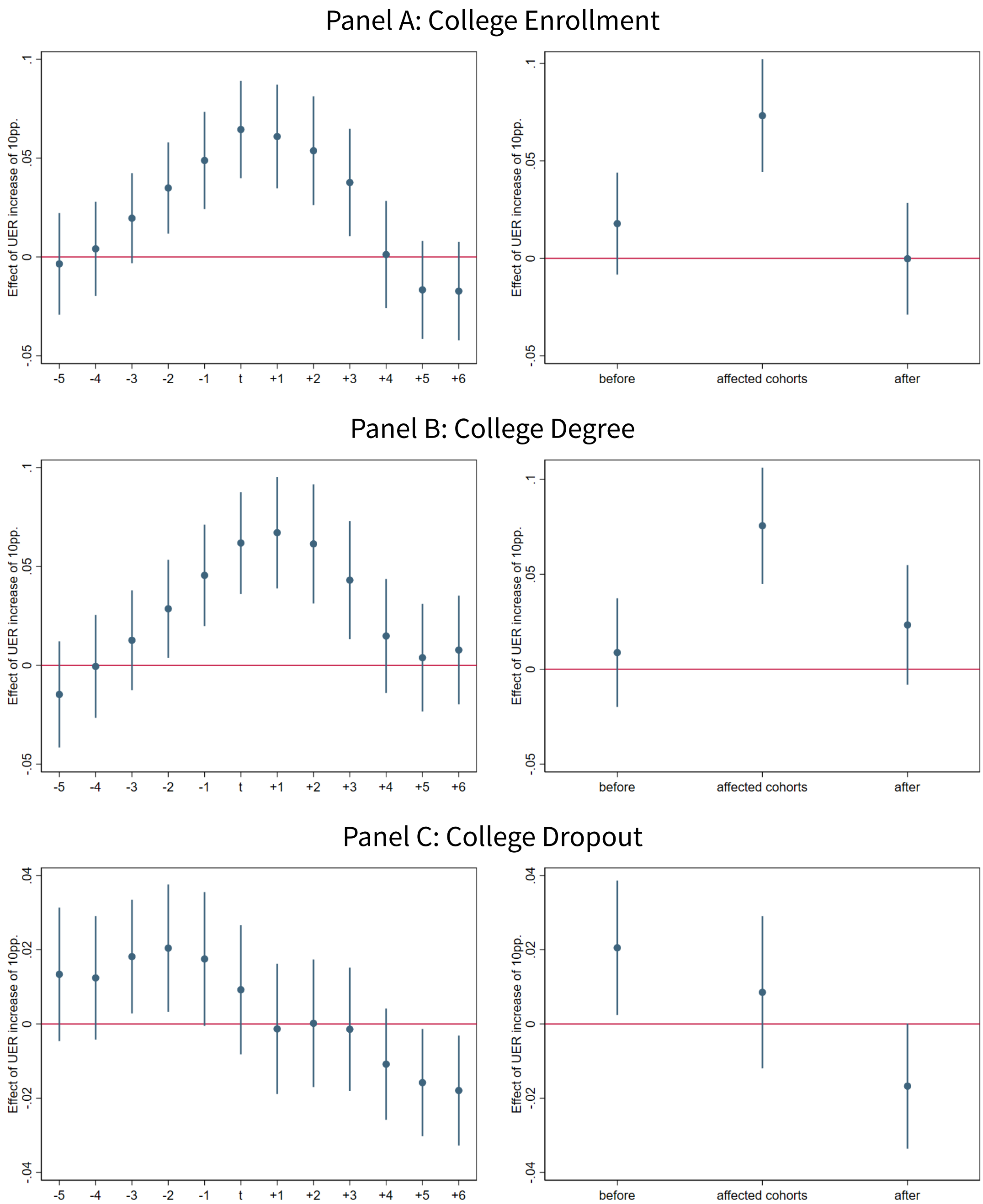
Notes: Figure reports enrollment rates in upper secondary education (high school), by distance to hypothetical graduation age. The enrollment share one year prior to the hypothetical graduation age is normalized to 100. Panel A shows average enrollment rates across the pooled sample of countries, Panel B shows enrollment rates for each country separately. No information on enrollment rates available for Canada an the United States. *Data source:* OECD Education at a Glance (2015); OECD (1999), Classifying Educational Programmes – Manual for ISCED-97 Implementation in OECD Countries, see `http://www.oecd.org/education/1841854.pdf`; UNESCO Institute for Statistics.

Figure 3.2 : Hypothetical Age of College Decision-Making



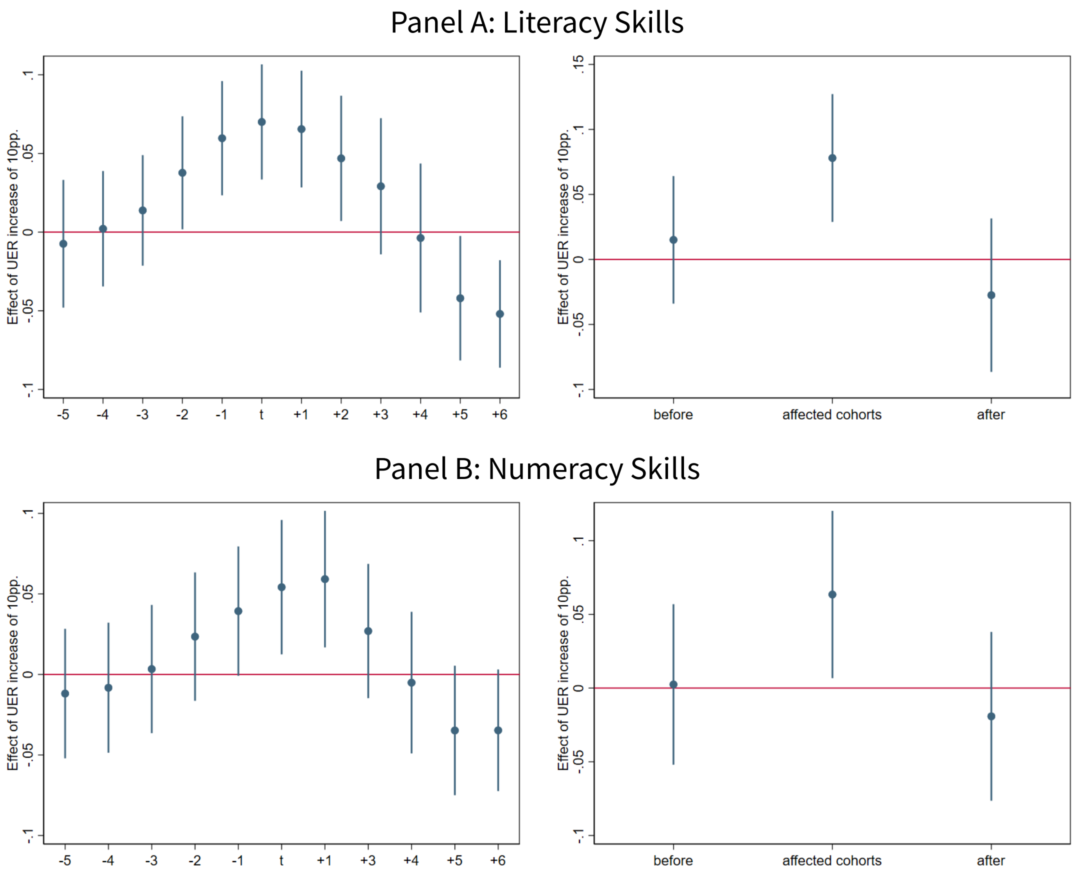
Notes: Figure reports hypothetical age when individuals decide on their tertiary education investments, separately for men and women. Values derived by the hypothetical age when graduating from high school plus years of military service in countries with compulsory military service. In all countries, except Israel, only men were considered for conscription. Conscription ended during our observational period in the Netherlands (1991), Belgium (1992), Czech Republic (1992), France (1996), Spain (2001), Slovenia (2003), and Italy (2004). Thus, country-averages of hypothetical years when deciding on tertiary education investments differ from integer ages in these countries. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. *Data source:* CIA Factbook, country-specific information about compulsory military service, OECD (1999), Classifying Educational Programmes – Manual for ISCED-97 Implementation in OECD Countries, see `http://www.oecd.org/education/1841854.pdf`; UNESCO Institute for Statistics.

Figure 3.3 : Business Cycle Effects on College Outcomes



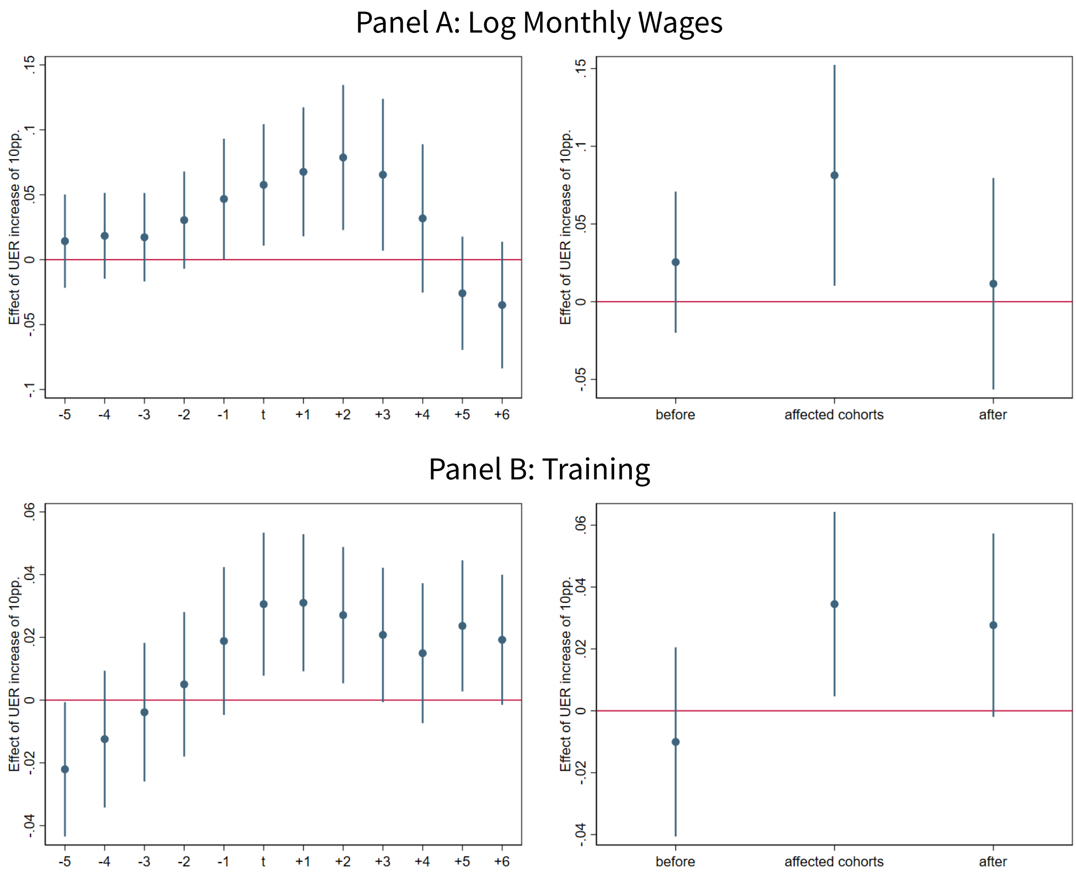
Notes: Coefficient plot. Separate OLS regressions of college variables on unemployment rate in the year indicated on the horizontal axis (t denoting the hypothetical year when deciding on tertiary education investments, see Figure 3.2). Dependent variable: college enrollment (Panel A), college degree (Panel B), college dropout (Panel C). Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Unemployment rate is divided by 10 throughout. Dots indicate the estimated coefficient size, vertical lines the 95 percent confidence interval. Right hand side: "before" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "affected cohorts" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t-1, t, t+1, t+2; "after" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t+3, t+4, t+5, and t+6. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. In regressions using college dropouts, information about currently enrolled students as well as information for individuals in the United States is missing. *Data source:* PIAAC 2012/2015.

Figure 3.4 : Business Cycle Effects on Cognitive Skills



Notes: Coefficient plot. Separate OLS regressions of skills on unemployment rate in the year indicated on the horizontal axis (t denoting the hypothetical year when deciding on tertiary education investments, see Figure 3.2). Dependent variable: literacy skills (Panel A), numeracy skills (Panel B). Skills are standardized to have mean 0, SD 1, within each country. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Unemployment rate is divided by 10 throughout. Dots indicate the estimated coefficient size, vertical lines the 95 percent confidence interval. Right hand side: "before" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "affected cohorts" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t-1, t, t+1, t+2; "after" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t+3, t+4, t+5, and t+6. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects.
Data source: PIAAC 2012/2015.

Figure 3.5 : Business Cycle Effects on Labor-Market Outcomes



Notes: Coefficient plot. Separate OLS regressions of labor-market outcomes on unemployment rate in the year indicated on the horizontal axis (t denoting the hypothetical year when deciding on tertiary education investments, see Figure 3.2). Dependent variable: log monthly wage (Panel A), participation in adult learning activities (Panel B). Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad and current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. Unemployment rate is divided by 10 throughout. Dots indicate the estimated coefficient size, vertical lines the 95 percent confidence interval. Right hand side: "before" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "affected cohorts" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t-1, t, t+1, t+2; "after" presents the estimated coefficient of a regression using the simple average of unemployment rates in years t+3, t+4, t+5, and t+6. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. *Data source:* PIAAC 2012/2015.

Table 3.1 : Business Cycle Effects on College Outcomes

	(1)	(2)	(3)	(4)
Panel A: College Enrollment				
UER – before	.018			−.006
	(.013)			*(.014)*
UER – affected cohorts		.073***		.077***
		(.015)		*(.016)*
UER – after			−.000	−.014
			(.015)	*(.014)*
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	51241	51241	51241	51241
Panel B: College Degree				
UER – before	.009			−.010
	(.015)			*(.016)*
UER – affected cohorts		.076***		.077***
		(.016)		*(.017)*
UER – after			.023	.008
			(.016)	*(.016)*
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	51241	51241	51241	51241
Panel C: College Dropout				
UER – before	.021**			.016*
	(.009)			*(.010)*
UER – affected cohorts		.009		.006
		(.010)		*(.011)*
UER – after			−.017*	−.014
			(.009)	*(.009)*
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	43149	43149	43149	43149

Notes: Ordinary least squares estimations. Dependent variable: college enrollment (Panel A), college degree (Panel B), college dropout (Panel C). Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. "UER – before" presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; "UER – after" presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. In regressions using college dropouts, information about currently enrolled students as well as information for individuals in the United States is missing. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table 3.2 : Business Cycle Effects on Cognitive Skills

	(1)	(2)	(3)	(4)
Panel A: Literacy Skills				
UER – before	.015			–.018
	(.025)			*(.028)*
UER – affected cohorts		.078***		.091***
		(.025)		*(.027)*
UER – after			–.027	–.047
			(.030)	*(.031)*
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	51241	51241	51241	51241
Panel B: Numeracy Skills				
UER – before	.002			–.026
	(.028)			*(.029)*
UER – affected cohorts		.063**		.077***
		(.029)		*(.030)*
UER – after			–.019	–.038
			(.029)	*(.030)*
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	51241	51241	51241	51241

Notes: Ordinary least squares estimations. Dependent variable: literacy skills (Panel A), numeracy skills (Panel B). Skills are standardized to have mean 0, SD 1, within country. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. "UER – before" presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; "UER – after" presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table 3.3 : Business Cycle Effects on Labor-Market Outcomes

	(1)	(2)	(3)	(4)
Panel A: Log Monthly Wage				
UER – before	.025			.007
	(.023)			*(.024)*
UER – affected cohorts		.081**		.079**
		(.036)		*(.037)*
UER – after			.012	.000
			(.035)	*(.034)*
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	30638	30638	30638	30638
Panel D: Training				
UER – before	−.010			−.016
	(.016)			*(.016)*
UER – affected cohorts		.034**		.037**
		(.015)		*(.016)*
UER – after			.028*	.018
			(.015)	*(.016)*
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	44488	44488	44488	44488

Notes: Ordinary least squares estimations. Dependent variable: log monthly wage (Panel A), participation in adult learning activities (Panel B). Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad and current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. “UER – before” presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; “UER – affected cohorts” presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; “UER – after” presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother’s and father’s education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table 3.4 : Heterogeneity in Business Cycle Effects

	(1)	(2)
Panel A: College Enrollment		
UER – affected cohorts	.067***	.070***
	(.017)	*(.016)*
Female × UER – affected cohorts	.020	
	(.013)	
Uni Parents × UER – affected cohorts		.039***
		(.012)
Panel B: College Degree		
UER – affected cohorts	.070***	.072***
	(.018)	*(.018)*
Female × UER – affected cohorts	.015	
	(.013)	
Uni Parents × UER – affected cohorts		.040***
		(.012)
Panel C: Literacy Skills		
UER – affected cohorts	.063**	.094***
	(.030)	*(.028)*
Female × UER – affected cohorts	.054**	
	(.025)	
Uni Parents × UER – affected cohorts		.032
		(.025)
Panel D: Numeracy Skills		
UER – affected cohorts	.048	.081**
	(.033)	*(.032)*
Female × UER – affected cohorts	.058**	
	(.026)	
Uni Parents × UER – affected cohorts		.017
		(.023)
Panel E: Log Monthly Wage		
UER – affected cohorts	.050	.075*
	(.037)	*(.039)*
Female × UER – affected cohorts	.064**	
	(.032)	
Uni Parents × UER – affected cohorts		.029
		(.020)
Panel F: Training		
UER – affected cohorts	.022	.029*
	(.017)	*(.016)*
Female × UER – affected cohorts	.029**	
	(.013)	
Uni Parents × UER – affected cohorts		.037**
		(.015)
Country FE	✓	✓
Cohort FE	✓	✓

Notes: Ordinary least squares estimations. Dependent variable: college enrollment (Panel A), college degree (Panel B), literacy skills (Panel C), numeracy skills (Panel D), log monthly wage (Panel E), participation in adult learning activities (Panel F). Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for unemployment rates before and after the affected period, gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table 3.5 : IV Analysis – Effect of Recession-Induced College Education

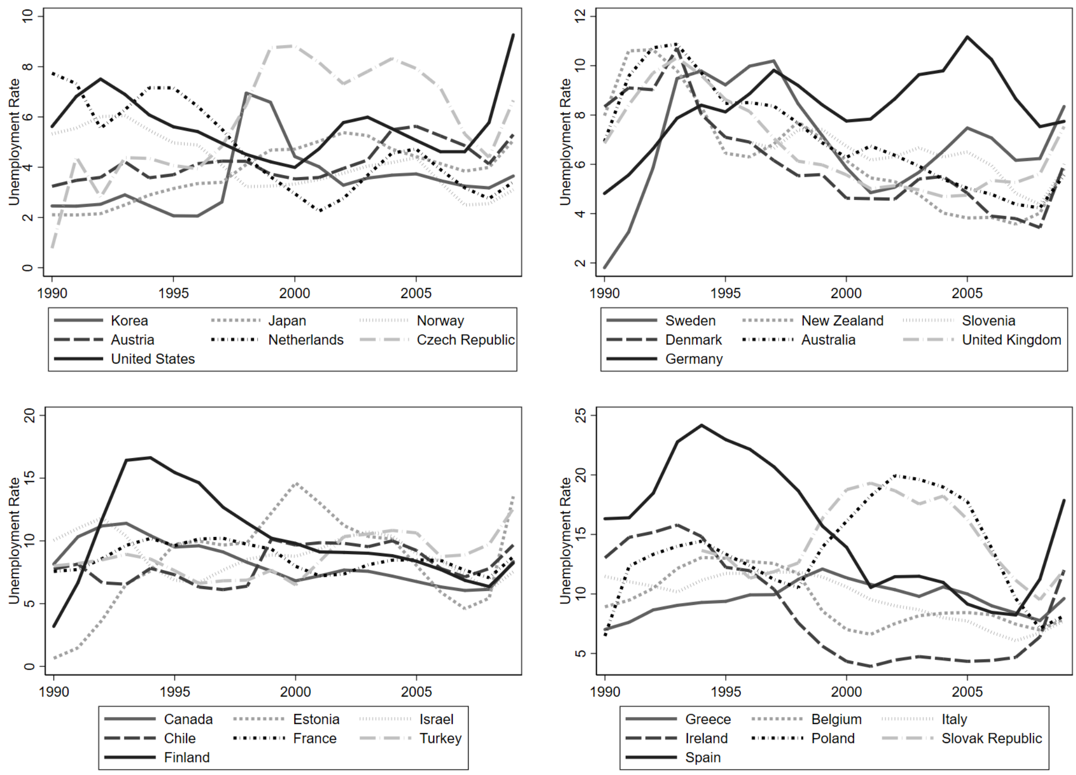
	Literacy	Numeracy	Wage	Training
Panel A: OLS				
College Enrollment	.617***	.623***	.297***	.217***
	(.011)	*(.011)*	*(.013)*	*(.006)*
Panel B: 2SLS				
College Enrollment	1.066***	.867**	1.121**	.440**
	(.326)	*(.373)*	*(.538)*	*(.193)*
Instrument F stat.	24.75	24.75	10.28	22.45
Observations	51241	51241	30638	44488
Panel C: 2SLS, conditional on UER before & after				
College Enrollment	1.178***	1.000***	.940**	.438**
	(.346)	*(.378)*	*(.445)*	*(.187)*
Instrument F stat.	24.65	24.65	13.32	24.37
Observations	51241	51241	30638	44488

Notes: Ordinary least squares (Panel A) and two stage least squares estimations (Panel B and C). Dependent variables indicated in the column header. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. College enrollment is instrumented by "UER – affected cohorts", which presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Appendix

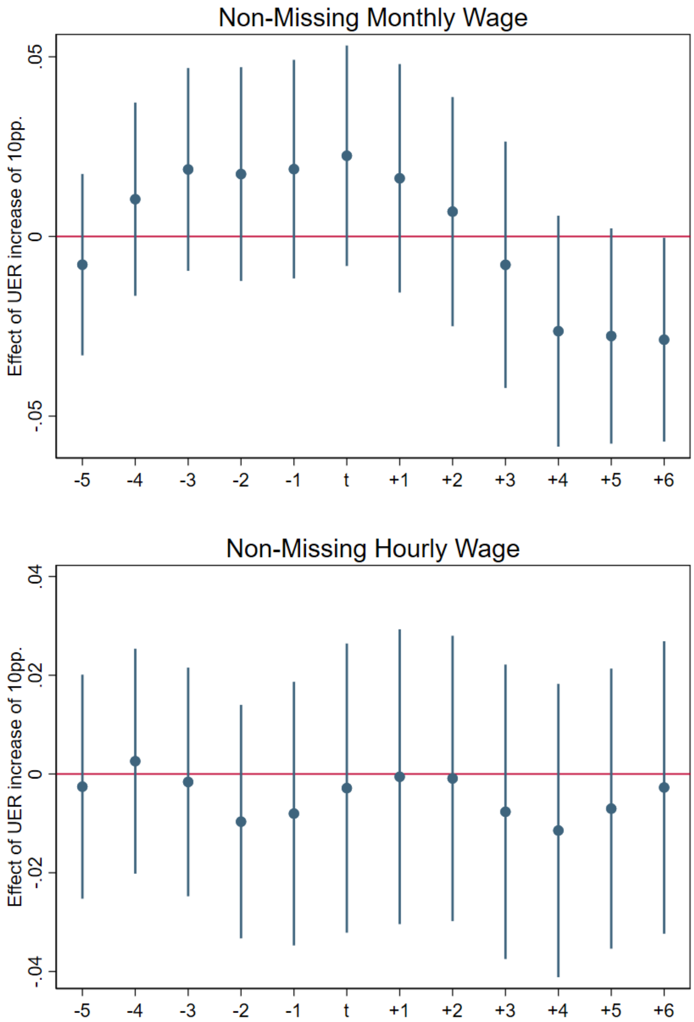
Appendix A3.1 Appendix Figures and Tables

Figure A3.1 : Variation in Unemployment Rate Across Countries and Over Time



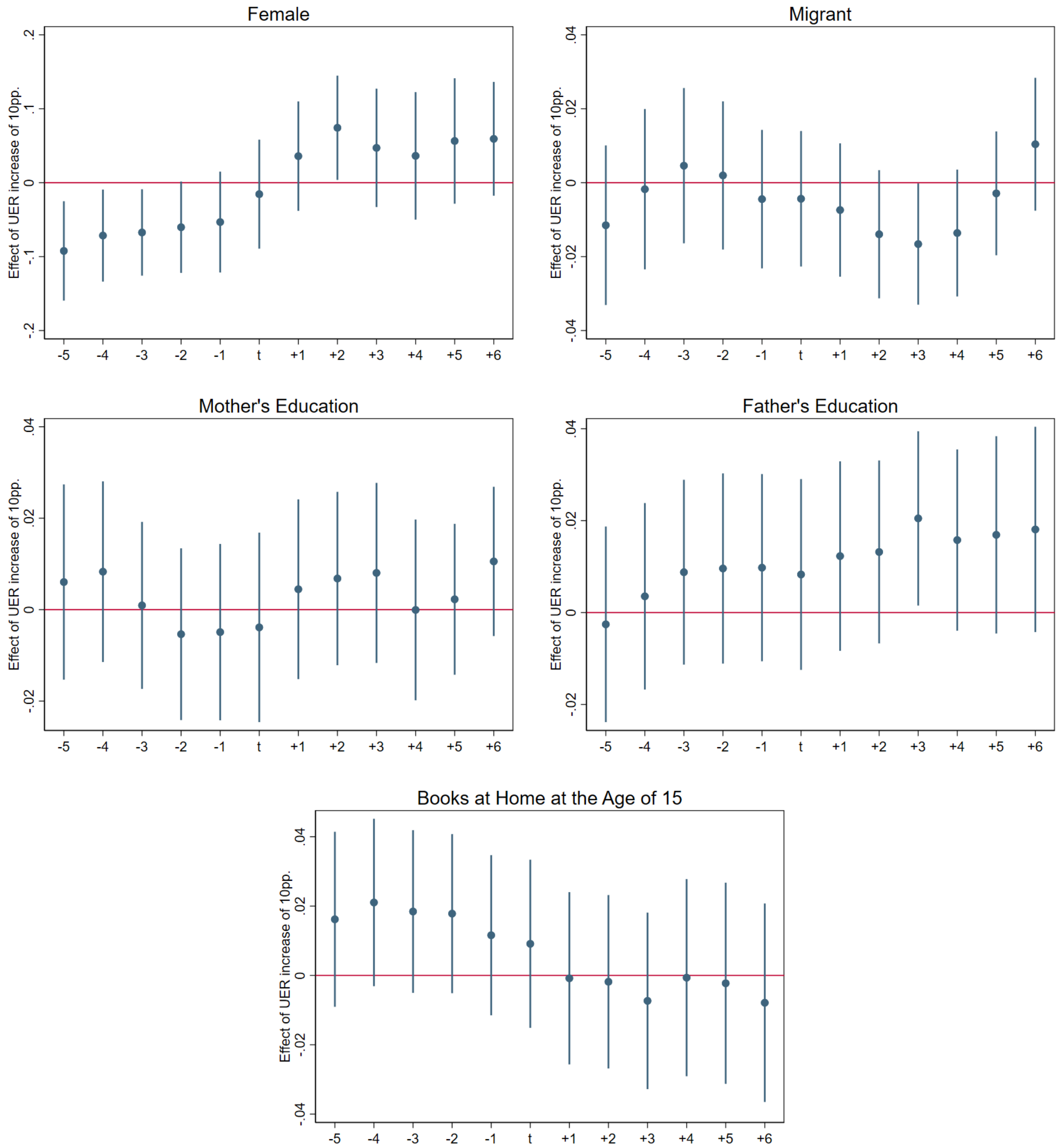
Notes: Figure denotes development of annual national unemployment rate between 1990 until 2009 for each of the 28 countries in our sample. Partly missing information on unemployment rates in the Slovak Republic (before 1994) and Slovenia (1996). Countries are displayed in ascending order of the mean unemployment rate across the observational period (lowest quartile of countries in upper left panel, highest quartile in lower right panel). *Data source:* OECD.

Figure A3.2 : Balancing of Availability of Wage Information



Notes: Coefficient plot. Separate OLS regressions of indicators for availability of wage information on unemployment rate in the year indicated on the horizontal axis (t denoting the hypothetical year when deciding on tertiary education investments, see Figure 3.2). Dependent variable indicated in each figure's title. The indicator for non-missing monthly (hourly) wage takes the value 1 if the individual has non-missing monthly (hourly) wage information, zero otherwise. The indicator for hourly wage information only considers full-time employed individuals. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Unemployment rate is divided by 10 throughout. Dots indicate the estimated coefficient size, vertical lines the 95 percent confidence interval. All specifications include country and birth-year fixed effects. *Data source:* PIAAC 2012/2015.

Figure A3.3 : Balancing of Background Variables



Notes: Coefficient plot. Separate OLS regressions of control variables on unemployment rate in the year indicated on the horizontal axis (t denoting the hypothetical year when deciding on tertiary education investments, see Figure 3.2). Dependent variable indicated in each figure's title. Mother's and father's education is a dummy that takes the value 1 if the mother/father holds a college degree, zero otherwise. Books at home at the age of 15 is a dummy which takes the value 1 if the respondent states to having had more than 100 books at home, zero otherwise. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Unemployment rate is divided by 10 throughout. Dots indicate the estimated coefficient size, vertical lines the 95 percent confidence interval. All specifications include country and birth-year fixed effects. *Data source:* PIAAC 2012/2015.

Table A3.1 : Descriptive Statistics – Main Variables

Country	Obs.	Enrollment	Degree	Dropout (abs.)	Dropout (rel.)	Literacy skills	Numeracy skills	Monthly wage (nat. curr.)	Training	Unemploment rate (t)
Australia	2185	0.56	0.46	0.09	0.16	291.9	280.2	5.3	0.56	7.9
Austria	1267	0.33	0.21	0.07	0.18	283.2	289.4	2.6	0.56	4.2
Belgium	1281	0.57	0.48	0.12	0.20	291.5	295.4	3.1	0.54	10.2
Canada	7189	0.68	0.60	0.13	0.22	284.6	276.2	4.2	0.59	8.8
Chile	1610	0.52	0.38	0.13	0.27	231.5	220.0	697.2	0.54	8.3
Czech Republic	1446	0.39	0.28	0.11	0.26	287.1	286.0	25.5	0.53	6.9
Denmark	1572	0.58	0.49	0.09	0.15	283.9	288.9	83.5	0.67	6.3
Estonia	1727	0.59	0.45	0.13	0.22	285.2	282.7	1.2	0.61	10.4
Finland	1516	0.62	0.48	0.10	0.15	308.5	301.1	2.8	0.69	11.3
France	1839	0.46	0.41	0.07	0.13	276.4	268.8	2.1	0.38	8.9
Germany	1417	0.34	0.23	0.07	0.18	280.2	281.3	2.4	0.53	8.6
Greece	1535	0.41	0.35	0.04	0.09	255.9	256.1	1.0	0.25	10.0
Ireland	2209	0.53	0.44	0.10	0.17	274.7	264.3	2.8	0.49	9.5
Israel	1845	0.66	0.51	0.12	0.19	266.1	263.2	19.4	0.47	8.4
Italy	1374	0.34	0.21	0.08	0.27	258.0	258.9	1.8	0.27	10.3
Japan	1555	0.58	0.55	0.03	0.04	309.0	297.5	302.2	0.46	3.7
Korea	2004	0.69	0.59	0.06	0.07	288.5	278.7	2,533.8	0.60	3.6
Netherlands	1246	0.52	0.41	0.10	0.18	297.7	292.5	3.0	0.68	5.1
New Zealand	1641	0.64	0.54	0.10	0.18	288.6	279.5	15.3	0.66	5.9
Norway	1411	0.55	0.46	0.08	0.13	290.0	287.3	35.3	0.66	4.3
Poland	2194	0.54	0.45	0.08	0.15	277.3	269.4	3.4	0.43	15.6
Slovak Republic	859	0.40	0.30	0.08	0.18	278.6	279.9	1.7	0.34	17.7
Slovenia	804	0.63	0.33	0.17	0.25	270.4	273.7	1.3	0.49	5.9
Spain	1892	0.49	0.40	0.10	0.19	263.4	257.5	2.3	0.50	16.9
Sweden	1201	0.45	0.39	0.06	0.12	290.0	288.0	26.6	0.66	7.4
Turkey	2147	0.25	0.21	0.03	0.10	232.0	227.4		0.24	8.6
United Kingdom	2783	0.56	0.49	0.08	0.13	281.8	269.8	2.2	0.53	7.1
United States	1492	0.44	0.44			275.0	259.9	3.8	0.59	5.6
Total	51241	0.51	0.42	0.09	0.16	278.5	273.6	153.8	0.52	8.3

Notes: Table reports country means for main variables. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. College enrollment is a binary indicator which equals 1 if individual ever attended college. College dropout indicates whether the individual dropped out of college, as overall share (abs.) and as percentage of enrolled students (rel.). Literacy and numeracy skills are measured on a 500-point scale. Monthly wages (gross) include bonuses and are for wage and salary earners and self-employed; reported in national currency, divided by 1000. Training is a dummy which takes the value 1 if the individual has taken part in any kind of adult learning activities during the year prior to the interview, zero otherwise. Unemployment rate corresponds to the mean annual unemployment rate at the hypothetical age when deciding on tertiary education investments (see Figure 3.2). Statistics weighted by sampling weights. *Data source:* PIAAC 2012/2015.

Table A3.2 : Descriptive Statistics – Control Variables

Country	Obs.	Female	Migrant	Age	Education mother	Education father	Books at home
Australia	2185	0.51	0.25	31.9	1.80	1.85	3.52
Austria	1267	0.50	0.07	32.0	1.75	1.99	3.36
Belgium	1281	0.49	0.06	32.0	1.88	1.97	2.82
Canada	7189	0.51	0.28	32.0	2.15	2.13	3.28
Chile	1610	0.50	0.06	32.0	1.64	1.75	2.22
Czech Republic	1446	0.48	0.04	30.0	1.98	2.10	3.99
Denmark	1572	0.50	0.14	32.0	1.99	2.04	3.88
Estonia	1727	0.50	0.06	30.5	2.24	2.15	4.15
Finland	1516	0.49	0.06	32.0	1.90	1.86	3.84
France	1839	0.51	0.11	32.0	1.66	1.78	3.21
Germany	1417	0.48	0.18	32.0	2.06	2.21	3.59
Greece	1535	0.50	0.09	32.0	1.50	1.56	2.56
Ireland	2209	0.52	0.26	32.0	1.67	1.62	3.12
Israel	1845	0.50	0.18	32.0	2.10	2.06	3.37
Italy	1374	0.49	0.11	32.0	1.34	1.37	2.86
Japan	1555	0.49	0.00	32.0	2.17	2.16	2.92
Korea	2004	0.47	0.02	32.0	1.51	1.76	3.18
Netherlands	1246	0.50	0.13	32.0	1.56	1.82	3.54
New Zealand	1641	0.53	0.28	31.9	1.88	1.97	3.52
Norway	1411	0.49	0.18	32.1	1.98	2.06	4.07
Poland	2194	0.50	0.00	30.0	1.98	2.01	3.30
Slovak Republic	859	0.48	0.00	28.0	1.89	1.94	3.20
Slovenia	804	0.46	0.10	28.5	1.91	1.95	2.93
Spain	1892	0.50	0.17	32.0	1.31	1.45	3.18
Sweden	1201	0.50	0.18	32.0	2.03	1.95	4.08
Turkey	2147	0.49	0.00	32.0	1.08	1.18	1.80
United Kingdom	2783	0.50	0.19	32.0	1.91	2.00	3.36
United States	1492	0.51	0.18	31.9	2.13	2.11	2.98
Total	51241	0.50	0.13	31.7	1.81	1.87	3.27

Notes: Table reports country means for all control variables included in the estimation equations. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Migrant is a dummy variable which takes the value 1 if the individual is a first-generation migrant. Mother's and father's highest level of education in 3 categories: 1 (ISCED 1, 2, and 3C short), 2 (ISCED 3 and 4), 3 (ISCED 5 and 6). Books at home when the individual was 15 in 6 categories: 1 (10 books or less) to 6 (more than 500 books). Statistics weighted by sampling weights. *Data source:* PIAAC 2012/2015.

Table A3.3 : Business Cycle Effects – Unemployment Rate in Single Years

	t-5	t-4	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4	t+5	t+6
Panel A: College Enrollment												
UER	–.003	.004	.020*	.035***	.049***	.064***	.061***	.054***	.038***	.001	–.017	–.017
	(.013)	*(.012)*	*(.012)*	*(.012)*	*(.012)*	*(.013)*	*(.013)*	*(.014)*	*(.014)*	*(.014)*	*(.013)*	*(.013)*
Observations	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241
Panel B: College Degree												
UER	–.015	–.001	.013	.029**	.045***	.062***	.067***	.061***	.043***	.015	.004	.008
	(.014)	*(.013)*	*(.013)*	*(.013)*	*(.013)*	*(.013)*	*(.014)*	*(.015)*	*(.015)*	*(.015)*	*(.014)*	*(.014)*
Observations	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241
Panel C: Literacy Skills												
UER	–.007	.002	.014	.038*	.060***	.070***	.065***	.047*	.029	–.004	–.042*	–.052**
	(.025)	*(.022)*	*(.021)*	*(.022)*	*(.022)*	*(.022)*	*(.023)*	*(.024)*	*(.026)*	*(.029)*	*(.024)*	*(.021)*
Observations	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241
Panel D: Numeracy Skills												
UER	–.012	–.008	.003	.023	.039	.054**	.059**	.045*	.027	–.005	–.035	–.035
	(.024)	*(.024)*	*(.024)*	*(.024)*	*(.024)*	*(.025)*	*(.026)*	*(.025)*	*(.025)*	*(.027)*	*(.024)*	*(.023)*
Observations	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241	51241
Panel E: Log Monthly Wage												
UER	.014	.018	.017	.030	.047*	.058**	.068**	.079**	.065*	.032	–.026	–.035
	(.022)	*(.020)*	*(.021)*	*(.023)*	*(.028)*	*(.028)*	*(.030)*	*(.034)*	*(.035)*	*(.035)*	*(.026)*	*(.030)*
Observations	30638	30638	30638	30638	30638	30638	30638	30638	30638	30638	30638	30638
Panel F: Training												
UER	–.022*	–.012	–.004	.005	.019	.031**	.031**	.027**	.021	.015	.024*	.019
	(.013)	*(.013)*	*(.013)*	*(.014)*	*(.014)*	*(.014)*	*(.013)*	*(.013)*	*(.013)*	*(.014)*	*(.013)*	*(.013)*
Observations	44488	44488	44488	44488	44488	44488	44488	44488	44488	44488	44488	44488

Notes: Ordinary least squares estimations. Dependent variable: college enrollment (Panel A), college degree (Panel B), literacy skills (Panel C), numeracy skills (Panel D), log monthly wage (Panel E), participation in adult learning activities (Panel F). Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. Unemployment rate is measured in the year indicated in the column header, where t denotes the hypothetical year when individuals decide on tertiary education investments (see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.4 : Business Cycle Effects on Working-time and Hourly Wage

	(1)	(2)	(3)	(4)
Panel A: Full-Time Employment				
UER – before	−.025			−.036**
	(.017)			(.017)
UER – affected cohorts		.028*		.039**
		(.017)		(.017)
UER – after			.012	−.003
			(.016)	(.017)
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	38315	38315	38315	38315
Panel B: Hours worked				
UER – before	.623			.380
	(.420)			(.417)
UER – affected cohorts		1.278***		1.154**
		(.479)		(.488)
UER – after			.176	.134
			(.570)	(.598)
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	30882	30882	30882	30882
Panel C: Log Hourly Wage (full-time employed workers only)				
UER – before	.038*			.036
	(.021)			(.022)
UER – affected cohorts		.046*		.033
		(.026)		(.027)
UER – after			.018	.023
			(.024)	(.026)
Country FE	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓
Observations	21224	21224	21224	21224

Notes: Ordinary least squares estimations. Dependent variable: Dummy indicating whether individual is full-time employed (Panel A), hours worked per week (Panel B), log hourly wage (Panel C). Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad and current students. Canada is excluded from the analysis because of missing information on full-time employment and hours worked. Hours worked is not reported for PIAAC respondents in Australia and Austria, hence these countries are excluded in specifications using hours worked. Specifications using hourly wage are restricted to full-time employed workers. "UER – before" presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; "UER – after" presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.5 : Business Cycle Effects – Coefficients on Control Variables

	Enrollment	Enrollment	Degree	Degree	Literacy	Literacy	Numeracy	Numeracy	Wage	Wage	Training	Training
UER – before	−.009	−.006	−.015	−.010	−.014	−.018	−.004	−.026	.043*	.007	−.010	−.016
	(.015)	*(.014)*	*(.017)*	*(.016)*	*(.031)*	*(.028)*	*(.032)*	*(.029)*	*(.026)*	*(.024)*	*(.016)*	*(.016)*
UER – affected cohorts	.080***	.077***	.081***	.077***	.097***	.091***	.078**	.077***	.083*	.079**	.040**	.037**
	(.016)	*(.016)*	*(.018)*	*(.017)*	*(.030)*	*(.027)*	*(.033)*	*(.030)*	*(.043)*	*(.037)*	*(.016)*	*(.016)*
UER – after	−.007	−.014	.016	.008	−.044	−.047	−.044	−.038	−.013	.000	.018	.018
	(.016)	*(.014)*	*(.017)*	*(.016)*	*(.033)*	*(.031)*	*(.035)*	*(.030)*	*(.037)*	*(.034)*	*(.016)*	*(.016)*
Female		.061***		.084***		−.036***		−.235***		−.350***		−.054***
		(.005)		*(.006)*		*(.010)*		*(.011)*		*(.013)*		*(.006)*
Migrant		.036***		.037**		−.599***		−.548***		−.124***		−.081***
		(.013)		*(.016)*		*(.027)*		*(.031)*		*(.016)*		*(.010)*
Mother Educ. – upper secondary		.092***		.080***		.173***		.151***		.077***		.049***
		(.008)		*(.007)*		*(.013)*		*(.014)*		*(.013)*		*(.008)*
Mother Educ. – tertiary		.182***		.173***		.296***		.291***		.108***		.075***
		(.009)		*(.009)*		*(.017)*		*(.017)*		*(.016)*		*(.010)*
Father Educ. – upper secondary		.089***		.075***		.119***		.123***		.055***		.041***
		(.007)		*(.007)*		*(.013)*		*(.014)*		*(.012)*		*(.008)*
Father Educ. – tertiary		.230***		.225***		.270***		.272***		.149***		.100***
		(.009)		*(.009)*		*(.019)*		*(.018)*		*(.014)*		*(.010)*
Books at home – 11 to 25		.109***		.089***		.314***		.311***		.063***		.055***
		(.009)		*(.008)*		*(.020)*		*(.020)*		*(.020)*		*(.010)*
Books at home – 26 to 100		.214***		.178***		.525***		.532***		.138***		.119***
		(.009)		*(.008)*		*(.017)*		*(.018)*		*(.016)*		*(.009)*
Books at home – 101 to 200		.286***		.233***		.696***		.716***		.183***		.155***
		(.011)		*(.010)*		*(.020)*		*(.020)*		*(.018)*		*(.010)*
Books at home – 201 to 500		.345***		.283***		.857***		.847***		.197***		.187***
		(.011)		*(.011)*		*(.022)*		*(.023)*		*(.021)*		*(.012)*
Books at home – more than 500		.349***		.291***		.882***		.880***		.196***		.189***
		(.012)		*(.012)*		*(.025)*		*(.026)*		*(.024)*		*(.015)*
Country FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	51241	51241	51241	51241	51241	51241	51241	51241	30638	30638	44488	44488

Notes: Ordinary least squares estimations. Dependent variable indicated in the column header. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. “UER – before” presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; “UER – affected cohorts” presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; “UER – after” presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include country and birth-year fixed effects. Omitted category for father’s and mother’s highest level of education is “primary or lower secondary education”. Omitted category for books at home at the age of 15 is “less than 10 books”. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.6 : Business Cycle Effects – Gender-Specific Cohort Fixed Effects

	Enrollment	Degree	Literacy	Numeracy	Wage	Training
UER – before	−.006	−.009	−.019	−.026	.006	−.015
	(.014)	*(.016)*	*(.028)*	*(.029)*	*(.024)*	*(.015)*
UER – affected cohorts	.077***	.075***	.090***	.075**	.074**	.035**
	(.016)	*(.017)*	*(.028)*	*(.030)*	*(.037)*	*(.016)*
UER – after	−.014	.011	−.046	−.034	.010	.021
	(.015)	*(.016)*	*(.031)*	*(.030)*	*(.033)*	*(.016)*
Observations	51241	51241	51241	51241	30638	44488

Notes: Ordinary least squares estimations. Dependent variable indicated in the column header. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. "UER – before" presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; "UER – after" presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, country fixed effects and gender-specific birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.7 : Business Cycle Effects – High-School Graduation Years not Adjusted for Compulsory Military Service

	(1)	(2)	(3)	(4)
Panel A: College Enrollment				
UER – before	.018			–.003
	(.013)			*(.015)*
UER – affected cohorts		.064***		.066***
		(.015)		*(.016)*
UER – after			.007	–.005
			(.015)	*(.015)*
Observations	51241	51241	51241	51241
Panel B: College Degree				
UER – before	.003			–.014
	(.015)			*(.016)*
UER – affected cohorts		.062***		.065***
		(.015)		*(.017)*
UER – after			.026	.011
			(.016)	*(.016)*
Observations	51241	51241	51241	51241
Panel C: Literacy Skills				
UER – before	.017			–.015
	(.025)			*(.030)*
UER – affected cohorts		.074***		.083***
		(.025)		*(.028)*
UER – after			–.012	–.030
			(.030)	*(.033)*
Observations	51241	51241	51241	51241
Panel D: Numeracy Skills				
UER – before	.005			–.018
	(.027)			*(.029)*
UER – affected cohorts		.056*		.065**
		(.029)		*(.030)*
UER – after			–.003	–.018
			(.029)	*(.030)*
Observations	51241	51241	51241	51241
Panel E: Log Monthly Wage				
UER – before	.028			.017
	(.023)			*(.024)*
UER – affected cohorts		.055		.047
		(.036)		*(.038)*
UER – after			.015	.012
			(.033)	*(.032)*
Observations	30638	30638	30638	30638
Panel F: Training				
UER – before	–.010			–.016
	(.015)			*(.015)*
UER – affected cohorts		.027*		.030*
		(.016)		*(.016)*
UER – after			.029*	.020
			(.016)	*(.017)*
Observations	44488	44488	44488	44488

Notes: Ordinary least squares estimations. Dependent variable indicated in the Panel title. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. "UER – before" presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; "UER – after" presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes here the year when individuals graduate from high-school). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.8 : Business Cycle Effects – Control for Early Unemployment Rates

	Enrollment	Degree	Literacy	Numeracy	Wage	Training
UER (t-10)	−.002	−.006	−.005	.001	−.001	.004
	(.003)	*(.004)*	*(.005)*	*(.006)*	*(.005)*	*(.003)*
UER (t-9)	.003	.010	.009	.007	.004	.000
	(.006)	*(.007)*	*(.009)*	*(.009)*	*(.009)*	*(.005)*
UER (t-8)	.001	−.006	−.009	−.009	−.004	−.008
	(.007)	*(.008)*	*(.010)*	*(.012)*	*(.011)*	*(.006)*
UER (t-7)	−.006	−.004	−.006	−.004	−.009	.003
	(.007)	*(.006)*	*(.010)*	*(.011)*	*(.011)*	*(.006)*
UER (t-6)	.005	.004	.011	.007	.027**	.002
	(.006)	*(.006)*	*(.009)*	*(.009)*	*(.011)*	*(.006)*
UER (t-5)	−.003	−.007	−.007	−.003	−.029***	−.003
	(.005)	*(.006)*	*(.010)*	*(.010)*	*(.011)*	*(.006)*
UER (t-4)	.001	.007	.007	.002	.020	.001
	(.006)	*(.006)*	*(.010)*	*(.011)*	*(.012)*	*(.006)*
UER (t-3)	.001	−.004	−.009	−.003	−.004	.003
	(.005)	*(.006)*	*(.010)*	*(.010)*	*(.011)*	*(.005)*
UER (t-2)	−.001	−.000	.005	.000	−.005	−.005
	(.004)	*(.004)*	*(.007)*	*(.007)*	*(.006)*	*(.004)*
UER – affected cohorts	.056***	.066***	.059	.067*	.087**	.078***
	(.021)	*(.023)*	*(.037)*	*(.039)*	*(.039)*	*(.022)*
Country FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Observations	48324	48324	48324	48324	28963	41862

Notes: Ordinary least squares estimations. Dependent variable indicated in column header. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. “UER – affected cohorts” presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother’s and father’s education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.9 : Business Cycle Effects – Include Country-Specific Linear Time Trends

	Enrollment	Degree	Literacy	Numeracy	Wage	Training
UER – before	−.030	−.003	−.041	−.012	−.035	−.003
	(.022)	*(.024)*	*(.048)*	*(.045)*	*(.037)*	*(.027)*
UER – affected cohorts	.030	.061**	.035	.097*	.048	.023
	(.024)	*(.026)*	*(.048)*	*(.050)*	*(.039)*	*(.030)*
UER – after	−.009	.030	.012	.014	.050	−.002
	(.020)	*(.024)*	*(.040)*	*(.043)*	*(.052)*	*(.026)*
Observations	51241	51241	51241	51241	30638	44488

Notes: Ordinary least squares estimations. Dependent variable indicated in the column header. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. "UER – before" presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; "UER – after" presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, country and birth-year fixed effects, and country-specific linear time trends. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.10 : Business Cycle Effects – Exclude Country Groups

	Enrollment	Degree	Literacy	Numeracy	Wage	Training
Panel A: exclude countries without precise age information (Canada, New Zealand, US)						
UER – before	–.010	–.011	–.024	–.036	.008	–.016
	(.014)	*(.016)*	*(.029)*	*(.030)*	*(.023)*	*(.016)*
UER – affected cohorts	.076***	.076***	.090***	.076**	.085**	.038**
	(.016)	*(.017)*	*(.027)*	*(.030)*	*(.037)*	*(.016)*
UER – after	–.012	.009	–.048	–.037	.022	.018
	(.015)	*(.017)*	*(.031)*	*(.031)*	*(.033)*	*(.016)*
Country FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Observations	40919	40919	40919	40919	23849	35678
Panel B: exclude former communist countries (Czech R., Estonia, Poland, Slovak R., Slovenia)						
UER – before	–.023	–.036**	–.034	–.034	.001	–.013
	(.015)	*(.018)*	*(.032)*	*(.034)*	*(.028)*	*(.018)*
UER – affected cohorts	.074***	.077***	.113***	.085**	.077*	.053***
	(.017)	*(.018)*	*(.032)*	*(.035)*	*(.046)*	*(.019)*
UER – after	–.009	.021	–.054	–.053	–.012	.004
	(.016)	*(.019)*	*(.035)*	*(.035)*	*(.040)*	*(.019)*
Country FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓
Observations	44211	44211	44211	44211	26846	38544

Notes: Ordinary least squares estimations. Dependent variable indicated in the column header. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. "UER – before" presents the estimated coefficient of the simple average of unemployment rates in years t-5, t-4, t-3 and t-2; "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2; "UER – after" presents the estimated coefficient of the simple average of unemployment rates in years t+3, t+4, t+5, and t+6 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.11 : Business Cycle Effects – Exclude Countries

	Enrollment	Degree	Literacy	Numeracy	Wage	Training
Australia	.078***	.078***	.088***	.074**	.081**	.035**
	(.016)	(.017)	(.027)	(.030)	(.037)	(.016)
Austria	.079***	.082***	.090***	.077**	.072*	.038**
	(.016)	(.017)	(.028)	(.030)	(.038)	(.016)
Belgium	.077***	.077***	.086***	.065**	.077*	.031*
	(.016)	(.017)	(.028)	(.030)	(.039)	(.016)
Canada	.076***	.077***	.090***	.076**	.078**	.037**
	(.016)	(.017)	(.027)	(.030)	(.037)	(.016)
Chile	.071***	.075***	.079***	.074**	.048	.035**
	(.015)	(.017)	(.028)	(.030)	(.030)	(.016)
CzechRepublic	.077***	.076***	.093***	.065**	.074*	.039**
	(.016)	(.017)	(.028)	(.029)	(.038)	(.016)
Denmark	.080***	.081***	.092***	.078**	.078**	.037**
	(.015)	(.017)	(.028)	(.030)	(.038)	(.016)
Estonia	.080***	.078***	.104***	.071**	.077**	.041**
	(.016)	(.018)	(.027)	(.031)	(.039)	(.016)
Finland	.068***	.063***	.102***	.095***	.071*	.030*
	(.016)	(.017)	(.029)	(.031)	(.039)	(.017)
France	.080***	.080***	.086***	.076**	.082**	.037**
	(.015)	(.017)	(.028)	(.030)	(.038)	(.016)
Germany	.077***	.080***	.095***	.081***	.072*	.038**
	(.016)	(.017)	(.028)	(.030)	(.039)	(.016)
Greece	.082***	.083***	.089***	.077***	.074**	.044***
	(.016)	(.017)	(.028)	(.030)	(.037)	(.016)
Ireland	.089***	.086***	.081***	.083***	.088**	.034**
	(.016)	(.018)	(.029)	(.031)	(.038)	(.016)
Israel	.079***	.079***	.092***	.079**	.090**	.033**
	(.016)	(.017)	(.028)	(.031)	(.039)	(.016)
Italy	.079***	.079***	.092***	.078***	.082**	.037**
	(.016)	(.017)	(.028)	(.029)	(.038)	(.016)
Japan	.077***	.075***	.094***	.082***	.087**	.029*
	(.016)	(.017)	(.027)	(.030)	(.040)	(.016)
Korea	.077***	.082***	.093***	.078***	.088**	.040**
	(.016)	(.017)	(.028)	(.030)	(.039)	(.015)
Netherlands	.076***	.076***	.091***	.082***	.080**	.039**
	(.016)	(.017)	(.028)	(.030)	(.038)	(.016)
NewZealnd	.077***	.077***	.091***	.077***	.079**	.037**
	(.016)	(.017)	(.027)	(.030)	(.037)	(.016)
Norway	.080***	.081***	.096***	.085***	.086**	.036**
	(.015)	(.017)	(.028)	(.030)	(.038)	(.016)
Poland	.064***	.070***	.081***	.097***	.086**	.042**
	(.016)	(.018)	(.031)	(.033)	(.041)	(.016)
SlovakRepublic	.081***	.085***	.097***	.082***	.080**	.035**
	(.015)	(.017)	(.028)	(.030)	(.038)	(.016)
Slovenia	.077***	.077***	.092***	.077**	.080**	.037**
	(.016)	(.017)	(.028)	(.030)	(.037)	(.016)
Spain	.069***	.076***	.095***	.058*	.088**	.028
	(.018)	(.020)	(.032)	(.034)	(.042)	(.017)
Sweden	.079***	.077***	.101***	.091***	.079**	.038**
	(.016)	(.017)	(.028)	(.030)	(.038)	(.016)
Turkey	.071***	.063***	.069**	.047	.079**	.042***
	(.016)	(.017)	(.028)	(.030)	(.037)	(.016)
UnitedKingdom	.076***	.078***	.088***	.076**	.077**	.038**
	(.016)	(.017)	(.027)	(.030)	(.037)	(.016)
United States	.078***	.077***	.091***	.078***	.086**	.038**
	(.016)	(.017)	(.028)	(.030)	(.037)	(.016)
Country FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓

Notes: Ordinary least squares estimations. Dependent variable indicated in column header. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. Each row shows the coefficient of “UER – affected cohorts” for the full sample excluding the country indicated in the left column. Unemployment rate is divided by 10 throughout. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.12 : Business Cycle Effects – Exclude Age Groups

	Enrollment	Degree	Literacy	Numeracy	Wage	Training
25	.083***	.079***	.096***	.081**	.068*	.037**
	(.016)	(.017)	(.027)	(.031)	(.038)	(.017)
26	.076***	.079***	.101***	.088***	.094**	.035**
	(.017)	(.018)	(.029)	(.031)	(.038)	(.016)
27	.075***	.076***	.079***	.062*	.097***	.032*
	(.016)	(.018)	(.029)	(.031)	(.037)	(.017)
28	.069***	.070***	.072**	.063**	.080**	.028*
	(.015)	(.017)	(.028)	(.032)	(.037)	(.016)
29	.077***	.078***	.099***	.076**	.078**	.035**
	(.017)	(.017)	(.030)	(.031)	(.038)	(.016)
30	.079***	.082***	.097***	.091***	.094**	.053***
	(.016)	(.017)	(.029)	(.030)	(.038)	(.015)
31	.080***	.079***	.097***	.075**	.092**	.039**
	(.016)	(.017)	(.028)	(.031)	(.037)	(.016)
32	.075***	.077***	.093***	.080***	.084**	.034**
	(.016)	(.017)	(.028)	(.030)	(.037)	(.016)
33	.075***	.079***	.083***	.072**	.080**	.036**
	(.016)	(.017)	(.028)	(.030)	(.039)	(.016)
34	.079***	.076***	.099***	.087***	.088**	.034**
	(.017)	(.018)	(.029)	(.032)	(.041)	(.017)
35	.066***	.067***	.087***	.065**	.075*	.039**
	(.016)	(.018)	(.030)	(.031)	(.041)	(.017)
36	.070***	.072***	.089***	.082**	.067**	.038**
	(.016)	(.018)	(.028)	(.032)	(.031)	(.017)
37	.081***	.079***	.082***	.079**	.072*	.032**
	(.016)	(.018)	(.029)	(.031)	(.041)	(.016)
38	.072***	.072***	.087***	.076**	.087**	.044***
	(.017)	(.018)	(.028)	(.030)	(.040)	(.016)
39	.080***	.079***	.088***	.077**	.048	.041**
	(.015)	(.017)	(.027)	(.030)	(.030)	(.016)
Country FE	✓	✓	✓	✓	✓	✓
Cohort FE	✓	✓	✓	✓	✓	✓

Notes: Ordinary least squares estimations. Dependent variable indicated in column header. Sample: PIAAC respondents 25–39 years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. Each row shows the coefficient of "UER – affected cohorts" for the full sample excluding the age indicated in the left column. The 5-year age cohort (including the omitted age) of countries without precise age information is excluded as well. Unemployment rate is divided by 10 throughout. All specifications include controls for unemployment rates before and after the affected period, gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A3.13 : Business Cycle Effects – Expanding Age Groups

	Enrollment	Degree	Literacy	Numeracy	Wage	Training
Panel A: Aged 25–39						
UER – affected cohorts	.077***	.077***	.091***	.077***	.079**	.037**
	(.016)	*(.017)*	*(.027)*	*(.030)*	*(.037)*	*(.016)*
Observations	51241	51241	51241	51241	30638	44488
Panel B: Aged 25–59						
UER – affected cohorts	.041***	.038***	.059**	.060**	.021	.022*
	(.012)	*(.013)*	*(.026)*	*(.026)*	*(.023)*	*(.012)*
Observations	111766	111766	111766	111766	70050	102296

Notes: Ordinary least squares estimations. Dependent variable indicated in the column header. Sample: PIAAC respondents 25–39 (Panel A) and 25–59 (Panel B) years old, excluding individuals who achieved their highest educational level abroad. Specifications on labor-market outcomes additionally exclude current students. Monthly wages are not reported for PIAAC respondents in Turkey, hence the country is excluded in specifications using monthly wages. "UER – affected cohorts" presents the estimated coefficient of the simple average of unemployment rates in years t-1, t, t+1, t+2 (t denotes the hypothetical year when individuals decide on tertiary education investments, see Figure 3.2). Unemployment rate is divided by 10 throughout. All specifications include controls for the unemployment rate before and after affected cohorts, gender, migrant status, mother's and father's education, books at home at the age of 15, as well as country and birth-year fixed effects. Regressions weighted by sampling weights. Robust standard errors, clustered at the country times year of birth level, in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

4 The Role of Cognitive Skills in International Differences in University Wage Premia[1]

4.1 Introduction

It is widely documented that university graduates earn more than other workers around the world (see e.g., Montenegro and Patrinos 2014). While this reduced-form pattern may arise for a variety of reasons, standard theories in labor economics highlight the importance of higher marketable skills. University wage premia may reflect higher-skilled individuals being more likely to attend university or university education increasing productive skills.

This paper is the first to investigate the role of cognitive skills in explaining international differences in university wage premia. Our analysis is based on internationally comparable data on cognitive skills, educational attainment, and labor-market outcomes for representative samples of adults in 32 developed countries. We estimate wage returns to university education and investigate to what extent higher skills contribute to higher wages of university graduates.[2] To explore the mechanism that drives the contribution of skills to university wage premia, we conduct a series of additional analyses. In particular, we study the extent to which wage premia are driven by selectivity into university and how far this accounts for international differences in university wage premia. Thereafter, we conduct international difference-in-differences estimations, where we compare the skills of individuals at university entry age and post-graduation age with and without university education. These estimations show the effect of university education on skill formation, controlling for unobserved differences across countries (e.g., in educational systems and labor-market institutions) and age cohorts (e.g., international business cycles or skill-age development). We also complement our cross-country analysis with detailed micro-level evidence. Making use of expanded information on university attendance in Germany, we exploit arguably exogenous variation in university proximity to identify the effect of university education on skills.

Our results show that university wage premia vary substantially across countries. On average across our sample countries, workers with a university degree earn 44 percent higher hourly wages than workers with lower educational attainment. While returns are relatively low in the Nordic countries – 20 percent in Sweden and between 20 and 30 percent in Denmark, Norway, and Finland – countries such as Chile, Indonesia, and Singapore have returns above 85 percent. When conditioning on cognitive skills, university wage premia decrease by 11

[1] This chapter was coauthored by Guido Schwerdt, University of Constance, and Simon Wiederhold, Catholic University Eichstaett-Ingolstadt.

[2] For expositional purposes, we use the terms *university wage premia*, *university wage returns*, and *wage returns to university education* interchangeably.

percentage points (24 percent) on average. However, the degree to which skills contribute to university premia also varies considerably across countries. While skills explain less than 10 percent of the university wage premium in Greece and Cyprus, more than one-third of the wage differential between workers with and without university degree is explained by skills in Germany, Sweden, Israel, and Singapore. In fact, controlling for skills reduces the international country-level variance in university wage premia by 16 percent.

However, the observation that cognitive skills explain international differences in university wage premia does not necessarily imply that university education increases skills. An alternative explanation is that higher-skilled individuals are simply more likely to start university education. Put differently, skill differences of persons with and without university education may already exist before entering university. Since our data contain information on cohorts of adults before and after potential university attendance, we can investigate this potential explanation further. We indeed find substantial skill gaps between university students and others already at university entry age, indicating skill-based selection into university. However, international variation in these skill gaps is unrelated to university wage premia, suggesting that differing degrees of university selectivity cannot explain cross-country differences in university wage premia.

At the same time, we provide evidence that skills are developed through university education. Our international difference-in-differences analysis reveals that skills increase more between cohorts at university entry age and cohorts at post-graduation age for university-educated individuals than for individuals without university education. We further show that these university skill premia are positively related to university wage premia across countries.[3] These results suggest that (i) skills contribute to university wage premia beyond selective university enrollment and (ii) part of the international differences in wage premia are driven by variation in the extent to which university education increases productive skills.

Further evidence for a skill-effect of university education comes from complementary micro-level evidence for Germany. We exploit variation in the probability to enroll in university due to the distance of a high-school graduate's home town to the nearest university campus in an instrumental variable (IV) model. Our IV estimates of the skill effect of university education are large and significant, corroborating the result from the international analysis that university education increases skills.[4]

Overall, we find that cognitive skills are an important driver of international differences in labor-market returns to university education. While we can show that skills of university enrollees

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[3] For the ease of exposition, we refer to *university wage premium* and *university skill premium* throughout the paper, although the underlying empirical methodology differs. University wage premia are estimated in OLS models, while university skill premia are estimated in a difference-in-differences or instrumental variable setting.

[4] Using university proximity as an instrument for university attendance has several well-known caveats, which we discuss in detail in Section 4.5.3 and Appendix A4.1. See also Card (2001); Kling (2001); Currie and Moretti (2003); Carneiro, Heckman and Vytlacil (2011).

are already higher than those of non-enrollees before the start of university education, we also show that this skill-based selection into university is largely orthogonal to international differences in university wage premia. Instead, our results from difference-in-differences and IV analyses are consistent with the idea that cross-country differences in university wage premia partly stem from differences in university quality, that is, the ability of universities to raise labor-market relevant skills. While our international data – due to their cross-sectional nature – have limitations in rigorously testing some of the identifying assumptions of our estimation strategies, the consistency of results across models and sources of identifying variation is clearly reassuring.

The remainder of the paper is structured as follows. Section 4.2 describes the conceptual background and reviews related literature. Section 4.3 introduces the PIAAC data. Section 4.4 presents our international results on the returns to university education and the contribution of skills to university wage premia. Section 4.5 explores the mechanisms driving the contribution of skills to university wage premia. Section 4.5.1 reports results on the degree of selection into university based on cognitive skills. Section 4.5.2 explores to what extent university education raises skills, by applying a difference-in-differences framework using variation across age cohorts. In Section 4.5.3, we exploit arguably exogenous variation in university enrollment in Germany to provide additional evidence on skill-enhancing effects of tertiary education. Section 4.6 concludes.

4.2 Literature Review

It is well documented that university-educated adults earn more than lower-educated workers and that this gap has been widening in recent decades (see e.g., Katz and Autor 1999; Goldin and Katz 2007; Autor 2014). In particular, Autor (2014) documents a substantial increase in wage premium associated with university education and cognitive ability, and discusses underlying reasons for the persistent rise in university wage premia across most industrialized countries. His analysis of U.S. Census data shows that the economic payoff to university education rose steadily between 1979 and 2012, almost doubling during this period. In addition, this increase in the university wage premium explains a large part of the wage dispersion in the United States (Lemieux 2006; Goldin and Katz 2007; Autor 2014).

In descriptive analyses of university wage premia, Carnevale, Rose and Cheah (2011) show for the United States that in 1999 an adult with a bachelor's degree earned 75 percent more over the lifetime than a high-school graduate. By 2009, the premium had grown to 84 percent. Avery and Turner (2012) find that – on average – a student graduating from a U.S. university in 2009 has lifetime earnings of about 1.2 million USD net of tuition expenses, compared to 780,000 USD for a high-school graduate. This corresponds to a university wage premium of 54 percent. Looking at the extensive margin, university graduates have a much lower unemployment risk than lower educated individuals in all countries (Oreopoulos and Petronijevic, 2013).

Montenegro and Patrinos (2014) present comparable estimates on the wage returns to schooling for 139 economies. They mainly confirm previous findings (e.g., Harmon, Oosterbeek and Walker 2003; Psacharopoulos and Patrinos 2004; Colclough, Kingdon and Patrinos 2010) by showing that returns to education are generally positive with a cross-country average rate of return to an additional year of schooling of approximately 10 percent, and that returns seem to be higher in low-income and middle-income countries. Moreover, in contrast to previous studies, Montenegro and Patrinos (2014) find that university education has higher returns than primary and secondary education.[5]

Admittedly, these international findings are descriptive in nature. For instance, higher wages of university graduates observed in simple Mincer regressions may stem from productivity (i.e., skill) differences that already existed before the individual's enrollment in university. This is commonly referred to as ability bias – wage differences across individuals that exist at every level of education due to underlying differences in ability. A growing number of studies attempts to estimate causal wage effects of university education, addressing endogeneity concerns in simple Mincerian earnings regressions (see e.g., Card 1999; Heckman, Lochner and Todd 2006). Card (1993) exploits variation in university proximity in an IV analysis of the returns to university education in the United States and finds an increase in earnings for each year of tertiary education of 10–14 percent. IV estimates on the returns to university education are 25–60 percent higher than the corresponding OLS estimates. Angrist and Chen (2011) use the "GI Bill" combined with the introduction of a draft lottery for conscription to the Vietnam war in 1969 to estimate returns to tertiary education in the United States. This policy induced some cohorts of young men to obtain more education than others by providing financial and institutional support for Vietnam war veterans who attended post-secondary institutions. Randomly drafted veterans who attended university due to the GI Bill experienced earnings increases of approximately 9 percent for each additional year of education. In a related study for Canada, Lemieux and Card (2001) find 15 percent higher earnings per year for veterans who attended university. A more recent study by Zimmerman (2014) applies a regression discontinuity design to estimate returns to university education for students at the margin of going to university. Using rich data from the Florida State University System, he compares high-school graduates who were just above the threshold for being admitted to one of the

[5] While measures of educational attainment reflect the quantity of education, a related stream of literature focuses on cognitive skills as a qualitative measure of education. Hanushek et al. (2015) were the first to estimate wage returns to skills across 23 developed countries. Using data from the PIAAC Survey of Adult Skills over the full working life-cycle, the authors show that, on average, a one-standard-deviation increase in numeracy skills is associated with an 18 percent wage increase among prime-age workers. However, there is considerable heterogeneity in estimated returns to skills across countries; returns are systematically lower in countries with higher union density, stricter employment protection, and larger public-sector shares. Extending their previous analysis to 32 countries, Hanushek et al. (2017*b*) find that returns to skills are larger in faster growing economies, consistent with the hypothesis that skills are particularly important for the adaptation to economic change. Hampf, Wiederhold and Woessmann (2017) estimate the effects of higher skills on employment and find that a one-standard-deviation increase in skills is associated with an average increase in the probability of being employed by almost 8 percentage points, ranging from 2.4 percentage points in Indonesia to 14 percentage points in the Slovak Republic and Spain.

state's public universities with graduates just below the cutoff. Wage returns to one year at a four-year university for these marginal students is 8.7 percent, which is almost equivalent to the university wage premium for the average graduate in Florida.[6]

Our paper is the first to shed light on the role of labor-market-relevant skills in explaining international differences in university wage premia by exploiting high-quality skill assessment data. We contribute to the literature by providing internationally comparable evidence on differences in university wage premia across a large set of countries and on the contribution of skills in interpreting the observed differences in university wage premia.

4.3 The PIAAC Data

One of the core features of this paper is its use of new and consistent international data on cognitive skills of the adult population, which stem from the PIAAC Survey of Adult Skills, administered by the Organisation of Economic Co-Operation and Development (see OECD 2016). This survey was designed to provide representative measures of the cognitive skills possessed by adults aged 16 to 65 years in 32 participating countries.[7] In each country, a representative sample of at least 5,000 adults participated in the PIAAC assessment, which was primarily computer-based. However, respondents without sufficient computer knowledge could change survey mode and do a pencil-and-paper based survey.

In the empirical analysis, we focus on numeracy skills, which we deem most comparable across countries. However, our results do not depend on the choice of a particular measure of cognitive skills, but are robust to using literacy instead of numeracy skills, both skill domains simultaneously, or instrumenting numeracy skills by literacy skills to account for domain-specific measurement error (see Section 4.4).[8] According to OECD (2016), numeracy skills

[6] Most of the existing evidence on the wage returns to university education considers primarily four-year universities. However, other types of tertiary education programs exist with varying length and quality which may affect wages of graduates differently. Some recent studies estimate the returns to other forms of tertiary education institutions such as two-year (i.e., community) universities (Kane and Rouse 1995; Jepsen, Troske and Coomes 2014; Riegg Cellini and Chaudhary 2014; Riegg Cellini and Turner 2016). Overall, evidence suggests that wage returns to one year at a two-year university are slightly lower than those to one year at a four-year university.

[7] The first round of data collection took place in 2011/2012 in the following countries: OECD countries: Australia, Austria, Canada, Czech Republic, Denmark, England/Northern Ireland (UK), Estonia, Finland, Flanders (Belgium), France, Germany, Ireland, Italy, Japan, Korea, the Netherlands, Norway, Poland, Slovak Republic, Spain, Sweden, United States; Non-OECD countries: Cyprus and Russian Federation. A second round of PIAAC with nine additional countries took place in 2014/2015. The following countries participated in the second round: OECD countries: Chile, Greece, Israel, Lithuania, New Zealand, Slovenia, Turkey; Non-OECD countries: Indonesia (Jakarta only), Singapore. We do not include data from the Russian Federation due to concerns about the representativeness because of missing data for people living in the Moscow municipal area.

[8] In addition to numeracy and literacy skills, a third skill domain was tested in PIAAC: Problem solving in technology-rich environments, often referred to as ICT skills. The assessment of ICT skills in PIAAC was an international option. Cyprus, France, Indonesia, Italy, and Spain did not take part in the ICT skills assessment.

measure the ability to access, use, interpret, and communicate mathematical information and ideas in order to engage in and manage the mathematical demands of a range of situations in adult life. Literacy skills are defined as the ability to understand, evaluate, use, and engage with written texts to participate in society, to achieve one's goals, and to develop one's knowledge and potential.

Each skill domain is measured on a 500-point scale.[9] In the regression analysis, we standardize skills to have mean zero and standard deviation (SD) one within each country.[10] For illustration, one SD in numeracy skills corresponds to one out of five proficiency levels in PIAAC, which is roughly twice the skill difference between PIAAC respondents with lower and upper secondary education (see also Hanushek et al. 2017*b*). Our estimations employ the sample weights provided in PIAAC; in regressions pooling all countries, each country receives the same weight.

PIAAC made use of leading international expertise to develop valid comparisons of skills across countries and cultures. Hence, the PIAAC data are superior to previous surveys in various dimensions. Many international data on individuals' competences do not include objectively measured skills alongside with information about earnings and educational background in a comparable fashion. Hence, existing literature focusing on cognitive skills relies on self-reported measures of skills or proxies thereof, such as skill use (for a discussion and related literature, see Falck, Heimisch and Wiederhold 2016). Especially when survey participants are asked to report their skills themselves, severe measurement issues may arise.[11] If classical in nature, such measurement error will lead to the well-known attenuation bias in OLS estimations. However, even non-classical measurement errors may arise, for instance, if university graduates over-estimate their skills by a larger extent than non-graduates do. Moreover, self-reported skill measures would also suffer from cross-country differences in answering behavior. Using objectively measured, internationally comparable PIAAC scores substantially reduces the problem of measurement error.[12]

In addition to cognitive skills scores, PIAAC offers information on the respondents' demographic characteristics, education, and labor-market outcomes from an internationally harmonized background questionnaire. Our wage measure in the international analysis refers to

Furthermore, not all respondents in the remaining countries took the test, which raises concerns about a positively selected sample within each country regarding computer competences (and possibly other unobserved characteristics). See Falck, Heimisch and Wiederhold (2016) for further details.

9 Throughout, we use the first of overall ten plausible values of the PIAAC scores.

10 Estimation results are very similar when standardizing skills in the entire international sample.

11 Such error may stem from the fact that survey questions referring to skills are often very crude with a limited number of response categories, suffer from reporting bias, and are used under the assumption that respondents are aware of the full skill distribution in the population.

12 This is not to say that PIAAC scores do not suffer from measurement error. As described in detail in Hanushek et al. (2015), measurement error in PIAAC may stem from PIAAC respondents having a bad testing day or getting distracted during the test. However, we consider this measurement error as less likely to be non-classical than the measurement error in self-reported skills.

gross hourly earnings.[13] Our indicator for holding a university degree equals one if a respondent's highest level of formal education is ISCED 5 or 6, and is zero otherwise.

To obtain a homogeneous sample of workers with strong labor-force commitment in the estimation of university wage premia, we limit the sample to survey respondents aged 35–54 who work full-time at the time of the survey (i.e., working at least 30 hours per week).[14] Our overall sample consists of 45,037 observations. Descriptive statistics for all countries and variables used in the empirical analysis can be found in Table A4.1 in the appendix. 40 percent of individuals in our sample hold a university degree, varying from 13 percent in Italy to 62 percent in Israel. Average numeracy (literacy) skills amount to 273.2 (273.8) PIAAC points across all countries. 43 percent of respondents in our sample are women and average work experience is 21.9 years.

4.4 International Variation in Returns to Education and the Contribution of Skills to University Wage Premia

4.4.1 Empirical Strategy

Our baseline empirical model to estimate wage returns to university education is a simple Mincer-type equation (Mincer, 1974), in which our earnings measure is regressed on a binary variable indicating whether a respondent has obtained university education, and a set of control variables:

$$lnY_i = \beta_0 + \beta_1 UNI_i + \beta_2 E_i + \beta_3 E_i^2 + \beta_4 G_i + \varepsilon \tag{4.1}$$

In equation 4.1, lnY_i is the log hourly wage of individual i and UNI_i is a dummy variable indicating whether the individual holds a university degree. We further include a quadratic polynomial in actual work experience, E_i, and a gender indicator, G_i, as control variables. ε is a stochastic error. The coefficient of interest is β_1, indicating the wage premium (in percent) for university graduates. When we investigate the role of skills in explaining wage differences between university graduates and non-graduates, we augment equation 4.1 by cognitive skills, C, which leads to:

[13] The earnings data in the Public Use File are reported only in deciles for Austria, Canada, Sweden, and the United States (first round) as well as for Singapore and Turkey (second round). For these countries, we assign the median wage of each decile of the country-specific wage distribution (obtained from the OECD) to each person belonging to the respective decile. Hanushek et al. (2015) show that using decile medians has no substantive impact on estimated returns to skills for those countries with continuous wage data. To limit the influence of outliers, we trim the bottom and top one percent of the wage distribution in each country.

[14] In employing these sample restrictions, we follow Hanushek et al. (2015). When investigating selection into university and the university skill premium (see Section 4.5.1 and 4.5.2), we work with an adjusted sample, which will be described in the respective section.

$$lnY_i = \beta_{0*} + \beta_{1*}UNI_i + \beta_{2*}E_i + \beta_{3*}E_i^2 + \beta_{4*}G_i + \beta_{5*}C_i + \vartheta \tag{4.2}$$

We refer to the estimate of the university wage premium in this modified specification as β_{1*}, and calculate the following ratio:

$$r_1 = \frac{\beta_1 - \beta_{1*}}{\beta_1} \tag{4.3}$$

The numerator of equation 4.3 is the difference between the university wage premium estimates from equations 4.1 (not conditioning on skills) and 4.2 (conditioning on skills). The denominator is the estimated wage premium from equation 4.1. The ratio in equation 4.3 shows to which extent cognitive skills account for the university wage premium. Hypothetically, if the university wage premium would be entirely driven by higher productivity and our skill measure would perfectly capture these productivity differences, β_{1*} would be zero, and r_1 would equal 1. In our analysis, however, we expect to find a ratio (substantially) below 1 because (i) cognitive skill measures in PIAAC do not capture all skill dimensions relevant for labor market success and are measured with error; and (ii) universities might not only increase wages through higher skills but also through other channels, for instance, signaling and networking effects (see e.g., Lang and Kropp 1986; Altonji and Pierret 2001; Bedard 2001; Mayer and Puller 2008).

Since we do not exploit any exogenous variation, our estimated university wage premia as well as the ratio in equation 4.3 should be seen as purely descriptive. It is well known that simple OLS estimations of wages on the level of education and a set of observables is likely to result in a biased estimate of the true returns to education. First, individuals may have unobserved characteristics (e.g., innate ability or non-cognitive skills) which enable them to earn higher wages at any level of education. If these characteristics are also associated with more schooling or the probability to attend university, OLS estimates will be upward biased. Furthermore, based on principles of comparative advantage, individuals who are most likely to select into university may also benefit most from university ("positive selection hypothesis"). As opposed to this, Brand and Xie (2010) conjecture that individuals who are least likely to attend university benefit the most from it ("negative selection hypothesis"). However, to the extent that any bias of the absolute returns to university education is similar in all countries, our analysis still gives a correct picture of the cross-country pattern of wage premia.

In addition to omitted variable bias and reverse causality, another concern in our analysis is measurement error, which potentially leads to a downward bias in OLS estimations. First, our indicator of university attendance represents a rather broad definition of all tertiary education programs within a country. However, countries differ in their education systems and which occupations require university education (for instance, nursing requires university education in the United States, but vocational education in Germany). Second, as in all skill assessments, skill scores are also measured with error (see Hanushek et al. 2015). To address domain-specific measurement in our skills measure, we instrument numeracy skills by literacy skills in a robustness specification, exploiting only the variation in skills that is common to both skill domains.

4.4.2 Results

Our estimates in Table 4.1, Row 1 consistently indicate a significantly positive university wage premium. In the pooled model, university graduates earn 44 percent higher wages than their lower educated peers. Estimated returns vary substantially across countries. All Nordic countries in our sample – Sweden, Denmark, Norway and Finland – have university wage premia below 30 percent, with Sweden having the lowest premium at 20 percent. On the other end of the spectrum, labor markets in Turkey, Singapore, Chile, and Indonesia considerably reward holding a university degree, with wage premia above 70 percent. Singapore is at the top of the international league table with a university wage premium of 88 percent.[15]

Our results are similar to previous country-specific evidence on the wage returns to university education. For the United States, we find a university wage premium of 53 percent, which is almost identical to the estimated lifetime earnings advantage of university graduates in Avery and Turner (2012) and only slightly lower than the estimate in Carnevale, Rose and Cheah (2011). Our university wage premium estimate of 45 percent for Germany is somewhat lower than results shown in Piopiunik, Kugler and Woessmann (2017), who estimate university wage premia between 58 percent and 89 percent (depending on the type of university attended) based on information from the German Micro Census.[16]

As described in Section 4.3, we restrict our sample to full-time workers aged 35–54 to obtain a homogeneous sample of individuals with strong labor force attachment. We argue that the university wage premium estimated in this sample is most informative about lifetime earnings. However, our results are robust to estimating the university wage premium for the entire age range of full-time employed workers (see Table A4.2). Across countries, university graduates earn 42 percent higher wages than non-graduates, which is only slightly smaller than the corresponding estimate of 44 percent in our prime-age sample. The cross-country pattern of university wage premia remains unchanged – holding a university degree is rewarded least in Nordic countries and most in Turkey, Singapore, Chile, and Indonesia. Point estimates of

[15] The cross-country correlation between the university wage premium and the share of university graduates is -0.298 and not significant at 5 percent or better.

[16] Piopiunik, Kugler and Woessmann (2017) estimate the university wage premium relative to workers who completed an apprenticeship by means of present value calculations, assuming a discount rate of 3 percent. The estimated premium amounts to 89 percent for university graduates and 58 percent for graduates from Universities of Applied Science.

university wage premia in most countries barely change or decrease only marginally compared to the prime-age sample.[17]

Row 2 of Table 4.1 reports university wage premia when conditioning on numeracy skills (see equation 4.2). Across sample countries, university wage premia decrease by an average of 11 percentage points. Put differently, cognitive skills account for almost one-quarter of the wage gradient of university education (Row 3 of Table 4.1, see equation 4.3).[18] Both university wage premia – unconditional and conditional on numeracy skills – are shown in Figure 4.1. While we clearly observe substantial variation in university wage premia across countries (dark grey bars), the variation decreases as soon as numeracy skills are accounted for (light grey bars). In fact, controlling for skills reduces the country-level variance in university wage premia by 16 percent.

The degree to which university wage premia can be explained by skills differs considerably across countries. Figure 4.2 illustrates the skill contribution to university wage premia calculated in Row 3 of Table 4.1, by country. While less than 10 percent of the wage premium can be attributed to numeracy skills in Greece and Cyprus, these skills account for more than one-third of the wage differential between workers with and without university degree in Sweden, Israel, and Singapore. Hence, the share of the university wage premium explained by higher numeracy skills is almost identical in the country with the lowest university wage premium (Sweden) and the country with the highest premium (Singapore).

Our results are robust to changes in the skill control used to estimate the contribution of skills to the university wage premium. When conditioning on literacy skills in Table A4.3, results are very similar as for numeracy skills. Wage returns to university education decrease by an average of 9 percentage points, once we control for literacy skills. Put differently, the contribution of literacy skills amounts to 21 percent. Controlling for both skill domains simultaneously yields a skill contribution of 25 percent across all sample countries (Table A4.4).[19]

A straightforward approach to address potential attenuation bias arising from domain-specific measurement error is to use literacy skills as an instrument for numeracy skills. Results of this IV specification are shown in Table A4.5. The estimated contribution of skills to the university wage premium increases by 12 percent to 27 percent across countries, indicating that our preferred specification slightly underestimates the contribution of skills to university wage returns due to measurement error in the skill variable. Compared to Table 4.1, the contribution of skills to the wage premium increases quite substantially in countries such as Poland (23 percent), the Netherlands (24 percent), Indonesia (26 percent), and Czech Republic

[17] The Czech Republic and Denmark are the only countries in which we find slightly higher university wage premia in the full-age sample than in the prime-age sample; however, the difference is less than 4 percentage points.
[18] Note that skill coefficients are statistically significant in all countries in Row 2 of Table 4.1 (not shown).
[19] Figure A4.1 and A4.2 replicate Figure 4.1 and 4.2 for specifications using literacy skills (Panel A) and both skill domains (Panel B).

(35 percent). Attenuation bias due to domain-specific measurement error seems to be most severe in Turkey, where the contribution of skills increases by almost 56 percent, once we instrument numeracy skills by literacy skills.[20] However, the general pattern of countries with high and low skill contribution to the university wage premium does not change substantially (Figure A4.2, Panel C). Only a very small fraction of the wage premium can be attributed to numeracy skills in Greece and Cyprus, while Sweden and Israel have skill contributions of more than 40 percent.

Our results show that the skill contribution to university wage premia is quite substantial. This is especially interesting when we consider the fact that tested skills in PIAAC are designed to capture rather general cognitive skills. The main aim of tertiary education programs, however, is to teach students more specific skills, which are often industry- or occupation-related, preparing students to work in specific types of jobs. While cognitive skills measured in PIAAC clearly do not reflect the entire spectrum of labor market relevant skills, the strong association of PIAAC skills with wages discussed above indicates that the labor market rewards possessing these skills. Although general in nature, PIAAC skills also have field-specific components; for instance, programmers and other IT professionals have the highest ICT skills in PIAAC (Falck, Heimisch and Wiederhold, 2016). Moreover, we provide direct evidence that university education fosters skills measured by PIAAC test scores in Sections 4.5.2 and 4.5.3.

Our international evidence on university wage premia suggests that a considerable part of differences in university wage premia across countries is driven by international differences in the skill endowment between workers with and without university education. When Hanushek et al. (2015) include a control for years of schooling in their international analysis of returns to skills, both coefficients – of skills and schooling – are significant and the estimated returns to one year of schooling shrinks by about 21 percent compared to the estimation without skill control. This reduction in coefficient magnitude is very similar to our analysis, where we observe a 24 percent drop in university wage premium once we condition on skills. These results indicate that a large part of the schooling-earnings relation is related to the fact that individuals with more schooling also have higher skills. In the following section, we investigate whether the observed reduction in university wage premia when conditioning on skills reflect higher-skilled individuals being more likely to attend university (i.e., skill-based selection into university) or university education increasing productive skills rewarded on the labor market.

4.5 Explorations into the Role of Skills for University Wage Premia

To investigate potential mechanisms that drive the contribution of skills to university wage premia, we conduct a set of explorative analyses. Our aim in this section is to explore whether

[20] The estimated contribution of skills to the university wage premium decreases in only very few countries, compared to Table 4.1: Chile, Estonia, Greece, Japan, and Singapore.

the observed link between university attendance and adult skills is more likely to be driven by selective enrollment in tertiary education or by a causal effect of university education on skills. For this purpose, we start by investigating the observed variation in selective university attendance directly (Section 4.5.1). We then apply two distinct approaches to estimate effects of university attendance on skill development. In particular, we conduct a set of international difference-in-differences estimations in Section 4.5.2, while we exploit arguably exogenous variation in university enrollment due to university campus proximity in Germany in an IV framework in Section 4.5.3.

4.5.1 Selection into University Education

One potential reason for the positive association between university education and adult skills may be a positive selection into university, that is, a positive skill gap between students and non-students *before* entering university. To identify the degree of selectivity based on cognitive skills, we compare skills of students and non-students at university entry age. Unfortunately, PIAAC does not allow to identify the exact date when respondents enrolled in university. Hence, we approximate university freshmen based on their age. We then regress cognitive skills on a dummy which equals one if the PIAAC respondent reports to be currently enrolled in university, and zero otherwise, while additionally controlling for gender differences in cognitive skills.[21] Thus, the coefficient of the university attendance dummy is likely to capture the pre-existing gap in cognitive skills between university students and non-students.

Table 4.2 reports the estimated skill gap between students and non-students for various definitions of university entry age for the pooled sample of countries. Austria, Canada, New Zealand, Singapore and the United States are excluded from the analysis due to missing information for the respondent's exact age in the PIAAC Public Use File. When entry age is proxied by the age of 20 in Column 1, university entrants have 0.68 SD higher numeracy skills than individuals who do not attend university. The skill gap for literacy skills is 0.61 SD. However, restricting the sample to 20 year-old individuals has some limitations. A large fraction of young people does not enroll in university precisely at the age of 20 but at older ages. Across countries, the age of high-school graduation varies roughly between 18 and 19 years (OECD, 1999). Furthermore, grade retention increases the age of high-school graduation and consequently the potential university entry age. Thereby, the likelihood of grade retention varies quite substantially across countries in our sample (Ikeda and García, 2014). Finally,

[21] Information about the ISCED-level of currently enrolled students comes from variable *b_q02b_c* in PIAAC. Since Canada and the United States do not report this variable in the Public Use File and we observe a lot of missing values for this variable in other countries as well, we assume an individual also being currently enrolled in university (i) when it reports being currently enrolled in formal education without reporting the level of education or (ii) when it reports already holding some kind of university degree. We assume an individual as being a non-student when stating that she is currently not enrolled in formal education and does not hold a university degree. Despite losing a substantial amount of observations, the estimated relationship between pre-university skills and university enrollment is qualitatively unchanged if we use only the sample with exact information based on *b_q02b_c*.

due to compulsory military service in some countries, high-school graduates – mostly men – may need to postpone their enrollment decision even further.[22] Thus, we expand our sample step-wise to less restrictive age groups considering individuals aged 20–21, 20–22, 20–23 and 20–24 years (Column 2–5). In the last column, we approximate university freshmen by the age group 20-24 while implicitly assuming that the skills we observe for this group were not (or only partially) developed by university education, but rather represent pre-university skill endowments. The estimated numeracy skill gap within this age group between students and non-students is 0.67 SD for numeracy as well as literacy skills. This finding supports the validity of approximating university entry age by the age group of 20–24 year-old individuals when estimating the degree of selectivity into university. By doing so, we can augment our analysis by including also those five countries without precise age information – Austria, Canada, New Zealand, Singapore and the United States – because PIAAC provides information on whether the individual belongs to the 5-year-age cohort between 20–24. Furthermore, the increased sample size enables us to estimate the skill gap for each country separately.

Table 4.3 reports the corresponding results. On average across all countries, university entrants have 0.68 SD higher numeracy skills than young individuals who do not enroll in university. While we observe such skill selectivity in university attendance in all countries, the degree of selection varies substantially. The gap is relatively small for students in Israel, Turkey and Korea, but exceeds 0.90 SD in Belgium and Italy. The variation in university skill gaps does not change significantly when substituting numeracy by literacy skills.[23]

Interestingly, we find that the international variation in cognitive skill gaps at university entry ages between students and non-students as a measure of selection is not related to university wage premia (Figure 4.3).[24] This suggests that the international variation in wage premia is unlikely to be explained by differences in selective enrollment in university education across countries.

4.5.2 International Difference-in-Differences Estimations on University Skill Premia

To study potential effects of university attendance on skill development, we employ an international difference-in-differences estimation that compares the skill accumulation between cohorts at university entry age and post-graduation cohorts for university-educated individu-

[22] See also Hampf, Piopiunik and Wiederhold (2019).

[23] We estimate the degree of selection into university for individuals at university entry age at the time when the PIAAC survey was conducted. However, the university wage premium is estimated for individuals aged 35–54 years at the time of the survey. Due to the cross-sectional nature of our data, we assume that there were no systematic changes in selectivity across countries over time and hence the skill gap between students and non-students at university entry age estimated "today" is a good approximation of the skill gab between the two groups several years ago.

[24] The fitted line displays the correlation between the two estimated coefficients across countries. The university skill gap and age premium are not significantly correlated.

als as opposed to individuals without university education. More precisely, we estimate the following equation:

$$C_i = \alpha_0 + \alpha_1 OLD_i + \alpha_2 UNI_i + \alpha_3 OLD_i * UNI_i + \alpha_4 G_i + \delta_c + \epsilon \tag{4.4}$$

Our measure of cognitive skills, C_i, is regressed on a dummy which takes the value one if the individual is at university exit age (25–29 years), and zero otherwise (20 years)[25], OLD_i, a university attendance dummy, UNI_i, as well as their interaction, a gender indicator, G_i, and a full set of country dummies, δ_c.[26] The coefficient of the interaction term, α_3, is the coefficient of interest, representing the university skill premium (skill effect of university education) in the difference-in-differences framework.

Table 4.4 shows that skills increase more between cohorts at university entry age and cohorts at post-graduation age for university-educated individuals as opposed to individuals without university education.[27] When restricting the university entry cohort to 20 year-old individuals, the university skill premium corresponds to 0.10 SD for numeracy skills (Panel A) and 0.16 SD for literacy skills (Panel B). In the remaining columns of Table 4.4, we expand the age groups considered as university entry cohorts. The difference-in-differences estimate of the university skill premium tends to decrease slightly when also including older ages in the university entry cohort. This may reflect that broader definitions of the entry cohort are likely to include individuals with some university education. When comparing 20–24 year-old with 25–29 year-old individuals in Column 5, university graduates have 0.08 SD and 0.10 SD higher numeracy and literacy skills, respectively.[28]

However, skill premia of university education vary substantially across countries (Table A4.7).[29] While we do not observe any negative significant skill premium, we find significantly positive skill premia in France, Indonesia, Ireland, Israel, Singapore, Turkey and the United States.[30] In particular, U.S. universities significantly increase cognitive skills of students (0.29 SD), which is consistent with the common perception that university quality is very high in the United

[25] As discussed in Section 4.5.1, PIAAC does not provide information on the age of university entry. Hence, we define in our main specification cohorts at university entry age as those individuals aged 20 years, and post-graduation cohorts aged 25–29 years. However, we show that your results are robust to broader definitions of university entry and exit ages.

[26] The university attendance dummy equals 1 if the PIAAC respondent is either currently enrolled in formal education on the tertiary level (as defined in Section 4.5.1) or already holds a university degree, zero otherwise.

[27] We exclude countries without precise age information in Table 4.4, Column 1–4.

[28] Table A4.6 reports difference-in-differences estimation results when varying the age range for the post-graduation cohorts.

[29] To increase sample sizes in the single country regressions, we use the broadest definition of university entry age cohorts (20–24 years).

[30] The university skill premium in some countries may not be precisely estimated due to small sample sizes. However, it may also be the case that the pattern of positive skill premia of university education does not exist across all countries.

States.[31] In Israel and Singapore, the positive skill premium amounts to more than one third of a SD. On the contrary, German university graduates do not seem to benefit significantly from their educational investment in terms of higher numeracy and literacy skills. In general, countries with higher university skill premium in numeracy also have a higher skill premium in literacy.

Figure 4.4 plots the estimated university skill premia reported in Table A4.7 against university wage premia for each country in our sample. We find that estimated university skill premia are positively related to university wage premia across countries. The cross-country correlation between wage and skill premia is 0.45 and highly significant. This pattern is consistent with the idea that the effectiveness of university education in terms of increasing cognitive skills is one determinant of the size of the university wage premium.

The straightforward causal interpretation of the results presented in this subsection hinges on the assumption that counterfactual changes in skills over cohorts from age 20 to 30 are on average the same for university attendees and non-attendees. Arguably, this is a strong assumption as it rules out any changes in selective university attendance over cohorts. To further probe the causal interpretation of the link between university attendance and skill formation, we present another exploration into causality that rests on different identifying assumptions in the next subsection.

4.5.3 Instrumental Variable Estimations of the University Skill Premium Exploiting Campus Proximity in Germany

An alternative approach to identify skill effects of university education would be to exploit any exogenous variation in university enrollment. While we cannot implement such an approach for all countries in PIAAC because of data limitations, we can exploit variation in university proximity to identify the effect of interest in the German PIAAC data. More specifically, we apply an instrumental variable approach exploiting variation in the proximity of university campuses in Germany as an arguably exogenous determinant of enrollment.[32]

Distance to educational institutions – as a potential (financial) constraint to enrollment – is a popular and often used instrument in the economic literature. Several studies exploit distance measures orthogonal to unobserved individual characteristics to investigate labor market returns to education (e.g., Card 1993; Maluccio 1998; Siegler 2012; Kamhöfer, Schmitz and Westphal 2018). For our purpose, we use information for Germany from PIAAC-L, which comprise the original PIAAC survey plus three resurvey-waves in 2014, 2015, and 2016. In

[31] See e.g., Bruni (29.06.2014). However, we do not claim that students entering university are equally endowed with cognitive skills across countries. Hence, students in some countries may have much lower skill levels at university entry and hence a bigger scope for improvement.

[32] This section is a shortened exposition of the within-Germany analysis of the university skill premium, estimated in an IV framework. See Appendix A4.1 for more details regarding the related literature, institutional background, data, empirical strategy, and results.

addition to PIAAC 2012 participants (anchor persons), also household members of anchor persons were interviewed in PIAAC-L, which increases the number of observations available for our analysis. Furthermore, numeracy and literacy skills were re-tested in 2015. PIAAC-L provides information on the location of the highest secondary school attended by the individual and the respective graduation year, which enables us to calculate the relevant distance to the nearest university.[33] We calculate university proximity as linear distance (in kilometers) between the home municipality and the nearest university campus, while showing that our results are robust to using alternative distance measures such as travel distance or travel time (in minutes) by car (see Appendix A4.1). The final regression sample contains 3,976 observations, differing slightly from the sample used in the international analysis described in Section 4.3. Individuals who are still in secondary education and those with foreign high school diploma were excluded from our estimations. Furthermore, we consider individuals between 16 and 79 years old.[34]

The university skill premium is estimated by regressing skills on an indicator for university attendance, which is instrumented by the proximity measure. The distance to the nearest university refers to the time of high-school graduation, when individuals decide upon their further career paths. All regressions include controls for a set of individual socio-demographic characteristics (gender, migrant status), as well as birth-year and region fixed effects.[35] For identification, we use the fact that within each year, some students within the same region face higher costs of going to university than others because they have to commute further to reach the nearest university. Thereby, we assume that campus proximity affects the individual's education choice but has no other association with cognitive skills, once we control for a set of individual and regional characteristics. Additionally, no third (unobserved) factors are supposed to affect the location choice of individuals as well as the formation of cognitive skills.

Table 4.5 reports the results of our IV estimations. First-stage coefficients for varying model specifications in Panel A confirm a strong association between campus proximity and the decision to attend university. When controlling for differences across federal states – such as university enrollment shares and other institutional characteristics – results suggest that high-school graduates who live 10 kilometers further away from the campus are 1.6 percentage points less likely to enroll in university (5 percent). If we include region dummies in Column 2, the coefficient decreases slightly to 1.5 percentage points. The inclusion of region dummies as further controls is important if unobserved determinants of skills are fundamentally different

[33] Respondents in PIAAC-L report the municipality of residence when attending secondary school. Thus, under the reasonable assumption that the school location is close to the individual's home town, our data reveals the exact place of residence (municipality) of the individual at the time when deciding upon further schooling investments.

[34] The age range differs from the original age range between 16 and 65 years in PIAAC in 2012 because household members older than 65 were surveyed as well. Table A4.8 reports summary statistics for all relevant variables.

[35] The region dummies included in our preferred specification represent 40 administrative regions (so called *Regierungsbezirke*). See Appendix A4.1 for a detailed description of the regression equations.

in more or less densely populated regions with varying levels of average college proximity, within a state. Adding further controls for family background barely changes the association between university proximity and attendance (Column 3).[36] The reported F-statistic on the excluded instrument exceeds the conventional threshold for strong instruments throughout all specifications.

Two-stage least square estimates of the effect of university attendance on numeracy and literacy skills are shown in Panel B. When state-specific characteristics are controlled for in Column 1, the effect of university education on numeracy skills is 1.14 SD (58 PIAAC points). When we add region-specific fixed effects to account for geographical factors on a more disaggregated level, the estimated skill premium of university education is 1.13 SD. This effect barely changes when adding a full set of family background characteristics in Column 3.[37] We provide evidence that our results are robust to varying sample specifications, alternative measures of university proximity, and the inclusion of additional controls (see Appendix A4.1).

Overall, our IV estimates suggest a large university skill premium, exceeding the corresponding least square estimates. Thus, consistent with a growing number of studies of schooling choices, our findings suggest that the skill gap between more- and less-educated people may underestimate the true returns to schooling at least for the group of compliers in our sample.[38]

Note also, that taking our IV estimates at face value and multiplying them with the estimated wage returns to skills in Germany in Hanushek et al. (2015) of 24 percent per SD of skills, leads to an approximation of the university wage premium of 30 percent. This corresponds to approximately 2/3 of the estimated university wage premium in Germany, estimated in our international analysis in Table 4.1. Relative to the estimated contribution of skills to the university wage premium of 33 percent, this suggests that – if anything – the skill contribution to the overall wage premium is underestimated in simple OLS regressions.

Admittedly, the approach of exploiting geographical variation in the supply of educational institutions for identification is not free of concerns. Families with high educational aspirations may select themselves into areas with high-quality schools and a university nearby. At the same time, children of these families may have higher innate ability or motivation to invest in the formation of skills. Hence, higher cognitive skills of people living in municipalities with a nearby university could not be entirely attributed to university education. Likewise, universities may open more likely in regions with prospering labor markets or university locations attract more firms offering jobs that promote the development of skills.

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[36] These controls include dummies for whether the father and the mother were working when the individual was 15 years old and the person's birth order position. Results do not change when we include other background controls like books at home at the age of 15, number of brothers/sisters/siblings or the education level of each parent separately.

[37] Results for literacy skills are shown in Table A4.9.

[38] E.g. Angrist and Krueger 1991; Card 1993; Butcher and Case 1994; Kane and Rouse 1995 all find significantly higher estimates in IV settings than in OLS regressions on the returns to schooling.

While our analysis is limited in its possibilities to address all potential threats to the identification of the true university skill premium, we can address, however, several concerns. First, we account for unobserved heterogeneity in family characteristics related to university proximity by adding information about high-school exit exam grades for Math, German and first foreign language in Column 4 of Table 4.5. This does not change our first-stage results on the relationship between university proximity and the decision to attend university. Furthermore, two-stage least square estimates of the effect of university education on skills barely change. University graduates, who were induced to attend university because they live close to a campus, have 1.2 SD higher numeracy skills.

Addressing ability differences of high-school graduates prior to their university enrollment decision may raise some concerns. Living close to a university campus may not only affect individuals just at the point in time when they finish secondary education but may also influence effort exerted in school or even the choice of secondary school track. In this sense, controlling for exit exam grades would be a bad control because they might already be affected by campus proximity.[39] Still, controlling for high-school exit grades addresses potential threats to identification due to selective location of families close to universities.

In an ideal setting, our IV model would identify from within-municipality variation in university proximity to capture regional characteristics associated with university proximity as well as cognitive skill development. Due to data limitations (lack of sufficient over-time variation in university proximity within municipalities), our analysis is bounded to the inclusion of 40 dummies for geographical regions on a more aggregated level than municipalities. When we include an additional control for the distance to the nearest big city, our results do not change. However, the size and precision of estimated IV coefficients decrease substantially when we condition on the degree of urbanization (Table A4.10). Individuals, who were induced to attend university because they live close to a university, have 0.74 – 0.84 SD higher numeracy skills than lower-educated individuals (not significant).

Overall, however, our results are reasonably robust. Nevertheless, we shy away from claiming that we can provide clean causal evidence on positive skill effects of university attendance with this IV estimation alone. We rather interpret our results as another exploration into the causal link between skills and university education that adds credibility to the international difference-in-differences analysis presented above. Arguably, both approaches – the international difference-in-differences analysis and the German-based IV analysis – rely on rather strong assumptions, but these assumptions are also quite distinct from each other. Yet, both approaches of estimating the skill effect of university education independently provide evidence that university attendance fosters skill development. Thus, we feel comfortable to conclude, that the overall pattern of results produced by all explorative analyses into causality

[39] This concern is strengthened in our data when we regress the probability of holding an Abitur on the distance to the nearest tertiary institution. Individuals who live 10 kilometers closer to the nearest campus are 2.6 percentage points more likely to hold an Abitur. Thus, we do not include high-school exit exam grades in our preferred specification.

presented in Section 4.5 suggests that it is quite likely that differences in the degree to which university education leads to skill improvements account for some part of the differences in university wage premia across countries.

4.6 Conclusion

Income gaps between workers with and without university degree increased in many countries in recent years (Goldin and Katz, 2007). Broadly speaking, this increase in university wage premia may reflect a more pronounced selection of higher-skilled individuals into university education or a rise in the relative effectiveness of university education in increasing earnings. However, little is known about the role of cognitive skills in shaping university wage premia around the world.

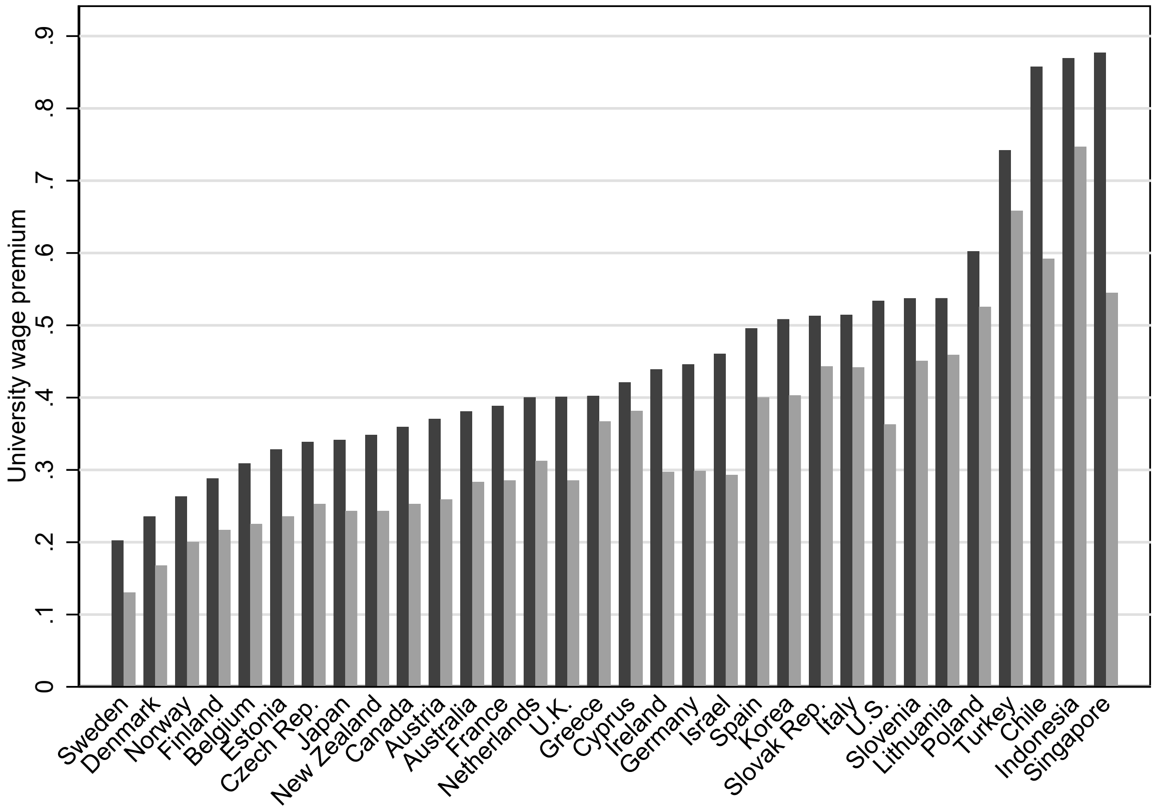
Using unique labor force data including objectively measured cognitive skills for a representative sample of the working age population across 32 countries, we provide first internationally comparable evidence on the contribution of skills to university wage premia. Our results suggest that university wage premia vary substantially across countries. On average across our sample, workers with a university degree earn 44 percent higher hourly wages than workers with lower educational attainment. While returns are relatively low in the Nordic countries, countries such as Chile, Indonesia, and Singapore have returns above 85 percent. Conditioning on skills reduces the estimated university wage premium considerably. On average, the reduction in the wage premia is 11 percentage points (24 percent), but there is a wide variation in the degree to which university premia can be attributed to differences in skills. Overall, these results indicate that cognitive skills are an important driver of international differences in wage returns to university education.

We explore the two most prominent mechanisms driving the contribution of cognitive skills to university wage premia: skill-based selection into university and university education developing skills. Concerning the first mechanism, we find substantial skill gaps between university students and others already at university entry age, indicating that higher-skilled individuals are more likely to start university education. However, international variation in these skill gaps as a measure of selection into university is not related to the difference in observed university wage premia. Instead, applying an international difference-in-differences framework as well as IV estimations using arguably exogenous variation in university attendance in Germany, we show that university attendance fosters skill development. The degree to which university education leads to skill improvements is positively related to university wage premia across countries, suggesting that skills contribute to university wage premia beyond selective university enrollment.

In sum, our results shed new light on the importance of cognitive skills in shaping differences in labor markets' returns to higher education across countries. At the same time, new research questions emerge from our findings, such as whether rising university wage premia within countries over time are partly driven by university education creating higher levels of skills, and whether wage inequality can be reduced by policy-makers by providing programs aiming at increasing skills after the end of formal education. Answering these questions holds great promise for deepening our understanding of the interplay between skills, education, and wage inequality.

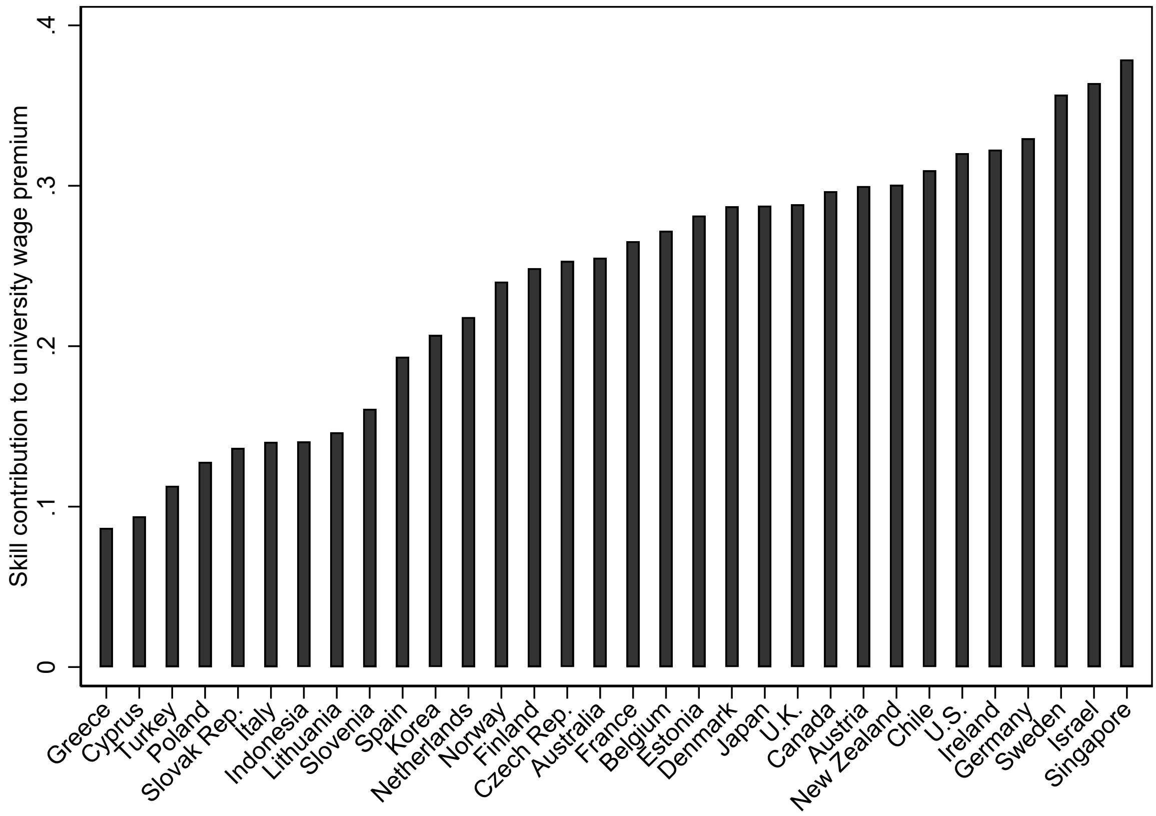
Figures and Tables

Figure 4.1 : University Wage Premia Around the World



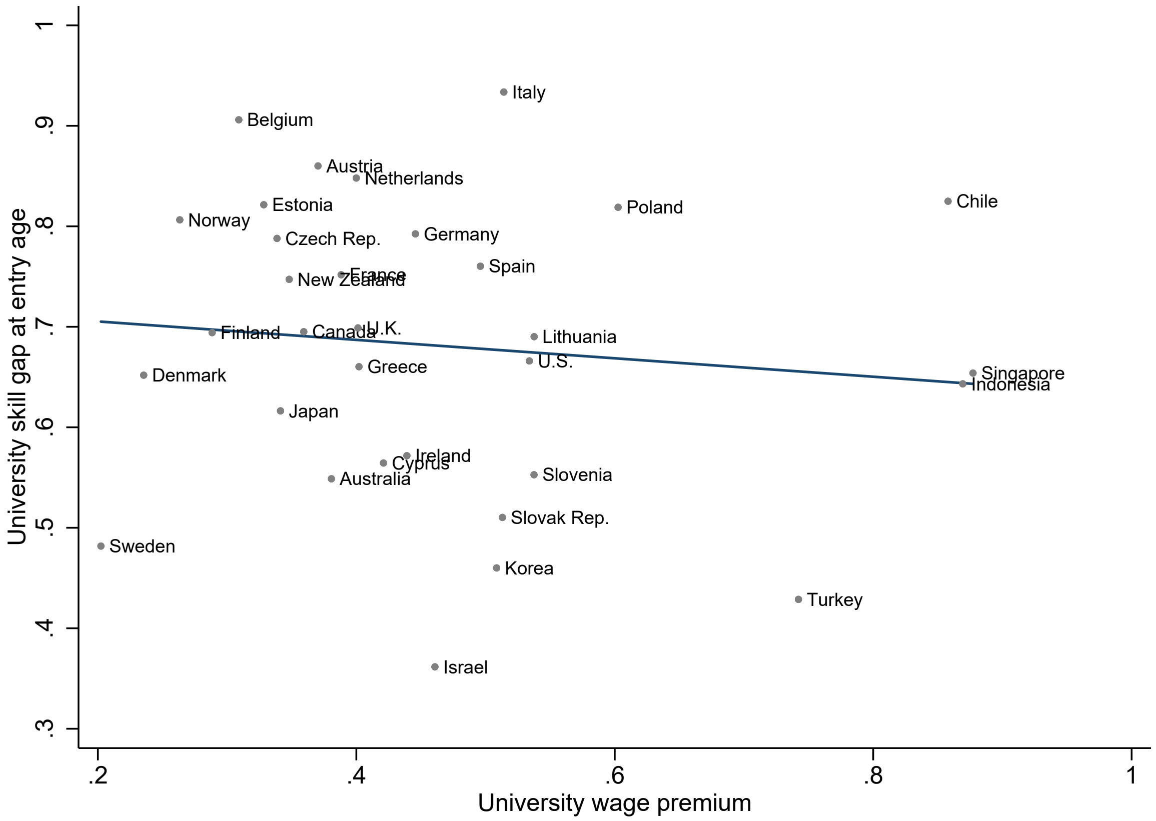
Notes: Wage returns to university education across PIAAC countries. Dark grey bars show wage premia as estimated in Table 4.1, row (1), light grey bars show wage premia after conditioning on numeracy skills, as estimated in row (2). *Data source:* PIAAC 2012/2015.

Figure 4.2 : The Contribution of Skills to University Wage Premia



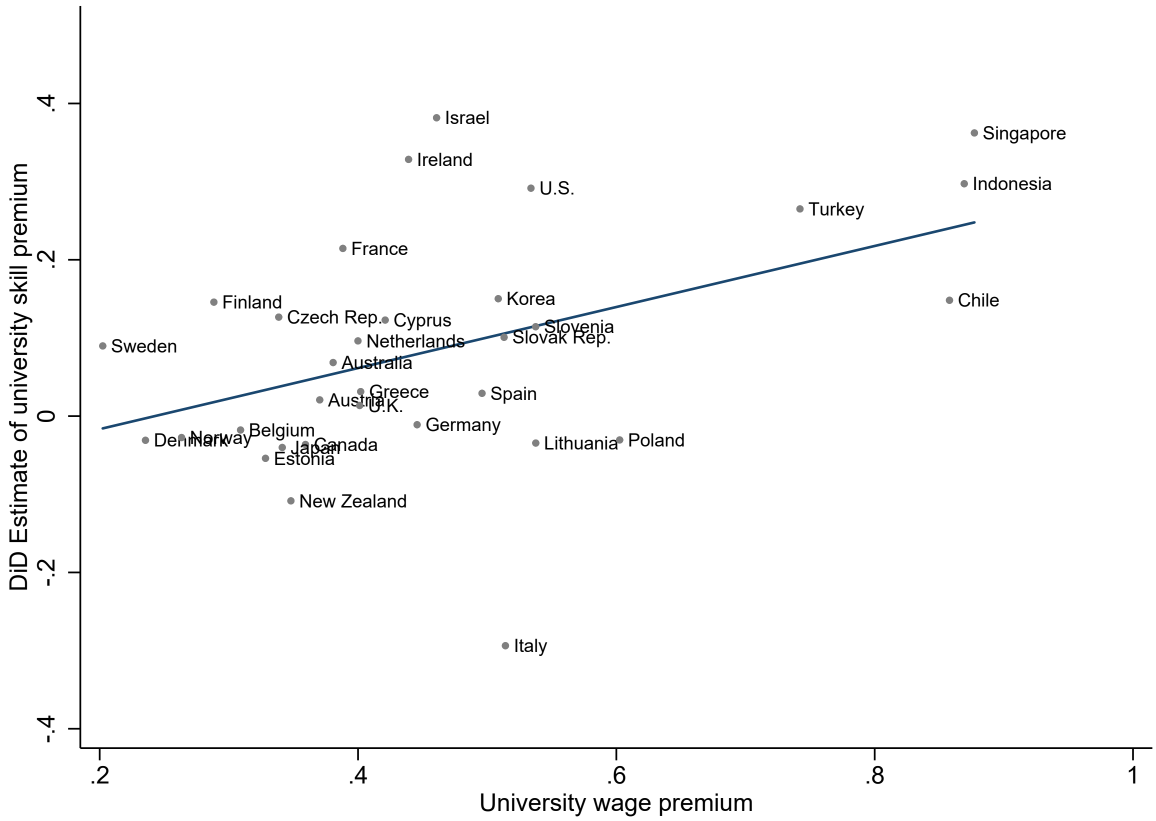
Notes: Contribution of numeracy skills to wage return to university education across PIAAC countries, as calculated in Table 4.1, row (3).
Data source: PIAAC 2012/2015.

Figure 4.3 : University Skill Gap at Entry Age & University Wage Premium



Notes: Scatter plot of the numeracy skill gap at university entry against wage returns to university education, by country. University skill gap at entry age is the estimated coefficient of a dummy indicating university attendance in a regression of numeracy skills on this dummy, including a gender control, for the sample of individuals aged 20-24 years in each country (replicates Table 4.3). Wage returns to university education report coefficients as estimated in Table 4.1. *Data source:* PIAAC 2012/2015.

Figure 4.4 : DiD Estimate of University Skill Premium & University Wage Premium



Notes: Scatter plot of the numeracy skill returns to university education against university wage premia, by country. Skill returns to university education is the estimated coefficient of the interaction term between a dummy indicating the age group (20–24 vs. 25–29) and a dummy for university attendance (either actual enrollment or graduation) in a regression of standardized numeracy skills on the age and university attendance dummies separately and their interaction, including gender controls, for the sample of individuals aged 20-29 years in each country. *Data source:* PIAAC 2012/2015.

Table 4.1 : University Wage Premia & the Contribution of Numeracy Skills

	Pooled	Australia	Austria	Belgium	Canada	Chile	Cyprus	Czech R.	Denmark	Estonia	Finland
(1) University wage premium without skill control	.440***	.381***	.370***	.309***	.359***	.858***	.421***	.339***	.235***	.328***	.288***
	(.005)	*(.026)*	*(.025)*	*(.018)*	*(.016)*	*(.058)*	*(.033)*	*(.052)*	*(.014)*	*(.024)*	*(.016)*
(2) University wage premium with skill control	.334***	.284***	.259***	.225***	.253***	.592***	.381***	.253***	.168***	.236***	.217***
	(.005)	*(.026)*	*(.025)*	*(.019)*	*(.016)*	*(.056)*	*(.034)*	*(.059)*	*(.015)*	*(.025)*	*(.018)*
(3) Contribution skills to wage premium	.241	.255	.300	.272	.297	.310	.094	.253	.287	.281	.249
Observations	45037	1433	1115	1220	7189	903	938	1065	1879	1763	1480
	France	**Germany**	**Greece**	**Indonesia**	**Ireland**	**Israel**	**Italy**	**Japan**	**Korea**	**Lithuania**	**Netherl.**
(1) University wage premium without skill control	.388***	.446***	.402***	.869***	.439***	.461***	.514***	.341***	.509***	.538***	.400***
	(.017)	*(.025)*	*(.041)*	*(.079)*	*(.036)*	*(.033)*	*(.039)*	*(.025)*	*(.027)*	*(.036)*	*(.022)*
(2) University wage premium with skill control	.285***	.299***	.367***	.747***	.297***	.293***	.442***	.243***	.403***	.459***	.313***
	(.019)	*(.025)*	*(.042)*	*(.081)*	*(.038)*	*(.036)*	*(.039)*	*(.027)*	*(.029)*	*(.039)*	*(.022)*
(3) Contribution skills to wage premium	.265	.330	.087	.141	.323	.364	.140	.288	.207	.146	.218
Observations	1714	1296	623	803	1033	908	1019	1319	1441	1258	1012
	New Zealand	**Norway**	**Poland**	**Singapore**	**Slovak R.**	**Slovenia**	**Spain**	**Sweden**	**Turkey**	**U.K.**	**U.S.**
(1) University wage premium without skill control	.348***	.263***	.603***	.877***	.513***	.538***	.496***	.202***	.742***	.401***	.534***
	(.024)	*(.014)*	*(.032)*	*(.029)*	*(.036)*	*(.022)*	*(.025)*	*(.015)*	*(.055)*	*(.029)*	*(.032)*
(2) University wage premium with skill control	.243***	.200***	.526***	.545***	.443***	.451***	.400***	.130***	.658***	.286***	.363***
	(.025)	*(.015)*	*(.033)*	*(.033)*	*(.036)*	*(.023)*	*(.027)*	*(.016)*	*(.056)*	*(.030)*	*(.035)*
(3) Contribution skills to wage premium	.301	.240	.128	.379	.137	.161	.193	.357	.113	.288	.320
Observations	1200	1519	817	1507	1193	1306	1191	1316	674	1782	1121

Notes: Each cell reports the coefficient from a separate regression. Regressions weighted by sampling weights. Sample: full-time employees aged 35–54 years. Dependent variable in specifications (1) and (2) is gross hourly wages (log). Specification (2) controls for numeracy skills. Scalars in (3) are calculated as ((1)-(2))/(1). All regressions control for a quadratic polynomial in actual work experience and gender. Observations refer to baseline specification. Numeracy skills standardized to mean 0 and SD 1 within country. Pooled specification includes country fixed effects and gives same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table 4.2 : University Skill Gap – Skill Difference Between University Students and Non-Students, for Various Age Groups

Dependent variable:	20	20–21	20–22	20-23	20–24
Numeracy skills	.676***	.643***	.680***	.671***	.674***
	(.048)	*(.034)*	*(.025)*	*(.022)*	*(.019)*
Literacy skills	.613***	.603***	.656***	.661***	.670***
	(.049)	*(.034)*	*(.026)*	*(.022)*	*(.020)*
Observations	2290	4827	8072	10968	13830

Notes: Each cell reports the coefficient from a separate regression. Regressions weighted by sampling weights. Sample: pooled sample of countries, including individuals in age groups as indicated in column header. Austria, Canada, New Zealand, Singapore and United States excluded due to missing information about exact age. Regression of cognitive skills on a dummy that equals 1 if the individual is enrolled in university, 0 otherwise, including a gender control. Numeracy and literacy skills standardized to mean 0 and SD 1 within country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table 4.3 : University Skill Gap – Skill Difference Between Students and Non-Students at University Entry Age (20-24 years)

Dependent variable:	Pooled	Australia	Austria	Belgium	Canada	Chile	Cyprus	Czech R.	Denmark	Estonia	Finland
Numeracy skills	.679***	.549***	.860***	.906***	.695***	.825***	.564**	.788***	.652***	.821***	.694***
	(.018)	*(.105)*	*(.087)*	*(.087)*	*(.070)*	*(.094)*	*(.220)*	*(.134)*	*(.075)*	*(.071)*	*(.084)*
Literacy skills	.676***	.568***	.864***	.824***	.731***	.792***	.423**	.859***	.672***	.669***	.551***
	(.018)	*(.098)*	*(.090)*	*(.090)*	*(.062)*	*(.101)*	*(.205)*	*(.177)*	*(.075)*	*(.069)*	*(.078)*
Observations	17433	484	398	327	2237	542	243	543	517	690	441
Dependent variable:	France	Germany	Greece	Indonesia	Ireland	Israel	Italy	Japan	Korea	Lithuania	Netherl.
Numeracy skills	.752***	.792***	.660***	.643***	.572***	.362***	.934***	.616***	.460***	.690***	.848***
	(.073)	*(.081)*	*(.156)*	*(.141)*	*(.096)*	*(.074)*	*(.169)*	*(.102)*	*(.109)*	*(.118)*	*(.087)*
Literacy skills	.774***	.921***	.621***	.671***	.594***	.340***	.996***	.524***	.545***	.686***	.763***
	(.073)	*(.083)*	*(.161)*	*(.122)*	*(.098)*	*(.074)*	*(.170)*	*(.099)*	*(.110)*	*(.126)*	*(.086)*
Observations	422	555	244	445	351	597	181	355	416	334	424
Dependent variable:	New Zealand	Norway	Poland	Singapore	Slovak R.	Slovenia	Spain	Sweden	Turkey	U.K.	U.S.
Numeracy skills	.747***	.806***	.819***	.654***	.510***	.553***	.760***	.482***	.429***	.699***	.666***
	(.082)	*(.092)*	*(.038)*	*(.085)*	*(.103)*	*(.151)*	*(.087)*	*(.075)*	*(.110)*	*(.101)*	*(.092)*
Literacy skills	.819***	.830***	.768***	.622***	.501***	.690***	.795***	.559***	.343***	.730***	.664***
	(.083)	*(.089)*	*(.038)*	*(.084)*	*(.106)*	*(.153)*	*(.085)*	*(.071)*	*(.111)*	*(.109)*	*(.091)*
Observations	618	445	2421	516	362	131	367	418	303	654	452

Notes: Each cell reports the coefficient from a separate regression. Regressions weighted by sampling weights. Sample: individuals aged 20–24 years. Regression of cognitive skills on a dummy that equals 1 if the individual is enrolled in university, 0 otherwise, including a gender control. Numeracy and literacy skills standardized to mean 0 and SD 1 within country. Pooled specification includes country fixed effects and gives same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table 4.4 : Difference-in-Differences Estimate of University Skill Premium

	20 vs. 25–29	20–21 vs. 25–29	20–22 vs. 25–29	20–23 vs. 25–29	20–24 vs. 25–29
Panel A: Numeracy Skills					
Old	.043	.038	.051**	.051**	.034*
	(.035)	*(.025)*	*(.022)*	*(.020)*	*(.018)*
University	.662***	.637***	.684***	.682***	.693***
	(.047)	*(.033)*	*(.025)*	*(.021)*	*(.017)*
Old X University	.101**	.125***	.074**	.074***	.078***
	(.050)	*(.037)*	*(.031)*	*(.028)*	*(.024)*
Observations	17433	19970	23215	26111	36159
Panel B: Literacy Skills					
Old	−.018	−.025	.004	.008	−.007
	(.035)	*(.025)*	*(.022)*	*(.021)*	*(.018)*
University	.607***	.604***	.659***	.669***	.683***
	(.047)	*(.033)*	*(.026)*	*(.022)*	*(.018)*
Old X University	.161***	.162***	.106***	.095***	.099***
	(.050)	*(.037)*	*(.031)*	*(.028)*	*(.024)*
Observations	17433	19970	23215	26111	36159

Notes: Each column reports the coefficients from a separate regression, using equation (4). "Old" is a dummy which takes the value 1 if the individual is aged between 25–29, and zero otherwise; "University" is a dummy for university attendance (either actual enrollment or graduation). Numeracy (Panel A) and literacy skills (Panel B) are regressed on the age and university attendance dummies and their interaction, including a gender control and country fixed effects. Sample: individuals aged as indicated in column header. Regressions weighted by sampling weights. Numeracy and literacy skills, standardized to mean 0 and SD 1 within country. Observations weighted such that each each country receives same weight. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table 4.5 : Within-Germany Analysis – IV Estimate of University Skill Premium

	Panel A: University Attendance			
First-Stage				
Distance	-.016***	-.015***	-.014***	-.014***
	(.004)	*(.004)*	*(.004)*	*(.004)*
1st-stage F stat.	17.49	14.15	12.72	12.81
	Panel B: Numeracy Skills			
OLS				
University Attendance	.743***	.734***	.725***	.589***
	(.030)	*(.030)*	*(.030)*	*(.030)*
Second-Stage				
University Attendance	1.137**	1.271**	1.286**	1.209**
	(.520)	*(.554)*	*(.589)*	*(.576)*
Federal state FE	✓			
Region FE		✓	✓	✓
Family background			✓	✓
High-school grades				✓
Observations	3976	3976	3976	3976

Notes: Each cell reports the coefficients from a separate regression. Sample: full sample of German PIAAC-L participants with available distance information. Panel A shows first-stage estimation results (dependent variable: university attendance). Panel B shows OLS and 2SLS estimation results (dependent variable: numeracy skills). All specifications control for gender, migrant status, parental education (Dummy which equals 1 if at least one parent has attained a university degree), and include birth year fixed effects. Region fixed effects refer to 40 administrative districts “Regierungsbezirke” in Germany. Family background characteristics include mother’s and father’s working status when the individual was 15 years old and position in birth order. High-school grades are final exam grades in high school in German, Mathematics, and first foreign language. Robust standard errors, clustered at the municipality level (1163 clusters), in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC-L 2014, 2015, 2016.

Appendix

Appendix A4.1 Instrumental Variable Estimations of the University Skill Premium Exploiting Campus Proximity in Germany

Literature Review

The geographic distance to a school or university may be one (financial) constraint to enrollment. Students who live far away from a university must leave home to attend university, which can be costly. Several studies provide evidence for the fact that students who grew up in an area outside the commuting distance to a university are far less likely to enroll. For the case of Germany, Spiess and Wrohlich (2010) examine whether the proximity of universities serves as a relevant determinant of an individual's educational decision and find that the distance to the next university at the time of completing high school significantly affects the decision to enroll. A 10 km difference in university proximity explains 2–3 percentage points of the difference in the probability of attending a university.[1]

Distance to cities, hospitals or educational institutions is a popular and often used instrument in the economic literature. Several studies exploit distance measures orthogonal to unobserved individual characteristics to investigate labor market returns to education (Card 1993; Maluccio 1998).[2] Card (1993) exploits variation in university proximity in an analysis of the returns to tertiary education in the United States. He shows that men who grew up in areas with a nearby university have significantly higher levels of education, even after controlling for regional and family background characteristics. IV results on the wage returns to university education are 25–60 percent higher than the corresponding OLS estimates. Thereby, the education and wage gains are concentrated among men with low educated parents. Using information on university openings during the German educational expansion and individual-level data from the German Socio-Economic Panel, Siegler (2012) estimates the effect of local university access on obtaining a university degree in a difference-in-differences framework. Results show that a university opening increases the share of university graduates in a county by 8 to 10 percentage points, mainly driven by increased enrollment of females and immigrants.

[1] Estimates of the relevance of university proximity for post-secondary educational decisions in other countries reveal similar results. See e.g. Card (1993) for the US, Frenette (2004, 2006, 2009) for Canada, Sá, Florax and Rietveld (2006) for the Netherlands, Flannery and Cullinan (2014) for Ireland, Rizzica (2013) for Italy and Suhonen (2014) for Finland. For England, Gibbons and Vignoles (2012) do not find a significant influence of geographical distance on enrollment decisions, but on institutional choice.

[2] Regarding other research areas, studies have used – for example – the distance to the nearest nursery (Attanasio, Di Maro and Vera Hernandez, 2013) or hospital (Baiocchi et al., 2010) to evaluate their causal impact on individual health outcomes. Currie and Moretti (2003) study the impact of maternal education on infant health at birth as well as various mechanisms underlying the effect for a large sample of white women in the US, using data on the availability of universities at the county level to instrument for maternal education. See Duflo (2001) and Muralidharan and Prakash (2017) for related research in development economics.

A recent study by Kamhöfer, Schmitz and Westphal (2018) is closely related to our analysis. Using variation in university accessibility induced by the German educational expansion in an IV approach and information from German NEPS data, the authors study returns to tertiary education on cognitive abilities, health and wages. Results suggest positive skill effects of university education.[3]

Institutional Background

Higher education institutions in Germany can be divided into two broad categories: traditional universities and Universities of Applied Sciences (*(Fach-)Hochschulen*). While high-school graduates are required to have a general university entrance qualification (*Hochschulreife, Abitur*) to enroll in university, Universities of Applied Sciences (UAS) can also be entered with a subject-linked university entrance qualification (*fachgebundene Hochschulreife, Fachabitur*). The Abitur exam is taken after completion of the Gymnasium, which is the highest secondary school track. During the second half of the 20th century, Germany experienced a substantial expansion of tertiary education institutions. Thereby, most of the expansion was driven by the openings of new UAS: the number increased from 68 in 1970, to 134 in 1980, up to 198 in 2009. This implies a decrease in costs of attending tertiary education and hence may have changed individuals' incentives towards enrollment.

Starting with relatively low levels of educational attainment after World War II, the process of educational expansion was triggered by several intertwined economic and social developments that led to a growing demand for high-skilled workers.[4] In a needs assessment for educational expenditures in 1961, the *Kultusministerkonferenz* (The Standing Conference of the Ministers of Education and Cultural Affairs of the Länder in the Federal Republic of Germany) proposed that educational expenditures should double until 1970. A large share of the additional funds was supposed to be invested – among others – in the expansion of existing tertiary educational institutions as well as in the new foundation of universities and UAS.

Kultusministerkonferenz (2011) analyzes the mobility of university and UAS entrants between 1980 and 2009 using information from the German university statistics (*Hochschulstatistik*). Overall, student mobility is increasing in Germany. Within the observed period, the share of students who changes federal state to attend university increased from 26 percent in 1980 to 35 percent in 2009. However, the majority of German student is still territorially immobile,

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[3] Jürges, Reinhold and Salm (2011) also exploit variation in universities access from the Germany educational expansion in an IV approach to estimate health effects of schooling. However, they focus on higher (academic track) secondary schooling. Beside the opening of new universities and Universities of Applied Sciences, the period of educational expansion was characterized by greater access to academic track schools and hence increased the share of individuals eligible for enrollment in university.

[4] See Kamhöfer and Westphal (2017) as well as `http://www.bpb.de/izpb/198031/bildungsexpansion-und-bildungschancen?p=all` and `https://www.kmk.org/kmk/aufgaben/geschichte-der-kmk.html`.

meaning that they attend university in the federal state where they graduated from high school.[5]

Heine, Krawietz and Sommer (2008) report results from a large representative survey of German university and UAS entrants conducted during the winter term 2006/2007. According to this sample of university entrants, the proximity of the campus to their hometown is a central determinant of the university choice. 65 percent of students categorized the proximity as important influencing factor. Financial reasons, which prevent students to move to another city, are relevant for almost one fifth of surveyed students. However, the importance of university proximity varies considerably across fields of study. Individuals who study to become a teacher as well as (science of) art students value proximity high – mostly due to family ties, financial reasons, having a partner at home, or familiarity with the hometown. Also, engineering students rank university locations close to their parents' home high due to financial reasons. First-semester law students also rank proximity high, however not because of financial constraints but because of family ties and familiarity with the city. Students in forestry, agricultural and nutritional science do not value location motives very high.

PIAAC and PIAAC-L Data

For our purpose, we use data for Germany from PIAAC-L, which comprise the original PIAAC survey (see Section 4.3) plus three resurvey-waves in 2014, 2015, and 2016. In addition to PIAAC 2012 participants (*anchor persons*), also household members of anchor persons were interviewed in PIAAC-L, which increases the number of observations available for our analysis. Besides the rich background questionnaire and additional skills testing in 2015, PIAAC-L contains information on the location of the highest secondary school attended by the individual and the respective graduation year. Assuming that the school location is close to the individual's home town, our data reveals the exact place of residence of the individual at the time when deciding upon further schooling investments. While all anchor persons, who could be re-surveyed, were also re-tested in 2015 by means of PIAAC test instruments, participating household members were tested by means of NEPS test instruments in 2015.[6] We combine z-standardized PIAAC and NEPS skill measures in our main IV specification to retain as many observations as possible. To address potential mean differences across the two test regimes, we control for the test regime throughout all regressions and show that our results are not sensitive to the exclusion of individuals with only NEPS skill measures.

[5] The share of immobile students is especially large in the three biggest states of Northrhine-Westfalia (80.2% in 2009), Bavaria (76.5%), and Baden-Wuerttemberg (68.9%). Even for the more mobile students, migration appears primarily between neighboring states. Thereby, women are slightly more mobile than male students which may be due to gender differences in the selection of fields of study (Kultusministerkonferenz, 2011). Overall, the mobility varies significantly by subject. The authors argue that varying admission procedures and entrance examinations across fields of study as well as regional variation in supply may cause the observed differences in mobility shares. The later cause is strengthened by the fact that e.g., veterinary medicine, human medicine, forestry, agricultural and nutritional science, and art show the highest mobility rates in Germany.

[6] National Education Panel Study, see Blossfeld (2011).

The campus proximity, measured as linear distance, is likely to be superior to measures used in several previous studies since we know the exact distance in kilometers or minutes of travel time from an individual's home municipality to the municipality of the nearest university campus. Using county-level information on university proximity may not always capture the relevant universities that should be included in a high-school graduate's choice set since a campus in the neighboring county may be even closer than the campus in the home county (e.g., Currie and Moretti 2003; Siegler 2012). Furthermore, we do not need to approximate the municipality of school attendance based on information on place of birth or place of residence at the time of the survey, which is more common in large-scale surveys.[7] If individuals move randomly from county to county, then these studies likely underestimate the effect of university accessibility in the first stage. However, if there is non-random migration – e.g., young people and their families move closer to a university in order to benefit from the proximity – this may threaten the identification of the true university skill premium.

The final regression sample contains 3,976 observations, differing slightly from the sample used in the international analysis described in Section 4.3. Individuals who are still in secondary education and those with foreign high-school diploma were excluded from our estimations. Furthermore, we consider individuals between 16 and 79 years old.[8] Table A4.8 reports summary statistics for all relevant variables. 32 percent of people in our sample went to university. On average, numeracy and literacy skills amount to 282.5 and 281.8 PIAAC points, respectively. The mean commuting distance to the nearest campus is 19.1 kilometers. The distribution of distance measures is also visualized in Figure A3 (coarsened due to confidentiality issues). 51 percent of individuals in our sample are female and one fifth is considered a first- or second-generation migrant. People graduated from high school between 1951 and 2015. 30 percent of people in our sample state that at least one parent has a tertiary education degree.

Empirical Strategy

To test the relevance of our instrument, the following first stage regression is estimated:

$$UNI_{itm} = \beta_0 + \beta_1 DISTANCE_{itm} + \boldsymbol{X_{itm}\beta_2} + \phi_t + \eta_r + \varepsilon \qquad \text{(A4.1)}$$

The outcome variable UNI_{itm} is a dummy which equals one if individual i graduating from high school in year t and living in municipality m ever enrolled in university, and zero otherwise.

[7] For instance, Currie and Moretti (2003) only observe the mother's residence at the time of the birth of the baby, not when she made her tertiary educational decision. Especially in the US this may result in substantial measurement errors since young women have high mobility rates. Toivanen and Väänänen (2016) approximate the residence at age 18 – which would be the preferred but unavailable information – by an individual's place of birth to calculate the distance to each engineering establishment in Finland.

[8] The age range differs from the original age range between 16 and 65 years in PIAAC in 2012 because household members older than 65 were surveyed as well.

To estimate the role of campus proximity for an individual's educational decision, we regress the dummy on a measure of campus proximity $DISTANCE_{itm}$. The distance to the nearest university is measured at the time of high-school graduation t, when individuals decides upon their further career paths. In our preferred specification, we control for a set of individual socio-demographic (gender, migrant status) and family background characteristics (parental education, position in birth order, work status of parents when the individual was 15), denoted by X_{itm}. Birth year (ϕ_t) and region fixed effects (η_r) are included throughout all specifications. The university skill premium is estimated by the following equation:

$$SKILLS_{itm} = \lambda_0 + \lambda_1 \widehat{UNI_{itm}} + \boldsymbol{X_{itm}\lambda_2} + \phi_t + \eta_r + \vartheta \tag{A4.2}$$

whereby $\widehat{UNI_{itm}}$ is instrumented by campus proximity. Standard errors are clustered at the municipality level to allow for potential correlations between the errors within each municipality.[9] For identification, we use the fact that within each year, some high-school graduates in the same region face higher costs of going to university than others because they have to commute further to reach the nearest campus. Thereby, we assume that campus proximity affects the individual's education choice but has no other association with cognitive skills, once we control for a set of individual and regional characteristics. Additionally, no third (unobserved) factors are supposed to affect the location choice of individuals as well as the formation of cognitive skills.

In our preferred specification, we include dummies for 40 administrative regions.[10] By doing so, we control for any unobserved characteristics common to all individuals living in one specific region. A desirable approach to capture as much regional heterogeneity as possible would be to identify the effect from variation in campus proximity over time within smaller regional areas than "Regierungsbezirke". Siegler (2012) uses a differences-in-differences setting to identify the effect of university openings on obtaining a university degree, exploiting county-level information on university openings in Germany. However, we are unable to estimate such specifications due to sample limitations. While we have precise information about the distance between the home town municipality (centroid) of the high-school graduate and the nearest university, we lack sufficient over-time variation in campus proximity within municipalities or counties to include regional fixed effects on such dis-aggregated level (1,224 municipalities, 375 counties).[11]

[9] Standard errors do not change substantially when they are clustered at the level of municipality times year of birth or municipality times high school graduation year. This may be due to our relatively small sample size. In only 632 out of 1161 municipalities we observe more than one individual.

[10] The region dummies included in our regression refer to "Regierungsbezirke". Germany is divided into 16 federal states and each federal state is divided in various Regierungsbezirke. Several counties are aggregated to one Regierungsbezirk.

[11] Due to the PIAAC sampling strategy, some municipalities are observed several times, however without variation in university proximity over time (zero distance, the municipality is likely a big city). Other municipalities are observed only once, which prevents the identification of the effect over time.

Admittedly, the approach of exploiting geographical variation in the supply of educational institutions for identification is not free of concerns. Several concerns may prevent the identification of a causal effect in an instrumental variable approach exploiting variation in proximity. Families with high educational aspirations may select themselves into areas with high-quality schools and a university nearby. At the same time, children of these families may have higher innate ability or motivation to invest in the formation of skills. Hence, higher cognitive skills of people living in municipalities with a nearby university could not be entirely attributed to university education. Likewise, universities may open more likely in regions with prospering labor markets or university locations attract more firms offering jobs that promote the development of skills. In addition, the presence of a tertiary education institution may be associated with higher quality of elementary and secondary schools in the same region. Hence, higher cognitive skills of people living in municipalities with a nearby university could not be entirely attributed to university education but may already result from attending high-quality schools.[12]

The availability of a large set of family background characteristics in our data allows us to address concerns arising from socio-economic differences between individuals which may affect an individual's location as well as the attained educational level.[13] However, concerns remain that observably equivalent families may have different unobserved tastes for education and choose their living location accordingly. In some specifications, we also control for exit exam grades at high school to address ability differences of high-school graduates before enrolling in tertiary education. However, addressing ability differences of high-school graduates prior to their university enrollment decision may raise some concerns. Living close to a university campus may not only affect individuals just at the point in time when they finish secondary education but may also influence effort exerted in school or even the choice of secondary school track. In this sense, controlling for exit exam grades would be a bad control because they might already be affected by campus proximity.

If university openings during the educational expansion occurred non-random with respect to regional variables, the distance to the nearest university would also be non-random and disqualify as valid exogenous variation in enrollment. To avoid picking up such regional differences with our instrument, we include dummies for the 40 administrative regions in Germany in our regressions. Kamhöfer and Westphal (2017) provide qualitative and quantitative evidence that university openings were not related to demographic or socio-economic regional

[12] In relation to this, Currie and Moretti (2003) argue that higher education institutions may tend to open in areas where residents' education is already increasing or is expected to increase, and therefore are not a cause but an effect of increasing education.

[13] Previous studies had limited access to family background information. Card (1993), for instance, uses an indicator for mother's and father's education, the interaction of mother's and father's education, as well as indicators for family structure at age 14. In contrast, Currie and Moretti (2003) control for median income and the percent urban in the county when the woman was 17. Kane and Rouse (1995) include region dummies, city-size dummies, and controls for family background (family income) and measured ability (high-school class rank, test scores) in the wage and earnings equations.

characteristics, such as population size, marital structure, fertility, share of migrants or unemployment rate, nor other reasons except local politicians' own motivation, e.g. to gain voters' support before local elections. Kamhöfer, Schmitz and Westphal (2018) compare counties (*Kreise*) with and without university openings using Micro Census 1962 data and do not find differences in terms of socio-demographics across the two groups. However, socio-economic indicators differed somewhat between regions with and without universities. According to their calculations, employees in areas with already existing universities (before 1958) had a higher income, were less likely to work in the agricultural sector and had a higher probability of being employed in academic occupations. If these characteristics are also related to cognitive skills, the IV estimate of the university skill premium would be biased.

Results

First-Stage Results
To verify the relevance of our instrument, we run OLS regressions of the probability of university attendance on our preferred measure of campus proximity and a set of control variables. Table 4.5, Panel A, shows the corresponding estimation results for varying model specifications. All specifications control for gender, migrant status, parental education, as well as birth-year fixed effects. When controlling for differences across federal states – such as university enrollment shares and other institutional characteristics – results suggest that high-school graduates who live 10 kilometers further away from the campus are 1.6 percentage points less likely to enroll in university (5 percent).[14] If we include region dummies in Column 2, the coefficient decreases slightly to 1.5 percentage points. Our results support previous descriptive evidence on the relevance of university proximity for an individual's enrollment decision. Adding further controls for family background barely changes the association between university proximity and attendance (Column 3).[15] The reported F-statistic clearly exceeds the conventional threshold for strong instruments throughout all specifications.

Second-Stage Results
Table 4.5, Panel B, reports ordinary least square and two-stage least square estimates of the effect of university attendance on numeracy and literacy skills. University students have 0.74 SD (38 PIAAC points) higher numeracy skills than individuals who did not attend university. The estimated skill return does not change when including region fixed effects (Column 2) or additional family background variables (Column 3). Thus, the average university skill premium for the sample of the German-based analysis is slightly lower than OLS results of numeracy

[14] Table A4.11 reports the identical first-stage regression results, including coefficients for main control variables. Males as well as high school graduates with high-educated parents are more likely to go to university.
[15] These controls include dummies for whether the father and the mother were working when the individual was 15 years old and the person's birth order position. Results do not change when we include other background controls like books at home at the age of 15, number of brothers/sisters/siblings or the education level of each parent separately.

skills on university degree in the international analysis, which amount to 0.85 SD (42 PIAAC points, not shown).[16]

Panel B further reports coefficients from two-stage least squares estimations of numeracy skills on university attendance, using exogenous variation in university attendance based on campus proximity. When state-specific characteristics are controlled for in Column 2, the effect of university education on numeracy skills is 1.14 SD (58 PIAAC points). When we add region-specific fixed effects to account for geographical factors on a more dis-aggregated level, the estimated skill premium of university education is 1.13 SD. This effect barely changes when adding a full set of family background characteristics in Column 3.[17]

As described before, the approach of exploiting geographical variation in the supply of educational institutions for identification is not free of concerns. While our analysis is limited in its possibilities to address all potential threats to the identification of the true university skill premium, we address several concerns. First, we address the unobserved heterogeneity in family characteristics related to university proximity by adding information about high-school exit exam grades for Math, German and their first foreign language. This does not change our first-stage results. However, we can observe that the impact of parental education decreases (Table A4.11, Column 4), which suggests that part of the relationship between parental education and university enrollment is driven by higher student achievement in secondary school. In addition, the OLS estimate of the university skill premium decreases. Conditional on ability at university entry (proxied by exit exam grades), the numeracy skill gap between university graduates and those who did not attend university is 0.59 SD. This suggests that a substantial part of the university skill premium estimated in Column 1–3 is driven by selective university attendance based on cognitive skills. However, two-stage least square estimates of the effect of university education on skills barely changes. University graduates, who were induced to attend university because they live close to a university, have 1.2 SD higher numeracy skills.

Addressing ability differences of high school graduates prior to their university enrollment decision may raise some concerns. Living close to a university campus may not only affect individuals just at the point in time when they finish secondary education but may also influence effort exerted in school or even the choice of secondary school track. In this sense, controlling for exit exam grades would be a bad control because they might already be affected by campus proximity.[18] Still, controlling for high-school exit grades addresses potential threats to identification due to selective location of families close to universities.

[16] However, a different set of control variables and a different sample is used in the international analysis (see Section 4.4.1). Across all countries, the university skill premium is 0.78 SD, when estimating Equation 4.1 substituting wages by numeracy skills.

[17] Results for literacy skills are shown in Table A4.9.

[18] This concern is strengthened in our data when we regress the probability of holding an Abitur on the distance to the nearest tertiary institution. Individuals who live 10 kilometers closer to the nearest campus are 2.6 percentage points more likely to hold an Abitur. Thus, we will not include high-school exit exam grades in our preferred specification.

In an ideal setting, our IV model would identify from within-municipality variation in university proximity. However, we cannot include municipality fixed effects due to data limitations. As explained above, our data lacks sufficient over-time variation in university proximity within municipalities. Although we include 40 dummies for geographical regions on a more aggregated level than municipalities, this may not fully capture unobserved regional characteristics associated with university proximity as well as cognitive skill development. When we include an additional control for distance to the nearest big city,[19] our results do not change. One way to further address this concern is to condition on the degree of urbanization, proxied by a dummy indicating whether the municipality is in a rather urban or rural area. Results are shown in Table A4.10. The size and precision of estimated IV coefficients decrease substantially when we condition on the degree of urbanization. Individuals, who were induced to attend university because they live close to a university, have 0.74 – 0.84 SD higher numeracy skills than lower-educated individuals. The point estimates exceed corresponding OLS coefficients by 12–14 percent. Our results suggest that our IV estimates may be biased due to unobserved heterogeneity that cannot be sufficiently accounted for. Although we shy away from claiming that we can provide clean causal evidence on positive skill effects of university attendance, we interpret our results as another exploration into the identification of skill effects of university education. While relying on arguably strong assumptions, our German-based IV analysis provides suggestive evidence that is consistent with the idea of a skill-enhancing effect of university education, matching our findings from the international difference-in-differences analysis.

Robustness Checks

In this section, we show that our results are robust to a series of alternative specifications. The estimated coefficients do not change substantially when we use alternative measures of university proximity (Table A4.12). Travel distance to the nearest university is the distance in kilometers traveled by car on roads as of today. This may induce some measurement imprecision because individuals in our sample make their university choice at various points in time and road networks may have expanded over time, which cannot be accounted for in our setting. Indeed, the estimated effect of university education on numeracy skills using travel distance is slightly lower (Panel A), suggesting small attenuation bias due to measurement error in our instrument. Travel time reports the time (in minutes) needed to travel to the nearest university by car on roads as of today. Again, estimated IV skill effects of university education decrease only negligibly.[20]

To assure that our results are not driven by a specific subgroup, we run our IV model on varying sample specifications. First, we exclude household members interviewed and tested in PIAAC-L and only consider PIAAC anchor persons, because the test instruments differed

[19] Cities are classified as being big with more than 200.000 inhabitants. In an alternative specification, big cities are classified as being big with more than 1 Mio inhabitants (incl. Leipzig).

[20] It may be the case that travel distance and time by car is not the relevant mean of transportation that is considered by young individuals when deciding upon university education investments. Often, train connections are more relevant for calculating commuting costs of attending university.

slightly between the two groups. Regressions using only the sample of PIAAC anchor persons yield qualitatively similar results despite the loss of almost 1000 observations (Table A4.13). Excluding individuals who have a university campus in their home town, or those who live in the city states of Hamburg, Bremen, and Berlin, or excluding all individuals from the former German Democratic Republic (GDR, East Germany) does not change substantially the association between university proximity and university attendance.[21]

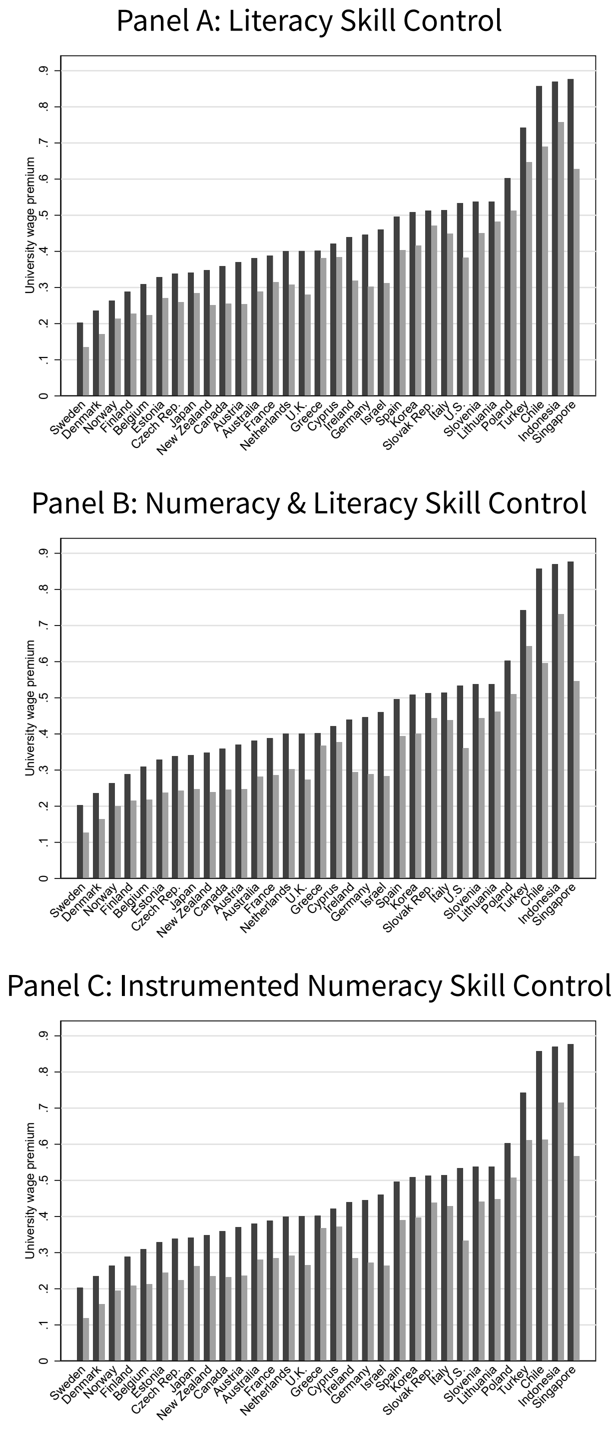
Subgroup analyses facilitate the identification of the population affected by our instrument. Due to sample size limitations, we are not able to conduct a fully elaborated analysis of compliers to the instrument of university proximity. However, we do not observe gender differences in the extent of being affected by the instrument (not shown). Older cohorts seem to be more affected, which can be explained by worse transportation networks and less financial aid for students some decades ago.[22] In contrast to previous studies (e.g., Card 1993; Frenette 2009), we do not find evidence that especially high school graduates from low-SES families are affected by university campus proximity (not shown). One potential reason are cost differences of university education between e.g., the United States and Germany. While tuition fees impose very high direct costs of education, universities in Germany are (almost) free of charge. IV estimates of the university skill premium show that females and high-SES students benefit more from attending university in terms of higher skills (not shown).

[21] Germany was separated into the Federal Republic of Germany (FRG or West Germany) and the German Democratic Republic (GDR or East Germany) between 1949 until 1990 – covering a big part of high school graduation years in our sample. Since very different regimes were present in the two parts of Germany, this might also have affected educational decisions of individuals living in these regimes. While West Germany had a social market economy like the German and many other countries' system today, East Germany was part of the Eastern bloc administered by the Soviets and hence was a socialist state. Consequently, people's educational careers were mostly pre-determined and from an ex ante perspective we would not expect to see any effect of campus proximity on university enrollment. Our sample includes only 665 individuals from East Germany before 1990.

[22] The importance of proximity as an instrument for university attendance reduces over time because travel, communication and room costs have become a relatively smaller share of the entire cost of university. Carneiro, Heckman and Vytlacil (2011) show that estimates based on campus proximity several decades ago, are outdated, since the share and types of students enrolling in university has changed substantially since then.

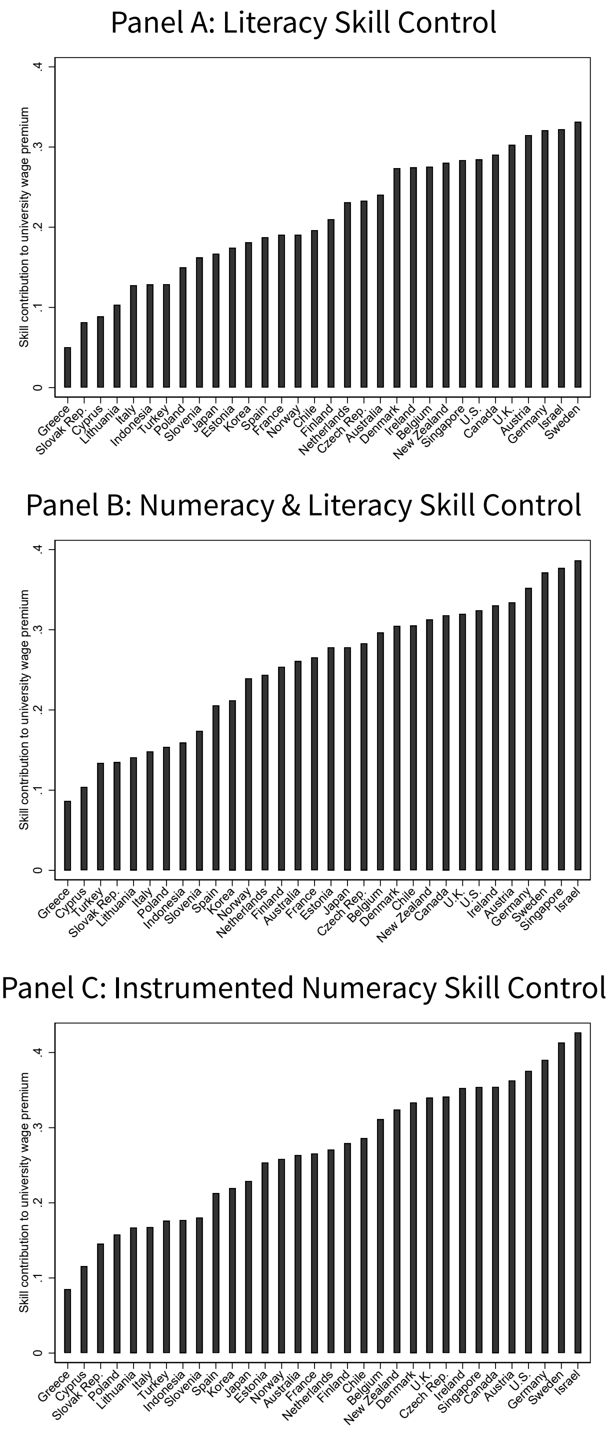
Appendix A4.2 Appendix Figures and Tables

Figure A4.1 : University Wage Premia Around the World



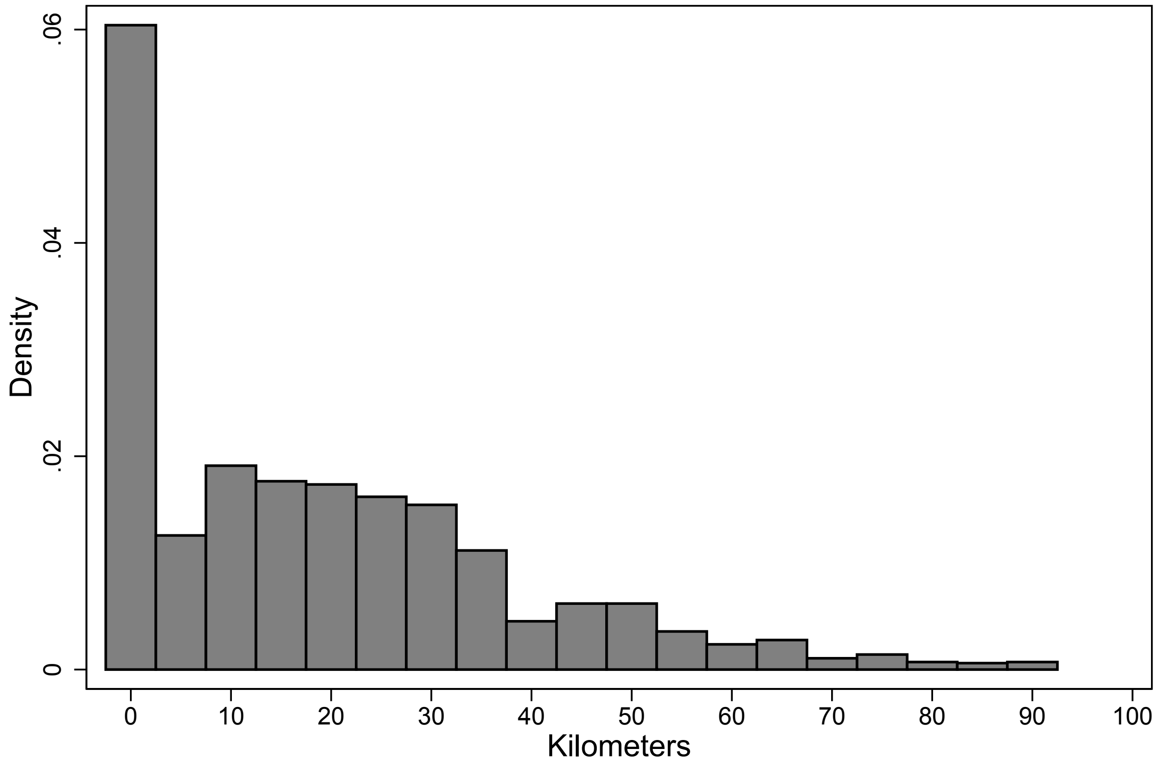
Notes: Wage returns to university education across PIAAC countries. Dark grey bars show wage premia as estimated in Table A4.3, A4.4 and A4.5, row (1), light grey bars show wage premia after conditioning on literacy skills (Panel A), on numeracy and literacy skills (Panel B), and on numeracy skills instrumented by literacy skills (Panel C), as estimated in row (2). *Data source:* PIAAC 2012/2015.

Figure A4.2 : The Contribution of Skills to University Wage Premia



Notes: Contribution of literacy skills (Panel A), numeracy and literacy skills (Panel B), and numeracy skills instrumented by literacy skills (Panel C) to wage return of university education across PIAAC countries, as calculated in Table A4.3, A4.4 , and A4.5, row (3). *Data source:* PIAAC 2012/2015.

Figure A4.3 : Distribution of Distance to Nearest University Campus



Notes: Figure shows the distribution of distance to nearest university campus in 5-kilometer intervals (capped at 90 kilometers). Sample: full sample of German PIAAC-L participants with available distance information. *Data source:* PIAAC-L 2014.

Table A4.1 : Summary Statistics

Country	Obs.	University degree	Numeracy skills	Literacy skills	Hourly wages (nat. currency)	Female	Work experience	Migrant	Parental education
Australia	1433	0.43	283.8	292.0	35.1	0.34	24.1	0.53	1.82
Austria	1115	0.22	282.1	273.0	15.4	0.34	24.9	0.27	1.84
Belgium	1220	0.45	290.4	282.3	17.9	0.39	23.2	0.11	1.79
Canada	7189	0.59	271.9	279.0	27.6	0.49	23.5	0.41	2.06
Chile	903	0.31	213.5	222.2	3295.5	0.38	18.5	0.05	1.64
Cyprus	938	0.42	273.8	275.8	11.9	0.46	22.0	0.13	1.44
Czech Republic	1065	0.18	274.2	272.0	117.3	0.50	22.4	0.13	1.97
Denmark	1879	0.46	291.8	280.9	198.3	0.49	24.4	0.11	1.97
Estonia	1763	0.46	275.2	276.8	5.1	0.56	22.0	0.36	1.98
Finland	1480	0.55	293.5	298.1	18.6	0.51	21.1	0.06	1.69
France	1714	0.34	262.8	266.1	13.8	0.44	22.5	0.21	1.64
Germany	1296	0.37	283.0	276.4	16.8	0.36	23.4	0.33	2.25
Greece	623	0.36	263.5	261.2	7.1	0.40	19.1	0.11	1.46
Indonesia	803	0.23	208.5	202.5	16782.3	0.20	16.5	0.00	1.07
Ireland	1033	0.49	271.6	278.0	21.2	0.40	22.0	0.21	1.68
Isreal	908	0.62	263.1	261.8	57.8	0.48	22.4	0.73	2.11
Italy	1019	0.13	256.0	256.1	11.8	0.36	20.8	0.10	1.26
Japan	1319	0.52	300.3	305.5	1902.4	0.35	21.3	0.01	2.04
Korea	1441	0.43	265.9	273.0	12962.6	0.40	16.2	0.03	1.48
Lithuania	1258	0.28	269.9	266.7	11.6	0.55	21.8	0.09	1.98
Netherlands	1012	0.42	292.7	294.0	19.7	0.28	22.9	0.19	1.68
New Zealand	1200	0.51	279.0	286.3	29.1	0.42	23.5	0.46	1.98
Norway	1519	0.48	294.3	290.2	235.9	0.44	22.2	0.15	2.03
Poland	817	0.31	262.0	267.1	16.5	0.47	20.7	0.06	1.82
Singapore	1507	0.56	259.0	255.2	23.8	0.43	19.9	0.61	1.52
Slovak Republic	1193	0.23	286.7	279.9	4.3	0.51	22.4	0.09	1.81
Slovenia	1306	0.31	263.1	259.2	7.7	0.49	22.7	0.23	1.77
Spain	1191	0.44	260.4	264.2	10.4	0.41	20.5	0.11	1.35
Sweden	1316	0.38	288.0	289.0	172.8	0.49	22.4	0.26	1.87
Turkey	674	0.23	233.7	233.8	11.8	0.22	16.0	0.04	1.13
United Kingdom	1782	0.44	274.2	281.9	14.3	0.39	24.6	0.23	1.95
United States	1121	0.45	262.7	276.5	24.6	0.47	24.0	0.23	2.20
Total	45037	0.40	273.2	273.8		0.43	21.9	0.22	1.79

Notes: Table shows number of observations and means of variables by country. Sample: full-time employees aged 35–54 years. University degree is a dummy which equals 1 if the individual has achieved a university degree, zero otherwise. All skills in PIAAC are measured on a 500-point scale. Gross hourly wages are reported in national currency (trimmed at the bottom and top one percent of the wage distribution in each country). Work experience reports years of paid work during lifetime. Migrant is a dummy which equals one if the individual is a first- or second-generation migrant. Parental education is classified in 3 categories: 1(neither parent has attained upper secondary education), 2 (at least one parent has attained secondary education), 3 (at least one parent has attained tertiary education). *Data source:* PIAAC 2012/2015.

Table A4.2 : University Wage Premium & the Contribution of Numeracy Skills – All Age Groups

	Pooled	Australia	Austria	Belgium	Canada	Chile	Cyprus	Czech R.	Denmark	Estonia	Finland
(1) University wage premium without skill control	.418***	.312***	.370***	.296***	.353***	.777***	.384***	.371***	.277***	.318***	.296***
	(.004)	*(.017)*	*(.018)*	*(.013)*	*(.012)*	*(.039)*	*(.023)*	*(.033)*	*(.011)*	*(.017)*	*(.011)*
(2) University wage premium with skill control	.324***	.238***	.277***	.219***	.280***	.563***	.333***	.297***	.224***	.234***	.241***
	(.004)	*(.017)*	*(.018)*	*(.014)*	*(.012)*	*(.041)*	*(.023)*	*(.037)*	*(.011)*	*(.017)*	*(.012)*
(3) Contribution skills to wage premium	.224	.239	.252	.260	.209	.276	.132	.199	.192	.262	.188
Observations	90736	2900	2091	2240	14718	1972	1893	2292	3795	3614	2887
	France	Germany	Greece	Indonesia	Ireland	Israel	Italy	Japan	Korea	Lithuania	Netherl.
(1) University wage premium without skill control	.357***	.449***	.333***	.767***	.386***	.420***	.460***	.320***	.506***	.470***	.394***
	(.012)	*(.020)*	*(.031)*	*(.053)*	*(.023)*	*(.025)*	*(.028)*	*(.018)*	*(.020)*	*(.024)*	*(.016)*
(2) University wage premium with skill control	.264***	.336***	.312***	.610***	.280***	.302***	.393***	.230***	.409***	.402***	.308***
	(.013)	*(.021)*	*(.033)*	*(.055)*	*(.023)*	*(.027)*	*(.028)*	*(.019)*	*(.022)*	*(.027)*	*(.016)*
(3) Contribution skills to wage premium	.261	.252	.065	.204	.273	.282	.147	.281	.192	.146	.220
Observations	3161	2563	1021	1714	1991	2173	1611	2595	2692	2469	1968
	New Zealand	Norway	Poland	Singapore	Slovak R.	Slovenia	Spain	Sweden	Turkey	U.K.	U.S.
(1) University wage premium without skill control	.292***	.257***	.517***	.825***	.459***	.505***	.468***	.190***	.729***	.396***	.494***
	(.016)	*(.011)*	*(.021)*	*(.019)*	*(.025)*	*(.017)*	*(.020)*	*(.011)*	*(.040)*	*(.020)*	*(.022)*
(2) University wage premium with skill control	.210***	.200***	.436***	.553***	.380***	.419***	.374***	.140***	.669***	.299***	.348***
	(.015)	*(.011)*	*(.022)*	*(.022)*	*(.025)*	*(.018)*	*(.021)*	*(.011)*	*(.041)*	*(.020)*	*(.024)*
(3) Contribution skills to wage premium	.280	.221	.157	.330	.174	.172	.202	.260	.082	.245	.294
Observations	2624	2846	3451	3102	2273	2114	2075	2563	1462	3485	2381

Notes: Each cell reports the coefficient from a separate regression. Regressions weighted by sampling weights. Sample: all full-time employees aged 16–65. Dependent variable in specifications (1) and (2) is gross hourly wages (log). Specification (2) controls for numeracy skills. Scalars in (3) are calculated as ((1)-(2))/(1). All regressions control for a quadratic polynomial in actual work experience and gender. Observations refer to baseline specification. Numeracy skills standardized to mean 0 and SD 1 within country. Pooled specification includes country fixed effects and gives same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A4.3 : University Wage Premium & the Contribution of Literacy Skills

	Pooled	Australia	Austria	Belgium	Canada	Chile	Cyprus	Czech R.	Denmark	Estonia	Finland
(1) University wage premium without skill control	.440***	.381***	.370***	.309***	.359***	.858***	.421***	.339***	.235***	.328***	.288***
	(.005)	*(.026)*	*(.025)*	*(.018)*	*(.016)*	*(.058)*	*(.033)*	*(.052)*	*(.014)*	*(.024)*	*(.016)*
(2) University wage premium with skill control	.347***	.289***	.254***	.224***	.255***	.689***	.384***	.260***	.171***	.271***	.228***
	(.005)	*(.026)*	*(.025)*	*(.019)*	*(.016)*	*(.061)*	*(.033)*	*(.058)*	*(.015)*	*(.025)*	*(.018)*
(3) Contribution skills to wage premium	.211	.240	.315	.276	.291	.196	.089	.233	.274	.175	.210
Observations	45037	1433	1115	1220	7189	903	938	1065	1879	1763	1480

	France	Germany	Greece	Indonesia	Ireland	Israel	Italy	Japan	Korea	Lithuania	Netherl.
(1) University wage premium without skill control	.388***	.446***	.402***	.869***	.439***	.461***	.514***	.341***	.509***	.538***	.400***
	(.017)	*(.025)*	*(.041)*	*(.079)*	*(.036)*	*(.033)*	*(.039)*	*(.025)*	*(.027)*	*(.036)*	*(.022)*
(2) University wage premium with skill control	.314***	.303***	.382***	.758***	.319***	.312***	.449***	.284***	.416***	.482***	.308***
	(.019)	*(.026)*	*(.043)*	*(.078)*	*(.036)*	*(.037)*	*(.039)*	*(.027)*	*(.029)*	*(.038)*	*(.022)*
(3) Contribution skills to wage premium	.191	.321	.051	.129	.275	.322	.128	.167	.181	.103	.231
Observations	1714	1296	623	803	1033	908	1019	1319	1441	1258	1012

	New Zealand	Norway	Poland	Singapore	Slovak R.	Slovenia	Spain	Sweden	Turkey	U.K.	U.S.
(1) University wage premium without skill control	.348***	.263***	.603***	.877***	.513***	.538***	.496***	.202***	.742***	.401***	.534***
	(.024)	*(.014)*	*(.032)*	*(.029)*	*(.036)*	*(.022)*	*(.025)*	*(.015)*	*(.055)*	*(.029)*	*(.032)*
(2) University wage premium with skill control	.250***	.213***	.512***	.628***	.471***	.450***	.403***	.135***	.646***	.280***	.382***
	(.025)	*(.015)*	*(.034)*	*(.033)*	*(.036)*	*(.024)*	*(.028)*	*(.016)*	*(.060)*	*(.030)*	*(.033)*
(3) Contribution skills to wage premium	.280	.191	.150	.284	.081	.162	.187	.331	.129	.303	.284
Observations	1200	1519	817	1507	1193	1306	1191	1316	674	1782	1121

Notes: Each cell reports the coefficient from a separate regression. Regressions weighted by sampling weights. Sample: full-time employees aged 35–54 years. Dependent variable in specifications (1) and (2) is gross hourly wages (log). Specification (2) controls for literacy skills. Scalars in (3) are calculated as ((1)-(2))/(1). All regressions control for a quadratic polynomial in actual work experience and gender. Observations refer to baseline specification. Literacy skills standardized to mean 0 and SD 1 within country. Pooled specification includes country fixed effects and gives same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A4.4 : University Wage Premium & the Contribution of Numeracy and Literacy Skills

	Pooled	Australia	Austria	Belgium	Canada	Chile	Cyprus	Czech R.	Denmark	Estonia	Finland
(1) University wage premium without skill control	.440***	.381***	.370***	.309***	.359***	.858***	.421***	.339***	.235***	.328***	.288***
	(.005)	*(.026)*	*(.025)*	*(.018)*	*(.016)*	*(.058)*	*(.033)*	*(.052)*	*(.014)*	*(.024)*	*(.016)*
(2) University wage premium with skill control	.330***	.281***	.247***	.217***	.245***	.596***	.377***	.243***	.164***	.237***	.215***
	(.005)	*(.026)*	*(.025)*	*(.019)*	*(.016)*	*(.056)*	*(.034)*	*(.060)*	*(.015)*	*(.025)*	*(.019)*
(3) Contribution skills to wage premium	.251	.261	.334	.297	.318	.305	.104	.283	.305	.278	.254
Observations	45037	1433	1115	1220	7189	903	938	1065	1879	1763	1480
	France	**Germany**	**Greece**	**Indonesia**	**Ireland**	**Israel**	**Italy**	**Japan**	**Korea**	**Lithuania**	**Netherl.**
(1) University wage premium without skill control	.388***	.446***	.402***	.869***	.439***	.461***	.514***	.341***	.509***	.538***	.400***
	(.017)	*(.025)*	*(.041)*	*(.079)*	*(.036)*	*(.033)*	*(.039)*	*(.025)*	*(.027)*	*(.036)*	*(.022)*
(2) University wage premium with skill control	.285***	.289***	.367***	.731***	.294***	.283***	.438***	.246***	.401***	.462***	.302***
	(.019)	*(.025)*	*(.042)*	*(.079)*	*(.037)*	*(.037)*	*(.039)*	*(.027)*	*(.029)*	*(.038)*	*(.022)*
(3) Contribution skills to wage premium	.266	.352	.087	.159	.330	.386	.148	.278	.212	.141	.244
Observations	1714	1296	623	803	1033	908	1019	1319	1441	1258	1012
	New Zealand	**Norway**	**Poland**	**Singapore**	**Slovak R.**	**Slovenia**	**Spain**	**Sweden**	**Turkey**	**U.K.**	**U.S.**
(1) University wage premium without skill control	.348***	.263***	.603***	.877***	.513***	.538***	.496***	.202***	.742***	.401***	.534***
	(.024)	*(.014)*	*(.032)*	*(.029)*	*(.036)*	*(.022)*	*(.025)*	*(.015)*	*(.055)*	*(.029)*	*(.032)*
(2) University wage premium with skill control	.239***	.200***	.510***	.546***	.444***	.444***	.394***	.127***	.643***	.273***	.361***
	(.025)	*(.015)*	*(.034)*	*(.032)*	*(.036)*	*(.023)*	*(.028)*	*(.016)*	*(.058)*	*(.030)*	*(.035)*
(3) Contribution skills to wage premium	.313	.240	.154	.377	.135	.174	.206	.371	.134	.320	.324
Observations	1200	1519	817	1507	1193	1306	1191	1316	674	1782	1121

Notes: Each cell reports the coefficient from a separate regression. Regressions weighted by sampling weights. Sample: full-time employees aged 35–54 years. Dependent variable in specifications (1) and (2) is gross hourly wages (log). Specification (2) controls for numeracy and literacy skills. Scalars in (3) are calculated as ((1)-(2))/(1). All regressions control for a quadratic polynomial in actual work experience and gender. Observations refer to baseline specification. Numeracy and Literacy skills standardized to mean 0 and SD 1 within country. Pooled specification includes country fixed effects and gives same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A4.5 : University Wage Premia & the Contribution of Numeracy Skills (Instrumented by Literacy Skills)

	Pooled	Australia	Austria	Belgium	Canada	Chile	Cyprus	Czech R.	Denmark	Estonia	Finland
(1) University wage premium without skill control	.440***	.381***	.370***	.309***	.359***	.858***	.421***	.339***	.235***	.328***	.288***
	(.005)	*(.026)*	*(.025)*	*(.018)*	*(.016)*	*(.058)*	*(.033)*	*(.052)*	*(.014)*	*(.024)*	*(.016)*
(2) University wage premium with skill control	.321***	.280***	.236***	.213***	.232***	.613***	.372***	.223***	.157***	.245***	.208***
	(.006)	*(.026)*	*(.026)*	*(.020)*	*(.017)*	*(.064)*	*(.034)*	*(.064)*	*(.015)*	*(.026)*	*(.020)*
(3) Contribution skills to wage premium	.270	.263	.363	.311	.354	.286	.116	.341	.334	.254	.279
Observations	45037	1433	1115	1220	7189	903	938	1065	1879	1763	1480
	France	**Germany**	**Greece**	**Indonesia**	**Ireland**	**Israel**	**Italy**	**Japan**	**Korea**	**Lithuania**	**Netherl.**
(1) University wage premium without skill control	.388***	.446***	.402***	.869***	.439***	.461***	.514***	.341***	.509***	.538***	.400***
	(.017)	*(.025)*	*(.041)*	*(.079)*	*(.036)*	*(.033)*	*(.039)*	*(.025)*	*(.027)*	*(.036)*	*(.022)*
(2) University wage premium with skill control	.285***	.272***	.368***	.715***	.284***	.264***	.428***	.263***	.397***	.448***	.292***
	(.020)	*(.027)*	*(.046)*	*(.082)*	*(.037)*	*(.039)*	*(.039)*	*(.028)*	*(.030)*	*(.041)*	*(.024)*
(3) Contribution skills to wage premium	.266	.390	.085	.177	.353	.427	.168	.229	.219	.167	.271
Observations	1714	1296	623	803	1033	908	1019	1319	1441	1258	1012
	New Zealand	**Norway**	**Poland**	**Singapore**	**Slovak R.**	**Slovenia**	**Spain**	**Sweden**	**Turkey**	**U.K.**	**U.S.**
(1) University wage premium without skill control	.348***	.263***	.603***	.877***	.513***	.538***	.496***	.202***	.742***	.401***	.534***
	(.024)	*(.014)*	*(.032)*	*(.029)*	*(.036)*	*(.022)*	*(.025)*	*(.015)*	*(.055)*	*(.029)*	*(.032)*
(2) University wage premium with skill control	.235***	.195***	.507***	.567***	.438***	.441***	.390***	.119***	.611***	.265***	.333***
	(.025)	*(.015)*	*(.034)*	*(.034)*	*(.038)*	*(.024)*	*(.028)*	*(.016)*	*(.063)*	*(.031)*	*(.036)*
(3) Contribution skills to wage premium	.324	.258	.158	.354	.145	.180	.213	.413	.176	.340	.376
Observations	1200	1519	817	1507	1193	1306	1191	1316	674	1782	1121

Notes: Each cell reports the coefficient from a separate regression. Regressions weighted by sampling weights. Sample: full-time employees aged 35–54 years. Dependent variable in specifications (1) and (2) is gross hourly wages (log). Specification (2) controls for numeracy skills, which is instrumented by literacy skills. Scalars in (3) are calculated as ((1)-(2))/(1). All regressions control for a quadratic polynomial in actual work experience and gender. Observations refer to baseline specification. Numeracy skills are standardized to mean 0 and SD 1 within country and instrumented by literacy skills. Pooled specification includes country fixed effects and gives same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A4.6 : Difference-in-Differences Estimate of University Skill Premium – Varying Old Age Group

	20–24 vs. 25	20–24 vs. 25–26	20–24 vs. 25–27	20–24 vs. 25–28	20–24 vs. 25–29
Panel A: Numeracy Skills					
Old	.008	.010	.021	.037*	.034*
	(.032)	*(.025)*	*(.021)*	*(.020)*	*(.018)*
University	.681***	.684***	.685***	.684***	.693***
	(.019)	*(.019)*	*(.019)*	*(.019)*	*(.017)*
Old X University	.043	.070**	.067**	.074***	.078***
	(.044)	*(.034)*	*(.030)*	*(.027)*	*(.024)*
Observations	16779	19919	23236	26076	36159
Panel B: Literacy Skills					
Old	.004	−.009	.001	.007	−.007
	(.033)	*(.025)*	*(.022)*	*(.020)*	*(.018)*
University	.679***	.680***	.679***	.677***	.683***
	(.019)	*(.019)*	*(.019)*	*(.019)*	*(.018)*
Old X University	.040	.077**	.068**	.083***	.099***
	(.045)	*(.034)*	*(.030)*	*(.027)*	*(.024)*
Observations	16779	19919	23236	26076	36159

Notes: Each column reports the coefficients from a separate regression, using equation (4). "Old" is a dummy which takes the value 1 if the individual is aged between 25–29, and zero otherwise; "University" is a dummy for university attendance (either actual enrollment or graduation). Numeracy (Panel A) and literacy skills (Panel B) are regressed on the age and university attendance dummies and their interaction, including a gender control and country fixed effects. Sample: individuals aged as indicated in column header. Regressions weighted by sampling weights. Numeracy and literacy skills, standardized to mean 0 and SD 1 within country. Observations weighted such that each each country receives same weight. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A4.7 : Difference-in-Differences Estimate of University Skill Premium – by Country

	Pooled	Australia	Austria	Belgium	Canada	Chile	Cyprus	Czech R.	Denmark	Estonia	Finland
Numeracy skills	.078***	.069	.021	−.018	−.036	.148	.123	.127	−.031	−.054	.146
	(.024)	*(.129)*	*(.121)*	*(.118)*	*(.096)*	*(.134)*	*(.236)*	*(.189)*	*(.126)*	*(.098)*	*(.123)*
Literacy skills	.099***	.035	.049	.202*	−.025	.099	.284	−.008	.043	.128	.219*
	(.024)	*(.124)*	*(.123)*	*(.116)*	*(.088)*	*(.141)*	*(.221)*	*(.223)*	*(.125)*	*(.097)*	*(.118)*
Observations	36159	1133	856	721	4333	1110	727	1207	963	1376	915
	France	Germany	Greece	Indonesia	Ireland	Israel	Italy	Japan	Korea	Lithuania	Netherl.
Numeracy skills	.215**	−.011	.031	.297*	.328**	.382***	−.294	−.040	.150	−.034	.096
	(.098)	*(.115)*	*(.198)*	*(.175)*	*(.133)*	*(.111)*	*(.206)*	*(.141)*	*(.143)*	*(.162)*	*(.129)*
Literacy skills	.140	−.076	−.131	.253*	.321**	.392***	−.218	.051	.012	−.001	.258*
	(.101)	*(.117)*	*(.215)*	*(.150)*	*(.135)*	*(.105)*	*(.212)*	*(.135)*	*(.143)*	*(.168)*	*(.132)*
Observations	970	1051	618	1116	910	1159	467	758	979	773	796
	New Zealand	Norway	Poland	Singapore	Slovak R.	Slovenia	Spain	Sweden	Turkey	U.K.	U.S.
Numeracy skills	−.108	−.027	−.031	.362***	.101	.114	.029	.090	.265*	.014	.292**
	(.126)	*(.135)*	*(.080)*	*(.115)*	*(.134)*	*(.171)*	*(.109)*	*(.119)*	*(.143)*	*(.147)*	*(.131)*
Literacy skills	−.192	−.021	.129	.407***	.036	.048	.034	.005	.311**	−.065	.307**
	(.131)	*(.134)*	*(.081)*	*(.115)*	*(.136)*	*(.181)*	*(.109)*	*(.117)*	*(.151)*	*(.149)*	*(.136)*
Observations	1161	864	3916	1038	907	493	862	795	783	1443	959

Notes: Each cell reports the coefficient from a separate regression. University skill premium is the estimated coefficient of the interaction term between a dummy indicating the age group (20–24 vs. 25–29) and a dummy for university attendance (either actual enrollment or graduation) in a regression of cognitive skills on the age and university attendance dummies separately and their interaction, including a gender control, for the sample of individuals aged 20-29 years in each country. Regressions weighted by sampling weights. Numeracy (row 1) and literacy skills (row 2), standardized to mean 0 and SD 1 within country. Pooled specification includes country fixed effects and gives same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC 2012/2015.

Table A4.8 : Within-Germany Analysis – Summary Statistics

	Mean	SD	Min	Max
3,976 observations				
University Attendance	.32	.47	0	1
Numeracy Skills (PIAAC 2015)	282.51	51.14	72.28	444.64
Literacy Skills (PIAAC 2015)	281.82	45.14	100.07	425.96
Distance	19.14	19.15		
Female	.51	.50	0	1
Migrant	.20	.40	0	1
High School graduation year	1988	14.46	1951	2015
Age	41.56	13.84	16	79
Parental education	.30	.46	0	1
Work status - mother	.53	.50	0	1
Work status - father	.85	.36	0	1
Position in birth order	2.02	1.11	1	5

Notes: University attendance is a dummy which equals 1 if the individual has ever attended or is currently enrolled in university. Numeracy and literacy skills are measured in 2015 on a 500-points scale. Distance to the nearest university campus is measured in kilometers. Due to the confidentiality of the data, extreme values cannot be displayed. Migrant is a dummy which equals 1 if the individual is a first- or second-generation migrant. Parental education is a dummy which equals 1 if at least one parent holds a university degree. The work status of mother and father is a dummy which equals 1 if the parent was employed when the individual was 15 years old. Position in birth order refers to the number of older siblings. *Data source:* PIAAC-L 2014, 2015, 2016.

Table A4.9 : IV Estimate of University Skill Premium – Literacy Skills

	Panel A: University Attendance			
First-Stage				
Distance	-.016***	-.015***	-.014***	-.014***
	(.004)	*(.004)*	*(.004)*	*(.004)*
1st-stage F stat.	17.49	14.15	12.72	12.81
	Panel B: Literacy Skills			
OLS				
University Attendance	.823***	.814***	.805***	.662***
	(.029)	*(.029)*	*(.029)*	*(.029)*
Second-Stage				
University Attendance	1.846**	1.969**	2.045**	1.959**
	(.533)	*(.573)*	*(.616)*	*(.612)*
Federal state FE	✓			
Region FE		✓	✓	✓
Family background			✓	✓
High-school grades				✓
Observations	3976	3976	3976	3976

Notes: Each cell reports the coefficients from a separate regression. Sample: full sample of German PIAAC-L participants with available distance information. Panel A shows first-stage estimation results (dependent variable: university attendance). Panel B shows OLS and 2SLS estimation results (dependent variable: literacy skills). All specifications control for gender, migrant status, parental education, and include birth year fixed effects. Region fixed effects refer to 40 administrative districts "Regierungsbezirke" in Germany. Family background characteristics include mother's and father's working status when the individual was 15 years old and position in birth order. High-school grades are final exam grades in high school in German, Mathematics, and first foreign language. Robust standard errors, clustered at the municipality level (1163 clusters), in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC-L 2014, 2015, 2016.

Table A4.10 : IV Estimate of University Skill Premium – Control for Degree of Urbanization

		Panel A: University Attendance	
First-Stage			
Distance	-.013***	-.013***	-.012***
	(.004)	*(.004)*	*(.004)*
1st stage F stat.	9.01	8.49	8.07
		Panel B: Numeracy Skills	
OLS			
University Attendance	.740***	.731***	.589***
	(.030)	*(.030)*	*(.030)*
Second-Stage			
University Attendance	.842	.815	.736
	(.699)	*(.731)*	*(.748)*
Federal state FE	✓	✓	✓
Urban-Dummy	✓	✓	✓
Family background		✓	✓
High-school grades			✓
Observations	3976	3976	3976

Notes: Each cell reports the coefficients from a separate regression. Sample: full sample of German PIAAC-L participants with available distance information. Panel A shows first-stage estimation results (dependent variable: university attendance). Panel B shows OLS and 2SLS estimation results (dependent variable: numeracy skills). All specifications control for gender, migrant status, parental education, include birth year fixed effects and a dummy indicating whether the municipality of school attendance was in an urban or rural region. Family background characteristics include mother's and father's working status when the individual was 15 years old and position in birth order. High-school grades are final exam grades in high school in German, Mathematics, and first foreign language. Robust standard errors, clustered at the municipality level (1163 clusters), in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC-L 2014, 2015, 2016.

Table A4.11 : Within-Germany Analysis – Campus Proximity and University Attendance

	(1)	(2)	(3)	(4)
Distance	-.016***	-.015***	-.014***	-.014***
	(.004)	*(.004)*	*(.004)*	*(.004)*
Female	-.055***	-.054***	-.053***	-.082***
	(.014)	*(.014)*	*(.014)*	*(.013)*
Migrant	.001	-.001	-.001	-.007
	(.018)	*(.018)*	*(.018)*	*(.018)*
Parental education	.303***	.302***	.299***	.255***
	(.018)	*(.018)*	*(.018)*	*(.017)*
Federal state FE	✓			
Region FE		✓	✓	✓
Family background			✓	✓
High-school grades				✓
Observations	3976	3976	3976	3976

Notes: Each column reports the coefficients from a separate regression. Sample: full sample of German PIAAC-L participants with available distance information. Dependent variable: university attendance. Distance to the nearest university campus (in kilometers) is divided by 10 throughout. All specifications control for gender, migrant status, parental education, and include birth year fixed effects. Region fixed effects refer to 40 administrative districts "Regierungsbezirke" in Germany. Family background characteristics include mother's and father's working status when the individual was 15 years old and position in birth order. Grades are final exam grades in high school in German, Mathematics and first foreign language. Robust standard errors, clustered at the municipality level (1163 clusters), in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC-L 2014, 2015, 2016.

Table A4.12 : IV Estimate of University Skill Premium – Alternative Proximity Measures

Panel A: University Attendance Instrumented by Travel Distance				
First-Stage				
Travel Distance	-.013***	-.012***	-.012***	-.012***
	(.003)	*(.003)*	*(.003)*	*(.003)*
Second-Stage				
University Attendance	1.096**	1.236**	1.257**	1.198**
	(.496)	*(.525)*	*(.557)*	*(.542)*
1st-stage F stat.	18.24	15.00	13.51	13.98
Panel B: University Attendance Instrumented by Travel Time				
First-Stage				
Travel Time	-.104***	-.099***	-.094***	-.092***
	(.023)	*(.024)*	*(.024)*	*(.023)*
Second-Stage				
University Attendance	1.013**	1.145**	1.146**	1.080**
	(.463)	*(.483)*	*(.507)*	*(.494)*
1st-stage F stat.	20.73	17.57	16.00	16.17
Federal state FE	✓			
Region FE		✓	✓	✓
Family background			✓	✓
High-school grades				✓
Observations	3976	3976	3976	3976

Notes: Each cell reports the coefficients from a separate regression. Sample: full sample of German PIAAC-L participants with available distance information. Panel A shows first-stage and 2SLS estimation results when university attendance is instrumented by travel distance. Panel B shows first-stage and 2SLS estimation results when university attendance is instrumented by travel time. Dependent variable in first-stage regressions: university attendance; in 2SLS regressions: numeracy skills. All specifications control for gender, migrant status, parental education, and include birth year fixed effects. Region fixed effects refer to 40 administrative districts "Regierungsbezirke" in Germany. Family background characteristics include mother's and father's working status when the individual was 15 years old and position in birth order. High-school grades are final exam grades in high school in German, Mathematics, and first foreign language. Robust standard errors, clustered at the municipality level (1163 clusters), in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC-L 2014, 2015, 2016.

Table A4.13 : IV Estimate of University Skill Premium – PIAAC Anchor Persons only

	Panel A: University Attendance			
First-Stage				
Distance	-.018***	-.017***	-.016***	-.015***
	(.004)	*(.004)*	*(.005)*	*(.004)*
1st stage F stat.	16.25	14.28	13.08	12.03
	Panel B: Numeracy Skills			
OLS				
University Attendance	.705***	.701***	.695***	.559***
	(.033)	*(.034)*	*(.034)*	*(.033)*
Second-Stage				
University Attendance	.838	1.015*	1.001*	.720
	(.542)	*(.563)*	*(.580)*	*(.592)*
Federal state FE	✓			
Region FE		✓	✓	✓
Family background			✓	✓
High-school grades				✓
Observations	2982	2982	2982	2982

Notes: Each cell reports the coefficients from a separate regression. Sample: sample of German PIAAC anchor participants with available distance information. Panel A shows first-stage estimation results (dependent variable: university attendance). Panel B shows OLS and 2SLS estimation results (dependent variable: numeracy skills). All specifications control for gender, migrant status, parental education, and include birth year fixed effects. Region fixed effects refer to 40 administrative districts "Regierungsbezirke" in Germany. Family background characteristics include mother's and father's working status when the individual was 15 years old and position in birth order. High-school grades are final exam grades in high school in German, Mathematics, and first foreign language. Robust standard errors, clustered at the municipality level (1163 clusters), in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC-L 2014, 2015, 2016.

5 Vocational vs. General Education and Employment over the Life-Cycle: New Evidence from PIAAC[1]

5.1 Introduction

Around the world, there is an increasing interest in expanded vocational education as a way to get youth quickly and effectively into the labor market by endowing them with occupation-specific skills. Earlier analysis of labor markets in the 1990s, however, suggested possible adverse impacts of vocational education on employment opportunities later in life due to limited adaptability to technological and structural change (Hanushek et al., 2017*a*). With the significant transformation of labor markets over the past two decades including such factors as globalization, technological change, altered training programs, and reforms of social security systems, it is important to revisit the potential efficacy of expanding vocational education in today's economic environment. This article provides new evidence whether the employment trade-off of vocational orientation over the life-cycle is still relevant today.

The ramifications of the deep changes that have occurred on labor markets for the employment effects of vocational education over the life-cycle are not obvious. On the one hand, the structural changes brought about by globalization and the rapid technological changes stemming from automation and digitalization (Autor, Dorn and Hanson, 2015) may make the obsolescence of occupation-specific skills over the life-cycle even more pronounced (cf. Krueger and Kumar 2004). In these changing environments, long-run employment prospects may be enhanced by general skills such as basic cognitive skills, social interaction skills, and skills that facilitate continuous learning such as transversal skills, adaptability, creativity, problem-solving, and critical thinking skills. On the other hand, reduced options of generous early retirement schemes in the social security systems of many countries may dampen the incidence of reduced employment at older ages, thereby reducing the scope for differential employment patterns between vocational and general education late in the life-cycle.[2]

This article uses the Programme for the International Assessment of Adult Competencies (PIAAC), conducted in 2011–2012, to estimate the employment effects of vocational vs. general education over the life-cycle on modern labor markets in a sample of 16 countries. To address

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[2] For example, in Germany the entitlement age for early retirement after 12 months of unemployment has been gradually raised from 60 to 63 years since 2006, and the terms of early retirement have become less generous. As a consequence, the share of those retiring before age 65 years (61 years) among all retirees has declined from 75% (56%) in 1995 to 57% (25%) in 2012 (Deutsche Rentenversicherung Bund, 2015).

concerns of selection into types of education, we employ the difference-in-differences model introduced by Hanushek et al. (2017*a*) that compares employment rates across age for people with general and vocational education. We make use of the individual skill measures available in PIAAC, among others, to account for potential differential changes in selectivity over time.

Our results confirm a strong trade-off between early advantages and late disadvantages in employment for individuals with vocational education. But there is strong heterogeneity depending on the specific institutional structure of schooling and work-based training in a country. While no significant pattern is detected in the six countries without sizeable vocational systems, the declining relative age–employment pattern of individuals with vocational education is found across the 10 countries with significant vocational systems, and it is strongest in countries with widely developed apprenticeship systems where industry is directly involved in education. In these apprenticeship countries, the cross-over age by which individuals with a general education have higher employment probabilities is as low as age 44 years, and somewhat higher around age 50 years for the group of vocational countries at large.

Our study contributes to a growing literature on the effects of vocational education on labor-market outcomes over the life-cycle. An extensive literature looks at the effect of vocational education on the school-to-work transition, with varying results (see Shavit and Müller 1998, Ryan 2001, and Zimmermann et al. 2013 for studies with an international focus and Malamud and Pop-Eleches 2010 for a study identified from a Romanian reform). Using the International Adult Literacy Survey (IALS) of the mid-1990s, Hanushek et al. (2017*a*) extended this perspective beyond the entry phase of the labor market, showing that the relative labor-market advantage of vocational over general education decreases with age. Several recent country-specific studies that go beyond the entry phase similarly show consistent age patterns by education type, including Cörvers et al. (2011) for Germany, the Netherlands, and Great Britain; Weber (2014) for Switzerland; and Brunello and Rocco (2017) for Great Britain. While Stenberg and Westerlund (2015) and Golsteyn and Stenberg (2017) also find such a pattern for Sweden, Hall (2016) is an exception that does not find a significant pattern based on the pilot of a Swedish reform in 1988–1993 that extended upper-secondary vocational programs by 1 year and increased their general content.

Our results extend the life-cycle analysis to a large sample of countries with recent data. While some have argued that pension reforms that limit early retirement may have dampened any relative employment effect at older ages, others have suggested that increasing globalization, automation, and digitization may have made adaptability to changing occupational structures ever more important. In fact, our results show a continuing trade-off for vocational education between ease of labor-market entry and limited adaptability at later ages that is very similar in size to the results in Hanushek et al. (2017*a*) for the mid-1990s. Apart from the updated period, the PIAAC data also provide a much richer testing of skills and a sample size that is almost twice as large as in IALS. Because we see our main contribution in showing that international results which refer to two decades ago also hold on today's labor markets, we keep the article intentionally short with a focus on the core results of employment over the life-cycle.[3]

Our analysis also extends the emerging literature that uses the PIAAC data to study different aspects of education and the labor market. Thus, Levels, van der Velden and Allen (2014) provide an analysis on mismatch; Hanushek et al. (2015) on returns to skills; Brunello and Rocco (2015) and Forster, Bol and van de Werfhorst (2016) on aspects of vocational education; Broecke, Quintini and Vandeweyer (2016) on inequality; Falck, Heimisch and Wiederhold (2016) on returns to information and communication technology (ICT) skills; and Kahn (2016) on employment protection.

In what follows, Section 5.2 introduces the PIAAC database. Section 5.3 describes the difference-in-differences model. Section 5.4 presents our main results on the employment effects of education type over the life-cycle and reports several robustness analyses indicating that results are not driven by varying selectivity into education types over time. Section 5.5 tests for heterogeneity across groups of countries with differing vocational systems. Section 5.6 concludes.

5.2 The PIAAC Data

Collected between August 2011 and March 2012, PIAAC was developed by the Organisation for Economic Co-operation and Development (OECD) to survey the skills of a representative sample of adults aged 16–65 years in each participating country. For our purposes, PIAAC provides internationally comparable data on individuals' type of education, labor-market status, and background variables in 16 countries.[4]

We classify the 16 countries into different categories according to the extent and intensity of vocationalization of their education systems using information from PIAAC and OECD's Education at a Glance (EAG) statistics.[5] We define *vocational countries* as those countries whose vocational share is at least 40% in PIAAC and at least 50% in EAG. Based on these criteria, 6 countries (Ireland, Japan, Korea, Spain, the United Kingdom, and the USA) are classified as *non-vocational countries* with limited vocational systems, whereas 10 countries are *vocational countries* with significant vocational systems. Among the latter, three countries (Austria, Denmark, and Germany) are *apprenticeship countries* with a share of combined school and work-based vocational programs that exceeds 40% in EAG. Together with these

3 Hanushek et al. (2017*a*) provide additional analyses of income and adult education over the life-cycle, lifetime earnings, within-occupational-group analysis using the German Microcensus, and analysis of exogenous variation from plant closures in Austrian administrative data.

4 Among the remaining eight PIAAC countries, the Russian data have issues of representativeness; Canada and Estonia do not provide data on educational attainment in the Public Use File; and Belgium, Cyprus, Italy, Poland, and the Slovak Republic do not provide consistent data on the type of education.

5 The categorization follows the one applied in Hanushek et al. (2017*a*), updated with the more recent statistics of PIAAC and EAG 2008.

three countries, the Czech Republic is also classified among the *non-school based vocational countries* that have a vocational sector with at least 25% in combined school and work-based programs. The remaining six vocational countries (Australia, Finland, France, the Netherlands, Norway, and Sweden) have mostly school-based vocational sectors.

Our sample includes all males aged 16–65 years who completed at least secondary education and are not currently in education.[6] The type of education is derived from responses to an internationally harmonized background questionnaire. For individuals with secondary education, the PIAAC data provide a variable indicating whether a respondent's highest level of education is vocationally oriented. For individuals with tertiary education, we follow Hanushek et al. (2017*a*) and Brunello and Rocco (2015) in classifying the largely theory-based tertiary-type A programs (ISCED 5A) that are designed to provide sufficient qualifications for entry to advanced research programs and professions with high skill requirements as general. The more practical, technical, and occupational specific tertiary-type B programs (ISCED 5B) that lead to professional qualifications are classified as vocational.[7]

Apart from the education type, PIAAC provides detailed tests of individuals' cognitive skills in numeracy, literacy, and 'problem solving in technology-rich environments'. These skill measures have been shown to have substantial returns on the labor market (Hanushek et al., 2015) and allow us to account for differential selectivity into education type by age. Test scores are normalized to have mean zero and standard deviation one within each country. Apart from the richer testing of skills, PIAAC also provides substantially larger sample sizes per country than the IALS data set of the mid-1990s, so that our full sample of 29,452 individuals is almost twice as large as in the IALS study by Hanushek et al. (2017*a*).

Table 5.1 provides descriptive statistics of the main variables of our analysis for the sample of 10 countries with significant vocational systems. On average, 64% of individuals have completed a vocational education program in this country sample. Country-specific inspection suggests that the shares of individuals who completed a vocational program is rather stable over age cohorts in most of these countries, with the exceptions of Denmark and Finland (and, to a lesser extent, France) indicating a decline in vocational attendance over time. Employment rates are 84% for individuals with a general degree and 77% for those with a vocational degree. Literacy and numeracy scores are also higher for individuals with a general education.

[6] The restriction to males with their historically stable aggregate labor-force participation patterns during prime age circumvent concerns raised about our identification by cohort-specific selection into work by females.

[7] While tertiary vocational programs are likely more heterogeneous in the mix of general skills obtained, our results are robust to restricting the analysis to the subsample of individuals completing just secondary education for whom PIAAC explicitly provides a classification of education type (not shown).

5.3 Empirical Model

We focus on the impact of vocational vs. general education types on employment over the life-cycle, with our main hypothesis being that any relative labor-market advantage of vocational over general education decreases with age. As developed in Hanushek et al. (2017*a*), our baseline model is a simple difference-in-differences approach that compares the age–employment patterns of workers of the two education types within each country:

$$E_i = \alpha_0 + \alpha_1 A_i + \alpha_2 A_i^2 + \beta_1 G_i + \beta_2 G_i \cdot A_i + X_i \gamma + \mu_c + \epsilon_i \tag{5.1}$$

where E_i is an indicator capturing whether individual i is employed (in paid work during the past week); age A and its square capture the normal age–employment pattern in the economy; G_i is an indicator for general (as opposed to vocational) education type; X is a vector of control variables including years of schooling and skills; and μ_c are country fixed effects.

Our main coefficient of interest is β_2, which captures the differential impact of general relative to vocational education on employment with each year of age. In addition, β_1 measures the initial employment probability of general relative to vocational education (normalized to age 16 years in the empirical application). While we doubt that β_1 adequately captures the impact of general education because it implicitly includes any selectivity into education types not captured by X, the identifying assumption for β_2 is the standard assumption of the difference-in-differences approach that the selectivity of people into general vs. vocational education (conditional on X) does not vary over time. Put differently, to interpret our cross-sectional analysis as a pattern over the life-cycle, we assume that conditional on the available observables, today's older people in each education type are a good proxy for today's younger people when they grow older.[8]

In our analysis below, we provide several tests of this assumption. First, to account for possible time-varying selection of individuals with differing ability into education types, we condition on the literacy and numeracy scores observed in PIAAC and, importantly, their interactions with age. Second, we control for two additional characteristics that may depict selection into education type and their interactions with age, namely, parental education and the number of books at home when a person was 15 years old. Third, given the cross-country nature of our main analysis, we can also condition on the share of each 10-year age cohort in a country that completed an education type, thereby holding overall changes in the size of each education type constant. Fourth, we use propensity score matching to identify a sample of individuals with vocational education that is observationally comparable to that for general education, thereby disregarding any individuals who do not have common support in the

[8] Reassuringly, Brunello and Rocco (2017) and Golsteyn and Stenberg (2017) confirm a trade-off of labor-market outcomes by education type over the life-cycle with longitudinal data in Britain and Sweden, indicating that age differences reflect actual age effects rather than cohort effects that are specific to education types.

other education type. Together, these analyses provide strong support for an interpretation of the cross-sectional analysis as a life-cycle result.[9]

In addition, we can perform a straightforward direct test of whether selectivity into education types changed over time in our setting: we can estimate whether the effect of observed predictors of choice of education type varies with individuals' age. As is evident from Table 5.2, both individual test scores and socioeconomic status at the time of making educational choices — proxied by the number of books at home when an individual was 15 years old — are strong predictors of education type. In particular, individuals with higher literacy scores and more books at home are more likely to select into general education programs. Numeracy score also enters significantly in the absence of literacy scores, whereas only literacy retains significance in a model that considers both of them jointly. Mothers' education is marginally significantly positive in a model without books at home, but loses significance with books at home. More importantly, the interaction terms of all these variables with individuals' age are statistically insignificant. That is, we can observe a number of significant predictors of choice of education type, but the effect of none of them varies with age in the very setting of our analysis. While this does not preclude the possibility that unobserved characteristics of individuals with different education types may have changed differently over time, this result provides plausibility to our identifying assumption that conditional time-varying selectivity into education types does not drive our results.

5.4 Employment Effects of Education Type over the Life-Cycle

Our results in Table 5.3 indicate that there is indeed a strong trade-off of employment patterns by education type over the life-cycle. Initially, individuals completing vocational education programs have higher employment probabilities. But with increasing age, this advantage declines and ultimately turns around into an employment advantage of individuals completing general education programs (see also the descriptive pattern in Figure 5.1).[10] Using the sample of 10 countries with significant vocational systems, the first column of Table 5.3 shows the simplest model that conditions only on country fixed effects, a quadratic in age, and years of schooling. At age 16 years, the employment probability of persons with a vocational education is 10.0 percentage points higher. But with every 10 years of age, this declines significantly by 3.2

[9] These analyses also address potential effects of changes in the extent to which the curricula of vocational programs contain general material. For example, reforms of vocational programs such as the Dutch reform studied by Oosterbeek and Webbink (2007) and the Swedish reform studied by Hall (2016) may have contained such curricular implications. To ensure that our findings are not driven by these reforms, we confirm that results are robust to excluding the Netherlands and Sweden from our analysis (not shown).

[10] It is apparent from the figure that the gap between the two curves moves to the advantage of general education in a rather linear fashion, favoring the linear-in-age interaction specification of the empirical model. However, specifications with interaction terms that are nonlinear in age indicate that the differential pattern of employment between vocational and general education is particularly pronounced starting in the mid-50 age range (not shown). While Figure 5.1 is based on a matched sample of vocationally educated and generally education individuals, the same qualitative pattern emerges in a purely descriptive figure of the full sample.

percentage points, which is even larger than the 2.1 percentage points found in the equivalent specification of Hanushek et al. (2017*a*) for the mid-1990s. The interacted specification implies that starting with age 48 years, persons with a general education have a higher employment probability.

As discussed above, the main concern with identification from the age gradient in relative employment in this difference-in-differences approach is that within countries, selectivity into the two education types may have changed over time. As a first check on this possibility, Column 2 adds the PIAAC literacy score and its interaction with age. On the one hand, this inclusion captures any change in selectivity of individuals with initially different basic skill levels into different education types that is reflected in differences in observed adult skills. On the other hand, these skills could in part be endogenous to specific education types and to work histories, thereby taking out more of the identifying variation than it should. Specifically, if the education programs and employment experiences of generally educated individuals lead them to gain and maintain more literacy skills relative to vocationally educated individuals, conditioning on adult literacy skills will lead to an underestimation of β_2, our coefficient of interest in Equation 5.1. In any event, while the association of literacy with employment indeed increases with age, the main pattern of results remains unchanged, with a slightly reduced coefficient on the type of education–age interaction. Given that the inclusion of controls for adult skills is likely to lead to conservative estimates in our setting, we keep including them throughout.[11]

While the inclusion of literacy scores follows the analysis with the IALS test in Hanushek et al. (2017*a*), PIAAC in fact provides considerably richer testing of skills which allows us to estimate our main equation conditional on the different domains of cognitive skills tested in PIAAC. When we add the PIAAC numeracy score in addition to the literacy score (Column 3), literacy in fact loses significance, which is fully captured by numeracy. However, our qualitative results do not change.[12]

As another control for potential differential selectivity into education over time, Column 4 adds controls for the education level of respondents' mothers and its interaction with age. These turn out insignificant and hardly change our substantive results.[13] The same is true in a model without literacy scores (considering the potential endogeneity of adult skills) or when adding the number of books at home at age 15 years as another background control, which enter the model significantly with or without skill controls (not shown).

[11] While the basic literacy and numeracy skills captured by the PIAAC tests may be part of the set of general skills of which general education programs provide more than vocational programs, they do not capture many other aspects of general skills such as other cognitive skills, social-interaction skills, and learning-to-learn skills.
[12] Despite the high correlation between literacy and numeracy (0.85), our results are effectively unchanged when including only numeracy or when using the average of literacy and numeracy. Interestingly, the new PIAAC domain of 'problem solving in technology-rich environments' (not available in France and Spain) does not enter our employment equation significantly (individually or jointly with the other domains) and does not affect our results.
[13] The same holds for father's education and parents' highest education, which are missing more observations.

To account for potential effects of changes in the aggregate composition of the labor force by type of education over time, Column 5 adds the percentage of each 10-year age cohort completing general education in each country; results are hardly affected.[14] In this main specification, for each 10 years of age, the relative employment chances of those with a general education increase by 2.2 percentage points relative to those with a vocational education, which is effectively the same as found in the base specification of Hanushek et al. (2017*a*) for the mid-1990s.

As another approach to address possible selection issues, Column 6 shows results of a model using propensity score matching to compare individuals with a vocational education only to observationally similar individuals with a general education. We use nearest-neighbor matching which, for each country, matches each individual with vocational education to one individual with general education based on age, years of schooling, literacy and numeracy scores, and mother's education, so that the estimate is only identified from common support between the two groups within each country. While this reduces the number of observations by 35%, our main result in fact becomes stronger, indicating that it is not driven by observations off the common support.

A final concern is selectivity at young ages because some young people are still in the education system, particularly in general programs. Thus, Columns 7 and 8 restrict the sample to persons at least 20 and 30 years of age, respectively. In fact, the age pattern of employment by education type gets stronger in these reduced samples (in contrast to Hanushek et al. 2017*a*).

5.5 Heterogeneity across Countries

As indicated, countries differ widely in the treatment intensity of their aggregate institutional vocationalization. While the previous results were restricted to the 10 countries with significant vocational systems, the first column of Table 5.4 shows that the main results also hold in the full sample of 16 countries, albeit at reduced coefficient size. In fact, Column 2 shows that the pattern is not at all visible in the nonvocational countries, with effectively no employment differences across education types. This result may reflect the vagueness of the definition of general vs. vocational types of education programs in countries with limited vocational systems.

In contrast, results are substantially stronger in countries with non-school based vocational systems (Column 4) and, in particular, in countries with extensive apprenticeship systems (Column 5). The heterogeneous results across country groups may reflect an increasing treatment intensity of vocational specificity: The apprenticeship programs with their substantial industry-based education tend to provide the highest intensity of vocational experience (cf. Wolter and Ryan 2011). The cross-over age from which on employment is higher for general

[14] The age pattern of employment by education type is also robust to adding the average skill scores of individuals with the particular education type by country and 10-year age cohort, as in Hanushek et al. (2017*a*).

than for vocational education is as low as 44 years on average across the apprenticeship countries. In fact, despite the smaller sample sizes, the main pattern is significantly visible in all three apprenticeship countries (Columns 6—8), with the Austrian results providing confirmation in a country that had not participated in IALS. The overall pattern across country groups is consistent with the employment effects of education types increasing with the treatment intensity of occupation-specific education in the vocational system.[15]

5.6 Conclusions

Using recent data on labor markets in a large sample of countries, we aim to provide a deeper understanding of the merits and limitations of different education types for employment in an increasingly globalized era. We find strong evidence that a life-cycle perspective is important: while individuals who completed vocational education programs initially have better employment opportunities than individuals who completed general education programs, this pattern turns around at older ages. While estimates vary across specifications, the estimated cross-over age by which individuals with a general education have a higher employment probability than individuals with a vocational education is around 50 years, and somewhat earlier around 45 years in the apprenticeship countries. These estimates are broadly in line with the range of estimates found for the mid-1990s in Hanushek et al. (2017*a*), although they tend to indicate a slightly earlier cross-over age in the early 2010s. The findings are also consistent with the general pattern suggested by a number of recent country studies that show a similar age pattern of labor-market outcomes by education type over the life-cycle.

The estimated impact of education type on the age–employment profile is consistent with vocational education improving the transition from school to work but reducing adaptability of older workers to economic change. This pattern is particularly pronounced in countries with apprenticeship systems, whose emphasis on industry-based education may provide the strongest treatment intensity of vocationalization.

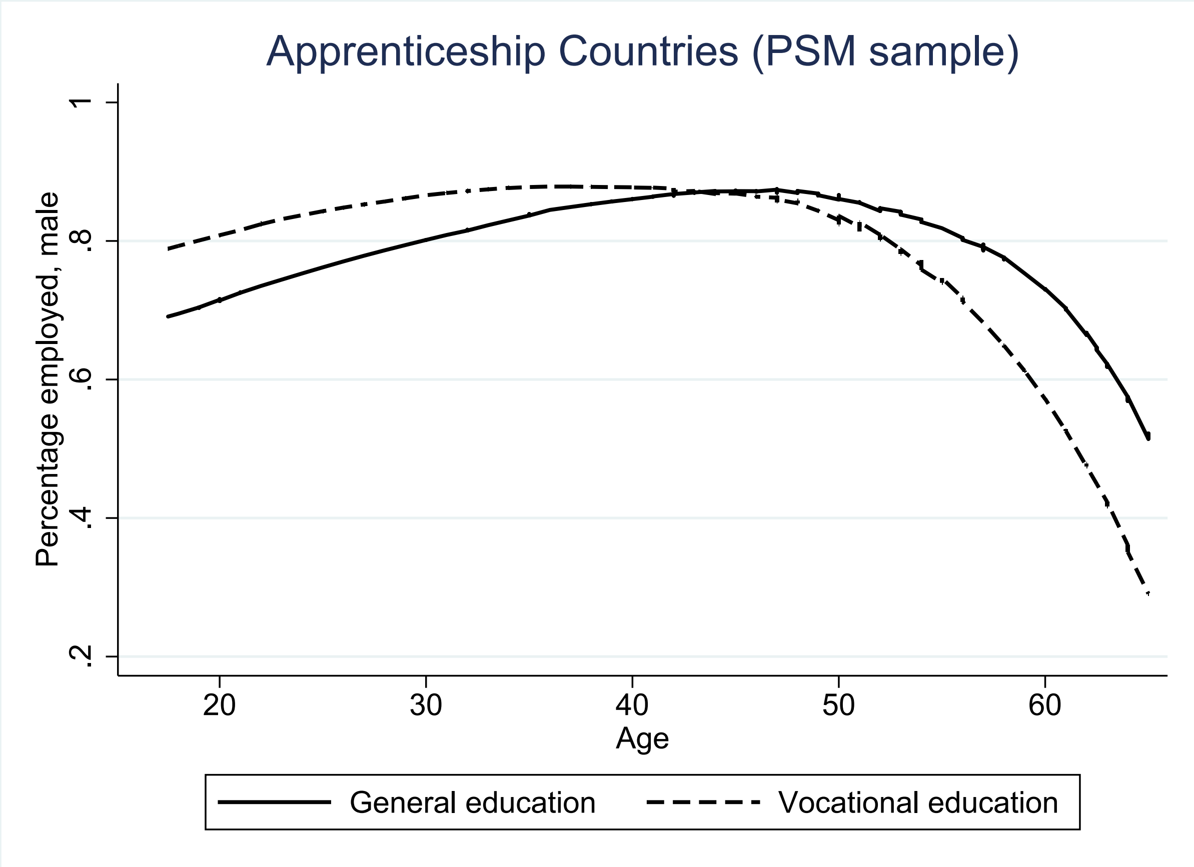
From an individual perspective, the results imply that people should be aware that there is a trade-off between early advantages and later disadvantages of vocational vs. general education programs over the employment life-cycle. The topics of facilitated entry vs. later adaptability indicate that there are both pros and cons of vocational education and of general education. The relative merits will depend on many factors, including the imminence of disruptions from technological or structural change in a specific sector or occupation in the country, the individual's inclination for adaptability and change in general, and the rate at which the individual discounts the future.

[15] These results suggest that the opposing interpretation in Forster, Bol and van de Werfhorst (2016) may stem from peculiarities in their standardized index of vocational systems, as well as their inclusion of countries with unclear identification of education types in PIAAC.

From a policy perspective, our results suggest caution about policies that concentrate just on the current employment situation and ignore the dynamics of growing economies. Current policy discussions often focus narrowly on issues of labor-market entry and youth unemployment. For a full assessment of how different education types affect the labor-market chances of workers, however, policy has to set the potential advantages of vocational programs in facilitating the transition from school to work against potential disadvantages when people have to adjust to changing conditions later in life. For countries with extensive vocational systems, the results may suggest that reducing the early specialization of students on specific occupational skills may be conducive to their long-run prospects on the labor market. In addition, the results indicate that it may be worth considering the establishment of a system for lifelong learning that does not only update workers' skills within their occupation but also conveys skills that facilitate their flexibility if changing labor-market conditions require occupational change.

Figures and Tables

Figure 5.1 : Employment by Age and Education Type in Apprenticeship Countries



Notes: Sample includes males who completed at least secondary education and are currently not students in the three "apprenticeship countries" (Austria, Denmark, and Germany), based on a matched sample that uses propensity score matching to ensure common support between persons with a vocational and a general education in each country (see text for details of the matching algorithm). Smoothed scatterplot using locally weighted regressions (Stata lowess). *Data source:* PIAAC.

Table 5.1 : Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)
	Full sample			Individuals with	
	Mean	Min	Max	Vocational education	General education
Employed	0.793	0	1	0.769	0.836
	(0.405)			*(0.421)*	*(0.371)*
General education	0.358	0	1	0	1
	(0.479)				
Age	44.36	17	65	44.64	43.86
	(12.62)			*(12.73)*	*(12.4)*
Years of schooling	13.97	9	22	12.95	15.81
	(2.309)			*(1.51)*	*(2.36)*
Literacy score	282.8	51.5	445.1	271.5	303.2
	(44.9)			*(42.3)*	*(42.2)*
Numeracy score	289.1	48.2	467	277.5	310
	(48.7)			*(45.9)*	*(46.6)*
Observations	18,938			12,164	6,774
Countries	10			10	10

Notes: Means, standard deviations (in parentheses), minimum, and maximum. Sample includes males aged 16 to 65 with at least secondary education in the 10 vocational countries. Data weighted by sampling weights, giving same weight to each country. *Data source*: PIAAC.

Table 5.2 : Correlates of General Education Type

	(1)	(2)	(3)
Literacy score	0.047***		0.054***
	(0.009)		*(0.017)*
Literacy score x Age	-0.001		-0.003
	(0.003)		*(0.005)*
Numeracy score		0.036***	-0.008
		(0.009)	*(0.017)*
Numeracy score x Age		-0.001	0.001
		(0.003)	*(0.005)*
Books at home at age 15	0.038***	0.041***	0.039***
	(0.01)	*(0.01)*	*(0.01)*
Books at home at age 15 x Age	-0.000	-0.000	-0.000
	(0.000)	*(0.000)*	*(0.000)*
Mother has high-school education	0.032	0.034	0.032
	(0.028)	*(0.028)*	*(0.028)*
Mother has high-school education x Age	-0.000	-0.000	-0.000
	(0.001)	*(0.001)*	*(0.001)*
Age	-0.008***	-0.008***	-0.008***
	(0.001)	*(0.002)*	*(0.002)*
Age^2	0.015***	0.014***	0.015***
	(0.002)	*(0.002)*	*(0.002)*
Years of schooling	0.120***	0.121***	0.121***
	(0.001)	*(0.002)*	*(0.002)*
Country fixed effects	✓	✓	✓
Observations	18,340	18,340	18,340
Countries	10	10	10
R^2 (adj.)	0.436	0.434	0.436

Notes: Linear probability model. Dependent variable: 1 = education type of individual is general; 0 = vocational. Sample includes males aged 16 to 65 with at least secondary education in the 10 vocational countries. Age variable subtracted by 16 and divided by 10. Regressions weighted by sampling weights, giving same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table 5.3 : Vocational vs. General Education and Employment over the Life-Cycle in PIAAC

	(1)	(2)	(3)	(4)	(5)	(6) Propensity-score matching	(7) 20+ age sample	(8) 30+ age sample
General education	-0.100***	-0.090***	0.085***	-0.082***	-0.084***	-0.090***	-0.093***	-0.135***
	(0.017)	*(0.018)*	*(0.018)*	*(0.019)*	*(0.018)*	*(0.027)*	*(0.018)*	*(0.026)*
General education x Age	0.032***	0.024***	0.022***	0.021***	0.022***	0.027***	0.025***	0.034***
	(0.006)	*(0.006)*	*(0.006)*	*(0.006)*	*(0.006)*	*(0.009)*	*(0.006)*	*(0.008)*
Age	0.270***	0.260***	0.257***	0.255***	0.260***	0.260***	0.252***	0.453***
	(0.013)	*(0.013)*	*(0.013)*	*(0.015)*	*(0.013)*	*(0.015)*	*(0.013)*	*(0.027)*
Age^2	-0.066***	-0.062***	-0.062***	-0.062***	-0.063***	-0.062***	-0.062***	-0.091***
	(0.002)	*(0.002)*	*(0.002)*	*(0.003)*	*(0.002)*	*(0.003)*	*(0.002)*	*(0.004)*
Years of schooling	0.021***	0.016***	0.015***	0.015***	0.015***	0.020***	0.015***	0.017***
	(0.002)	*(0.002)*	*(0.002)*	*(0.002)*	*(0.002)*	*(0.003)*	*(0.002)*	*(0.002)*
Literacy score		0.001	-0.002	-0.000	-0.003	0.028	-0.008	-0.017
		(0.009)	*(0.017)*	*(0.017)*	*(0.017)*	*(0.022)*	*(0.017)*	*(0.025)*
Literacy score x Age		0.014***	0.002	0.002	0.002	-0.007	0.004	0.007
		(0.003)	*(0.006)*	*(0.006)*	*(0.006)*	*(0.008)*	*(0.006)*	*(0.008)*
Numeracy score			0.006	0.003	0.007	-0.002	0.011	0.029
			(0.017)	*(0.017)*	*(0.017)*	*(0.021)*	*(0.017)*	*(0.025)*
Numeracy score x Age			0.014**	0.014**	0.014**	0.017**	0.012**	0.007
			(0.006)	*(0.006)*	*(0.006)*	*(0.008)*	*(0.006)*	*(0.008)*
Share of country cohort with general education					-0.133**	-0.125	-0.144**	0.178*
					(0.066)	*(0.080)*	*(0.066)*	*(0.093)*
Mother's education (2 indicators and their interaction with age)				✓				
Country fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	18,938	18,938	18,938	18,372	18,938	12,374	18,745	15,691
Countries	10	10	10	10	10	10	10	10
R^2 (adj.)	0.138	0.146	0.149	0.148	0.149	0.122	0.150	0.175

Notes: Linear probability model. Dependent variable: individual is employed. Sample includes males aged 16 to 65 with at least secondary education in the 10 vocational countries. Age variable subtracted by 16 and divided by 10. Regressions weighted by sampling weights, giving same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

Table 5.4 : Heterogeneity across Country Groups with Different Vocational Intensity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All countries	Non-vocational countries	Vocational countries	Non-school based vocational countries	Apprenticeship countries			
					All	Austria	Denmark	Germany
General education	-0.063***	-0.001	-0.084***	-0.123***	-0.134***	-0.083	-0.110**	-0.201***
	(0.014)	*(0.024)*	*(0.018)*	*(0.032)*	*(0.035)*	*(0.062)*	*(0.046)*	*(0.067)*
General education x Age	0.019***	-0.000	0.022***	0.041***	0.049***	0.064***	0.036**	0.043*
	(0.005)	*(0.009)*	*(0.006)*	*(0.011)*	*(0.012)*	*(0.022)*	*(0.015)*	*(0.022)*
Controls	✓	✓	✓	✓	✓	✓	✓	✓
Country fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
Observations	29,452	10,514	18,938	8,040	6,004	1,719	2,365	1,920
Countries	16	6	10	4	3	1	1	1

Notes: Linear probability model. All models include the same controls as column 5 of Table 3. Dependent variable: individual is employed. Sample includes males aged 16 to 65 with at least secondary education. See section 2 for country groups. Age variable subtracted by 16 and divided by 10. Regressions weighted by sampling weights, giving same weight to each country. Robust standard errors in parentheses. Significance levels: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. *Data source:* PIAAC.

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Professional Experience

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Intern, department Automotive, Section Business Development — March 2014 – August 2014

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Intern, department Global Automotive, Section Finance/Controlling — May 2011 – Oct. 2011

Teaching and research assistance

Experiments and Quasi-Experiments in Education Economics
Seminar supervisor, Bachelor seminar (taught in english) — March 2018 – June 2018

Bildungsökonomik (Economics of Education)
Tutor, Bachelor course (taught in German) — Oct. 2016 – Feb. 2017

School Systems and Student Achievement in International Perspective
Seminar supervisor, Bachelor seminar (taught in english) — March 2016 – July 2016

Conference presentations (selection)

2019 Jahrestagung des Vereins für Socialpolitik, Leipzig, Germany; North American Summer Meeting of the Econometric Society, Seattle, USA; Lisbon Research Workshop on the Economics & Statistics of Education, Lisbon, Portugal.

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