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The Effect of Compulsory Schooling on Skills: Evidence from a Reform in Germany

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The Effect of Compulsory Schooling on Skills: Evidence from a Reform in Germany*

Abstract

Based on high-quality skill data from PIAAC, this paper provides evidence on the effect of schooling on labor-market relevant cognitive skills. For identification, I exploit the staggered introduction of a compulsory ninth grade in basic track schools across German states, as well as a simultaneous reform that introduced short school years to harmonize the start of the school year nation-wide. Instrumental-variable results suggest that the additional year of compulsory schooling increased numeracy skills of basic-track students by about 0.2 standard deviations. Using superior skill data, the results contrast with previous evidence of zero skill effects of compulsory schooling in Germany.

JEL Code: I21, I24, C26

Keywords: Returns to education, compulsory schooling reform, skills, PIAAC

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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 resurvey *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.

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.¹

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 summarizes previous evidence on returns to compulsory schooling and skill effects of education. Section 3 provides background information on the German education system as well as on the two reforms affecting schooling duration. Section 4 introduces the data. Section 5 describes the empirical model used to identify the causal skill effect of schooling. Section 6 reports the results, including a variety of robustness checks. Section 7 concludes.

2 Literature Review

This section reviews the related literature. Section 2.1 summarizes studies examining the effect of compulsory schooling reforms. Section 2.2 outlines existing evidence on the skill effects of education.

2.1 Effects of Compulsory Schooling Reforms

2.1.1 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

¹I 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.

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.² 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).³

Harmon and Walker (1995) exploit exogenous 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).⁴

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, 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

²Hence, 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.

³Further 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).

⁴Exploiting 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.

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).

2.1.2 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.⁵ 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 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

⁵Several 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.

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.⁶ 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 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.⁷ 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).

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

⁶Kemptner, 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.

⁷According 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.

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.

3 Institutional Background

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.⁸ 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.⁹ 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 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.¹⁰ 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

⁸The 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.

⁹After grade 12, students can receive a field-specific university entry qualification (*fachgebundene Hochschulreife, Fachabitur*), which enables them to enter Universities of Applied Sciences.

¹⁰Track attendance is measured based on highest secondary school degree achieved.

Germany (*Kultusministerkonferenz*). Within this council, resolutions regarding changes in education policy have to be passed unanimously. Once a resolution is passed in the council, each federal state is responsible for its implementation (Hepp, 2011).

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 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.¹¹ 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.¹² 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-Westfalen, 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.¹³

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 disrup-

¹¹The year refers to the first year when basic track students graduated under the new compulsory schooling regime of 9 years (Table 1, Column 1).

¹²Due 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.

¹³Note 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 earliest birth cohort observed in Saarland is born in 1949. Hence, the inconsistencies in applied reform years are irrelevant in my analysis.

tions, 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.

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.¹⁴ 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.

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-Westfalen, Hessen, Rheinland-Pfalz, and Baden-Württemberg. Thereby, the period between April 1966 to summer 1967 was split into two compressed school years, from

¹⁴The 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).

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).

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.

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).¹⁵ 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.

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

¹⁵In 2015, 9 more countries took part in PIAAC.

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).¹⁶ 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.¹⁷ 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.¹⁸

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.¹⁹ Around 30 percent of anchor persons also received the NEPS test 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.

¹⁶Throughout, I use the first of overall ten plausible values of the PIAAC scores.

¹⁷See Section 4.3 for a description of the final regression sample.

¹⁸The 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).

¹⁹These 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.

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 Gehrler 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).²⁰

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).²¹ 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.²²

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 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.²³ 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:

²⁰The 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).

²¹PIAAC 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).

²²Results do not change quantitatively when using the original sample and average skill scores within each skill domain, but are less precise.

²³For the distribution of NEPS and PIAAC z-standardized test scores of both-takers see Figure A1.

$$T^{NEPS}_{new} = \frac{(T^{NEPS} - \overline{T^{NEPS}_{bothtaker}})}{SD^{NEPS}_{bothtaker}} * SD^{PIAAC}_{bothtaker} + \overline{T^{PIAAC}_{bothtaker}} \quad (1)$$

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.

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 A1 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.²⁴

The distribution of skill scores is shown in Figure 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 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 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

²⁴Individuals 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.

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.²⁵ 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 1), track shares in PIAAC seem to be quite representative of the overall population for the birth cohorts of interest (1945–1960).

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 result 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 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

²⁵Due to some implausibly high and low reported schooling years, the variable is trimmed at the 1st and 99th percentile within track.

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.²⁶

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:

$$Skills_i = \beta_0 + \beta_1 \widehat{Schooling}_i + \lambda_{state} + \delta_{cohort} + \chi'_i \beta_3 + \epsilon_i \quad (2)$$

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,

$$Schooling_i = \alpha_0 + \alpha_1 CSR + \alpha_2 SSY + \gamma_{state} + \mu_{cohort} + \chi'_i \alpha_3 + \varepsilon_i \quad (3)$$

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 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 .²⁷ All specifications control for differences across gender and test

²⁶Furthermore, 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.

²⁷Theoretically, 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

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.²⁸

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 for restricting the preferred sample to basic track students instead of considering the entire student sample across all tracks.²⁹

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 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

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.

²⁸Robustness specifications in Section 6.3 show that results are robust to excluding observations with approximated information on federal state of school attendance.

²⁹With 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 6.

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.³⁰ 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.

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.³¹

³⁰The statistical difference between estimated coefficients is tested using seemingly unrelated estimations.

³¹Standard errors increase slightly when clustering at the federal state level (10 clusters), which would be the most conservative way.

6 Results

This section summarizes findings on the effect of the compulsory schooling reform in Germany. Section 6.1 reports the estimated reform effect on length of schooling. Section 6.2 depicts reduced-form and IV results on the reform effect on skills. Various robustness checks are presented in section 6.3. Appendix A summarizes the results of a replication of the analysis using an alternative data set (the Adult Cohort of the National Education Panel Study, NEPS).

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 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.³²

Table 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 by the federal state at the time of the interview (see Section 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

³²Please 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 4), but length of schooling is equal across all observations for each individual.

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 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.³³ 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 A3). For instance, Panel A, Column 4, reports a coefficient of 0.49 years when being affected by the compulsory schooling 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 A4 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

³³Using 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.

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).³⁴ 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 schooling reform – 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.

6.2 Reform Effects on Skills

6.2.1 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 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.³⁵

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

³⁴The results are qualitatively the same when estimated in specifications with state-specific linear trends (not shown).

³⁵The 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 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.

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 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 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.

6.2.2 Two-Stage Least Squares Estimation Results

Two-stage least squares estimations in Table 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.³⁶ Panel A reports first-stage results, which are almost identically to those shown in Table 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.

³⁶An 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, 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.

The estimated reform effect on numeracy skills in Column 1 is very similar to the effect based on reduced-form estimations.³⁷ 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 3.8 percentage points higher rank for each additional year of schooling (Table A5). 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 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 A6, A7).

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 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

³⁷This is not surprising due to a first-stage reform effect of almost one year.

successful labor-market entry may be learned earlier than in grade nine. Unfortunately, my data does not allow to test this hypothesis directly.

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 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 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.³⁸

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 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.³⁹ 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.

³⁸Corresponding results for the alternative IV approach using hypothetical weeks of schooling is shown in Table A8.

³⁹Please 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.

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 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.⁴⁰ 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.⁴¹ 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 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 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

⁴⁰Specifications 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).

⁴¹This is the case for 18 basic track students (65 observations).

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 A9 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.⁴² 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.

7 Conclusion

The study analyzes skill effects of a compulsory schooling reform in West Germany between 1946 and 1969. Findings suggest that the German compulsory schooling reform significantly increased the length of schooling for basic track students. This additional education led to higher numeracy skills that are still measurable 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 (*compliers*), the complier population of the German reform constitutes an interesting sub-population. The reform was targeted at basic track 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 1 shows, the share of students affected by the reform was

⁴²I 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.

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.

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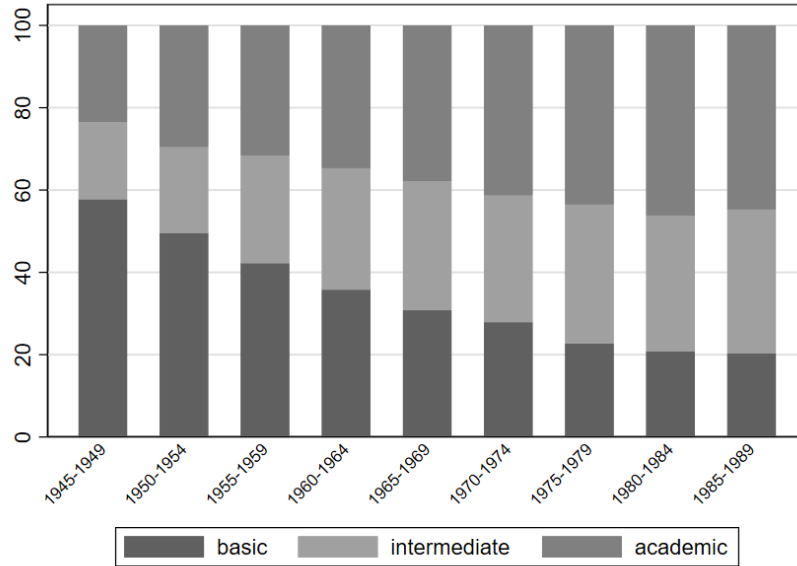
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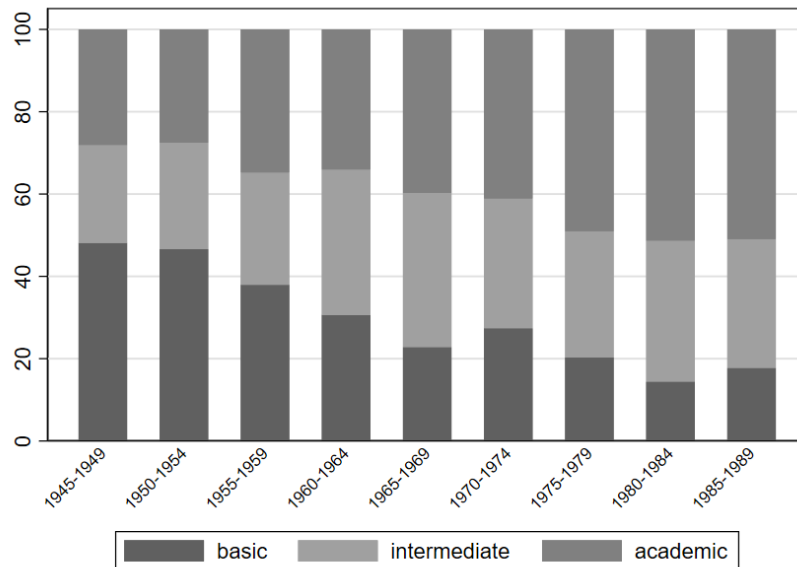
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Figure 1: Share of Students Across Secondary School Tracks

Panel A: German Micro Census, 2010



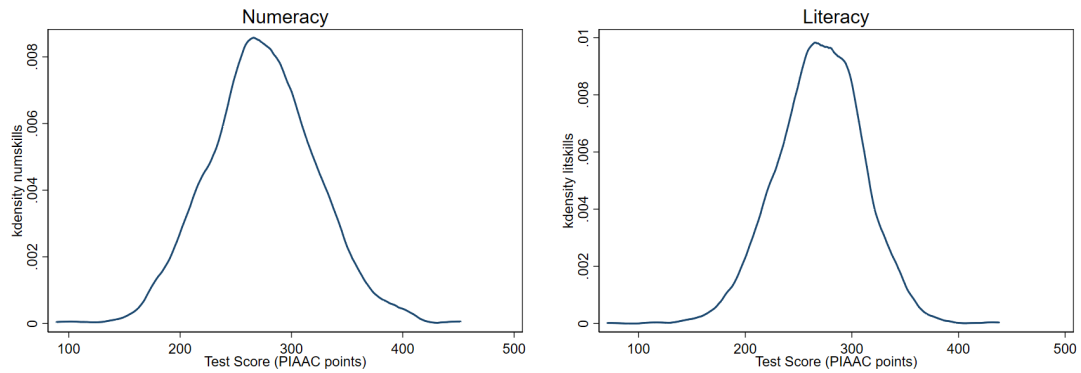
Panel B: PIAAC



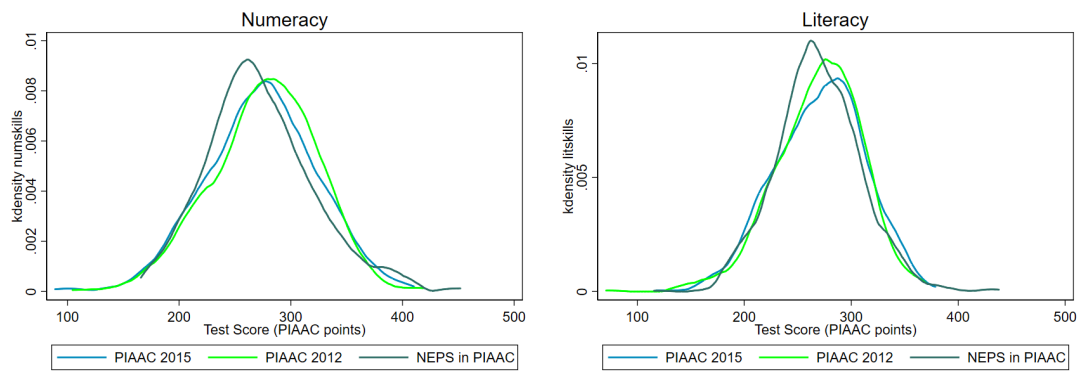
Notes: Share of students in each secondary school track by 5-year age cohorts, based on highest school degree achieved. Sample: birth cohorts born between 1945 and 1989. Data source: German Micro Census 2010, PIAAC.

Figure 2: Distribution of PIAAC Skill Scores

Panel A: Overall Distribution

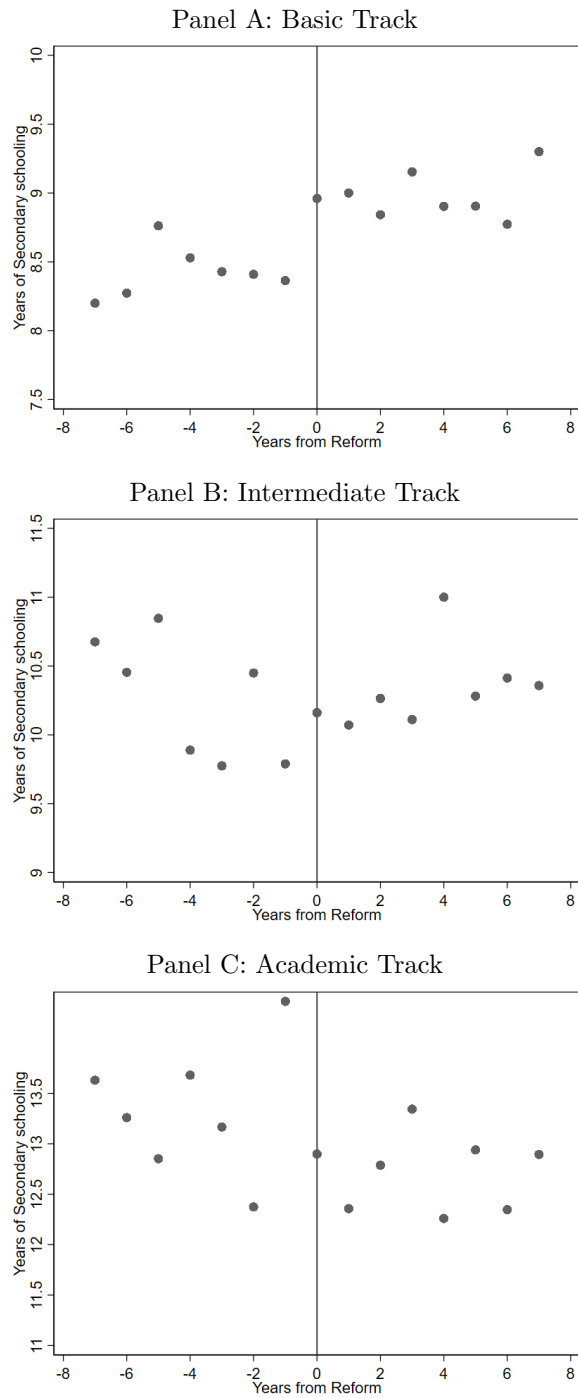


Panel B: Distribution by Test Regime



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 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 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: 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 4). *Data source:* PIAAC.

Table 3: Reform Effect on Length of Schooling

	Dependent variable: length of schooling		
	(1)	(2)	(3)
<i>Panel A: compulsory schooling reform & short school years</i>			
Compulsory Schooling Reform	.714*** (.182)	.969*** (.164)	.736*** (.207)
Short School Year		-.762*** (.184)	-.582** (.245)
<i>Panel B: hypothetical weeks of schooling</i>			
Weeks of schooling		.027*** (.004)	.021*** (.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 4: Reform Effect on Numeracy Skills – Reduced-Form Analysis

	Numeracy Skills		Numeracy Rank	
	(1)	(2)	(3)	(4)
<i>Panel A: effect of compulsory schooling reform & short school years</i>				
Compulsory Schooling Reform	.201*	.205	.085**	.074
	(.118)	(.163)	(.040)	(.056)
Short School Year	-.134	-.246**	-.049	-.088**
	(.094)	(.100)	(.032)	(.034)
<i>Panel B: effect of hypothetical weeks of schooling</i>				
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 5: Reform Effect on Literacy Skills – Reduced-Form Analysis

	Literacy Skills		Literacy Rank	
	(1)	(2)	(3)	(4)
<i>Panel A: effect of compulsory schooling reform & short school years</i>				
Compulsory Schooling Reform	.156 (.176)	.241 (.253)	.046 (.057)	.052 (.077)
Short School Year	.112 (.137)	.029 (.139)	.029 (.046)	.001 (.046)
<i>Panel B: effect of hypothetical weeks of schooling</i>				
Weeks of schooling	.002 (.004)	.004 (.006)	.001 (.001)	.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 6: Effect of Compulsory Schooling on Numeracy Skills – 2SLS Analysis I

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
<i>Panel A: First-Stage</i>				
Compulsory Schooling Reform	.969*** (.166)	.738*** (.210)	.969*** (.166)	.738*** (.210)
Short School Year	-.763*** (.187)	-.575** (.247)	-.763*** (.187)	-.575** (.247)
Dependent variable:	Numeracy Skills		Numeracy Rank	
	(1)	(2)	(3)	(4)
<i>Panel B: Second-Stage</i>				
Years of schooling	.207* (.119)	.277 (.224)	.088** (.041)	.101 (.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 7: Effect of Compulsory Schooling on Numeracy Skills – 2SLS Analysis II

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
<i>Panel A: First-Stage</i>				
Weeks of schooling	.027*** (.004)	.021*** (.006)	.027*** (.004)	.021*** (.006)
Dependent variable:	Numeracy Skills		Numeracy Rank	
	(1)	(2)	(3)	(4)
<i>Panel B: Second-Stage</i>				
Years of schooling	.197** (.096)	.325 (.199)	.080** (.034)	.117* (.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 8: Effect of Compulsory Schooling on Numeracy Skills I (Various Samples)

	Excl. 1st cohort	Excl. city states	1945 – 1970
<i>Panel A: First-Stage (Dependent Variable: Years of Schooling)</i>			
Compulsory Schooling Reform	.940*** (.215)	.967*** (.167)	.899*** (.161)
Short School Year	-.792*** (.195)	-.765*** (.187)	-.940*** (.163)
<i>Panel B: Second-Stage (Dependent Variable: Numeracy Skills)</i>			
Years of schooling	.321*** (.123)	.208* (.120)	.251* (.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 9: Effect of Compulsory Schooling on Numeracy Skills (Samples Based on State Identifiers)

	State of Residence Today	No Approx. State
<i>Panel A: First-Stage (Dependent Variable: Years of Schooling)</i>		
Compulsory Schooling Reform	.525** (.250)	.898*** (.169)
Short School Year	-.630*** (.220)	-.748*** (.169)
<i>Panel B: Second-Stage (Dependent Variable: Numeracy Skills)</i>		
Years of schooling	.056 (.301)	.201 (.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 A Replication – Effect of the Compulsory Schooling Reform on Length of Schooling Using NEPS

This Appendix replicates 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*.⁴³ 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).⁴⁴ 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.⁴⁵

Table A10 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, 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 A3 illustrates the share of students in each secondary schools track across 5-year birth cohorts from 1945 to 1989.

⁴³National 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.

⁴⁴From 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.

⁴⁵For 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).

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 A4 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 A11 reports coefficients of the reform effect on educational attainment. Following the empirical strategy outlined in Section 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.⁴⁶ 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 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

⁴⁶This is the case for 57 observations in the basic track sample (5.5 percent).

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 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 indicating 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.⁴⁷

Table A12 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 A13 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.⁴⁸ As mentioned in Section 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 of schooling decrease to 0.43 years (0.33 years with state-specific

⁴⁷Another 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.

⁴⁸The coefficient increases to 0.59 years in the specification controlling for state-specific linear trends (not shown).

linear trend). This suggests substantial inaccuracy in the treatment assignment of previous studies.

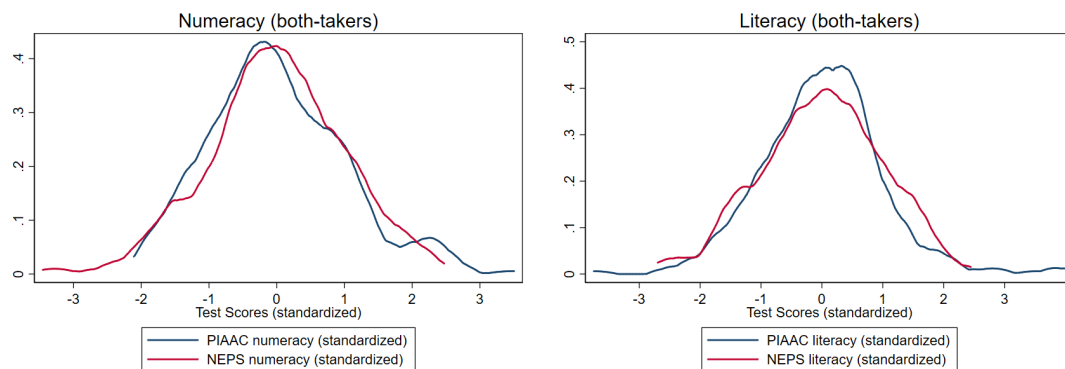
Reform Effects on Skills

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 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 A14 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.

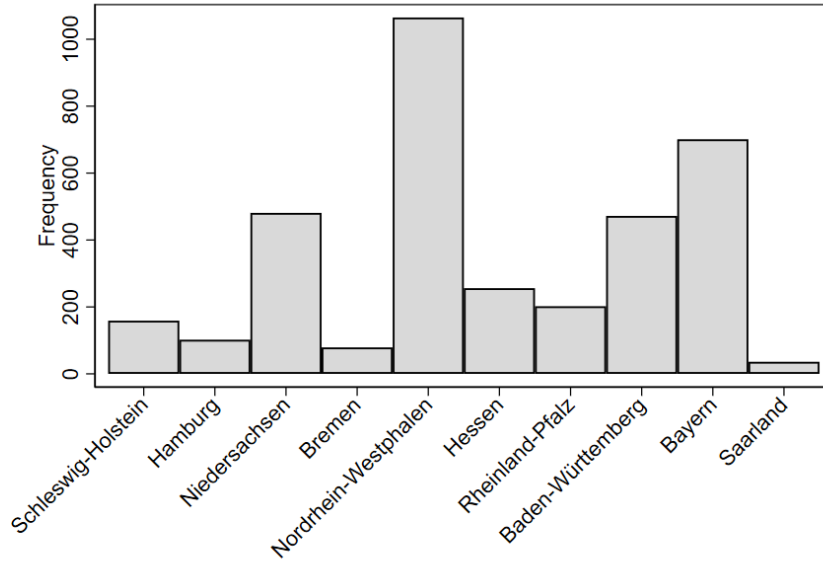
Figure A1: Distribution of PIAAC & NEPS Standardized Test Scores of Both-Takers in PIAAC 2015



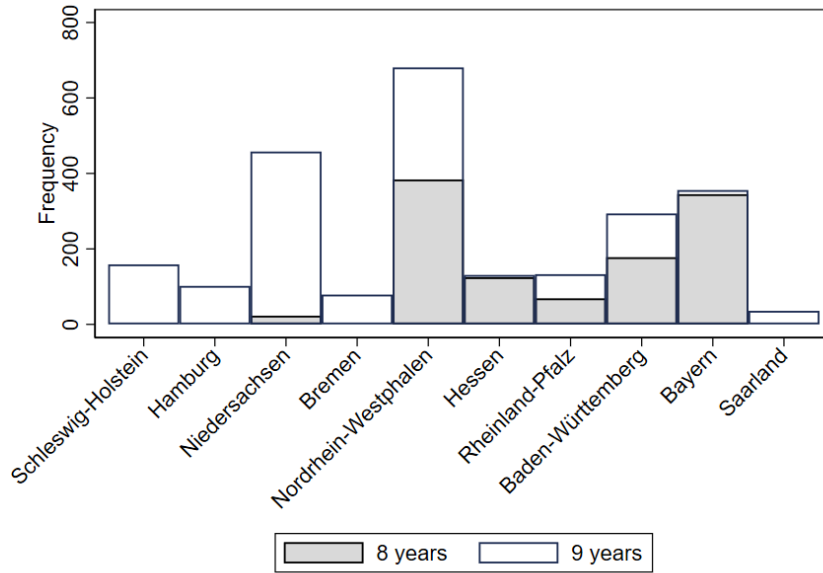
Notes: Distribution of PIAAC and NEPS numeracy and literacy skills (standardized on sample of both-takers for each skill domains). Sample: individuals born between 1945-1960 in West Germany, who were tested by means of PIAAC as well as NEPS test instruments in PIAAC 2015. *Data source:* PIAAC.

Figure A2: Distribution of Observations Across Federal States

Panel A: Across Federal States



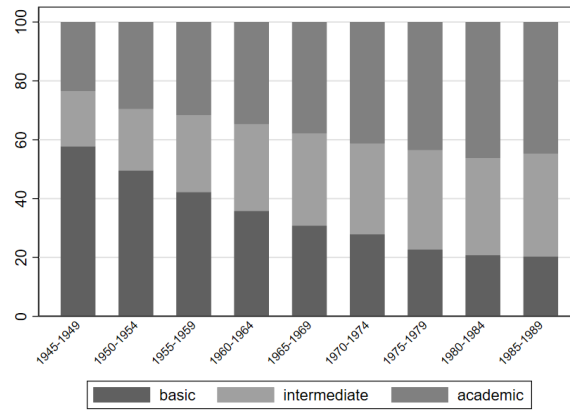
Panel B: Across Federal States & Compulsory Schooling Regime



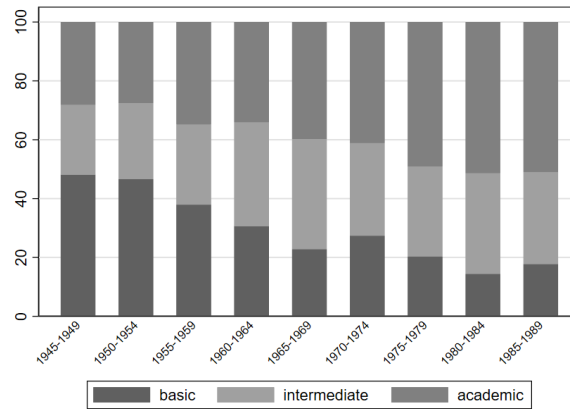
Notes: Panel A: distribution of observations across federal states. Panel B: distribution of observations in each compulsory schooling regime across federal states. Sample: individuals born between 1945-1960 in West Germany. Data source: PIAAC.

Figure A3: Share of Students Across Secondary School Tracks – NEPS Analysis

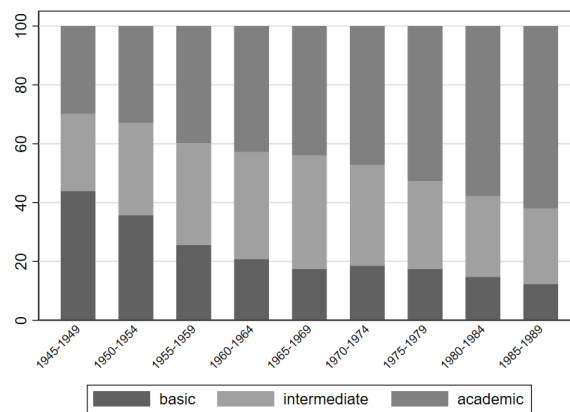
Panel A: German Micro Census, 2010



Panel B: PIAAC

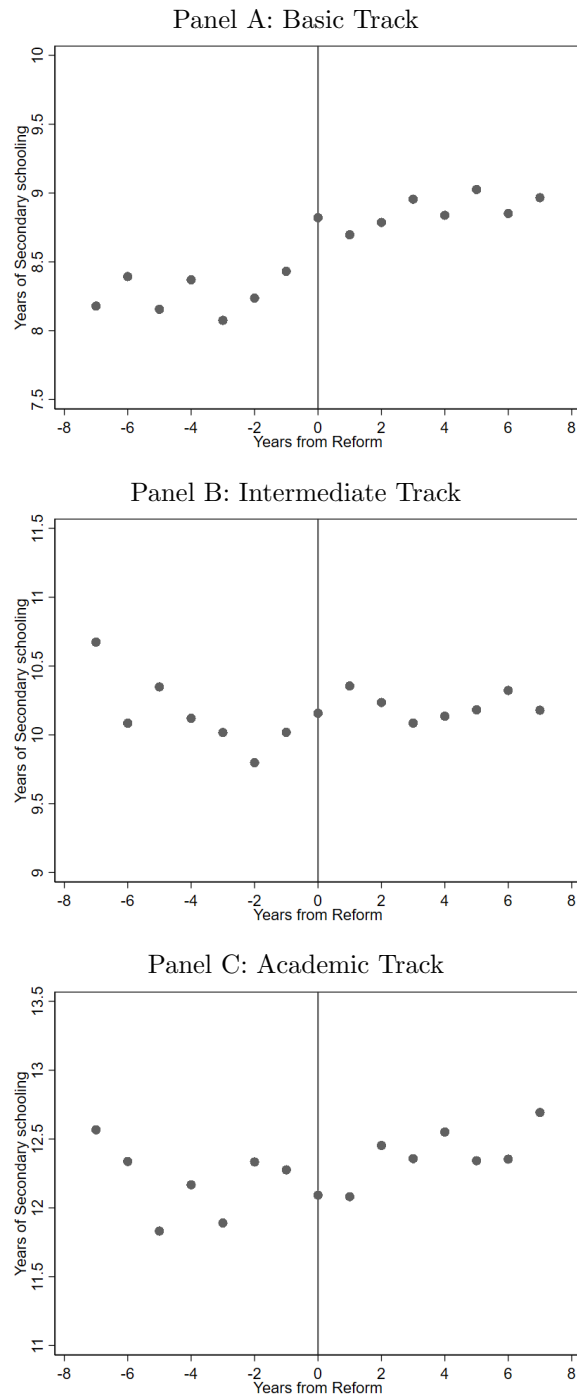


Panel C: NEPS



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 A4: 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 A1: 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: Reform Effect on Track Attendance

	(1)	(2)
<i>Panel A: Dependent variable: individual attends basic track</i>		
Compulsory Schooling Reform	.016 (.079)	.022 (.106)
<i>Panel B: Dependent variable: individual attends intermediate track</i>		
Compulsory Schooling Reform	.000 (.059)	.067 (.092)
<i>Panel C: Dependent variable: individual attends academic track</i>		
Compulsory Schooling Reform	-.016 (.068)	-.089 (.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 A3: Reform Effect on Length of Schooling, All Tracks

	Dependent variable: length of schooling		
	(1)	(2)	(3)
<i>Panel A: compulsory schooling reform & short school years</i>			
Compulsory Schooling Reform	.234 (.183)	.284 (.183)	.556*** (.212)
Short School Year		-.316** (.147)	-.237 (.168)
<i>Panel B: hypothetical weeks of schooling</i>			
Weeks of schooling		.019*** (.004)	.020*** (.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 A4: Reform Effect on Length of Schooling, by Secondary School Track

	Dependent variable: length of schooling		
	Basic track sample	Intermediate track sample	Academic track sample
<i>Panel A: compulsory schooling reform & short school years</i>			
Compulsory Schooling Reform	.969*** (.164)	.114 (.294)	-.367 (.535)
Short School Year	-.762*** (.184)	-.154 (.337)	.160 (.630)
<i>Panel B: hypothetical weeks of schooling</i>			
Weeks of schooling	.027*** (.004)	.005 (.013)	-.007 (.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 A5: The Relationship Between Length of Schooling and Skills

	Numeracy	Numeracy	Numeracy Rank	Numeracy Rank
Years of schooling	.118*** (.012)	.115*** (.012)	.038*** (.004)	.038*** (.004)
Observations	1770	1770	1770	1770
	Literacy	Literacy	Literacy Rank	Literacy Rank
Years of schooling	.139*** (.013)	.136*** (.013)	.045*** (.004)	.044*** (.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 A6: Effect of Compulsory Schooling on Literacy Skills – 2SLS Analysis I

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
<i>Panel A: First-Stage</i>				
Compulsory Schooling Reform	.961*** (.165)	.720*** (.207)	.961*** (.165)	.720*** (.207)
Short School Year	-.762*** (.185)	-.591** (.250)	-.762*** (.185)	-.591** (.250)
Dependent variable:	Literacy Skills		Literacy Rank	
	(1)	(2)	(3)	(4)
<i>Panel B: Second-Stage</i>				
Years of schooling	.163 (.178)	.335 (.365)	.048 (.058)	.072 (.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 A7: Effect of Compulsory Schooling on Literacy Skills – 2SLS Analysis II

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
<i>Panel A: First-Stage</i>				
Weeks of schooling	.027*** (.004)	.020*** (.006)	.027*** (.004)	.020*** (.006)
Dependent variable:	Literacy Skills		Literacy Rank	
	(1)	(2)	(3)	(4)
<i>Panel B: Second-Stage</i>				
Years of schooling	.062 (.142)	.209 (.279)	.020 (.047)	.048 (.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 A8: Effect of Compulsory Schooling on Numeracy Skills II (Various Samples)

	Excl. 1st cohort	Excl. city states	1945 – 1970
<i>Panel A: First-Stage (Dependent Variable: Years of Schooling)</i>			
Weeks of schooling	.027*** (.005)	.027*** (.004)	.029*** (.004)
<i>Panel B: Second-Stage (Dependent Variable: Numeracy Skills)</i>			
Years of schooling	.271*** (.096)	.198** (.096)	.166* (.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 A9: Effect of Compulsory Schooling on Exit Exam Grades

	Math	German
<i>Panel A: instrumented by compulsory schooling reform</i>		
Years of schooling	-.101 (.188)	.086 (.230)
Instrument F statistic	28.03	23.05
<i>Panel B: instrumented by hypothetical weeks of schooling</i>		
Years of schooling	-.204 (.179)	.017 (.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 A10: 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 A11: Reform Effect on Length of Schooling – NEPS Analysis

	Dependent variable: length of schooling		
	(1)	(2)	(3)
<i>Panel A: compulsory schooling reform & short school years</i>			
Compulsory Schooling Reform	.444*** (.100)	.533*** (.110)	.446*** (.101)
Short School Year		-.241** (.117)	.067 (.115)
<i>Panel B: hypothetical weeks of schooling</i>			
Weeks of schooling		.013*** (.003)	.008** (.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 A12: Reform Effect on Track Attendance – NEPS Analysis

	(1)	(2)
<i>Panel A: Dependent variable: individual attends basic track</i>		
Compulsory Schooling Reform	.026 (.043)	.042 (.053)
<i>Panel B: Dependent variable: individual attends intermediate track</i>		
Compulsory Schooling Reform	.042 (.030)	.059* (.033)
<i>Panel C: Dependent variable: individual attends academic track</i>		
Compulsory Schooling Reform	-.068 (.042)	-.101** (.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 A13: Reform Effect on Length of Schooling, All Tracks – NEPS Analysis

	Dependent variable: length of schooling		
	(1)	(2)	(3)
<i>Panel A: compulsory schooling reform & short school years</i>			
Compulsory Schooling Reform	.219** (.093)	.242** (.102)	.227 (.147)
Short School Year		-.108 (.104)	-.033 (.107)
<i>Panel B: hypothetical weeks of schooling</i>			
Weeks of schooling		.016*** (.002)	.015*** (.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 A14: Effect of Compulsory Schooling on Numeracy Skills – NEPS Analysis

Dependent variable:	Length of Schooling			
	(1)	(2)	(3)	(4)
<i>Panel A: First-Stage</i>				
Compulsory Schooling Reform	.683*** (.148)	.749*** (.148)	.581*** (.123)	.592*** (.133)
Short School Year	-.246* (.128)	-.237** (.111)	-.277** (.130)	-.115 (.138)
Dependent variable:	Numeracy Skills		Literacy Skills	
	(1)	(2)	(3)	(4)
<i>Panel B: Second-Stage</i>				
Years of schooling	.049 (.342)	.170 (.375)	-.104 (.194)	.002 (.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.