Lifetime consequences of lost instructional time in the classroom: Evidence from shortened school years

Kamila Cygan-Rehm*

Leibniz Institute for Educational Trajectories (LIfBi), CESifo, IZA, LASER

This version: May 6, 2022

Abstract: This study estimates the lifetime effects of lost instructional time in the classroom on labor market outcomes. For identification, I use historical shifts in the starting date of the school year in Germany, which substantially shortened the duration of the affected school years with no adjustments in the core curriculum. The lost in-school instruction was mainly substituted by additional homework and reduced emphasis on non-core subjects. I apply a difference-in-differences design to social security records, which allow me to follow the exposed individuals over the life cycle. I find adverse effects of the policy on earnings and employment nearly over the entire occupational career. Significantly lower levels of post-secondary education seem a plausible mechanism behind the deteriorated labor market outcomes. Survey data also reveal long-lasting imprints on cognitive skills and personality traits. The results are driven by men, for whom the policy also elevated income dispersion due to larger harm at the bottom of the income distribution.

*Contact: Kamila Cygan-Rehm, Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg, Wilhelmsplatz 3, 96047 Bamberg, Germany, Email: kamila.cygan-rehm@lifbi.de. This paper uses proprietary data that can be requested from the Research Data Centers (FDZ) at the Institute for Employment Research (FDZ-IAB), the Federal Pension Insurance (FDZ-RV), the German Institute for Economic Research (FDZ SOEP), the Federal Statistical Office (FDZ DESTATIS), and the Leibniz Institute for Educational Trajectories (FDZ-LIfBi). The author is willing to assist. I thank Mevlude Akbulut-Yuksel, Anton Barabasch, Kristiina Huttunen, Jan Marcus, Steve Pischke, Regina T. Riphahn, Katharina Wrohlich, Conny Wunsch, and Sebastian Vogler for helpful comments and suggestions. I also acknowledge the feedback from seminar participants at the FAU Erlangen-Nuremberg, Ausschuss für Sozialpolitik, EMAE-Meeting 2021, RES Conference 2022, and the IAAEU's 14th Workshop on Labour Economics. I also thank the Landesarchiv Baden-Württemberg for providing me a digital version of numerous historical records and Josefine Koebe for fruitful discussions on institutional details. Claudius Bauer and Marion Heinz provided excellent research assistance. I acknowledge financial support by the FAU's Office for Equality and Diversity.

1 Introduction

What are the long-run consequences of lost instructional time in the classroom? This question has recently gained prominence due to the outbreak of the COVID-19 pandemic, which led to school closures affecting millions of students around the world (UNESCO, 2021). To some extent, in-person classes have been substituted by remote instruction but its effectiveness has been often questioned (e.g., Artelt et al., 2020; Huebener et al., 2020; Kuhfeld et al., 2020; Bacher-Hicks et al., 2021; Halloran et al., 2022). Fast growing international evidence documents that students substantially reduced the time spent on school-related activities during lockdowns and made little to no progress while learning from home.[1] Unless re-mediated, these learning losses might have long-lasting detrimental consequences for the affected students and the economies in general (Hanushek and Woessmann, 2020). However, the magnitude of these effects is currently difficult to assess.

The scale of the COVID-19 pandemic and its impacts on various social and economic dimensions are unprecedented. Nevertheless, the potential implications of lost instructional time in school are also relevant in the context of many other situations that might keep students out of the classroom such as other pandemics (e.g., Ager et al., 2020), natural disasters (e.g., Sacerdote, 2012), inclement weather conditions (e.g., Marcotte and Hemelt, 2008; Goodman, 2014), teacher strikes (e.g., Belot and Webbink, 2010; Baker, 2013; Jaume and Willén, 2019), long summer holidays (for a recent review, see, Kuhfeld et al., 2020), or four-day school weeks (e.g., Thompson, 2021). In line with the theoretical predictions, most empirical studies on these events find that reduced instructional time impairs academic achievement. A similar conclusion arises from research relying on within-student variation across subjects or grades (e.g., Lavy, 2015; Rivkin and Schiman, 2015). The literature examining whether the effects carry over to the labor market is still scarce and inconclusive.[2]

[1] For example, Andrew et al. (2020); Anger et al. (2020); Bansak and Starr (2021); Grewenig et al. (2021); Engzell et al. (2021); Wößmann et al. (2020); Contini et al. (2021); Maldonado and De Witte (2021). For recent reviews, see Hammerstein et al. (2021); Werner and Woessmann (2021). Most studies emphasize unequal impacts of the pandemic-related school closures with largest effects for the most disadvantaged groups. Halloran et al. (2022) find significant interactions between learning losses and schooling mode in favor of in-person classes as opposed to hybrid or virtual instruction.

[2] For example, for Argentina, Jaume and Willén (2019) estimate that an average exposure to teacher strikes during primary school reduces labor earnings at ages 30-40 by 3.2% for males and 1.9% for women. In contrast, Ager et al. (2020) find no wage effects of the school closures during the 1918 flu pandemic in the U.S. Related and more extensive literature studies the labor market effects of exposure to increased instructional time, e.g., due to

This paper contributes to the literature by looking at the labor market effects of lost instructional time in the classroom from a lifetime perspective. Specifically, I evaluate the long-run impacts of two shortened school years that occurred in Germany in the 1960s as a result of moving the starting date of the school year (for details, see, e.g., Pischke, 2007; Koebe and Marcus, 2022). Each short school year compressed the instructional time in the classroom by one-third of a regular school year. Although there was much emphasis on leaving the curriculum unaffected, compensatory measures were mostly limited to assigning additional homework in core subjects such as mathematics and German. In contrast, instructional time in other subjects was heavily reduced and nearly all co-curricular activities canceled. In the media, the policy was popularized as a "large-scale experiment at the expense of the students" (Landesarchiv, 2020).

So far, there is only limited evidence on the consequences of the short school years for human capital formation and labor market performance, and the findings are largely inconclusive. Early studies produced mixed results by comparing cognitive skills (mostly reading, vocabulary, and mental arithmetic tests) between relatively small samples of exposed and non-exposed students (Kornadt and Meister, 1970; Meister, 1972; Thiel, 1973). If anything, the immediate effects on learning were small and not necessarily negative. However, more recently, Hampf (2019) documents long-lasting deficiencies in numeracy skills. For educational attainment, Drewek (2020) compares aggregate data on school drop-outs in the largest affected and non-affected states. He concludes that the time series do not support the expected harm. Using a difference-in-difference approach, Grätz (2021) does not find any significant effect on high school completion. This is in line with earlier results by Pischke (2007) from a similar design, who however found a significant increase in grade repetition rates in primary school and a lower probability of obtaining the intermediate secondary school degree. Nonetheless, his estimations for wages did not yield any detrimental effects.[3]

This study adds to this evidence by investigating the potential effects of the short school years on educational attainment and labor market outcomes from a life-cycle perspective. The last birth cohorts of students affected by the loss of in-school instruction in the 1960s are cur-

extensions of compulsory schooling (e.g., Stephens and Yang, 2014; Bhuller et al., 2017) or the term length (e.g., Parinduri, 2014; Fischer et al., 2020).

[3]For other outcomes, Koebe and Marcus (2022) document significant effects on the timing of marriage and parenthood. Related literature studies the effects of compressed duration of the German high school that resulted from the so-called "G8-reform" in the 2000's (e.g., Dahmann, 2017; Huebener et al., 2017; Marcus et al., 2020).

rently close to reaching the statutory retirement age. Thus, using social security records with detailed employment biographies, I can observe their earnings and employment over nearly entire occupational careers. I link the individual-level administrative data to a novel data set that includes relevant institutional details, which I compiled from primary sources for the purpose of this study. Similar to some earlier studies (Pischke, 2007; Hampf, 2019; Grätz, 2021), I identify the effects of interest by using a difference-in-differences design that leverages the variation in the exposure to the short school years across the federal states and birth cohorts. However, by exploring the exact date of birth and the state-specific cutoff dates for school enrollment, I rely on a more accurate assignment of the treatment status. I also apply an augmented model specification that allows me to control for the potentially confounding impacts of accompanying policy changes such as the parallel extensions of compulsory schooling in several states and the necessary adjustments of the cutoff rules for school enrollment during the transitory period.

I find adverse labor market effects of the exposure to the shortened school years nearly over the entire occupational career. Specifically, my estimation results imply that one year of lost instructional time in the classroom decreases lifetime earnings by nearly 3%, on average. This is partly driven by negative employment responses during the prime ages (a 2% reduction). The results are robust to various changes in the model specification and sample restrictions, and to accounting for potential drawbacks of my research design in case of treatment heterogeneity.[4] My findings sharply contrast a purely mechanical effect of an earlier graduation, which would imply more time spent in the labor market and thus, higher earnings accumulated over the life cycle. In fact, I show that the initial advantage form an accelerated labor market entry is substantial but vanishes quickly. In contrast, the subsequent negative effects are less pronounced but they persist until retirement ages. With respect to potential mechanisms, I find no effects on secondary school credentials. However, the affected individuals exhibit lower levels of post-secondary education other than college or university. Using complementary data, I also document long-lasting deficiencies in cognitive abilities of non-trivial magnitudes and long-run imprints on labor market relevant personality traits. The unfavorable effects on different aspects of human capital seem plausible channels through which the policy might have impaired labor market performance. Nevertheless, likely due to a generally low female labor force par-

[4]For recent reviews of the current advances in the econometrics of difference-in-differences methods, see, e.g., de Chaisemartin and D'Haultfœuille (2021); Roth et al. (2022).

ticipation among the cohorts under study, the earnings losses were entirely borne by men. For them, the policy also elevated income dispersion due to larger harm at the bottom of the income distribution.

The paper proceeds as follows. Section 2 provides the institutional details. Section 3 describes the data and Section 4 the empirical strategy. Section 5 discusses the main results, their robustness, and the potential operating channels. Section 6 concludes.

2 Institutional background

In Germany, the responsibility for educational policies lies with the federal states (see, e.g., Helbig and Nikolai, 2015). However, during the Nazi regime, the education system was centralized and among other things, the start of the school year was uniformly set to fall. After World War II, all states successively reinstated or reformed their own school laws and mostly shifted the start of the school year back to spring (see Table 1). This change was implemented through one shortened school year that began in fall and ended before the Easter break. In 1955, the Ministers of Education of all states agreed (within the so-called *Düsseldorf Accord*) that the school year should start on 1st April, which has however never been implemented in Bavaria (for details, see Koebe and Marcus, 2022).

In 1964, all states signed a further agreement aiming at the standardization of the school systems (the *Hamburg Accord*). One of its consequences was an introduction of a uniform school year schedule, starting on 1st August (Pischke, 2007). In this regard, Germany decided to follow other European countries, which commonly began a new school year after a longer summer break (DIE ZEIT, 1966).[5] Most states moved the begin of the school year back to fall within two shortened school years that started on 1st April and 1st December 1966, respectively.[6]

All children attending school during the transitory period experienced a shortened schooling duration save for Bavarian, Hamburgian, and Lower Saxonian students. Bavaria remained unaffected because it already started the school year in fall (see Table 1). Hamburg accomplished the change within one prolonged school year, which actually counted as two grades but implied no school entries and no graduations between April 1966 and August 1967. Moreover,

[5]In practice, summer vacations in Germany are staggered across the states, so that a new school year can begin from early August until mid September (KMK, 2020).

[6]More details are provided, e.g., in Pischke (2007) and Koebe and Marcus (2022).

the time losses were appended to students' final year in school, so that the switch did not affect the eventual schooling duration in this state. Same applied to the majority of students in Lower Saxony.[7] Effectively, each short school year reduced the amount of instructional time by one-third. Figure 1 illustrates that most students were affected over two grades with the exception of students who started schooling in the second short school year and those being in graduating classes during the first short school year. The figure also reveals that despite being a one-time change, the shift in schedule in 1966/67 affected a large number of birth cohorts because it had implications for millions of students who entered primary school long before 1966.

Due to data availability, I focus on school years from 1950 through 1970. During this period, the cutoff dates for school enrollment were state-specific. Thus, an individual's birth date and the state of school attendance largely determined the exposure to short school years. Figure 2 shows this variation at a monthly level for children who started schooling between 1950 and 1970, i.e., those born between January 1944 and December 1963. The state-specific figures illustrate the effective duration of compulsory schooling depending on the exposure to either one or two short school years.

Generally, this period includes two compulsory schooling regimes that required either eight or nine years of school attendance.[8] For example, both Schleswig-Holstein and Hamburg mandated nine years of compulsory schooling. However, while all cohorts in Hamburg enjoyed full nine years of compulsory education, Schleswig-Holstein's students born between April 1951 and November 1960 experienced compressed schooling duration due to the short school years. The downward deviations from the statutory requirement of one-third and two-thirds of a year

[7]Lower Saxony did neither enroll new students nor released graduates from the lowest secondary school track in fall 1966. For them, schooling duration was extended to spring 1967, so that they left after full nine years of schooling as usual. However, students from more advanced tracks graduated in fall 1966, thereby experiencing a shorter school year. More details provide Pischke (2007) and Koebe and Marcus (2022). In the main analysis, I consider Lower Saxony as a non-treated state (see Figure 1) but the results remain unchanged after excluding this state from the estimations (see Figure A.7 in Appendix A).

[8]Compulsory schooling laws in Germany are grade-based, i.e., they require individuals to complete a minimum number of years of education, independently of when they started schooling and of their age at leave. There are some inconsistencies regarding the exact timing of the German compulsory schooling extensions from eight to nine years in the literature (e.g., Pischke and von Wachter, 2008; Cygan-Rehm and Maeder, 2013; Piopiunik, 2014). My description largely follows Leschinsky and Roeder (1980). I validated their information against the original state laws (Makrolog, 2019), the official statistics on actual ninth grade attendance (DESTATIS, 2021), and numerous newspaper articles and historical documents from the State Archives of Baden-Württemberg (Landesarchiv, 2020). Furthermore, I compared and discussed the results of my background research with Josefine Koebe, who simultaneously and independently conducted institutional research on this period (Koebe and Marcus, 2022). All this makes me think that the information provided here is very accurate.

correspond to one and two short school years, respectively. A similar pattern of exposure applies to Bremen. Nordrhein-Westphalia, Hesse, Rhineland-Palatinate, and Baden-Wuerttemberg used the short school years 1966/67 to introduce the ninth grade. Nevertheless, the patterns are not identical because Hesse did it already in the first short school year while the other states waited until the second one. Moreover, in Baden-Wuerttemberg, students born before June 1945 also experienced compressed schooling due to an earlier shift of the school start from fall to spring in 1952. Bavaria was not affected by any changes in the schedule but extended compulsory schooling in the period under study. Finally, Saarland moved the start of the school year from fall to spring and then the other way around within a 10-year period, so that the majority of considered cohorts lost some instructional time due to short school years.[9]

Some students not exposed to short school years during compulsory schooling could have still been affected if they attended an advanced secondary school track during the transitory period. Typically, after four years in primary school[10], German students are tracked into one out of three secondary school types: basic track (*Hauptschule*), middle track (*Realschule*), and high school (*Gymnasium*)[11], which differ in duration and curriculum, and prepare for different professional careers.[12] Nevertheless, in the 1950s and 1960s, the vast majority of students left school after completing the compulsory requirements. For example, comparing the number of 13th-graders in 1965 to the number of school starters 13 years earlier suggests that less than 10% of a given enrollment cohort actually continued until the final high school year (see Figure A.1 in Appendix A). Approximately 70% of the 8th graders attended the basic track, 10% the middle track, and 15% the high school (see Figure A.2 in Appendix A).

Although the short school years substantially compressed the in-school instruction, there was much emphasis on leaving the core curriculum unaffected (Pischke, 2007). Still, contemporaneous sources suggest that the transition led to turbulent changes in the course of instruction

[9]Saarland joined the Federal Republic of Germany as a state in January 1957. Immediately thereafter, the begin of the school year was set to spring and had to be changed again to fall during 1966/67.

[10]Two exemptions were the city states of Hamburg and Bremen, where primary school comprised six grades.

[11]There are also alternative school types such as comprehensive schools without tracking (*Gesamtschule*) or schools for children with special needs (*Sonderschule, Förderschule*), but the vast majority of cohorts considered in this study participated in the traditional tripartite system (see Figure A.2 in Appendix A). The tracking depends on various criteria, which differ by state. Details are provided, e.g., in Lüdemann and Schwerdt (2013).

[12]The basic track prepares for an apprenticeship. In the period under study, its duration was determined by compulsory schooling requirements (i.e., 8 or 9 years depending on the state and birth date). The middle track typically ends after grade 10 and qualifies for an apprenticeship or training in white collar jobs. A successful completion of a high school, which lasts until grade 12 or 13, gives access to academic education in colleges or universities.

and in students' lives (Landesarchiv, 2020).[13] Indeed, a priority was given to teaching the usual material in math, German, and modern foreign languages (mostly English). The weekly amount of in-class instruction in these subjects increased to accommodate the necessary acceleration in pace. Typically, teachers also assigned additional homework.[14] In contrast, the number of instructional hours in other subjects such as geography, biology, history, and especially in music, arts, sports etc. was reduced, and nearly all co-curricular activities canceled. Many states also cut down the number of in-class tests and the final exam requirements in the core subjects. Generally, teachers, parents, and students complained about the increased pace of instruction. Much disputed in the press was also the increased stress level and anxiety among students due to learning under high pressure, and the potentially negative effects of the short school years on the academic performance (Landesarchiv, 2020). At the same time, contemporary witnesses report that teachers often turned a blind eye to knowledge deficiencies when deciding about grade progression, track recommendation, and grades in final exams (Drewek, 2020).

3 Data

3.1 Social security records (SIAB 1975-2017)

For the main analysis, I use individual register records from the Sample of Integrated Labour Market Biographies (Antoni et al., 2019).[15] The SIAB is a 2% sample of the population covered at least once by the social security system between 1975 and 2017 due to employment or take-up of public transfers such unemployment benefits and welfare. Since 2000, registered job seekers with no benefit eligibility and participants in active labor market programs have also been included. The original data cover approximately 80% of the total workforce in Germany because civil servants and the self-employed are not subject to social security. The SIAB is organized by spells and follows the sampled individuals as long as their activities appear in

[13] I refer here to two collections of numerous newspaper articles and historical documents (approximately 400 pages), which I obtained from the State Archives of Baden-Württemberg (Landesarchiv, 2020). These documents are not limited to the state of Baden-Württemberg.

[14] Surveys among teachers (Thiel, 1973) reveal that some of them also occasionally gave extra hours of instruction in math and writing, but this was rather an exception. It remains unclear whether parental involvement increased (Meister, 1972; Thiel, 1973). Minor compensatory measures on the part of the states included, e.g., radio broadcasting of English classes in Baden-Wurttemberg (Landesarchiv, 2020).

[15] Specifically, I use the weakly anonymous version of the SIAB 1975-2017 and accessed the data via a Scientific Use File at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) in Nuremberg.

social security records. The key advantage of the data, apart from the large sample size, is that the information on employment biographies, earnings, and birth date is very accurate.

I consider German citizens born between 1944 and 1963 to ensure long earnings histories. I focus on their outcomes measured at ages 20 through 64, which covers the potential working life span. The time frame of the data implies that my main estimates are based on an unbalanced panel because individuals born in 1944 are first recorded at age 31 and those born in 1963 are last recorded at age 54. I define lifetime outcomes as sums over the age interval 20 to 64 even if I miss the earliest or the latest career years for some cohorts. However, I also show results obtained for prime-age outcomes measured for ages from 31 to 54, which I observe for all included birth cohorts. For comparability, I restrict the estimation samples to individuals whom I observe at least once (in employment or unemployment) at ages 31-54.

The original earnings measure is stored as gross daily pay in EUR, which I deflate to 2015 prices using the consumer price index (OECD, 2020). Although the payroll information on earnings is highly reliable in general, the data include the actual gross pay only up to the legal social security contribution ceiling, which is relevant for the calculation of retirement pensions and unemployment insurance benefits. All earnings above the ceiling are top-coded, which affects approximately 5% of all spells. To impute the top-coded earnings, I use a two-step procedure as implemented in Dauth and Eppelsheimer (2020)[16] but my main results change little if I use the original top-coded values. I reshape the spell data into a yearly panel to calculate the annual sum of earnings for each individual. Using this measure, I determine an individual's lifetime earnings as the undiscounted sum of annual earnings over ages 20-64. To measure employment, I calculate the total number of days in employment at these ages. I construct similar measures for prime-age earnings and employment using the age range of 31-54 years.

Unfortunately, there is no detailed information on educational trajectories in the data. Thus, I do not observe the actual exposure to short school years, which depended on school attendance in affected states during the transitory period. Nevertheless, given that grade retention or advancement was rarely practiced back then, the exposure was largely predetermined by birth date, which determines the date of school enrollment. Thus, using the information on an individual's birth date and state-specific cutoffs for school enrollment, I can infer a potential exposure to

[16]A similar imputation procedure for earnings in German social security records has been previously applied, e.g., in Dustmann et al. (2009) and Card et al. (2013).

short school years. For this purpose, I created a data set with the relevant institutional details, which I describe in Section 3.2 below. However, given the sparse educational information in social security records, I do not observe the state of schooling or even the place of birth. Thus, I use the first state of residence ever reported for a given individual in the data as a proxy for the state of schooling.[17] The resulting measurement error should be limited as for the cohorts under study, cross-state mobility was generally low[18] and unrelated to the exposure to the short school years. Appendix B provides evidence on issue from auxiliary survey data. Thus, the measurement error due to the lacking information on the state of schooling (if anything) leads to an attenuation bias.[19]

Generally, educational variables in the German social security data are a byproduct of a reported employment or unemployment spell, and the focus is mainly on post-secondary education (Fitzenberger et al., 2006). Because the variable reporting school-leaving certificates lumps together the basic and the middle track graduates, I am not able to construct any measure of years of schooling. Nevertheless, to investigate the potential effects of short school years on educational attainment, I consider indicators for having a high school diploma, a college degree (incl. universities), and any vocational degree as auxiliary outcomes. The final sample comprises nearly 7.7 million annual observations on 278,797 individuals. Table A.1 in Appendix A displays the descriptive statistics.

3.2 Database with policy variables

I merge the social security data with a data set including relevant institutional details (described in Section 2), which I compiled from primary sources. Specifically, for each state and each school year between 1950/51 and 1978/79, I collected information on the statutory cutoff date for school enrollment, the start and end dates of the school year, and compulsory schooling

[17] Because for 5% individuals, the state of residence was never reported, I then use the state of the local employment agency or the first employer instead. This should not significantly increase the potential measurement error because the vast majority of employees in Germany work and live in the same state.

[18] Survey data from the National Educational Panel Study (NEPS) suggest that nearly 80% of individuals born 1944-1963 still live in their state of schooling at the time of the interview (i.e., at age 43 and above). For details, see Appendix B.

[19] I provide supportive evidence for this argument in a robustness test in Section 5.2 where I alternatively use the last state observed in the data as a proxy for state of schooling, which arguably increases the measurement error.

requirements.[20] Based on this information, for each combination of year and month of birth between January 1944 and December 1963, I assign a state-specific date of school entry according to the relevant cutoff rule. Similarly, for each birth cohort, I determine the date of the earliest possible school leave from compulsory schooling laws and the actual end dates of the corresponding school years. The difference between the date of the earliest possible school exit and the date of school enrollment corresponds to state- and cohort-specific compulsory schooling duration. Effective duration of less than the mandated eight or nine years indicates exposure to short school years (see Figure 2). Specifically, downward deviations of one-third and two-thirds of a year correspond to one and two short school years, respectively. Thus, my main policy variable of interest equals to 0.333 or 0.666 for cohorts exposed to short school years and zero otherwise, thereby measuring the amount of in-school instructional time lost due to the short school years.

I create additional control variables for my empirical analysis; first, the difference between the expected date of school enrollment and birth date yields the statutory age at school entry, which is also state and cohort-specific. This variable varies between 5.6 and 7.6 and captures differences in the expected school starting age across states and birth cohorts. Furthermore, I also calculate the size of the enrollment cohort measured as a number of birth months that were simultaneously scheduled for enrollment in a given state in a particular school year.[21]

I augment the data by adding the information on time-variant state-specific student-to-teacher ratios, which I transcribed from annual school statistics reported in statistical yearbooks (DESTATIS, 2021). I merge this aggregate measure as of the school year when a particular birth cohort was in the 1st, 4th, and 9th grade as proxies for the underlying state-specific differences in schooling quality at the time of school enrollment, shortly before tracking, and by the end of compulsory education.

[20] I retrieved the relevant details from public records which mostly included original state laws (Makrolog, 2019), dates of school vacations (KMK, 2020), aggregate administrative data on new school entrants, the ninth grade attendance, and school leavers (DESTATIS, 2021), and historical newspaper articles and policy documents (nearly 400 pages) obtained from the State Archives (Landesarchiv) of Baden-Württemberg.

[21] While a typical enrollment cohort comprises children born over a 12-month period, any shift of the cutoff date leads to a one-time change in the number of birth months scheduled for enrollment. For example, postponing the cutoff by one month from June 30th to July 31st implies a smaller enrollment cohort in the upcoming school year (11 birth months) because children born in July who would have to be enrolled according to the old cutoff are now held back from starting school for one year. All states affected by the short school years adjusted the enrollment cutoffs to the new schedule of the school year.

I link all policy variables to the individual level data from SIAB based on date of birth and state. Nearly one-third of individuals in my sample was exposed to at least one short school year. The bottom panel of Table A.1 in Appendix A summarizes the institutional data.

3.3 Complementary data sets

To address some limitations of the social security records, throughout the paper, I provide auxiliary analyses based on three complementary data sets. First, I use administrative records from the German Statutory Pension Insurance, which document all pension-relevant events (incl. employment) for a random sample of persons covered by the mandatory Pension Insurance. In contrast to the SIAB data starting in 1975, the Pension Insurance records follow each sampled individual from the age of 14 onward (irrespective of the calendar year), thereby allowing me to study the effects of the short school years directly upon the labor market entry. Although the Pension Insurance does not collect direct information on earnings, the statutory "pension points" might serve a close proxy; each year, the average earners gain exactly one additional point while lower or higher earnings contribute proportionately less or more points to an individual's pension account, respectively. The total sum of points accumulated until retirement determines the eventual pension entitlements. The data are stored in monthly spells, which I convert to a yearly panel spanning calendar years from 1958 through 2018. Consequently, for the first (last) considered birth cohort 1944 (1963), I obtain a balanced panel comprising ages from 14 through 64 (55). The estimation sample includes nearly 58,000 individuals.

Second, I use data from the German Micro Census, which is the largest national household survey. In contrast to the SIAB, the Micro Census also includes civil servants and self-employed, which allows me to show that my main conclusions hold after including groups that are not subject to social security contributions. The available income measure reports respondents' monthly net income, which comprises any income sources including labor, pensions, and public transfers. The survey also includes some additional measures of educational attainment, which allows me to look at the potential effects on years of schooling and the completed school degree. By pooling three cross-sectional waves of the Micro Census (2008, 2012, and 2016), I obtain a sample size of approximately 370,000 individuals.

Finally, I complement my results for labor market outcomes and educational attainment by studying various domains of cognitive and socio-emotional skills available in the German Socio-Economic Panel (SOEP) (Goebel et al., 2019). The SOEP measures cognitive ability using a symbol correspondence test (matching as many numbers and symbols as possible within 90 seconds according to a given correspondence list) and a word fluency test (naming as many different animals as possible within 90 seconds). For personality traits, I mainly draw on the Big Five inventory comprising openness to experience, conscientiousness, extroversion, agreeableness, and neuroticism. Note that the SOEP started to collect information on cognitive skills and personality traits in the mid-2000s, which allows me to look at the long-run effects on these outcomes (i.e., 40 to 50 years after the treatment). For completeness, I also consider other measures such as the locus of control, reciprocity, self-esteem, risk aversion, and trust. Extensive literature argues that personality traits and socio-emotional skills are strong predictors for labor income and other important outcomes.[22] Unfortunately, the outcomes of interest were collected only in selected SOEP waves, so that the available sample sizes are relatively small and vary between approximately 1,300 and 8,700 depending on the outcome. Appendix C provides more details on the complementary data and reports summary statistics for the estimation samples.

4 Estimation strategy

In my empirical approach, I exploit the variation in the exposure to short school years across states and birth dates. Specifically, I estimate the following equation

$$y_{ist} = \alpha SSY_{st} + \theta_s + \theta_t + \theta_f + Z'_{st}\gamma + \nu_{ist} \tag{1}$$

where y is an outcome of an individual i from state s and birth cohort t defined as a combination of birth year and month. As main outcomes, I consider lifetime earnings (in 2015 EUR or in logs) and days spent in employment. However, I also estimate age-specific regressions where the outcomes are measured annually at a given age. The main explanatory variable of interest is SSY measuring the amount of instructional time in school missed in years due to the exposure to one or two short school years. Thus, α is a dosage parameter, which accounts for treatment intensity. All regressions include state (θ_s) and birth date (θ_t) fixed effects and

[22] See, e.g., Bowles et al. (2001); Heineck and Anger (2010); Heckman and Kautz (2012); Cubel et al. (2016); Gensowski (2018); Collischon (2021). For a recent meta-analysis, see Alderotti et al. (2021).

a gender dummy (θ_f). Z represents a vector of additional policy variables, which I describe below, and ν_{ist} is an error term.

The coefficient of interest α is identified within a difference-in-differences (DiD) framework using the variation across states and birth dates.[23] Thus, the main identification assumption is that there were no other state-specific changes that could be correlated with both the introduction of short school years and the outcomes. The main threat to this assumption is that some states used the transitory period to extend compulsory schooling requirements, which potentially affected the outcomes in the opposite direction. Thus, in my main specification, Z includes a binary variable that indicates whether an individual was exposed to nine instead of eight years of compulsory schooling. Another challenge is that the shift of the start date of the school year from spring to fall automatically changed the timing of school entry for newly enrolled students. In fact, many states adjusted the cutoff rules for school enrollment to the new schedule. Thus, in my main specification, Z also includes a measure of the statutory age at school entry.

However, changes in the cutoff rules for school enrollment affect not only the age at school entry but also the size of a given enrollment cohort. While a typical enrollment cohort comprises children born over a 12-month period, a shift of the cutoff date introduces a one-time change in number of birth months contemporaneously enrolled. This might have long-run consequences, e.g., because it potentially affects the class size experienced by a given enrollment cohort over the entire school career. To account for such potentially confounding effects, I also control for the size of the enrollment cohort (measured in months) in Z. To mitigate any remaining concerns that beyond the policy change, there still could have been other factors that disproportionately affected the states over time, Section 5.2 shows that my main results hold after including further controls in Z such as time-variant state-specific student-to-teacher ratios or year of birth indicators that differ across more broadly defined geographical regions.

The estimated effect of SSY is mainly identified from the one-time introduction in 1966 and 1967.[24] Nevertheless, my empirical strategy exploits the staggered exposure to the treat-

[23]Note that α may not precisely correspond to the average effect in the population because it depends on the weight that particular states and birth cohorts carry in the entire sample (Borusyak and Jaravel, 2017; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021). However, reassuringly, in Section 5.2, I show that my results are similar across various alternative sample cuts, which suggests that weighting issues are not a major concern (Goodman-Bacon, 2021).

[24]The earlier occurrences of short school years in Baden-Wuerttemberg and Saarland (see Figure 2) apply to a minor percentage of my sample. I show that my results remain unchanged if I exclude them from the estimations.

ment across birth dates, which arises due to state-specific cut off rules for school enrollment. Recent contributions have raised concerns about the validity of staggered DiD designs if the treatment effects vary across regions or over time even if the parallel trends assumption holds (e.g., Callaway and Sant'Anna, 2020; de Chaisemartin and D'Haultfoeuille, 2020; Goodman-Bacon, 2021; Sun and Abraham, 2021). Most of the robust estimators recently proposed in the literature focus on a two-way fixed effects setting with region and cohort fixed effects and a binary treatment, which is weakly increasing over time and an absorbing state (de Chaisemartin and D'Haultfœuille, 2021; Roth et al., 2022). However, the problems also apply to setups with multi-valued discrete (or continuous) treatments, which might pose additional challenges if the responses differ across treatment intensity (e.g., Callaway et al., 2021).

To reassure that my main results from a conventional DiD estimation are not driven by a potential bias from treatment effect heterogeneity, I alternatively apply an imputation estimator in the vein of Borusyak et al. (2022). The authors suggest using the non-treated observations to construct valid contrafactual outcomes for the treated observations. Their procedure can be easily implemented in more complex setups (e.g., with non-binary and non-absorbing treatments), is transparent, possesses attractive efficiency properties, and allows for an analytic computation of conservative standard errors. Complementary, I apply the diagnostics suggested in de Chaisemartin and D'Haultfoeuille (2020) to assess the potential problem of negative weights attached to the relevant DiD comparisons between pairs of states and birth dates. Furthermore, I demonstrate that my results hold if I use a binary treatment definition, which disregards the different treatment intensity (i.e., exposure to one versus two short school years), and carefully investigate the potential effect heterogeneity across the different treatment doses. I also show that my conclusions do not change if I restrict my sample in the way that the treatment is an absorbing state (i.e., it does not switch off). Given the source of identifying variation, the standard errors are clustered at level of federal state but two-way clustering at level of state and school enrollment cohort leads to identical conclusions.

5 Results

5.1 Effects on labor market performance

I begin by investigating the lifetime effects of the short school years on labor market outcomes. Table 2 summarizes the results. Panel A documents the effects on the total sum of earnings at ages between 20 and 64. Following the literature on life-cycle effects of other educational policies (e.g., Fredriksson and Öckert, 2014; Bhuller et al., 2017), the sum of earnings includes zero-earners. The estimates are thus not biased by a potentially selective sorting into employment and capture both labor supply and wage responses to variations in the exposure to short school years. All regressions include state and birth date fixed effects, and a gender dummy.

The results in column 1 are from a simplified specification of equation 1 when the vector of other policy changes Z is omitted. The point estimate on SSY is negative but insignificant and negligible in magnitude; it translates to a reduction of lifetime earnings by 0.5% if compared to sample mean. In column 2, I additionally include an indicator for a ninth compulsory year to capture the potentially confounding effects of the parallel extensions of compulsory schooling requirements in some states, which substantially changes the conclusions. The coefficient on SSY implies that one lost year of in-school instruction decreases lifetime earnings by approximately 25K EUR or 2.8% relative to sample mean. This magnitude is not negligible because it corresponds to approximately 60% of the reduced-form effect of compulsory schooling extensions from eight to nine years estimated within the same regression.[25] The results remain nearly identical when I additionally control for the statutory age at school entry (column 3) and the size of the enrollment cohort (column 4). In column 5, I omit individuals born before 1946 and those from Saarland to omit the impacts of earlier occurrences of short school years in Baden-Wuerttemberg and Saarland (see Figure 2). Finally, in the last column, using the restricted sample, I apply the imputation estimator by Borusyak et al. (2022), which yields a somewhat stronger effect compared to the conventional DiD.

The dependent variable in panel B is the natural logarithm of lifetime earnings, which ex-

[25] Specifically, the point estimate on the indicator for a ninth compulsory schooling year is 40.012 with a standard error of 8.806. Earlier estimates of monetary returns to this compulsory schooling reform from survey data are largely imprecise and inconclusive. Pischke and von Wachter (2008) found no statistically significant wage returns, which has been both confirmed (Kamhöfer and Schmitz, 2016) and questioned (Cygan-Rehm, 2022). The most recent study finds an approximately 8% wage return to one year of compulsory schooling in Germany.

cludes zero-earners from the analysis. Again, the estimates on SSY in column 1 is close to zero and insignificant. However, it turns negative and statistically significant when I account for the accompanying policy changes. The coefficients in columns 2 through 4 imply that, conditional on employment, lifetime earnings decreased, on average, by 3% due to the exposure to short school years. Again, the last two columns yield somewhat larger effects from the restricted sample and from the imputation procedure. The patterns of employment responses in panel C mirror the evidence on earnings effects; save for column 1, I find that the short school years significantly reduced the number of days in employment by approximately 2%. Again, columns 5 and 6 confirm that my preferred results from column 4 provide conservative estimates. The estimates for prime-age outcomes measured as of ages 31 through 54 lead to very similar conclusions (see Table A.2 in Appendix A).

Overall, the results suggest that the exposure to short school years had negative consequences for labor market performance. Restricting the sample so that the identification comes solely from the one-time change in 1966/67 leads to the same conclusions (column 5). Furthermore, I find that the conventional DiD model tends to slightly underestimate the effect of interest in comparison to imputation procedure proposed in Borusyak et al. (2022), which accounts for a potential bias from treatment heterogeneity (column 6). The two approaches also provide very similar results when I assume a binary nature of the treatment and further restrict the sample so that the treatment is an absorbing state (see Table A.3 in Appendix A). Moreover, applying the diagnostics suggested in de Chaisemartin and D'Haultfoeuille (2020), I find that negative weighting issues are not a serious concern in my application and that the conventional DiD regressions are robust to heterogeneous treatment effects (see Table A.4 in Appendix A).[26]

Figure 3 plots the development of the earnings and employment effects obtained from corresponding event studies. These estimates are based on the restricted sample as in column 5 of Table 2 to avoid complications with the assignment of relative event time to birth cohorts affected by the pre-1966/67 changes in Saarland and Baden-Wuerttemberg. The graphs show the effect of the exposure to at least one short school year (defined as a binary treatment) across

[26]Specifically, for all outcomes, the sum of the negative weights attached to the treatment effect (ATT) in all the treated state and time periods is low and does not exceed 0.010. The relative number of state$\times$cohort cells with a negative weight is largest in the simplest specification (column 1) and smallest in the main specification when applied to the restricted sample (column 5). Furthermore, the summary measures ($\underline{\hat{\sigma}}_{fe}$ and $\underline{\underline{\hat{\sigma}}}_{fe}$) are very large suggesting that treatment effect heterogeneity is not a serious concern for the validity of the coefficient of interest.

birth cohorts. I define the relative event time in 12-month increments and assign $t = 1$ to the first treated cohort in each affected state, while earlier cohorts ($t \leq 0$) are not treated.[27] Thus, the estimates on the left-hand side allow for a graphical inspection of the common trends assumption. Irrespective of the outcome variable, we observe a slightly increasing pre-trend but all estimates in the the pre-treatment period are statistically insignificant. The right-hand side estimates are all negative but more pronounced and statistically significant only for the first eight event time periods ($1 \leq t \leq 8$). The effects seem to disappear thereafter, which is consistent with the treatment switching off for the later birth cohorts, who started schooling after the transitory period. The patterns for the prime-age outcomes are very similar though less precisely estimated (see Figure A.3 in Appendix A). Generally, the event studies confirm detrimental impacts of the exposure to the short school years and yield no consistent evidence of dynamic treatment effects.

Figure 4 shows how the effects on lifetime outcomes vary with the timing and the intensity of the treatment. As mentioned in Section 2, during the transitory period 1966/67, all school-aged children in the affected states were exposed to the policy. However, the total duration of the exposure depended on the grade because the graduating classes of fall 1966 and the school starters of December 1966 experienced only one shorter school year. Otherwise, the exposure spanned two consecutive grades. For comparison, the darkest (first) bars show the average effect of the exposure during grades one through nine. All estimates refer to a binary treatment definition and are related to the sample mean of the respective outcome.[28] Figure 4a suggests that the overall earnings losses are mostly driven by students affected in primary school (until grade four) and by the end of compulsory schooling (grades eight and nine). The patterns of employment effects in Figure 4b are similar. However, the corresponding confidence intervals overlap throughout, which implies that the estimates are generally under-powered and do not allow for any strong conclusions about the potential effect heterogeneity. Similarly, the effects of the exposure to only one versus two short school years do not significantly differ. The last bar (labeled "> 9") illustrates that a potential exposure beyond compulsory schooling (i.e., in higher grades of secondary school) did not generate any harm. The event studies for prime-age

[27] The event studies are based on a sample that is balanced in event time (i.e., $-4 \leq t \leq 11$), which yields 233,973 observations. However, the results are very similar when I use all 255,298 observations from the restricted sample, which is balanced for calender time (individuals born between January 1946 and December 1963).

[28] The detailed estimation results behind Figure 4 are documented in Table A.5 in Appendix A.

outcomes lead to very similar conclusions (see Figure A.4 in Appendix A).

Finally, Figure 5 investigates the development of the effects over the life cycle. Figure 5a displays the impact on age-earnings profiles. Each estimate comes from a separate linear regression of annual earnings at a given age using the full sample and my main model specification. The vertical dashed lines mark the prime-age interval 31-54, for which, the estimation samples include all individuals born between 1944 and 1963. Outside the prime-age range, the estimations miss some birth cohorts due to the time frame of the data (see Section 3.1). These results might to some extent reflect a different sample composition and thus, should be viewed with some caution. Generally, the figure confirms that the affected individuals experienced earnings losses that persist nearly over their entire occupational career. Only the point estimates at ages 20 and 21 are positive, which potentially reflect a mechanical effect from an earlier graduation. Indeed, when I re-run the analysis starting at age 14 using the Pension Insurance records (see, Figure A.5a in Appendix A), I find significant positive effects on the number of pension-relevant points earned between ages 15 and 20. This pattern confirms that the short school years speeded up the labor market entry.[29] The vast majority of the estimated effects on earnings (and pension points) later in life are negative and mostly significant. These effects remain relatively constant during prime ages and seem to extend beyond age 54 but are then less precisely estimated.[30] Figure 5b illustrates the age-specific impacts on the annual number of days spent in employment. The pattern confirms the mechanical increase in labor supply in early career, which is more clearly detectable in the the Pension Insurance data (see, Figure A.5b in Appendix A). During the prime ages, the patterns suggest lasting but rather small employment reductions, which persist until the statutory retirement age.[31]

I conclude this section by re-assessing the lifetime impacts of short school years when accumulated from age 14 onward using the Pension Insurance records. Table 3 summarizes the results. The first column begins with the estimated effect on the timing of labor market entry,

[29] Using the Pension Insurance records, I estimate that the affected students were on average nearly half a year younger upon labor market entry (see, column 1 of Table 3).

[30] The estimates for ages close to retirement could be biased if the short school years induced a different selection into early-retirement programs or affected mortality. However, I do not find any economically and statistically significant effects of the short school years on the probability of early retirement or death before the age of 55 (see Panels F and G of Table A.6 in Appendix A) or 65 (not shown to save space).

[31] The effects do not operate solely along the extensive margin as the probability of any employment at a given age is hardly affected (see Figure A.6 in Appendix A). Unfortunately, there is no information on working hours in German social security records to study the intensive margin in more detail.

which implies that affected students were nearly half a year younger while entering the labor force. The next two columns show the total impacts on the pension-relevant points and employment accumulated over ages 14 through 64. The results confirm that the total number of points stemming from labor earnings declined by 2.8%. However, as for labor supply, there is no net reduction when accumulated over the entire occupational career. In the last two columns, I cut for the age span at age 55, which leads to similar conclusions. Taken together, I find consistent evidence that despite an earlier graduation and theoretically a longer occupational career, individuals exposed to the short school years did not accumulate significantly more labor market experience over the life cycle. The initial advantage of an earlier labor market entry was nearly entirely offset by a lower labor supply later in life. Despite no effect on labor supply from a lifetime perspective, the affected individuals experienced significant monetary losses because the persistently lower earnings during prime ages eventually exceed the initial gains. The detrimental income effects extend beyond the working life due to the direct consequences for old-age pension entitlements.

5.2 Robustness analysis

This section assesses the robustness of my main findings to alternative model specifications and sample restrictions. Table 4 summarizes the results. For comparability, the top panel repeats the baseline results for lifetime and prime-age outcomes obtained from social security records.

I start with providing additional evidence on the potential bias from effect heterogeneity across the two different doses of the treatment in my main specification (e.g., Callaway et al., 2021). For this purpose, the regressions in Panel A omit individuals who were exposed to only one short school year. Reassuringly, the estimates change little. I arrive at similar conclusions when I repeat this exercise using a binary treatment definition. Taken together, the results mitigate the concern that the multi-valued nature of the treatment could threaten my main results.

Next, I estimate extended model specifications that should more flexibly capture potential differences across the states and developments over time. In panel B, I add interaction terms between the state fixed effects and month of-birth dummies to account for potentially different seasonality patterns across the states. In panel C, I augment the main specification by adding year-of-birth fixed effects that differ across more broadly defined geographical regions as suggested in Stephens and Yang (2014). For this purpose, I distinguish between north (Schleswig-Holstein, Hamburg, Lower Saxony, and Bremen) and south Germany (remaining states).[32] The regressions in panel D include state-specific and time-variant student-to-teacher ratios measured when an individual was in the 1st, 4th, and 9th grade to account for potentially different trends in school quality across the states. The extended specifications generally lead to similar conclusions. Thus, my main results are not primarily driven by unobserved state-specific factors or differential developments in contemporaneous trends across the states.

Next, I assess the robustness of my results to various changes in sample restrictions. Panel E tests whether the results change if I limit my sample to individuals born in 1947 and thereafter, thereby excluding the first three birth years from my analysis. This sample omits the compulsory schooling reform in Lower Saxony and Bremen, and the earlier short school years in Baden-Wuerttemberg as well (see Figure 2). In Panel F, I exclude the last three birth cohorts. This specification uses only pre-treatment cohorts as a control group. The relative effect sizes are comparable to the baseline results throughout. I also exclude single states from the analysis. Figure A.7 in Appendix A shows that the effects remain relatively stable and statistically significant across the various samples.

Main results crucially depend on whether I control for the parallel extensions of compulsory schooling in some states (see Table 1). Thus, the estimate on the short school years could be susceptible to any bias in the estimated effect of compulsory schooling extensions.[33] To eliminate such possibility, in the next two panels, I cut the estimation samples so that they do not include any changes in compulsory schooling requirements. Specifically, panel G excludes all individuals born before July 1952 and those from Bavaria, which was the last state that extended compulsory schooling to nine years (see Figure 2). Alternatively, panel H considers only individuals born after June 1947 and from the states Schleswig-Holstein, Hamburg, Lower-Saxony, Bremen, and Saarland, which adopted the ninth compulsory year prior to the period under study. The results are less precisely estimated due to the substantially limited sample sizes but they strongly support my main conclusions. To address the potential bias from effect heterogeneity

[32] While aggregating the West German states into broader regions might seem arbitrary, this split into northern and southern states corresponds to two (to some extent competing) fractions within the Standing Conference of the Ministers of Education during the 1950s and 1960s (see e.g., DER SPIEGEL, 1966).

[33] This effect is identified within a staggered DiD design and might suffer from a potential bias from treatment effect heterogeneity (e.g., de Chaisemartin and D'Haultfœuille, 2021; Roth et al., 2022). The potential bias could affect the estimated coefficient on *SSY*.

in the full sample, Panel I shows results from an extended specification that allows the effect of compulsory schooling extensions to vary across states and over time.[34] The estimated earnings losses due to the short school years are even larger than in my baseline specification.

To reduce the measurement error in the treatment assignment that results from limited geographical information, panel J excludes individuals who entered the social security system after the fall of the Berlin Wall (November 9, 1989). I do so to omit individuals who attended school in the former GDR and moved to West Germany after the fall of the Wall. The results remain largely unchanged. In Panel K, I use the last (instead of the first) state of residence as a proxy for an individual's state of schooling. This approach increases the measurement error in treatment variable because the determining state is now measured much later in life (on average, at age 57 instead of 24). Not surprisingly, the estimated effects decrease somewhat in magnitude suggesting that (if anything) the measurement error from interstate mobility leads to an attenuation bias in my baseline results.[35]

A remaining issue is that some students who did not experience the short school years during compulsory schooling could still have been affected beyond the ninth grade. This applies to students who attended a more advanced grade in the middle track or high school during the transitory period. Unfortunately, I cannot identify individuals who attended the middle track in the data but for them, the measurement error should be limited because this track lasted only one year longer than compulsory schooling. Nevertheless, I do observe who eventually graduated from the highest track, which required up to four years of additional school attendance. Thus, in panel L, I exclude the high school graduates,[36] which yields even somewhat stronger results.

Finally, in column 1 of Table A.7 in Appendix A, I replicate the earnings results in the Micro Census. The income measures differ across the data sets so that the magnitude of the coefficients is not directly comparable. However, the effect sizes are very similar if related to the respective sample mean. Specifically, the Micro Census yields a 2.3% decrease in the monthly net income (measured, on average, at age 57). The effect size changes little after excluding self-employed and civil servants, who are not subject to social security contributions. This is not surprising

[34]Specifically, I add interaction terms of the indicator for ninth compulsory schooling year with state dummies and linear trends in birth date.

[35]The attenuation bias is in line with no effects of the short school years on cross-state mobility (see Appendix B).

[36]This restriction could lead to an endogenous sample selection if the short school years affected high school graduation rates, which is apparently not the case (see Section 5.3 and Table 5).

given that in columns 2 and 3, I do not find a different sorting into these occupations due to the policy. Using the detailed information on the completed school degree in the Micro Census, I can additionally assign the potential exposure to the short school years beyond grade nine. Table A.8 in Appendix A reveals that the refined coding generates even larger effects, which is consistent with the results in Panel L of Table 4. Overall, the various robustness checks render credibility to my main conclusions.

5.3 Potential mechanisms and heterogeneities

To shed some light on the potential mechanisms through which the short school years negatively affected earnings and employment in the long run, I start with investigating their impact on educational attainment. From a theoretical point of view (e.g., Hanushek, 2020), the short school years might have affected a student's education production function mainly through a reduction in the input factors on the part of the school (e.g., teachers' time and attention, curriculum). However, the educational output also depends on other factors such as family inputs (e.g., parental time and support) and a student inputs (e.g., motivation and effort). While parents might have filled some gaps left by the schools, contemporary surveys do not provide consistent evidence that parental involvement increased (Meister, 1972; Thiel, 1973). There is also no evidence on how well students coped with the necessary adjustments save for some newspaper articles pointing to increased stress levels and anxiety (Landesarchiv, 2020).

Table 5 documents the estimated effects on educational attainment from social security records. The information on the highest completed degree is missing for approximately 2% of individuals in my sample but column 1 reveals that the missings are not correlated with the treatment. Thus, endogenous sample selection should not be an issue. Columns 2 and 3 imply no effects on high school graduation and the probability of obtaining a tertiary degree from a college or university, respectively. However, the last two columns suggests that the short school years prevented some students from a successful completion of other types of vocational education or training. I find similar patterns in the Micro Census (see Table A.7 in Appendix A), which also uncovers no effects on graduation rates from the basic and middle tracks. Consequently, years of schooling remained unchanged, which is broadly in line with earlier findings.[37] No effects on secondary school credentials might simply reflect more lenient practices in grading and track recommendations during the transitory period (Drewek, 2020). However, the lower levels of post-secondary education suggest that the short school years did nonetheless affect the acquisition of important skills.

While there is no data that would allow to study the cognitive development of the relevant cohorts during the critical period, using the SOEP data, I can look at their cognitive abilities assessed much later in live (in their 50s). Table 6 reports the results for the symbol correspondence and the word fluency test.[38] Each of the outcomes is measured after the first 30, 60, and 90 seconds of the test's run-time. Only the effects on the symbol correspondence test are statistically significant and imply a loss in cognition of approximately 0.25 standard deviation (SD), which is substantial. For comparison, the average learning achievement during one school year is typically estimated to be of roughly one-quarter to one-third of a SD (Werner and Woessmann, 2021). Anger and Heineck (2008) show that the symbol correspondence test is positively related to earnings of German workers even after controlling for educational attainment while verbal fluency does not matter either way. Thus, the decline in cognitive skills could be a potential channel through which the short school years impaired post-secondary education and eventually labor market outcomes. My results are also consistent with prior evidence by Hampf (2019) who found negative impacts of the short school years on numeracy skills but no effects on literacy skills (both measured at similar ages as in my SOEP sample).[39]

The short school years could have also affected other domains of human capital such as personality traits and socio-emotional skills (e.g., due to more homework-oriented learning, less interactions with school peers, less weight on extracurricular activities). Extensive research argues that personality traits respond to experiences during childhood and adolescence but remain relatively stable later in life (e.g., Almlund et al., 2011; Cobb-Clark and Schurer, 2012; Allen

[37] Pischke (2007) found increased repetition rates in aggregate school statistics and a negative effect on the probability of attending the middle track using earlier waves of the Micro Census. Nevertheless, he argues that the effects on educational paths were of a temporary nature. Recently, Grätz (2021) looks at the effects on high school graduation and confirms no effect, which also holds across socioeconomic backgrounds.

[38] The sample sizes for the two tests differ because the symbol correspondence test was performed in 2006, 2012, 2014, 2016, and 2018 and the word fluency test only in 2006, 2012, and 2016. The samples are generally small because testing was possible only within computer assisted personal interviews (CAPI), out of which the participation rate was of approximately 75%. While selective participation could bias my estimates, in auxiliary regressions, I did not find any evidence that participation was correlated with the exposure to short school years.

[39] Hampf (2019) used a much smaller sample (ca. 300 individuals) from the Survey of Adult Skills (PIAAC).

et al., 2015; Fletcher and Schurer, 2017). Table 7 shows the effects on the Big Five inventory. The estimates imply a significant decrease in extroversion (i.e., increased introversion) and a higher level of neuroticism of approximately 0.08 and 0.11 SD, respectively. I do not find any significant effects on the remaining personality traits or other socio-emotional skills available in the data (see Table A.9 in Appendix A), though the estimates generally suffer from a low statistical power. According to the APA (2021), introversion refers to the focus on inner thoughts, ideas, and feelings, rather than what's happening externally. Neuroticism describes the tendency to respond poorly to negative experiences and psychological distress. Both introversion and neuroticism seem to be negatively related to wages in Germany (Heineck and Anger, 2010; Collischon, 2020), though there are some gender-specific differences.

Generally, for the generation under study, educational choices and labor market biographies fundamentally differed by gender. Describing the striking under-representation of girls among high school graduates and university entrants in Germany in the 1960s, Van De Graaff (1967) points to the traditional social roles as inhibitors of women's academic and professional ambitions. Thus, the short school years could have generated different responses among men and women. Splitting the main estimates by gender in Table 8 uncovers that the negative labor market effects are entirely driven by men. This is reassuring because men are unlikely to be affected by a potentially selective labor force participation or endogenous fertility effects.[40] For women, the point estimates are even positive but statistically insignificant. I find similar patterns in the Micro Census and in the Pension Insurance records (see Table A.10 in Appendix A).

Table 9 documents the corresponding effects on educational credentials. Column 1 reassures that endogenous sample selection due to missing information on the outcomes in not an issue. The remaining columns confirm that the short school years did not affect educational attainment save for a lower probability of obtaining any post-secondary degree. The effect is statistically significant for men but even slightly larger in magnitude for women. The Micro Census data corroborate this conclusion (see Table A.11 in Appendix A). A potential explanation for why the educational effects carried over to the labor market only for men is that for the cohorts under study, the female labor force participation was generally low due to the prevailing social norms. Another reason could be gender-specific differences in the effects on skill formation. Indeed,

[40]Koebe and Marcus (2022) document that the short school years affected the timing of marriage and parenthood.

Table 10 suggests that the cognitive decline and the increase in neuroticism were larger for men. In contrast, women exposed to the short school years seem to have developed higher levels of conscientiousness, which is typically positively related to wages (e.g., Collischon, 2020). Although consistent with the heterogenous earnings responses, the gender-specific results on skills should be interpreted with some caution due to the small samples.

An important source of heterogeneity might also stem from the socioeconomic background of the students, e.g., if better educated parents were more likely or capable to compensate the lost in-school instruction with home schooling. Unfortunately, due to data limitations, I cannot provide any direct evidence on this issue.[41] Nevertheless, the intergenerational mobility of the socioeconomic status in Germany is relatively low (e.g., OECD, 2018), so that looking at the effects at the lower and upper tails of the income distribution might provide some insight into the potentially different responses among students from deprived and privileged households. If in-school instruction serves as an equalizer and the short school years implied more learning at home, we might expect larger disadvantages at the bottom of the income distribution. Figure A.8 in Appendix A plots the results from unconditional quantile regressions. The estimates for the lowest centiles are very imprecise and do not follow a clear pattern, but the top earners remained apparently unaffected. Splitting the sample by gender (see Figure A.9) confirms that the policy generally did not harm women. In contrast, among men, the low earners suffered most while to the right of the income distribution, the effects are close to zero. These findings suggest that the short school years could also have affected income inequality. To test this conjecture, I investigate the effects on two basic measures of income dispersion: the index of dispersion and the coefficient of variation.[42] Table A.12 in Appendix A documents an increased income dispersion among men and no significant impact on income inequality among women.

Overall, the results suggest that the short school years did not impede the acquisition of

[41] There is no information on parental background characteristics in the social security records and in the Micro Census. Although the SOEP collects information on parental education, the information is missing for nearly 15% of the relevant sample, and among the valid responses, there is only limited variation because the vast majority of mothers and fathers (ca. 80%) obtained only the basic school degree. This and the generally small sample size do not allow for a reliable analysis using sample splits by parental education.

[42] I measure the earnings dispersion within birth date-state-gender cells. The index of dispersion relates the interquartile range in earnings in each cell to the respective median value. The coefficient of variation is computed by divining the standard deviation of earnings to the corresponding mean value. The regressions are run on data aggregated by birth date, state, and gender and reweighed using the number of individuals in each cell. Both outcomes are standardized to facilitate comparisons.

school credentials but they did affect the subsequent educational paths. They also left long-lasting imprints on cognitive abilities and some labor market relevant personality traits. The adverse effects on various aspects of human capital are plausible channels for the worse labor market outcomes. The results are driven by men, for whom the policy also elevated income dispersion due to larger harm at the bottom of the income distribution.

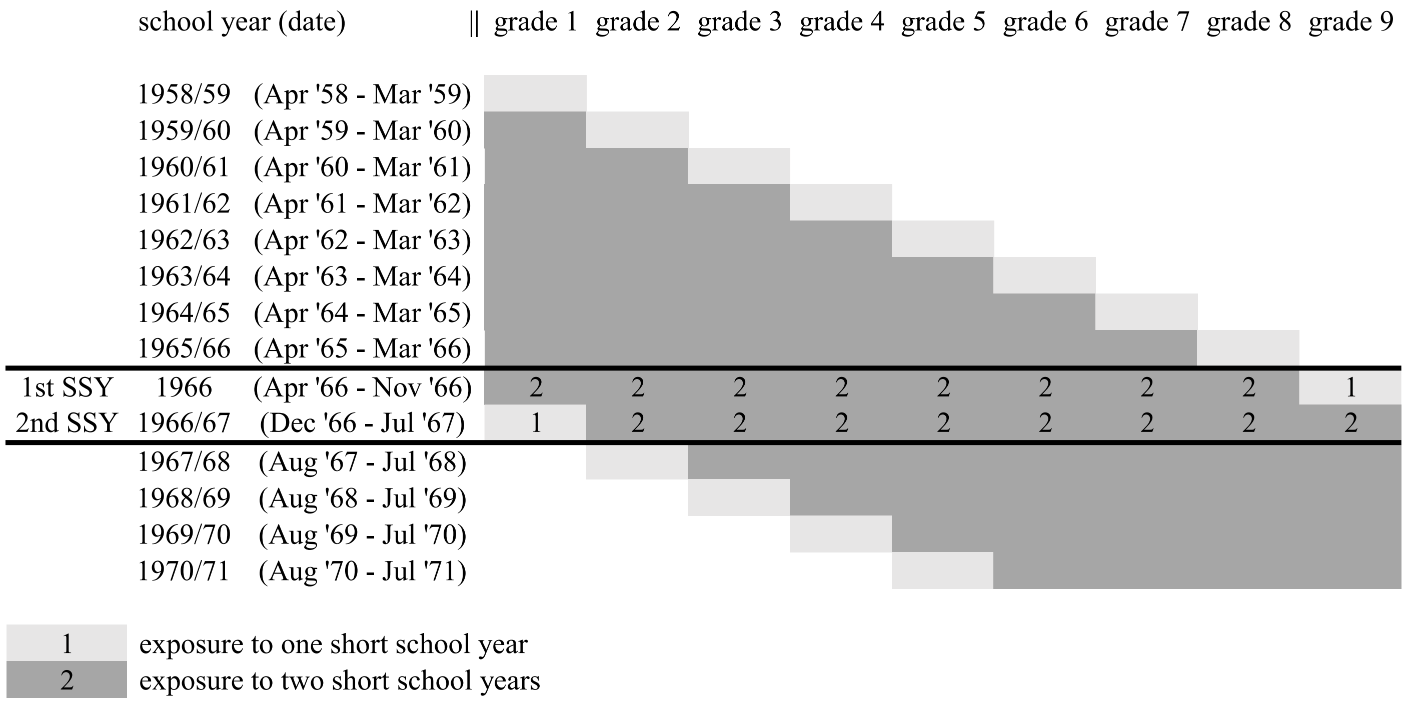
6 Conclusions

This paper investigates the lifetime effects of exposure to reduced instructional time in the classroom on earnings and employment. Specifically, I evaluate the long-run consequences of a German policy that substantially shortened the duration of two school years in the 1960s while leaving the core curriculum unaffected. The lost in-school instruction was mainly compensated by assigning additional homework in core subjects such as mathematics and German, shifting the emphasis away from other subjects, and canceling co-curricular activities. So far, the evidence on long-run effects of the short school years is scarce and inconclusive. For example, while Pischke (2007) found negative impacts on some educational outcomes but no effects on wages, Hampf (2019) documented long-lasting deficiencies in numeracy skills. The last birth cohorts of students affected by this policy are currently close to old-age retirement, which allows me to study their labor market responses from a life-cycle perspective.

Using social security records with detailed employment biographies linked to a novel data set on institutional details, I find adverse effects of the exposure to the short school years over nearly entire occupational career. My estimates imply that one year of lost instructional time in the classroom reduces lifetime earnings, on average, by nearly 3%. Assuming that a typical school year in Germany includes effectively 37 weeks of instruction, my results suggest that each month of lost in-school instruction decreases lifetime labor income by 0.3%. This is not negligible given that the policy was accompanied by a strong emphasis on maintaining the usual core curriculum. Interestingly, I do not find any significant effects on secondary school credentials, which could be due to the short-term increases in repetition rates (Pischke, 2007) and/or teachers letting the marginal students slide through (Drewek, 2020). Nevertheless, the policy had detrimental consequences for subsequent vocational education. Survey data also reveal that four to five decades after the reform, the affected students perform worse on cognitive tests and are more likely to be introverted and neurotic. Taken together, I find consistent evidence of unfavorable consequences for human capital formation. Nevertheless, only men carried these effects over to the labor market presumably due to the generally low female labor force participation. For men, the policy also elevated income dispersion because of larger harm at the bottom of the income distribution, which might reflect more severe implications of lost in-school instruction for anyway disadvantages students.

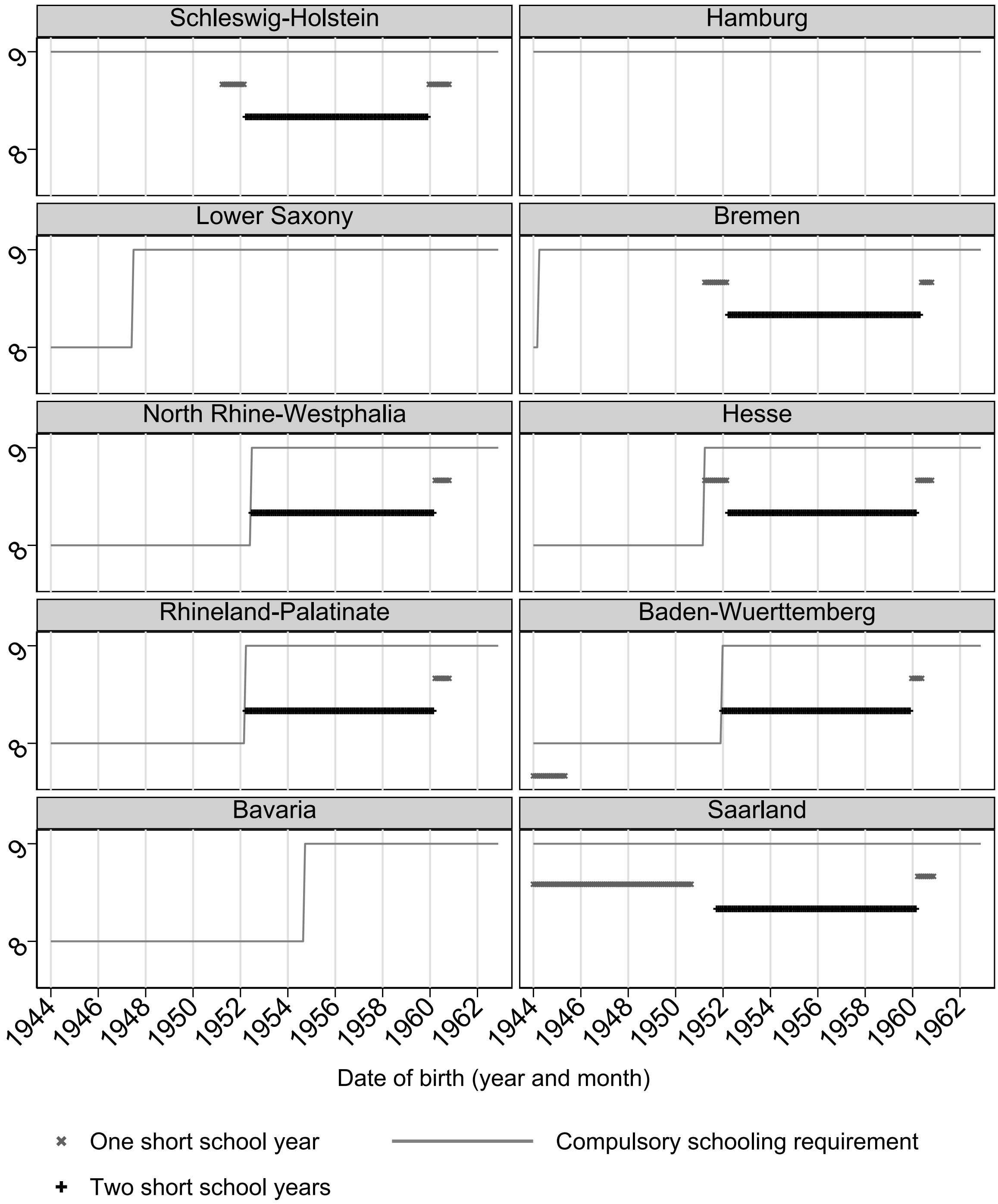
The shortened school years in the 1960s led to turbulent changes in the course of instruction and students' lives, and my results suggest that this has left persistent imprints on important skills and labour market performance. Some circumstances during the relevant period seem to resemble the recent situation during the COVID-19-related school closures (Drewek, 2020; Wößmann, 2020). Yet, the COVID-19 pandemic also led to additional shocks that extend far beyond the lost instructional time in the classroom such as economic uncertainty, social isolation, and a tangible health threat (Kuhfeld et al., 2020). All these aspects might impair the learning process and personal development either independently or through a multiplier effect. On the other hand, many governments intervened promptly by introducing various remedying interventions. Thus, it is difficult to extrapolate my results to the long-run impacts that we might expect to arise from the recent school closures. Nonetheless, more broadly, my findings call for immediate interventions to remedy any developmental disadvantages that occur when students are kept out of the classroom.

Figure 1: Exposure to the short school years 1966/67 during compulsory schooling



Note: Own illustration.

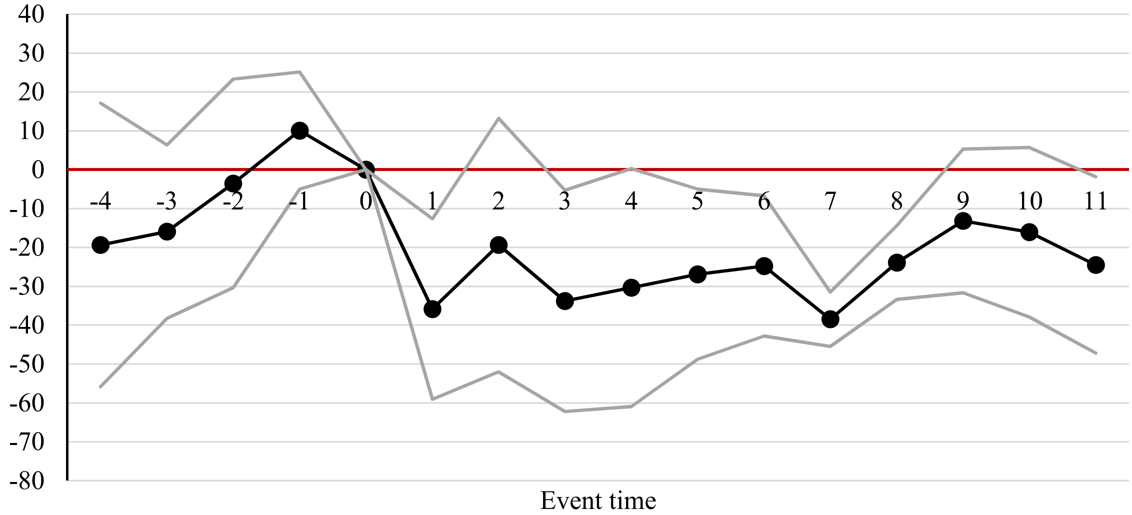
Figure 2: Exposure to short school years for children enrolled between 1950 and 1970



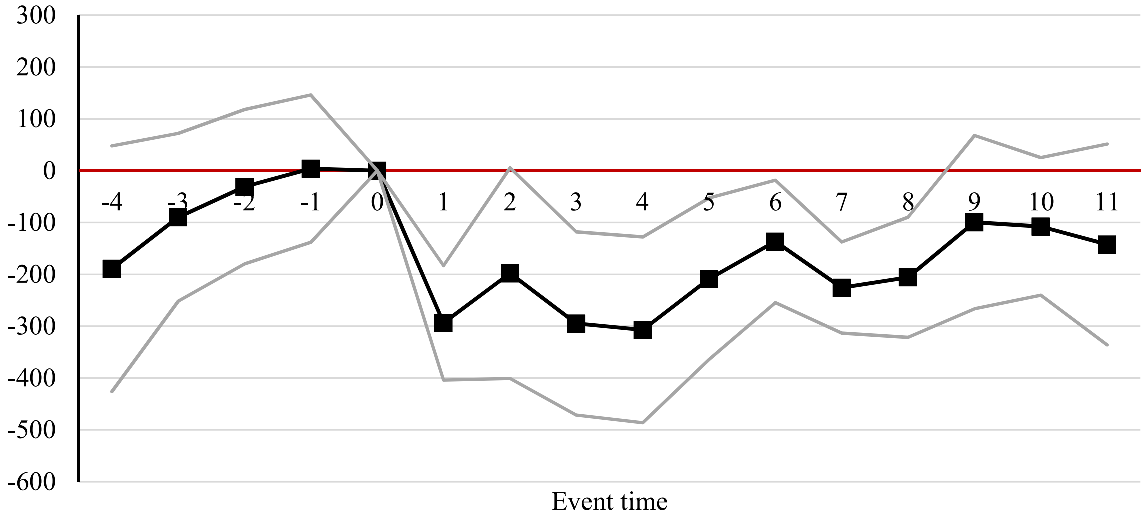
Note: The figure shows the duration of compulsory schooling depending on the exposure to short school years. For individuals exposed to one (two) short school year(s), the effective schooling duration was shorter than the statutory requirement by one-third (two-thirds) of a year. Schooling duration is calculated as a difference between the date of the earliest possible school leave (according to compulsory schooling laws and the actual end dates of the relevant school year) and the date of school enrollment (according to cutoff rules for school enrollment and the actual start dates of the relevant school year).
Source: State-specific laws from Makrolog (2019). State-specific start and end dates of school years from KMK (2020). Further details available on request.

Figure 3: Event time studies for the effect on lifetime outcomes (ages 20-64)

(a) Effects on earnings (in 1,000 EUR)



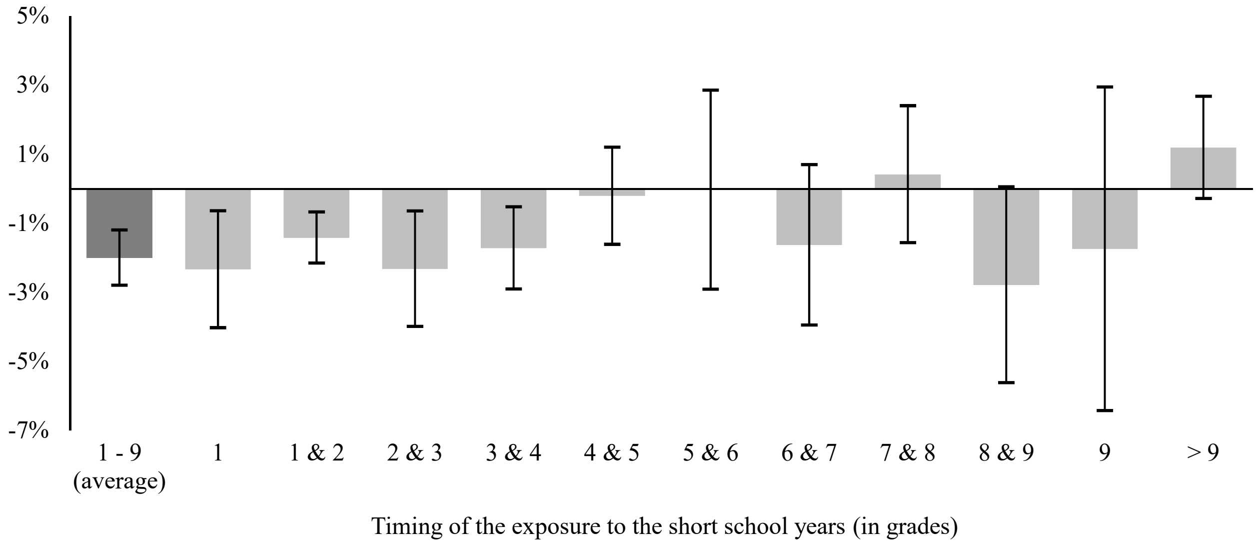
(b) Effects on the number of days in employment



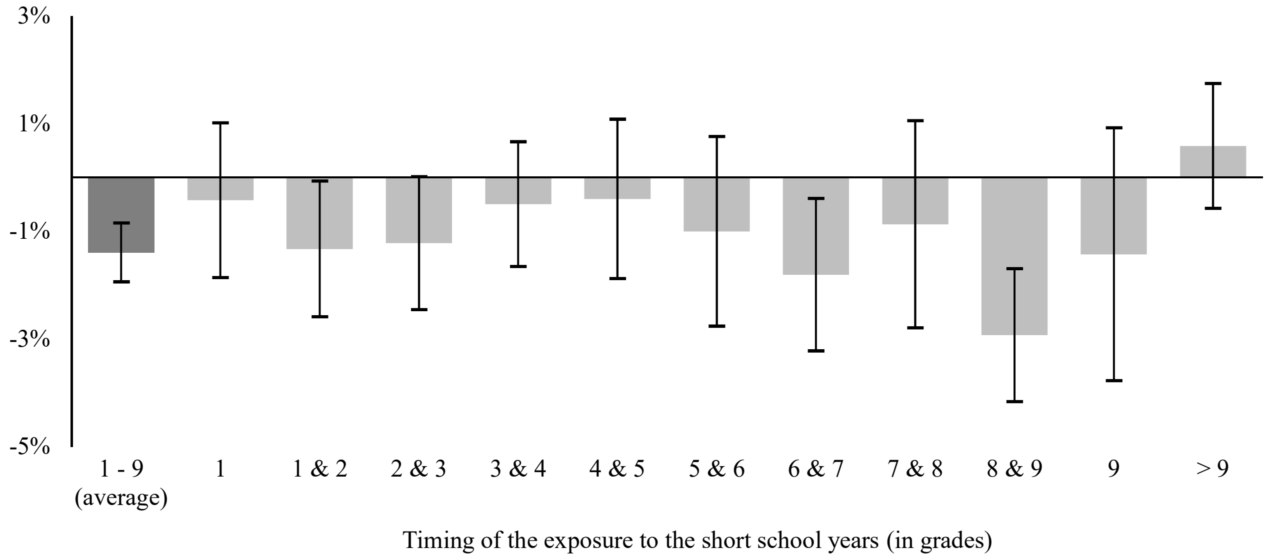
Note: The figures show the results from event time studies where the event time (t) is measured in 12-month increments. The first 12 treated birth months in each affected state are assigned $t = 1$. Each figure plots the event time estimates from a separate linear regression of the outcome on event time dummies, state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). The grey lines show 95% confidence intervals based on standard errors clustered at the state level.
Source: SIAB 1975-2017; own calculations.

Figure 4: Relative effects on lifetime outcomes (ages 20-64) depending on treatment timing

(a) Effects on earnings (in 1,000 EUR)



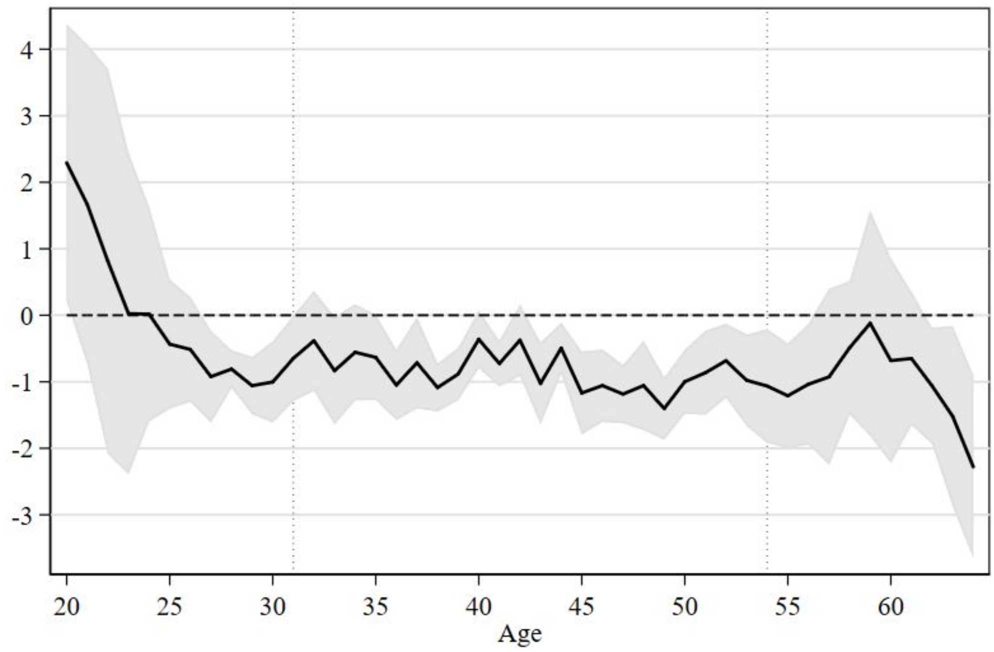
(b) Effects on the number of days in employment



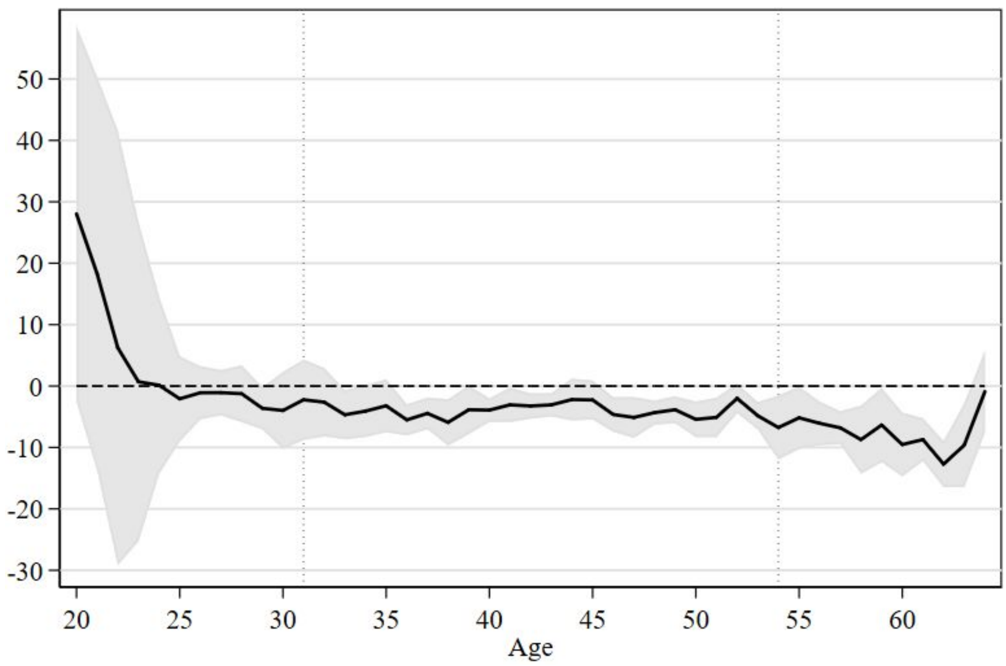
Note: The bars represent the estimated effects of the exposure to the short school years (defined as a binary treatment) relative to the mean of the outcome. The darkest bar is based on a linear regression of equation (1) where SSY is a dummy variable. The brighter bars are a based on a separate linear regression of equation (1) where SSY is split into eleven dummy variables indicating the expected grade attended at the time of the treatment. All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). The error bars show 95% confidence intervals based on standard errors clustered at the state level. The point estimates and standard errors behind the figures are reported in Table A.5 in Appendix A.
Source: SIAB 1975-2017; own calculations.

Figure 5: Effects of the short school years over the live cycle

(a) Effects on annual earnings (in EUR)



(b) Effects on the annual number of days in employment



Note: The figures plot the age-specific estimates on SSY in equation (1). Each estimate is from a separate linear regression of the outcome at a given age on state, state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Shaded areas show 95% confidence intervals based on standard errors clustered at the state level. Source: SIAB 1975-2017; own calculations.

Table 1: Start dates of the school year by state

school year	**Schleswig-Holstein**	**Hamburg**	**Lower Saxony**	**Bremen**	**North Rhine-Westphalia**	**Hesse**	**Rhineland-Palatinate**	**Baden-Wurttemberg**	**Bavaria**	**Saarland**
since 1922	spring	spring	spring	spring	spring	spring	spring	spring	spring	spring
Nazi regime	fall	fall	fall	fall	fall	fall	fall	fall	fall	fall
1945 - 1947	spring	spring	fall	fall	fall	fall	fall	fall	fall	fall
1948 - 1949	spring	spring	spring	spring	spring	spring	fall	fall	fall	fall
1950 - 1951	spring	spring	spring	spring	spring	spring	spring	fall	fall	fall
1952 - 1956	spring	spring	spring	spring	spring	spring	spring	spring	fall	fall
1957 - 1965	spring	spring	spring	spring	spring	spring	spring	spring	fall	spring
1966	spring	spring	spring	spring	spring	spring	spring	spring	fall	spring
1966/67	Dec		Dec	Dec	Dec	Dec	Dec	Dec		Dec
since 1967	fall	fall	fall	fall	fall	fall	fall	fall	fall	fall

Source: The information until 1965 is from "Umstellung von Ostern auf Herbstbeginn: Kurzschuljahr zehrt an der neunten Klasse" by Horst-Dieter Schiele in Mannheimer Morgen Nr. 51 from March 3, 1966. Since 1966, the details are from state-specific laws (Makrolog, 2019) and dates of school vacations (KMK, 2020).

Table 2: Lifetime effects on labor market outcomes (ages 20-64)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample				Restricted sample	
Panel A: Earnings (in 1,000 EUR as of 2015)						
SSY	-4.187	-24.948***	-24.419***	-24.304***	-25.756***	-30.421***
	(6.038)	(8.288)	(8.560)	(8.734)	(7.746)	(5.605)
	[-0.5%]	[-2.8%]	[-2.7%]	[-2.7%]	[-2.9%]	[-3.4%]
Mean dep.	888.496				896.972	
Obs.	278,797				255,298	
Panel B: Log earnings						
SSY	0.003	-0.030**	-0.030**	-0.030**	-0.044***	-0.047***
	(0.011)	(0.015)	(0.015)	(0.015)	(0.016)	(0.009)
Mean dep.	13.142				13.161	
Obs.	276,854				253,451	
Panel C: Employment (in days)						
SSY	-73.331	-172.881***	-175.316***	-175.856***	-193.410***	-203.925***
	(54.921)	(57.600)	(58.447)	(57.827)	(50.117)	(25.704)
	[-0.9%]	[-2.0%]	[-2.0%]	[-2.1%]	[-2.3%]	[-2.4%]
Mean dep.	8560.277				8668.693	
Obs.	278,797				255,298	
Ninth compulsory year	no	yes	yes	yes	yes	yes
Age at school entry	no	no	yes	yes	yes	yes
Enrollment cohort size	no	no	no	yes	yes	yes
BJS estimator	no	no	no	no	no	yes

Note: Each cell is based on a separate linear regression of equation (1). All regressions include state, state and birth date fixed effects, and a gender dummy. Standard errors in parentheses are clustered at the state level. The estimated effect relative to the mean of the outcome is reported in brackets. SSY = short school year. Restricted sample omits individuals born before 1946 and those from Saarland. BJS estimator uses the imputation procedure by Borusyak et al. (2022).
Source: SIAB 1975-2017; own calculations.

Table 3: Evidence on lifetime effects from the Pension Insurance records

	(1) Age at labor market entry	(2) Pension points (total sum over ages 14 - 64)	(3) Employment (total sum over ages 14 - 64)	(4) Pension points (total sum over ages 14 - 55)	(5) Employment (total sum over ages 14 - 55)
SSY	-0.431 ***	-0.628 **	66.501	-0.625 **	52.171
	(0.119)	(0.255)	(108.183)	(0.212)	(94.106)
	[-2.4%]	[-2.8%]	[0.8%]	[-3.0%]	[0.7%]
Mean dep.	18.225	22.161	8,080.162	20.837	7,634.434
Obs.			52,970		

Note: Pension points refer to the statutory points stemming from labor market earnings that determine future pension entitlements. Employment is measured in days. Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level. The estimated effect relative to the respective sample mean of the outcome is reported in brackets. SSY = schort school years
Source: VSKT-SUFs 2004-2018; own calculations.

Table 4: Sensitivity analysis

	Lifetime (ages 20-64)		Prime-age (ages 31-54)	
	earnings	employment	earnings	employment
Baseline	-24.304 ***	-175.856 ***	-17.609 ***	-77.487 **
(Obs. 278,797)	(8.734)	(57.827)	(5.830)	(31.898)
	[-2.7%]	[-2.1%]	[-2.8%]	[-1.4%]
A: Excl. if exposed to one SSY	-22.145 **	-188.307 ***	-16.254 ***	-88.188 **
(Obs. 266,218)	(9.057)	(58.001)	(6.113)	(36.151)
	[-2.5%]	[-2.2%]	[-2.6%]	[-1.6%]
B: Add birth month FE x state FE	-24.920 ***	-178.670 ***	-17.996 ***	-79.001 **
(Obs. 278,797)	(8.688)	(56.247)	(5.898) ***	(31.029)
	[-2.8%]	[-2.1%]	[-2.9%]	[-1.4%]
C: Add north x birth year FE	-22.424 ***	-173.500 ***	-16.107 **	-85.948 ***
(Obs. 278,797)	(7.822)	(49.970)	(4.220)	(21.870)
	[-2.5%]	[-2.0%]	[-2.6%]	[-1.5%]
D: Add student-to-teacher ratios	-25.137 **	-202.572 ***	-17.368 ***	-88.826 **
(Obs. 278,797)	(10.115)	(57.874)	(6.632)	(34.542)
	[-2.8%]	[-2.4%]	[-2.8%]	[-1.6%]
E: Born 1947-1963	-28.165 ***	-200.887 ***	-21.582 ***	-112.492 ***
(Obs. 228,099)	(7.560)	(58.580)	(4.726)	(31.156)
	[-3.1%]	[-2.3%]	[-3.4%]	[-2.0%]
F: Born 1944-1960	-24.806 *	-174.745 **	-14.828 *	-52.219
(Obs. 225,524)	(12.701)	(84.896)	(8.580)	(42.700)
	[-2.8%]	[-2.0%]	[-2.4%]	[-0.9%]
G: Born after June 1952 & w/o Bavaria	-27.958 ***	-174.408 **	-23.499 ***	-81.505 ***
(Obs. 146,018)	(8.960)	(80.544)	(8.922)	(26.386)
	[-3.1%]	[-2.0%]	[-3.7%]	[-1.4%]
H: Born after June 1947 & only S-H, HH,	-41.121 ***	-186.620	-19.670 **	-97.114
Bremen, Lower-Saxony, and Saarland	(8.390)	(139.679)	(7.954)	(97.624)
(Obs. 51,804)	[-4.8%]	[-2.2%]	[-3.3%]	[-1.7%]
I: $C9$ effect varies across states	-38.767 ***	-143.931 *	-27.515 ***	-113.546 ***
and over time (Obs. 278,797)	(11.692)	(85.584)	(8.687)	(40.431)
	[-4.4%]	[-1.7%]	[-4.4%]	[-2.0%]
J: Entered before the fall of Berlin Wall	-24.285 ***	-169.952 ***	-17.451 ***	-70.900 **
(Obs. 251,538)	(8.172)	(56.167)	(5.805)	(27.659)
	[-2.6%]	[-1.9%]	[-2.6%]	[-1.2%]
K: Last state observed as proxy for	-18.269 ***	-141.207 ***	-18.874 ***	-52.127 *
state of schooling (Obs. 279,871)	(6.654)	(49.883)	(4.478)	(28.776)
	[-2.1%]	[-1.7%]	[-2.1%]	[-0.9%]
L: W/o high school graduates	-29.301 ***	-221.455 ***	-19.448 ***	-93.365 **
(Obs. 214,616)	(10.592)	(68.938)	(6.469)	(37.969)
	[-3.7%]	[-2.5%]	[-3.6%]	[-1.7%]

Note: Earnings are measured in 1,000 EUR and employment in days. Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling (save for Panel G and H), statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level. The estimated effect relative to the respective sample mean of the outcome is reported in brackets. SSY = schort school years, $C9$ = ninth compulsory schooling year, FE = fixed effects, S-H = Schleswig-Holstein, HH = Hamburg.
Source: SIAB 1975-2017; own calculations.

Table 5: Effects on highest educational attainment

	(1) Missing information	(2) High school degree	(3) College/Univ. degree	(4) Vocational degree	(5) Any post-secondary
SSY	-0.000	0.009	0.004	-0.014 *	-0.011 **
	(0.002)	(0.006)	(0.005)	(0.008)	(0.005)
Mean dep.	0.019	0.270	0.189	0.760	0.925
Obs.	278,797	271,496	271,496	271,496	271,496

Note: Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level. The estimated effect relative to the respective sample mean of the outcome is reported in brackets.
Source: SIAB 1975-2017; own calculations.

Table 6: Effects on performance in cognitive tests

	(1)	(2)	(3)	(4)	(5)	(6)
	Symbol correspondence test			Word fluency test		
	30 sec	60 sec	90 sec	30 sec	60 sec	90 sec
SSY	-0.251 ***	-0.255 ***	-0.237 ***	-0.025	0.035	0.023
	(0.069)	(0.077)	(0.078)	(0.089)	(0.064)	(0.082)
Mean age		55.3			51.0	
Obs.		2,930			1,252	

Note: The outcome variables are standardized. Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, a gender dummy, age at interview (linear and quadratic), indicators for survey year, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level.
Source: SOEP 1984-2019 (v36); own calculations.

Table 7: Effects on personality traits (Big Five)

	(1) Openness	(2) Conscien-tiousness	(3) Extra-version	(4) Agree-ableness	(5) Neuro-ticism
SSY	0.016	0.001	-0.075 *	0.004	0.108 **
	(0.054)	(0.070)	(0.045)	(0.122)	(0.048)
Mean age			54.2		
Obs.			8,651		

Note: See Table 6.
Source: SOEP 1984-2019 (v36); own calculations.

Table 8: Gender-specific effects on labor market outcomes

	Lifetime outcomes (ages 20-64)			Prime-age outcomes (ages 31-54)		
	earnings (in 1,000 EUR)	log earnings	employment (in days)	earnings (in 1,000 EUR)	log earnings	employment (in days)
Men	-49.121 ***	-0.070 ***	-270.067 ***	-34.676 ***	-0.056 **	-145.665 ***
	(12.835)	(0.023)	(70.565)	(10.896)	(0.026)	(33.217)
	[-4.1%]		[-2.9%]	[-4.0%]		[-2.4%]
Mean dep.	1203.396	13.559	9198.421	866.355	13.154	6165.763
Obs.	142,996	142,180	142,996	142,996	142,180	142,996
Women	1.226	0.016	-66.144	0.002	0.028	3.202
	(6.105)	(0.022)	(101.110)	(3.694)	(0.026)	(47.311)
	[0.2%]		[-0.8%]	[0.0%]		[0.1%]
Mean dep.	556.852	12.702	7887.756	370.336	12.127	5100.012
Obs.	135,801	133,182	135,801	135,801	133,182	135,801

Note: Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level. The estimated effect relative to the respective sample mean of the outcome is reported in brackets.
Source: SIAB 1975-2017; own calculations.

Table 9: Gender-specific effects on highest educational attainment

	(1) Missing information	(2) High school degree	(3) College/Univ. degree	(4) Vocational degree	(5) Any post-secondary
Men	0.002	0.010	-0.003	-0.005	-0.008 **
	(0.004)	(0.009)	(0.006)	(0.006)	(0.003)
Mean dep.	0.019	0.270	0.189	0.760	0.950
Obs.	142,996	140,251	140,251	140,251	140,251
Women	-0.002	0.009	0.011	-0.024	-0.013
	(0.002)	(0.009)	(0.007)	(0.015)	(0.009)
Mean dep.	0.034	0.200	0.108	0.790	0.899
Obs.	135,801	131,245	131,245	131,245	131,245

Note: Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level.
Source: SIAB 1975-2017; own calculations.

Table 10: Gender-specific effects on skills

	(1) Symbol correspondence test	(2) Openness	(3) Conscientiousness	(4) Extraversion	(5) Agreeableness	(6) Neuroticism
Men	-0.230 *	0.002	-0.097	-0.107	0.000	0.168 **
	(0.128)	(0.078)	(0.105)	(0.073)	(0.144)	(0.072)
Mean age	55.5			53.8		
Obs.	1,366			4,364		
Women	-0.149	0.041	0.123 **	-0.002	-0.002	0.050
	(0.099)	(0.076)	(0.062)	(0.041)	(0.137)	(0.056)
Mean age	55.1			54.6		
Obs.	1,564			4,287		

Note: The outcome variables are standardized. Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, age at interview (linear and quadratic), indicators for survey year, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level.
Source: SOEP 1984-2019 (v36); own calculations.

References

Ager, P., K. Eriksson, E. Karger, P. Nencka, and M. A. Thomasson (2020). School Closures During the 1918 Flu Pandemic. NBER Working Paper 28246, National Bureau of Economic Research, Cambridge, MA.

Alderotti, G., C. Rapallini, and S. Traverso (2021). The big five personality traits and earnings: A meta-analysis. GLO Discussion Paper No. 902, Global Labor Organization (GLO), Essen.

Allen, M. S., S. A. Vella, and S. Laborde (2015). Sport participation, screen time, and personality trait development during childhood. *British Journal of Developmental Psychology 33*(3), 375–390.

Almlund, M., A. L. Duckworth, J. Heckman, and T. Kautz (2011). Chapter 1 - Personality Psychology and Economics. In E. A. Hanushek, S. J. Machin, and L. Woessmann (Eds.), *Handbook of the Economics of Education*, Volume 4, pp. 1–181. Amsterdam: Elsevier.

Andrew, A., S. Cattan, M. Costa Dias, C. Farquharson, L. Kraftman, S. Krutikova, A. Phimister, and A. Sevilla (2020). Inequalities in Children's Experiences of Home Learning during the COVID-19 Lockdown in England. *Fiscal Studies 41*(3), 653–683.

Anger, S., H. Dietrich, A. Patzina, M. Sandner, A. Lerche, S. Bernhard, and C. Toussaint (2020). School closings during the COVID-19 pandemic: findings from German high school students. IAB-Forum May 2020, Institute for Employment Research of the Federal Employment Agency (IAB), Nuremberg.

Anger, S. and G. Heineck (2008). Cognitive abilities and earnings–first evidence for germany. *Applied Economics Letters 17*(7), 699–702.

Antoni, M., A. Schmucker, S. Seth, and P. vom Berge (2019). Sample of Integrated Labour Market Biographies (SIAB) 1975 - 2017. FDZ data report, 02/2019 (en), Nürnberg, DOI:10.5164/IAB.FDZD.1902.en.v1.

APA (2021). APA Dictionary of Psychology. Digital version. Available online at https://dictionary.apa.org/ [Last accessed: 03.11.2021], American Psychological Association (APA), Washington, DC.

Artelt, C., I. Wolter, L. Nusser, M. Attig, and S. Fackler (2020). Corona-bedingte Schulschließungen... und nun funktioniert alles digital? NEPS Corona & Bildung Bericht Nr. 1, September 2020, Leibniz-Institut für Bildungsverläufe e.V., Bamberg.

Bacher-Hicks, A., J. Goodman, and C. Mulhern (2021). Inequality in household adaptation to schooling shocks: Covid-induced online learning engagement in real time. *Journal of Public Economics 193*, 104345.

Baker, M. (2013). Industrial actions in schools: strikes and student achievement. *Canadian Journal of Economics 46*(3), 1014–1036.

Bansak, C. and M. Starr (2021). COVID-19 shocks to education supply: How 200,000 US households dealt with the sudden shift to distance learning. *Review of Economics of the Household 19*(1), 63–90.

Belot, M. and D. Webbink (2010). Do teacher strikes harm educational attainment of students? *Labour 24*(4), 391–406.

Bhuller, M., M. Mogstad, and K. G. Salvanes (2017). Life-cycle earnings, education premiums, and internal rates of return. *Journal of Labor Economics 35*(4), 993–1030.

Blossfeld, H.-P. and H.-G. Roßbach (Eds.) (2019). *Education as a Lifelong Process-The German National Educational Panel Study (NEPS). Edition ZfE (2nd ed.).* Heidelberg: Springer VS.

Borusyak, K. and X. Jaravel (2017). Revisiting Event Study Designs, with an Application to the Estimation of the Marginal Propensity to Consume. Working paper.

Borusyak, K., X. Jaravel, and J. Spiess (2022). Revisiting event study designs: Robust and efficient estimation.

Bowles, S., H. Gintis, and M. Osborne (2001). The determinants of earnings: A behavioral approach. *Journal of economic literature 39*(4), 1137–1176.

Callaway, B., A. Goodman-Bacon, and P. H. C. Sant'Anna (2021). Difference-in-differences with a continuous treatment.

Callaway, B. and P. H. Sant'Anna (2020). Difference-in-differences with multiple time periods. *Journal of Econometrics*.

Card, D., J. Heining, and P. Kline (2013). Workplace heterogeneity and the rise of West German wage inequality. *The Quarterly Journal of Economics 128*(3), 967–1015.

Cobb-Clark, D. A. and S. Schurer (2012). The stability of big-five personality traits. *Economics Letters 115*(1), 11–15.

Collischon, M. (2020). The returns to personality traits across the wage distribution. *Labour 34*(1), 48–79.

Collischon, M. (2021). Personality traits as a partial explanation for gender wage gaps and glass ceilings. *Research in Social Stratification and Mobility 73*, 100596.

Contini, D., M. L. Di Tommaso, C. Muratori, D. Piazzalunga, L. Schiavon, et al. (2021). The covid-19 pandemic and school closure: Learning loss in mathematics in primary education. IZA Discussion Paper 14785, Institute of Labor Economics (IZA), Bonn.

Cubel, M., A. Nuevo-Chiquero, S. Sanchez-Pages, and M. Vidal-Fernandez (2016). Do personality traits affect productivity? evidence from the laboratory. *The Economic Journal 126*(592), 654–681.

Cygan-Rehm, K. (2022). Are there no wage returns to compulsory schooling in Germany? A reassessment. *Journal of Applied Econometrics 37*(1), 218–223.

Cygan-Rehm, K. and M. Maeder (2013). The effect of education on fertility: Evidence from a compulsory schooling reform. *Labour Economics 25*, 35–48.

Dahmann, S. C. (2017). How does education improve cognitive skills? Instructional time versus timing of instruction. *Labour Economics 47*, 35–47.

Dauth, W. and J. Eppelsheimer (2020). Preparing the sample of integrated labour market biographies (SIAB) for scientific analysis: a guide. *Journal for Labour Market Research 54*(1), 1–14.

de Chaisemartin, C. and X. D'Haultfoeuille (2020). Two-way fixed effects estimators with heterogeneous treatment effects. *American Economic Review 110*(9), 2964–96.

de Chaisemartin, C. and X. D'Haultfœuille (2021). Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey.

DER SPIEGEL (1966). Schuljahr: Grenze des Erträglichen. Nr. 4/1966. Available online at https://magazin.spiegel.de/epubdelivery/spiegel/pdf/46265355 [last acsessed: 17.12.2020], SPIEGEL-Verlag, Hamburg.

DESTATIS (2021). Statistisches Jahrbuch für die Bundesrepublik Deutschland. Digital version. Available online at http://resolver.sub.uni-goettingen.de/purl?PPN514402342 [Last accessed: 05.03.2021], Hrsg. Statistisches Bundesamt (DESTATIS), Stuttgart.

DIE ZEIT (1966). Elfmal eins macht eins. Der mühsame Weg der Bundesländer zu einheitlichem Schulbeginn. Nr. 42/1966. Available online at https://www.zeit.de/1966/42/elfmal-eins-macht-eins [last acsessed: 17.12.2020], ZEIT ONLINE GmbH, Hamburg.

Drewek, P. (2020). Bildungsdefizite coronabedingter Schulschließungen? Eine bildungshistorische Analyse. ZEW Discussion Papers 20-073, ZEW - Leibniz Centre for European Economic Research, Mannheim.

Dustmann, C., J. Ludsteck, and U. Schönberg (2009). Revisiting the German wage structure. *The Quarterly Journal of Economics 124*(2), 843–881.

Engzell, P., A. Frey, and M. D. Verhagen (2021). Learning loss due to school closures during the covid-19 pandemic. *Proceedings of the National Academy of Sciences 118*(17).

Fischer, M., M. Karlsson, T. Nilsson, and N. Schwarz (2020). The long-term effects of long terms–compulsory schooling reforms in sweden. *Journal of the European Economic Association 18*(6), 2776–2823.

Fitzenberger, B., A. Osikominu, and R. Völter (2006). Imputation rules to improve the education variable in the iab employment subsample. *Schmollers Jahrbuch: Journal of Applied Social Science Studies/Zeitschrift für Wirtschafts-und Sozialwissenschaften 126*(3), 405–436.

Fletcher, J. M. and S. Schurer (2017). Origins of adulthood personality: The role of adverse childhood experiences. *The BE Journal of Economic Analysis & Policy 17*(2).

Fredriksson, P. and B. Öckert (2014). Life-cycle effects of age at school start. *The Economic Journal 124*(579), 977–1004.

Gensowski, M. (2018). Personality, iq, and lifetime earnings. *Labour Economics 51*, 170–183.

Goebel, J., M. M. Grabka, S. Liebig, M. Kroh, D. Richter, C. Schröder, and J. Schupp (2019). The german socio-economic panel (soep). *Jahrbücher für Nationalökonomie und Statistik 239*(2), 345–360.

Goodman, J. (2014). Flaking out: Student absences and snow days as disruptions of instructional time. NBER Working Paper 20221, National Bureau of Economic Research (NBER), Cambridge, MA.

Goodman-Bacon, A. (2021). Difference-in-differences with variation in treatment timing. *Journal of Econometrics 225*(2), 254–277.

Grätz, M. (2021). Does more schooling lead to less or more inequality of educational opportunity?

Grewenig, E., P. Lergetporer, K. Werner, L. Woessmann, and L. Zierow (2021). COVID-19 and educational inequality: How school closures affect low- and high-achieving students. *European Economic Review 140*, 103920.

Halloran, C., R. Jack, J. C. Okun, and E. Oster (2022). Pandemic Schooling Mode and Student Test Scores: Evidence from US States. *American Economic Review: Insights* (forthcoming).

Hammerstein, S., C. König, T. Dreisörner, and A. Frey (2021). Effects of COVID-19-Related School Closures on Student Achievement - A Systematic Review. PsyArXiv Preprint, 6 June 2021, doi:10.3389/fpsyg.2021.746289.

Hampf, F. (2019). The effect of compulsory schooling on skills: Evidence from a reform in germany. Ifo Working Paper No. 313, Munich.

Hanushek, E. A. (2020). Chapter 13 - Education Production Functions. In S. Bradley and C. Green (Eds.), *The Economics of Education (Second Edition): A Comprehensive Overview*, pp. 161–170. London: Academic Press.

Hanushek, E. A. and L. Woessmann (2020). The economic impacts of learning losses. OECD Education Working Papers, No. 225, Paris.

Heckman, J. J. and T. Kautz (2012). Hard evidence on soft skills. *Labour economics 19*(4), 451–464.

Heineck, G. and S. Anger (2010). The returns to cognitive abilities and personality traits in Germany. *Labour Economics 17*(3), 535–546.

Helbig, M. and R. Nikolai (2015). *Die Unvergleichbaren: der Wandel der Schulsysteme in den deutschen Bundesländern seit 1949*. Bad Heilbrunn: Verlag Julius Klinkhardt.

Huebener, M., S. Kuger, and J. Marcus (2017). Increased instruction hours and the widening gap in student performance. *Labour Economics 47*, 15–34.

Huebener, M., C. K. Spieß, and S. Zinn (2020). SchülerInnen in Corona-Zeiten: Teils deutliche Unterschiede im Zugang zu Lernmaterial nach Schultypen und-trägern. *DIW Wochenbericht 87*(47), 865–875.

Jaume, D. and A. Willén (2019). The long-run effects of teacher strikes: evidence from Argentina. *Journal of Labor Economics 37*(4), 1097–1139.

Kamhöfer, D. A. and H. Schmitz (2016). Reanalyzing zero returns to education in Germany. *Journal of Applied Econometrics 31*(5), 865–872.

KMK (2020). Archiv der Ferienregelungen. Available online at https://www.kmk.org/service/ferien/archiv-der-ferientermine.html [last acsessed: 17.12.2020], Ständige Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland (KMK), Berlin.

Koebe, J. and J. Marcus (2022). The length of schooling and the timing of family formation. *CESifo Economic Studies 68*(1), 1–45.

Kornadt, H.-J. and H. Meister (1970). Kurzschuljahre und Schulleistungen in der Grundschule. *Bildung und Erziehung 23*, 321–333.

Kuhfeld, M., J. Soland, B. Tarasawa, A. Johnson, E. Ruzek, and J. Liu (2020). Projecting the potential impact of COVID-19 school closures on academic achievement. *Educational Researcher 49*(8), 549–565.

Landesarchiv (2020). Bestände der Ministerien und anderer zentraler Dienststellen seit 1945. Signatur EA1/106 Bü 808 und EA1/106 Bü 818. Landesarchiv Baden-Württemberg, Hauptstaatsarchiv Stuttgart.

Lavy, V. (2015). Do differences in schoolsâ€™ instruction time explain international achievement gaps? evidence from developed and developing countries. *The Economic Journal 125*(588), F397–F424.

Leschinsky, A. and P. M. Roeder (1980). Didaktik und Unterricht in der Sekundarschule I seit 1950 - Entwicklung der Rahmenbedingungen. In J. Baumert, A. Leschinsky, J. Naumann, J. Raschert, and P. Siewert (Eds.), *Bildung in der Bundesrepublik Deutschland - Daten und Analysen, Band 1: Entwicklungen seit 1950*, Chapter 4, pp. 283–392. Stuttgart: Klett-Cotta.

Lüdemann, E. and G. Schwerdt (2013). Migration background and educational tracking. *Journal of Population Economics 26*(2), 455–481.

Makrolog (2019). Online-Plattform für amtliche Verkündungsblätter. Available online at https://www1.recht.makrolog.de [last acsessed: 20.12.2019], Recht für Deutschland GmbH, Wiesbaden.

Maldonado, J. E. and K. De Witte (2021). The effect of school closures on standardised student test outcomes. *British Educational Research Journal*.

Marcotte, D. E. and S. W. Hemelt (2008). Unscheduled school closings and student performance. *Education Finance and Policy 3*(3), 316–338.

Marcus, J., S. Reif, A. Wuppermann, and A. Rouche (2020). Increased instruction time and stress-related health problems among school children. *Journal of Health Economics 70*, 102256.

Meister, H. (1972). Die Unangemessenheit des Anfangsunterrichts in der Grundschule. *Vergleichende Untersuchung des Einflusses der Kurzschuljahre auf Schulleistungen*.

OECD (2018). *A Broken Social Elevator? How to Promote Social Mobility*. OECD Publishing, Paris.

OECD (2020). Inflation (CPI) (indicator). doi: 10.1787/eee82e6e-en. [last acsessed: 21.10.2020].

Parinduri, R. A. (2014). Do children spend too much time in schools? Evidence from a longer school year in Indonesia. *Economics of Education Review 41*, 89–104.

Piopiunik, M. (2014). Intergenerational transmission of education and mediating channels: Evidence from a compulsory schooling reform in Germany. *Scandinavian Journal of Economics 116*(3), 878–907.

Pischke, J.-S. (2007). The impact of length of the school year on student performance and earnings: Evidence from the German short school years. *Economic Journal 117*(523), 1216–1242.

Pischke, J.-S. and T. von Wachter (2008). Zero returns to compulsory schooling in Germany: Evidence and interpretation. *Review of Economics and Statistics 90*(3), 592–598.

Rivkin, S. G. and J. C. Schiman (2015). Instruction time, classroom quality, and academic achievement. *The Economic Journal 125*(588), F425–F448.

Roth, J., P. H. C. Sant'Anna, A. Bilinski, and J. Poe (2022). What's trending in difference-in-differences? a synthesis of the recent econometrics literature.

Sacerdote, B. (2012). When the saints go marching out: Long-term outcomes for student evacuees from Hurricanes Katrina and Rita. *American Economic Journal: Applied Economics 4*(1), 109–35.

Stephens, M. and D.-Y. Yang (2014). Compulsory education and the benefits of schooling. *American Economic Review 104*(6), 1777–1792.

Sun, L. and S. Abraham (2021). Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Journal of Econometrics 225*(2), 175–199.

Thiel, B. (1973). *Die Auswirkung verkürzter Unterrichtszeit auf die Schulleistung: Untersuchung zur Problematik der Kurzschuljahre*. Doctoral dissertation, Eberhard-Karls-Universitaät Tübingen.

Thompson, P. N. (2021). Is four less than five? Effects of four-day school weeks on student achievement in Oregon. *Journal of Public Economics 193*, 104308.

UNESCO (2021). Global monitoring of school closured caused by COVID-19. Available at https://en.unesco.org/covid19/educationresponse. [last acsessed: 16.03.2021].

Van De Graaff, J. H. (1967). West Germany's abitur quota and school reform. *Comparative Education Review 11*(1), 75–86.

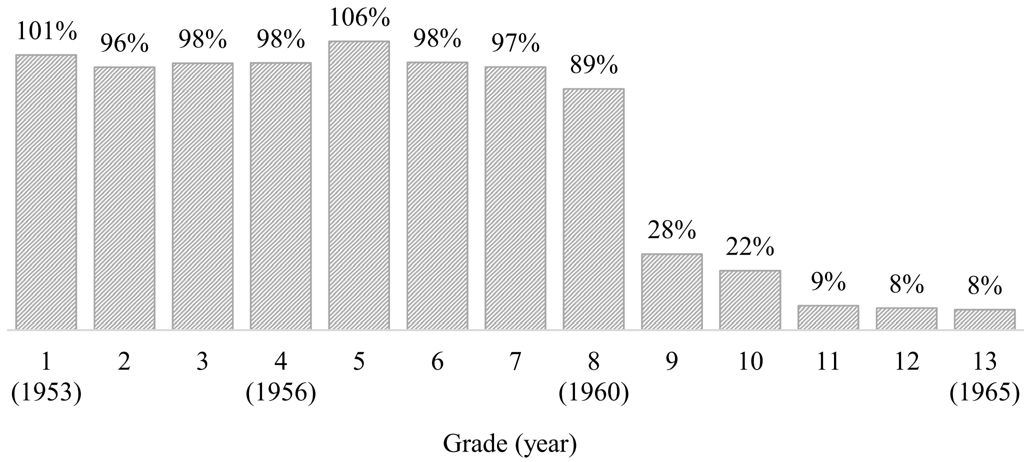
Werner, K. and L. Woessmann (2021). The Legacy of Covid-19 in Education. CESIfo Working Paper 9358, CESifo, Munich.

Wößmann, L. (2020). Folgekosten ausbleibenden Lernens: Was wir über die Corona-bedingten Schulschließungen aus der Forschung lernen können. *ifo Schnelldienst 73*(06), 38–44.

Wößmann, L., V. Freundl, E. Grewenig, P. Lergetporer, K. Werner, and L. Zierow (2020). Bildung in der Coronakrise: Wie haben die Schulkinder die Zeit der Schulschließungen verbracht, und welche Bildungsmaßnahmen befürworten die Deutschen? *ifo Schnelldienst 73*(09), 25–39.

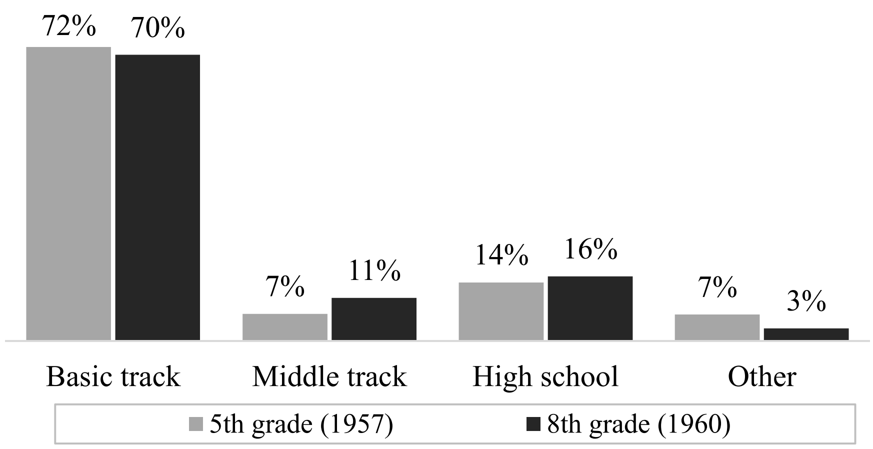
Appendix A: Additional Figures and Tables

Figure A.1: Grade progression for enrollment cohort 1953



Note: The figure shows the raw number of students in a particular grade (and the relevant calender year in parenthesis) relative to the number of students enrolled in 1953. Grade 4 corresponds to the final year in primary school. Grade 8 (9 in Schleswig- Holstein, Hamburg, and Bremen) marks the end of compulsory schooling. Grades 10 and 13 represent the final year in the middle track and high school, respectively. The numbers include downgrading, upgrading, mortality, and migration. Only West German states (w/o Berlin and Saarland) are included.
Source: DESTATIS (2021).

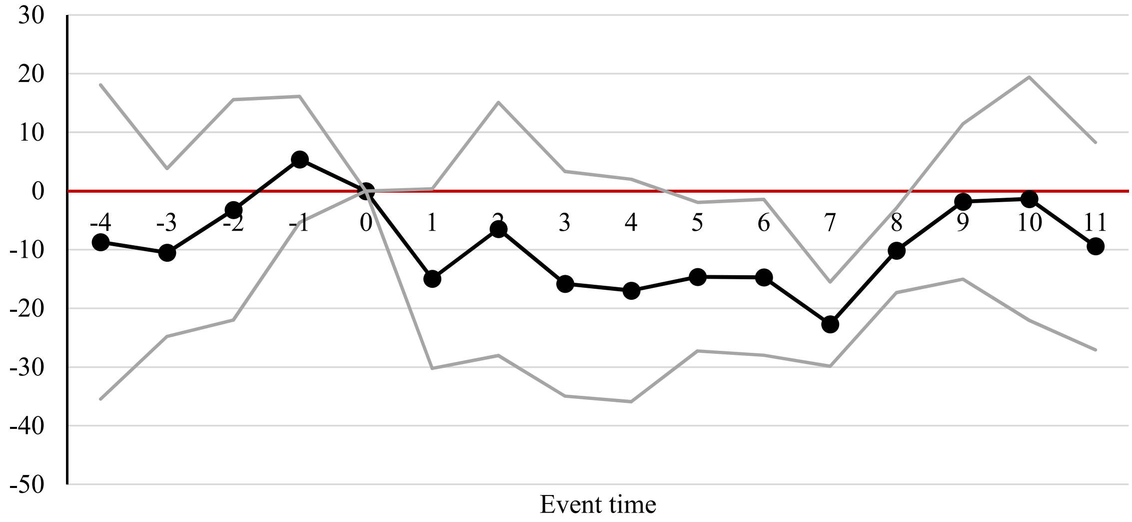
Figure A.2: Distribution of students across secondary school tracks



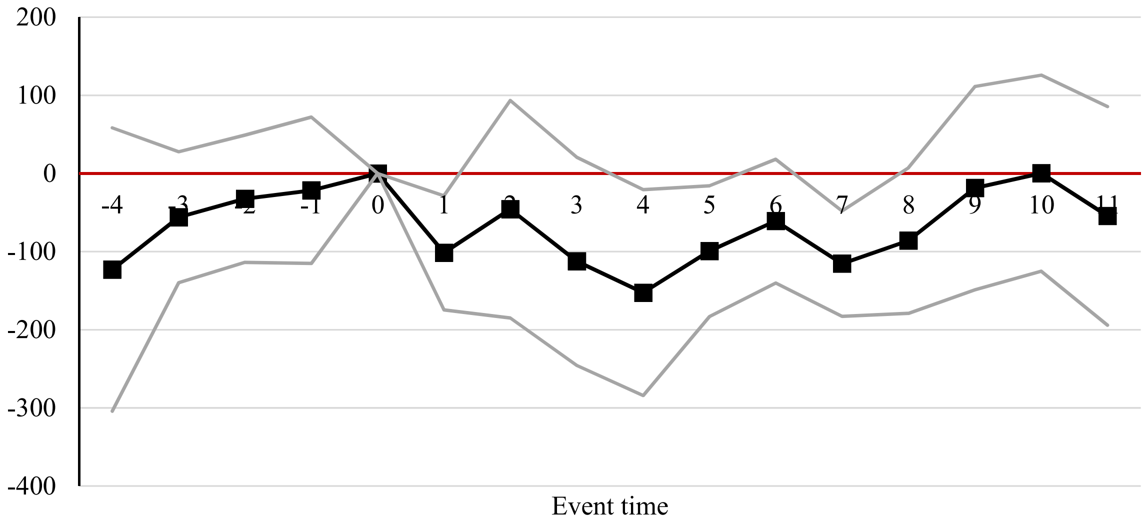
Note: The figure shows the distribution of 5th-graders in 1957 and 8th-graders in 1960 across tracks. Students who attended the 5th grade in 1957 and the 8th grade in 1960 had been enrolled in 1953 assuming that they progressed continuously. Only West German states (w/o Berlin and Saarland) are included.
Source: DESTATIS (2021).

Figure A.3: Event time studies for the effect on prime-age outcomes (ages 31-54)

(a) Effects on earnings (in 1,000 EUR)



(b) Effects on the number of days in employment



Note: The figures show the results from event time studies where the event time (t) is measured in 12-month increments. The first 12 treated birth months in each affected state are assigned $t = 1$. Each figure plots the event time estimates from a linear regression of the outcome on event time dummies, state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). The grey lines show 95% confidence intervals based on standard errors clustered at the state level.
Source: SIAB 1975-2017; own calculations.

Figure A.4: Relative effects on prime-age outcomes (ages 31-54) depending on treatment timing

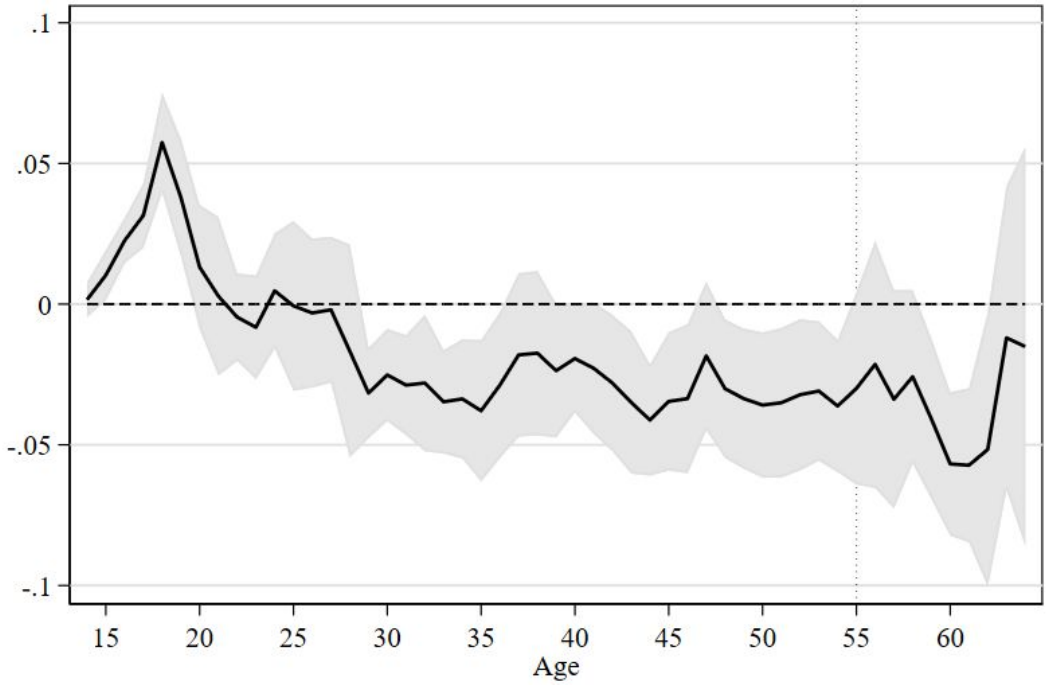
(a) Effects on earnings (in 1,000 EUR)

(b) Effects on the number of days in employment

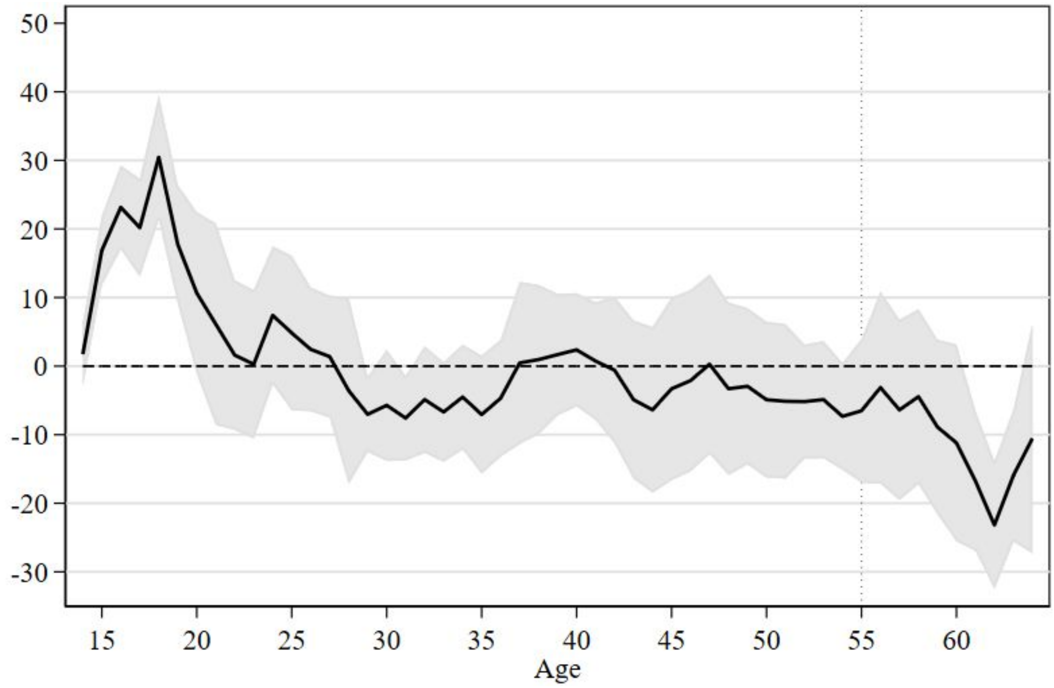
Note: The bars represent the estimated effects of the exposure to the short school years (defined as a binary treatment) relative to the mean of the outcome. The darkest bar is based on a linear regression of equation (1) where SSY is a dummy variable. The brighter bars are a based on a separate linear regression of equation (1) where SSY is split into eleven dummy variables indicating the expected grade attended at the time of the treatment. All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). The error bars show 95% confidence intervals based on standard errors clustered at the state level. The point estimates and standard errors behind the figures are reported in Table A.5 in Appendix A.
Source: SIAB 1975-2017; own calculations.

Figure A.5: Effects of the short school years over the live cycle

(a) Effects on annual pension points stemming from labor earnings

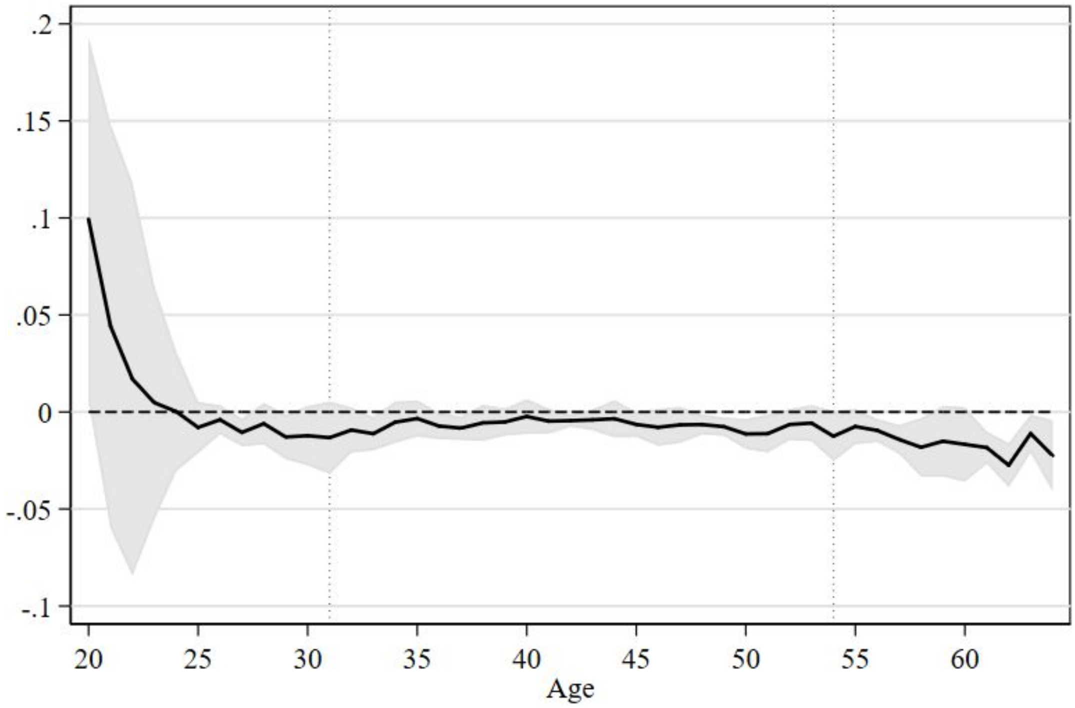


(b) Effects on the annual number of days in employment



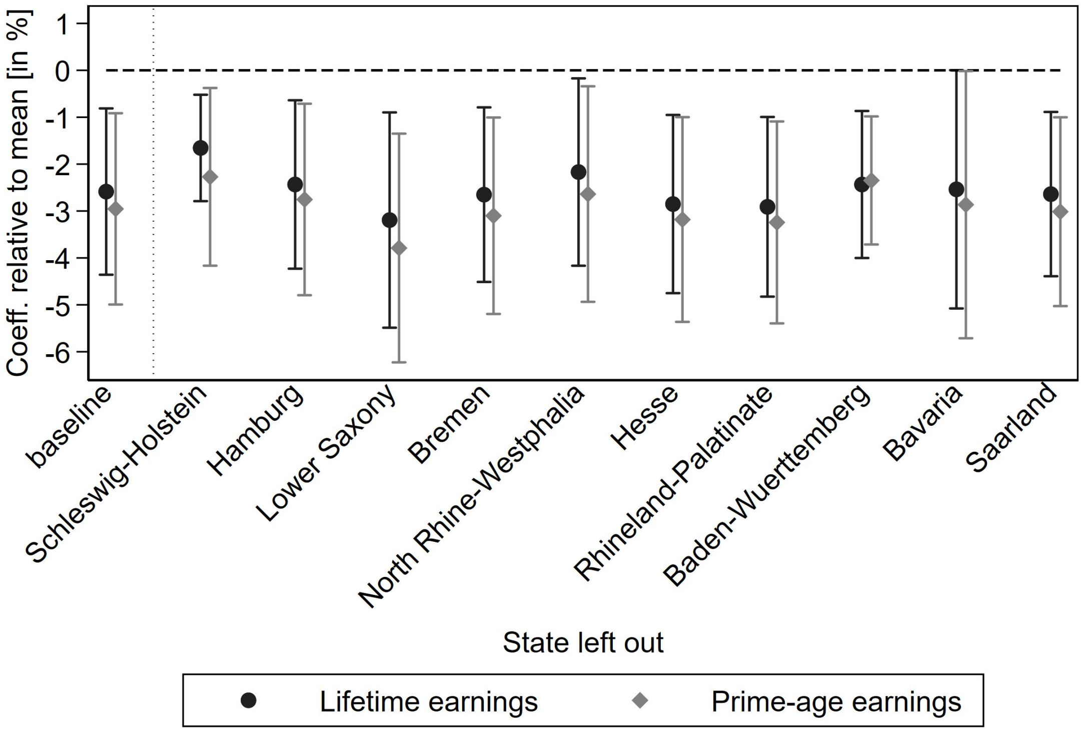
Note: Pension points refer to the statutory points stemming from labor market earnings that determine future pension entitlements. The figures plot the age-specific estimates on SSY in equation (1). Each estimate is from a separate linear regression of the outcome at a given age on state, state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Shaded areas show 95% confidence intervals based on standard errors clustered at the state level.
Source: VSKT-SUFs 2004-2018; own calculations.

Figure A.6: Effects of the short school years on the employment probability at a given age



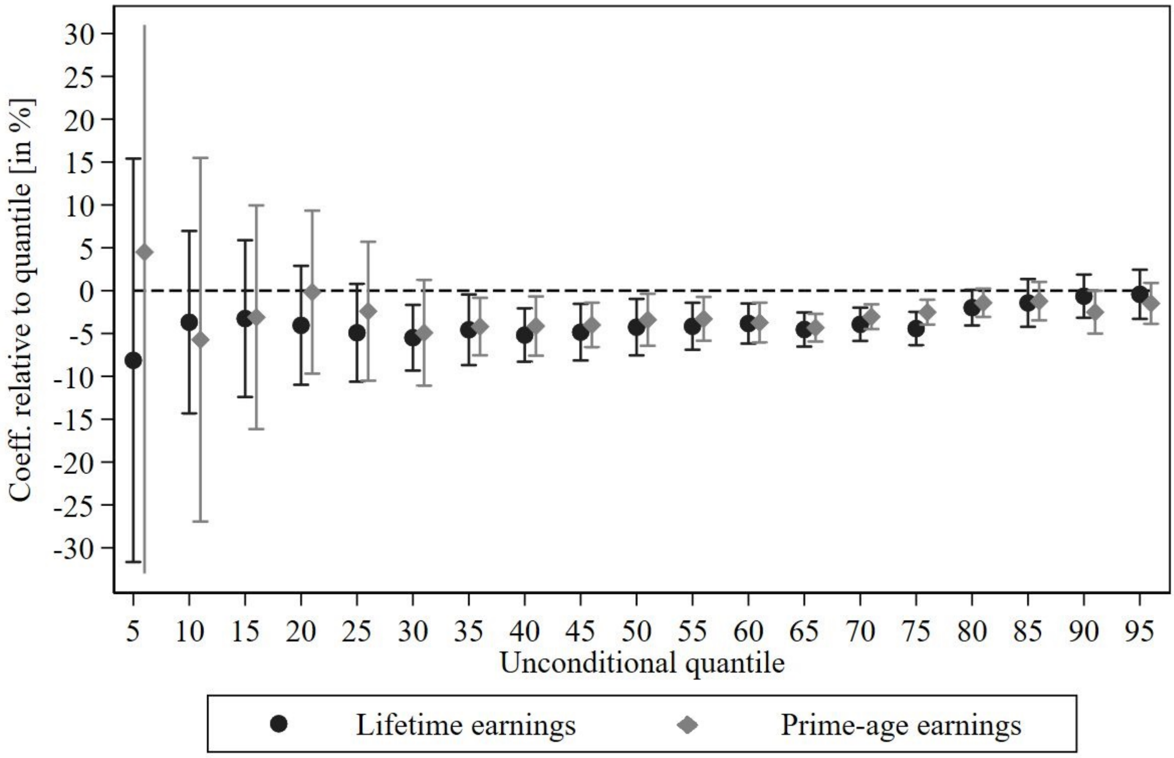
Note: The figure plots the age-specific estimates on SSY in equation (1). Each estimate is from a separate linear regression of the outcome at a given age on state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Shaded areas show 95% confidence intervals based on standard errors clustered at the state level. Source: SIAB 1975-2017; own calculations.

Figure A.7: Sensitivity analysis: excluding single states



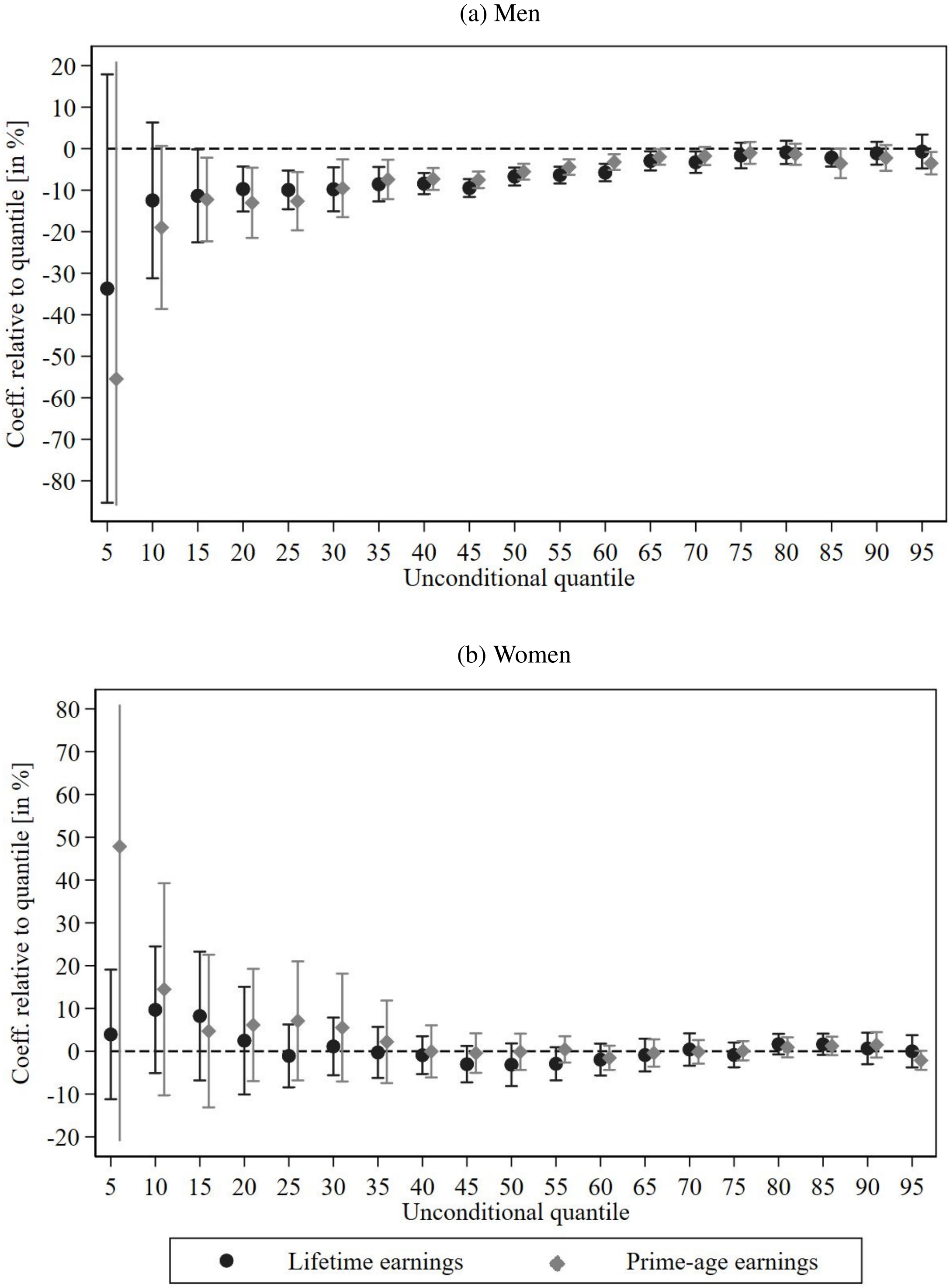
Note: The figure plots the relative effects of short school years on lifetime/prime-age earnings after excluding single states. The relative effects are estimated coefficients on SSY in equation (1) divided by a corresponding sample mean. Each estimate is from a separate linear regression of the outcome on state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). The capped spikes show 95% confidence intervals based on standard errors clustered at the state level.
Source: SIAB 1975-2017; own calculations.

Figure A.8: Earnings effects across the income distribution



Note: The figure plots the relative effects of short school years on lifetime/prime-age earnings along the respective distribution of the outcome. The relative effects correspond to the estimated coefficients on SSY in equation (1) divided by a respective quantile. Each estimate is from a separate unconditional quantile regression of the outcome on state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). The capped spikes show 95% confidence intervals based on standard errors clustered at the state level. For display purposes, the 95% confidence intervals around the estimated effect on prime-age earnings at the 5th percentile are trimmed.
Source: SIAB 1975-2017; own calculations.

Figure A.9: Gender-specific effects across the income distribution



Note: See Figure A.8.
Source: SIAB 1975-2017; own calculations.

Table A.1: Sample means

Variable	Person-level data	Person-year-level data (pooled)
Outcomes		
Lifetime earnings (in 1,000 EUR as of 2015)	888.50 (791.91)	
Lifetime log earnings	13.14 (1.37)	
Lifetime employment (in days)	8560.28 (4270.03)	
Prime-age earnings (in 1,000 EUR as of 2015)	624.77 (599.60)	
Prime-age log earnings	12.66 (1.59)	
Prime-age employment (in days)	5646.82 (3003.93)	
Annual earnings (in 1,000 EUR as of 2015)		32.39 (28.58)
Annual log earnings		10.12 (0.97)
Annual employment (in days)		312.04 (113.01)
Employed (0/1)		0.93
High school degree (0/1)	0.24	0.22
College/university degree (0/1)	0.15	0.13
Vocational degree (0/1)	0.77	0.81
Any post-secondary education (0/1)	0.95	0.94
Missing educational attainment (0/1)	0.03	0.01
Basic characteristics		
Year of birth	1954.50 (5.71)	1954.74 (5.51)
Month of birth	6.41 (3.42)	6.42 (3.43)
Female	0.49	0.46
Age		41.63 (11.41)
Schleswig-Holstein	0.04	0.04
Hamburg	0.03	0.02
Lower Saxony	0.12	0.13
Bremen	0.01	0.01
North Rhine-Westphalia	0.28	0.28
Hesse	0.09	0.09
Rhineland-Palatinate	0.07	0.07
Baden-Wurttemberg	0.15	0.16
Bavaria	0.19	0.19
Saarland	0.01	0.01
Policy variables		
Exposure to short school years (in years)	0.20 (0.30)	0.21 (0.30)
Exposure to short school years (0/1)	0.32	0.34
Nine years of compulsory schooling (0/1)	0.70	0.72
Statutory age at school entry (in years)	6.48 (0.33)	6.48 (0.33)
Size of enrollment cohort (in months)	11.68 (1.37)	11.69 (1.35)
Student-to-teacher ratio 1st grade	36.82 (4.82)	36.67 (4.70)
Student-to-teacher ratio 4th grade	34.74 (4.19)	34.70 (4.16)
Student-to-teacher ratio 9th grade	31.46 (5.44)	31.36 (5.45)
Observations	278,797	7,648,008
Individuals	278,797	278,797

Notes: Sample restricted to (West-)German citizens born 1944-1963. Standard deviations in parentheses.
Source: SIAB 1975-2017; own calculations.

Table A.2: Effects on labor market outcomes during prime-ages (31-54)

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample				Restricted sample	
Panel A: Earnings (in 1,000 EUR as of 2015)						
SSY	-4.887	-17.891***	-17.610***	-17.609***	-19.175***	-21.978***
	(4.689)	(5.516)	(5.669)	(5.830)	(4.475)	(3.735)
	[-0.8%]	[-2.9%]	[-2.8%]	[-2.8%]	[-3.1%]	[-3.5%]
Mean dep.	624.767				625.901	
Obs.	278,797				255,298	
Panel B: Log earnings						
SSY	0.006	-0.016	-0.017	-0.017	-0.033**	-0.029**
	(0.014)	(0.018)	(0.018)	(0.018)	(0.016)	(0.015)
Mean dep.	12.665				12.658	
Obs.	274,241				250,920	
Panel C: Employment (in days)						
SSY	-34.413	-73.016**	-76.417**	-77.487**	-100.465***	-94.691***
	(27.936)	(32.242)	(33.259)	(31.898)	(24.327)	(14.933)
	[-0.6%]	[-1.3%]	[-1.4%]	[-1.4%]	[-1.8%]	[-1.7%]
Mean dep.	5646.822				5657.561	
Obs.	278,797				255,298	
Ninth compulsory year	no	yes	yes	yes	yes	yes
Age at school entry	no	no	yes	yes	yes	yes
Enrollment cohort size	no	no	no	yes	yes	yes
BJS estimator	no	no	no	no	no	yes

Note: Each cell is based on a separate linear regression of equation (1). All regressions include state and birth date fixed effects, and a gender dummy. Standard errors in parentheses are clustered at the state level. The estimated effect relative to the mean of the outcome is reported in brackets. SSY = short school year. Restricted sample omits individuals born before 1946 and those from Saarland. BJS estimator uses the imputation procedure by Borusyak et al. (2022).
Source: SIAB 1975-2017; own calculations.

Table A.3: Lifetime effects (ages 20-64) - binary treatment definition

	(1)	(2)	(3)	(4)	(5)
	Full sample	Restricted sample 1		Restricted sample 2	
Panel A: Earnings (in 1,000 EUR as of 2015)					
$SSY(0/1)$	-17.372***	-17.919***	-17.529***	-21.448***	-18.934***
	(4.173)	(3.660)	(3.647)	(6.110)	(4.089)
	[-2.0%]	[-2.0%]	[-2.0%]	[-2.3%]	[-2.1%]
Mean dep.	888.496	896.972		906.669	
Obs.	278,797	255,298		200,210	
Panel B: Log earnings					
$SSY(0/1)$	-0.020**	-0.028***	-0.030***	-0.034***	-0.030***
	(0.008)	(0.008)	(0.005)	(0.012)	(0.005)
Mean dep.	13.142	13.161		13.156	
Obs.	276,854	253,451		198,764	
Panel C: Employment (in days)					
$SSY(0/1)$	-103.155***	-120.803***	-127.172***	-144.406***	-132.928***
	(31.219)	(24.245)	(17.187)	(33.337)	(32.300)
	[-1.2%]	[-1.4%]	[-1.5%]	[-1.6%]	[-1.5%]
Mean dep.	8560.277	8668.693		8768.400	
Obs.	278,797	255,298		200,210	
BJS estimator	no	no	yes	no	yes

Note: Each cell is based on a separate linear regression of equation (1) where SSY is defined as a dummy variable. All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level. The estimated effect relative to the mean of the outcome is reported in brackets. SSY = short school year. Restricted sample 1 omits individuals born before 1946 and those from Saarland. Restricted sample 2 additionally omits individuals born after 1961, so that the treatment is an absorbing state. BJS estimator refers to the imputation procedure by Borusyak et al. (2022). Last column estimated using the *did_imputation* Stata command.
Source: SIAB 1975-2017; own calculations.

Table A.4: Diagnostics suggested in de Chaisemartin and D'Haultfoeuille (2020)

	(1)	(2)	(3)	(4)	(5)
		Full sample			Restricted sample
Total no. of ATTs	864	864	864	864	655
No. of ATTs receiving a negative weight	96	36	35	44	19
Sum of negative weights	-0.008	-0.009	-0.009	-0.008	-0.010
Panel A: Earnings (in 1,000 EUR as of 2015)					
SSY	-4.068	-24.916	-24.346	-24.248	-25.639
$\underline{\hat{\sigma}}_{fe}$	10.502	57.612	55.563	56.411	58.628
$\underline{\underline{\hat{\sigma}}}_{fe}$	45.365	247.902	229.590	267.512	257.211
Panel B: Log earnings					
SSY	0.003	-0.030	-0.030	-0.030	-0.044
$\underline{\hat{\sigma}}_{fe}$	0.009	0.069	0.069	0.071	0.100
$\underline{\underline{\hat{\sigma}}}_{fe}$	0.038	0.298	0.286	0.337	0.439
Panel C: Employment (in days)					
SSY	-73.466	-172.903	-175.373	-175.899	-193.348
$\underline{\hat{\sigma}}_{fe}$	189.643	399.791	400.238	409.221	442.131
$\underline{\underline{\hat{\sigma}}}_{fe}$	819.172	1720.285	1653.822	1940.605	1939.695
Ninth compulsory year	no	yes	yes	yes	yes
Age at school entry	no	no	yes	yes	yes
Enrollment cohort size	no	no	no	yes	yes

Note: Restricted sample omits individuals born before 1946 and those from Saarland. All results estimated using the *twowayfeweights* Stata command. All regressions include state and birth date fixed effects and control for gender. Because the gender dummy varies within the state×cohort cells, the command uses its average value at the state×cohort level. The point estimate on SSY corresponds to the weighted sum of all ATTs. $\underline{\hat{\sigma}}_{fe}$ and $\underline{\underline{\hat{\sigma}}}_{fe}$ are summary measures of the robustness of the estimated coefficient on SSY to treatment effect heterogeneity defined in Corollary 1 in de Chaisemartin and D'Haultfoeuille (2020).
Source: SIAB 1975-2017; own calculations.

Table A.5: Effects depending on the timing of the exposure to the short school years

	Lifetime (ages 20-64)		Prime-age (ages 31-54)	
	earnings	employment	earnings	employment
Panel A: Average effects of the exposure to at least one short school year during grades 1 - 9				
$SSY(0/1)$	-17.919 ***	-120.803 ***	-12.900 ***	-60.153 ***
	(3.660)	(24.245)	(2.194)	(10.468)
	[-2.0%]	[-1.4%]	[-2.1%]	[-1.1%]
Panel B: Effects depending on the timing and the duration of the exposure				
Grade 1	-20.941 ***	-36.804	-21.489 ***	-64.552
	(7.746)	(63.527)	(5.387)	(42.643)
Grades 1 & 2	-12.705 ***	-115.16 **	-13.223 ***	-103.633 ***
	(3.371)	(55.700)	(3.840)	(25.995)
Grades 2 & 3	-20.79 ***	-105.8 **	-18.785 ***	-86.679 ***
	(7.650)	(54.591)	(5.582)	(25.244)
Grades 3 & 4	-15.413 ***	-43.112	-18.14 ***	-73.745
	(5.434)	(51.235)	(5.099)	(44.906)
Grades 4 & 5	-1.849	-34.785	-8.479 *	-58.153 **
	(6.425)	(65.439)	(4.375)	(27.713)
Grades 5 & 6	-0.286	-86.979	-7.544	-68.217
	(13.166)	(77.817)	(6.657)	(46.792)
Grades 6 & 7	-14.558	-156.738 **	-11.789 *	-83.211 **
	(10.613)	(62.635)	(6.782)	(41.201)
Grades 7 & 8	3.735	-75.552	0.42	-26.667
	(9.066)	(85.045)	(7.524)	(57.806)
Grades 8 & 9	-24.944 *	-254.068 ***	-19.17 *	-145.264 ***
	(12.961)	(54.629)	(11.179)	(52.808)
Grade 9	-15.632	-123.949	-3.749	-8.979
	(21.416)	(103.789)	(15.906)	(62.724)
Grades > 9	10.692	50.634	-0.946	-33.125
(beyond compulsory schooling)	(6.773)	(51.234)	(6.498)	(40.647)
Mean dep.	896.972	8,668.693	625.901	5,657.561
Obs.	255,298			

Note: Earnings are measured in 1,000 EUR and employment in days. In Panel A, each cell is based on a separate linear regression of equation (1) where SSY is defined as a binary treatment variable. The estimated effect relative to the respective sample mean of the outcome is reported in brackets. In Panel B, each column is from a separate linear regression of equation (1) where SSY is replaced by eleven dummies indicating the exposure to the treatment at a given grade or two consecutive grades. All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level.
Source: SIAB 1975-2017; own calculations.

Table A.6: Effects on educational attainment and other outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample				Restricted sample	
Panel A: Missing information on educational attainment						
SSY	0.000	-0.000	0.000	0.000	0.001	0.002 **
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)
Mean dep.			0.026			0.026
Obs.			278,797			255,298
Panel B: High school degree						
SSY	0.005	0.010	0.009	0.009	0.006	0.008 ***
	(0.007)	(0.006)	(0.006)	(0.006)	(0.004)	(0.003)
Mean dep.			0.236			0.244
Obs.			271,496			248,698
Panel C: College/university degree						
SSY	0.002	0.003	0.003	0.004	0.003	0.004
	(0.005)	(0.005)	(0.005)	(0.005)	(0.004)	(0.003)
Mean dep.			0.150			0.154
Obs.			271,496			248,698
Panel D: Vocational degree						
SSY	-0.005	-0.014 *	-0.014 *	-0.014 *	-0.013 **	-0.015 ***
	(0.009)	(0.008)	(0.008)	(0.008)	(0.005)	(0.005)
Mean dep.			0.775			0.774
Obs.			271,496			248,698
Panel E: Any post-secondary degree						
SSY	-0.003	-0.011 **	-0.011 **	-0.011 **	-0.010 ***	-0.011 ***
	(0.005)	(0.005)	(0.005)	(0.005)	(0.003)	(0.003)
Mean dep.			0.925			0.928
Obs.			271,496			248,698
Panel F: Early retirement (before age 65)						
SSY	-0.000	0.003	0.002	0.003	-0.001	0.001
	(0.005)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)
Mean dep.			0.097			0.088
Obs.			278,797			255,298
Panel G: Death before age 55						
SSY	-0.002	-0.001	-0.001	-0.001	-0.001	-0.000
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Mean dep.			0.019			0.018
Obs.			278,797			255,298
Ninth compulsory year	no	yes	yes	yes	yes	yes
Age at school entry	no	no	yes	yes	yes	yes
Enrollment cohort size	no	no	no	yes	yes	yes
BJS estimator	no	no	no	no	no	yes

Note: Each cell is based on a separate linear regression of equation (1). All regressions include state and birth date fixed effects, and a gender dummy. Standard errors in parentheses are clustered at the state level. The estimated effect relative to the mean of the outcome is reported in brackets. SSY = short school year. Restricted sample omits individuals born before 1946 and those from Saarland. BJS estimator uses the imputation procedure by Borusyak et al. (2022).
Source: SIAB 1975-2017; own calculations.

Table A.7: Comparison with results from the German Micro Census

	(1) Income measure	(2) Self-employed	(3) Public servant	(4) Basic degree	(5) Middle degree	(6) High school	(7) Years of schooling	(8) Univ./ College	(9) Vocational degree	(10) Any post-secondary
Social security records (SIAB)										
SSY	-24.304***	excl.	excl.	n.a.	n.a.	0.009	n.a.	0.004	-0.014*	-0.011**
	(8.734)					(0.006)		(0.005)	(0.008)	(0.005)
	[-2.7%]									
Y-mean	888.496					0.236		0.150	0.775	0.925
Obs.	278,797					271,496		271,496	271,496	271,496
Micro Census - all										
SSY	-41.242***	0.003	0.004	-0.009	0.003	0.005	0.014	-0.002	-0.020*	-0.022**
	(15.556)	(0.004)	(0.003)	(0.009)	(0.013)	(0.006)	(0.019)	(0.004)	(0.012)	(0.009)
	[-2.3%]									
Y-mean	1826.601	0.125	0.078	0.502	0.244	0.254	10.073	0.177	0.682	0.859
Obs.	351,519	370,223	370,223	370,223	370,223	370,223	370,223	370,223	370,223	
Micro Census after excl. self-employed & public servants										
SSY	-45.231**	excl.	excl.	-0.001	0.004	0.006	0.021	-0.002	-0.020*	-0.021**
	(22.939)			(0.010)	(0.012)	(0.004)	(0.019)	(0.003)	(0.012)	(0.010)
	[-2.8%]									
Y-mean	1598.293			0.559	0.250	0.190	9.806	0.115	0.724	0.839
Obs.	283,690			295,173	295,173	295,173	295,173	295,173	295,173	295,173

Note: The income measure is the lifetime labor income (in 1,000 EUR) in the SIAB data and personal current monthly net income (in EUR) in the Micro Census. Self-employed and public servant status in the Micro Census refer to the current employment (if working) or the last employment (if not working). Each cell is based on a separate linear regression of equation (1). All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). The Micro Census regressions additionally control for age at interview (linear and squared) and survey year. Standard errors in parentheses are clustered at the state level. The estimated effect relative to the mean of the outcome is reported in brackets. SSY = short school year.
Source: SIAB 1975-2017, German Micro Census 2008, 2012, 2016; own calculations.

Table A.8: Robustness to refined treatment assignment in the German Micro Census

	(1)	(2)	(3)	(4)
SSY	-41.242 ***	-61.841 ***	-45.231 **	-81.469 ***
	(15.556)	(17.065)	(22.939)	(15.048)
	[-2.3%]	[-3.4%]	[-2.8%]	[-5.1%]
Obs.	351,519	351,519	283,690	283,690
Coding based on track	no	yes	no	yes
Excl. self-employed & public servants	no	no	yes	yes

Note: The income measure is the current monthly net income (in EUR). The treatment coding based on school track additionally account for the potential exposure beyond grade 9 (i.e., in grades 10-13). Self-employed and public servant status refer to the current employment (if working) or the last employment (if not working). Each cell is based on a separate linear regression of equation (1). All regressions include state and birth date fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, the size of the enrollment cohort (in months), age at interview (linear and squared), and survey year. Standard errors in parentheses are clustered at the state level. The estimated effect relative to the mean of the outcome is reported in brackets.
Source: German Micro Census 2008, 2012, 2016; own calculations.

Table A.9: Effects on various socio-emotional characteristics

	(1) Self-esteem	(2) External LOC	(3) Positive reciprocity	(4) Negative reciprocity	(5) Patience	(6) Risk aversion	(7) Trust
SSY	-0.032	0.064	0.007	0.047	0.020	-0.047	-0.018
	(0.074)	(0.065)	(0.052)	(0.077)	(0.085)	(0.031)	(0.113)
Mean age	56.9	53.5	53.5	53.5	56.3	53.8	52.7
Obs.	5,533	6,631	6,666	6,663	6,565	9,855	7,984

Note: The outcome variables are standardized. Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, a gender dummy, age at interview (linear and quadratic), indicators for survey year, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level. LOC=Locus of Control
Source: SOEP 1984-2019 (v36); own calculations.

Table A.10: Gender-specific estimates from alternative data sets

	(1) Micro Census Net income (in EUR)	(2) Pension Insurance records Pension points (ages 14-64)	(3) Pension Insurance records Pension points (ages 14-55)
Men	-81.583 **	-1.299 **	-1.294 **
	(32.33)	(0.591)	(0.535)
	[-3.3%]	[-4.2%]	[-4.4%]
Mean dep.	2501.308	30.942	29.082
Obs.	172,039	25,225	
Women	1.700	0.357	0.310
	(11.089)	(0.496)	(0.460)
	[0.1%]	[2.5%]	[2.3%]
Mean dep.	1179.867	14.177	13.340
Obs.	179,480	27,745	

Note: Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, an indicator for nine years of compulsory schooling, statutory age at school entry, the size of the enrollment cohort (in months), age at interview (linear and squared), and survey year. Standard errors in parentheses are clustered at the state level.
German Micro Census 2008, 2012, 2016, VSKT-SUF 2004-2018; own calculations.

Table A.11: Gender-specific effects on educational attainment from the Micro Census

	(1) High school degree	(2) Years of schooling	(3) College/Univ. degree	(4) Vocational degree	(5) Any post-secondary
Men	0.009	0.022	-0.002	-0.008	-0.010 **
	(0.006)	(0.026)	(0.005)	(0.006)	(0.004)
Mean dep.	0.307	10.245	0.226	0.685	0.911
Obs.			182,913		
Women	0.001	0.000	-0.003	-0.032 *	-0.034 **
	(0.008)	(0.021)	(0.007)	(0.019)	(0.014)
Mean dep.	0.203	9.905	0.130	0.679	0.809
Obs.			187,310		

Note: Each cell is based on a separate linear regression and shows the estimate on SSY in equation (1). All regressions include state and birth date fixed effects, an indicator for nine years of compulsory schooling, statutory age at school entry, the size of the enrollment cohort (in months), age at interview (linear and squared), and survey year. Standard errors in parentheses are clustered at the state level.
German Micro Census 2008, 2012, 2016; own calculations.

Table A.12: Effects on earnings dispersion

	All		Men		Women	
	Lifetime earnings	Prime-age earnings	Lifetime earnings	Prime-age earnings	Lifetime earnings	Prime-age earnings
Panel A: Index of dispersion						
SSY	0.012	0.001	0.206 ***	0.156 ***	-0.042	-0.044
	(0.038)	(0.051)	(0.047)	(0.044)	(0.049)	(0.074)
Obs.	4,796	4,795	2,398	2,398	2,398	2,397
Panel B: Coefficient of variance						
SSY	0.031	0.020	0.182 ***	0.173 **	-0.080	-0.088
	(0.072)	(0.048)	(0.054)	(0.068)	(0.145)	(0.114)
Obs.	4,796	4,795	2,398	2,398	2,398	2,397

Note: The data is aggregated to birth date-state-gender cells. The index of dispersion relates the interquartile range in earnings in each cell to the respective median value. The coefficient of variation is computed by divining the standard deviation of earnings to the corresponding mean value. The outcomes are standardized. Each estimate comes from a separate linear regression of equation (1). All regressions include state and birth date fixed effects, a gender dummy (save for columns 3-6), an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Standard errors in parentheses are clustered at the state level. The regressions are reweighed using the number of individuals in each cell.
Source: SIAB 1975-2017; own calculations.

Appendix B: Potential effects on regional mobility

The German social security records do not include any information on an individual's place of schooling. Thus, in my main analysis, I use the first state of residence ever observed for a given individual in the data as a proxy for the state of school attendance. This yields measurement error in the treatment variable. This Appendix provides evidence on the extent of the resulting measurement error and it's potential threats to the internal validity of my main results. For this purpose, I use survey data from the National Educational Panel Study (NEPS; see Blossfeld and Roßbach, 2019). The NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi, Germany) in cooperation with a nationwide network. Specifically, I draw on the Starting Cohort Adults (NEPS-SC6), which includes self-reported information on both the state of school attendance and the state of residence later in life for the relevant birth cohorts.

The NEPS-SC6 study started in 2007/8 as a representative sample of individuals born between 1956 and 1986 living in private households in Germany. In 2009/10 (second wave), the sample has been extended to birth cohorts 1944-1955, and since then, the survey was conducted annually. During the fist interview, all respondents provide retrospective information on their educational careers including the location of each educational institution. This allows me to use the state of school enrollment for the treatment assignment. Each survey (from 2007/8 through 2018/19) also reports the respondents' current state of residence. In addition, while entering the sample, all participants provide retrospective information on their employment biographies including the job's venue. The employment spells are then updated by corresponding information collected in the following survey years.[43] Taken together, the NEPS allows me to study the extent to which an individual's state of schooling matches his/her state of residence and state of employment later in life. Specifically, I focus on the first and the last state of residence and employment available in the data for a given individual.

Similarly to my main analysis, I restrict the sample to individuals born between 1944 and 1963 and include those who started school in one of the ten West German states (excl. Berlin),

[43]Unfortunately, most respondents are reluctant to report their past and current earnings. Thus, the earnings spells are very intermittent, which does not allow for any reliable analysis of this issue in the NEPS data.

which yields a sample of 6,131 individuals. They were between 43 and 68 years old at the time of the first interview. Table B.1 below provides descriptive statistics. Almost 80% of sampled individuals lived in their state of schooling at the time of the first interview (i.e., on average, at age 54). This percentage remained nearly unchanged when measured at the last available interview (i.e., on average, at age 60). Along the same line, 85% of individuals started their working career (on average, at age 20) in the same state where they entered primary school. The match between the state of schooling and the last state of employment is 75%, which suggests that for the cohorts under study, the cross-state mobility increased somewhat during their prime ages but was generally at a relatively low level.

For my main analysis, this descriptive evidence from the NEPS implies that the first state ever observed for a given individuals in social security records is potentially a good proxy for the state of school attendance. Although limited, the measurement error in the treatment assignment could be nonetheless problematic if the exposure to short school years changed cross-state mobility patterns. Table B.2 below investigates this issue using the same estimation approach as described in Section 4. Similarly to the main analysis for labor market outcomes in Section 5.1 (see Table 2), I estimate various specifications using the full sample (columns 1 through 4) and a restricted sample that omits the earlier occurrences of short school years in Baden-Wuerttemberg and Saarland (columns 5 and 6).

The vast majority of the point estimates in Table B.2 are negative suggesting an increase in interstate mobility among the treated individuals. However, most of effects are small in magnitude and none of them implies a statistically significant effect of the short school years on regional mobility later in life. Thus, if anything, my main results for labor market outcomes suffer from an attenuation bias due to the measurement error in the treatment variable in social security records. I show supportive evidence for this argument in Section 5.2, which presents a robustness test using the last (instead of the first) state observed in social security records for treatment assignment, thereby increasing the measurement error.

Table B.1: Sample means - NEPS

Variable	Mean (Std. Dev.)
Outcomes	
State of schooling matches the state of residence at the first interview (0/1)	0.79
State of schooling matches the state of residence at the last interview (0/1)	0.78
State of schooling matches the state of the first employment (0/1)	0.85
State of schooling matches the state of the last employment (0/1)	0.76
Basic characteristics	
Year of birth	1,955.01 (5.52)
Month of birth	6.43 (3.45)
Female	0.50
Age at first interview	54.01 (6.33)
Age at last interview	59.84 (6.15)
Age at first employment	20.12 (4.81)
Age at last employment	56.29 (8.44)
State of school enrollment:	
Schleswig-Holstein	0.04
Hamburg	0.03
Lower Saxony	0.14
Bremen	0.01
North Rhine-Westphalia	0.29
Hesse	0.08
Rhineland-Palatinate	0.07
Baden-Wurttemberg	0.15
Bavaria	0.17
Saarland	0.02
Policy variables	
Exposure to short school years (in years)	0.21 (0.30)
Exposure to short school years (0/1)	0.34
Nine years of compulsory schooling (0/1)	0.74
Statutory age at school entry (in years)	6.49 (0.33)
Size of enrollment cohort (in months)	11.72 (1.37)
Observations	6,131

Notes: Sample restricted to individuals born 1944-1963 who were enrolled in school in a (West-)German state. Standard deviations in parentheses.
Source: NEPS-SC6:11.1.0; own calculations.

Table B.2: The effect of exposure to short school years on interstate mobility

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample				Restricted sample	
Panel A: State of schooling matches the state of residence at the first interview (0/1)						
SSY	-0.017	-0.004	-0.001	-0.006	-0.011	-0.030
	(0.025)	(0.026)	(0.026)	(0.028)	(0.023)	(0.020)
Mean dep.	0.787				0.790	
Mean age	54.0				54.1	
Panel B: State of schooling matches the state of residence at the last interview (0/1)						
SSY	-0.015	0.001	0.005	0.001	-0.003	-0.027
	(0.024)	(0.023)	(0.024)	(0.025)	(0.022)	(0.019)
Mean dep.	0.779				0.782	
Mean age	59.8				59.3	
Panel C: State of schooling matches the state of the first employment (0/1)						
SSY	-0.005	-0.022	-0.022	-0.022	-0.022	-0.033
	(0.031)	(0.030)	(0.030)	(0.031)	(0.023)	(0.028)
Mean dep.	0.851				0.853	
Mean age	20.1				20.2	
Panel D: State of schooling matches the state of the last employment (0/1)						
SSY	0.003	-0.006	-0.003	-0.007	-0.007	-0.025
	(0.029)	(0.029)	(0.031)	(0.032)	(0.030)	(0.024)
Mean dep.	0.763				0.766	
Mean age	56.3				56.1	
Obs.	6,131				5,685	
Ninth compulsory year	no	yes	yes	yes	yes	yes
Age at school entry	no	no	yes	yes	yes	yes
Enrollment cohort size	no	no	no	yes	yes	yes
Restricted sample	no	no	no	no	yes	yes
BJS estimator	no	no	no	no	no	yes

Note: Each cell is based on a separate linear regression of equation (1). All regressions include state, state and birth date fixed effects, and a gender dummy. Standard errors in parentheses are clustered at the state level. SSY = short school year. Restricted sample omits individuals born before 1946 and those from Saarland. BJS estimator uses the imputation procedure by Borusyak et al. (2022).
Source: NEPS-SC6:11.1.0; own calculations.

Appendix C: Detailed description of auxiliary data sets

Pension Insurance Records (VSKT-SUFs 2004-2018)

The Research Data Centre of the German Federal Pension Insurance (*Deutsche Rentenversicherung*) administers a 1% random sample of persons aged 30-67 who ever contributed to the Statutory Pension Insurance (*Versicherungskontenstichprobe* - VSKT). The initial sample was drawn in 1983 but only since 2002, the data is available for researchers. I start with the wave 2004, which is the first one including information on federal state. Each following calendar year, the VSKT excludes the oldest birth cohort turning 68 and adds the youngest cohort turning 30 to the original sample. The last available wave is currently 2018, which covers birth cohorts 1950-1987. Each wave provides basic demographic characteristics (e.g., gender, birth date) and retrospective information on pension-relevant spells (e.g, (un)employment, vocational training, military service, parental leave, invalidity) at a monthly level starting from January of the calendar year when a given individual turns 14 years old.

I use the Scientific Use Files (SUFs) 2004-2018, each including a 25% subsample of the entire VSKT for the respective calendar year. The SUFs are newly drawn from the corresponding VSKT every year, which implies that a given individual might randomly enter the SUFs in various years. For example, according to the Research Data Centre, out of all individuals drawn for the SUF 2018, 25% had been also included in 2018 and 10% in 2016. Unfortunately, the Research Data Centre does not provide personal identifiers that would allow me to follow individuals across the SUFs. Thus, to minimize multiple occurrences per person and still obtain a reasonably large estimation sample, I pool the data according to a specific scheme shown in Table C.1. Specifically, for a given birth cohort, I pool three SUFs using every other wave. Thus, my estimation sample might include a particular person up to three times, which is however very unlikely. I verified that several alternative sampling schemes generate very similar results.

Otherwise, following my main sample restrictions, I focus on German citizens from West German states (excl. Berlin). Given that the data do not include any information on the state of school attendance, I additionally omit individuals with pension entitlements obtained in the former East Germany or with entitlements according to the law on foreign pensions to exclude potential immigrants. My main outcome of interest is the total number of pension points gained from employment spells subject to social security[44] but I also investigate the effects on the number of days spent in employment and an individual's age at labor market entry. The latest is derived from the starting date of the first employment or unemployment spell and allows me to test whether the exposure to short school years actually speeded up the labor force entry, which would be an expected "first stage" effect. Table C.2 displays summary statistics.

Table C.1: Number of individuals in the estimation sample by birth year and data wave

Birth	Wave of the VSKT-SUF									
year	2004	2006	2008	2009	2011	2013	2014	2016	2018	Total
1944	847	839	875	0	0	0	0	0	0	2,561
1945	866	860	837	0	0	0	0	0	0	2,563
1946	875	856	847	0	0	0	0	0	0	2,578
1947	826	852	839	0	0	0	0	0	0	2,517
1948	849	836	837	0	0	0	0	0	0	2,522
1949	823	797	790	0	0	0	0	0	0	2,410
1950	0	0	0	802	766	787	0	0	0	2,355
1951	0	0	0	853	827	848	0	0	0	2,528
1952	0	0	0	839	835	807	0	0	0	2,481
1953	0	0	0	845	811	801	0	0	0	2,457
1954	0	0	0	843	823	793	0	0	0	2,459
1955	0	0	0	857	865	867	0	0	0	2,589
1956	0	0	0	884	862	839	0	0	0	2,585
1957	0	0	0	0	0	0	872	869	864	2,605
1958	0	0	0	0	0	0	899	861	841	2,601
1959	0	0	0	0	0	0	926	892	915	2,733
1960	0	0	0	0	0	0	951	929	956	2,836
1961	0	0	0	0	0	0	1,038	1,019	1,010	3,067
1962	0	0	0	0	0	0	1,051	1,077	1,081	3,209
1963	0	0	0	0	0	0	1,117	1,102	1,095	3,314
Total	5,086	5,040	5,025	5,923	5,789	5,742	6,854	6,749	6,762	52,970

Notes: Sample restricted to (West-)German citizens born 1944-1963.
Source: VSKT-SUF 2004-2018; own calculations.

[44]Self-employment is generally not subject to mandatory contributions to the Statutory Pension Insurance. Nevertheless, some self-employed pay voluntary contributions and are therefore included in the data. Voluntary contributors are generally rare and potentially highly selective. Thus, I omit the points earned from self-employment spells while calculating the main outcome but their inclusion leads to very similar results.

Table C.2: Sample means - Pension Insurance Records

Variable	Person-level data	Person-year-level data (pooled)
Outcomes		
Age at labor market entry	18.23 (5.52)	
Lifetime pension-relevant points	22.16 (18.42)	
Lifetime employment (in days)	8080.16 (4896.67)	
Annual pension-relevant points		0.50 (0.59)
Annual employment (in days)		181.28 (174.18)
Basic characteristics		
Year of birth	1,953.91 (5.91)	1,953.63 (5.90)
Month of birth	6.38 (3.44)	6.38 (3.44)
Female	0.52	0.52
Age at sample drawing	57.57 (2.95)	
Age		35.88 (13.03)
Schleswig-Holstein	0.04	0.04
Hamburg	0.02	0.02
Lower Saxony	0.12	0.12
Bremen	0.01	0.01
North Rhine-Westphalia	0.30	0.30
Hesse	0.09	0.09
Rhineland-Palatinate	0.06	0.06
Baden-Wurttemberg	0.14	0.14
Bavaria	0.18	0.18
Saarland	0.02	0.02
Policy variables		
Exposure to short school years (in years)	0.19 (0.29)	0.19 (0.29)
Exposure to short school years (0/1)	0.31	0.31
Nine years of compulsory schooling (0/1)	0.66	0.64
Statutory age at school entry (in years)	6.50 (0.33)	6.49 (0.33)
Size of enrollment cohort (in months)	11.67 (1.40)	11.67 (1.40)
Observations	52,970	2,360,981
Individuals	52,970	52,970

Notes: Sample restricted to (West-)German citizens born 1944-1963. Standard deviations in parentheses.
Source: VSKT-SUF 2004-2018; own calculations.

The German Micro Census (2008, 2012, 2016)

The German Micro Census is a 1% representative sample of households living in Germany. Consequently, the data include civil servants and the self-employed, who are not subject to the mandatory social security contributions. The main aim of the annual surveys is an ongoing monitoring of the socio-demographic structure of the population and the labor market. The data are provided by the Research Data Centers of the Statistical Offices of the Federation and the Federal States. The Micro Census counts to Germany's official statistics and the participation in the survey is mandated by law so that non-participation is not an issue. The study is designed as a rotating panel with a quarter of the sample being replaced each year. Unfortunately, the data released for research purposes do not include personal identifiers, which would allow following individuals over time. Thus, to avoid multiple occurrences, I use every fourth survey year starting from the most recent wave, i.e., 2016, 2012, and 2008, which yields a pooled cross-sectional sample. I cannot include earlier waves (2004, 2000 etc.) because they do not provide information on individuals' month of birth.

Each year, the data include more than 120,000 German citizens from the relevant birth cohorts (1944-1963) who live in the West German states (excl. Berlin). Similar to social security records, the Micro Census does not include any information on the state of school attendance. Thus, I use the current state of residence as a proxy. I further restrict my estimation sample by omitting individuals who were born abroad and those who obtained educational credentials specific to the former East Germany. I also drop a small number of observations with missing information on educational attainment (less than 1%).

The available income measure refers to a respondent's monthly net income, which comprises any income sources including labor, pensions, and public transfers. This is not necessarily a disadvantage given that in the included survey years, I observe the relevant birth cohorts relatively late in life (on average at age 57) and some of them already draw retirement benefits. The income variable is originally reported in 24 brackets and I assign each individual the value corresponding to the midpoint of the respective bracket converted into 2015 prices. To assess whether the short school years lead to a different sorting into jobs being subject to social security contributions, I consider indicators for being self-employed or public servant as further outcomes. These variables refer to the current employment status for working individuals and to the last occupation for those currently not working.

The Micro Census also includes information on several educational outcomes. First, using information on the highest completed school degree, I consider three mutually exclusive indicators for the basic, middle, and high school degree. Second, I compute a proxy for completed years of schooling by assigning each school degree the typical number of years needed to obtain a particular school leaving certificate (i.e., 8 or 9 years if basic degree depending on compulsory schooling regime, 10 years if middle degree, and 12 years if high school diploma). Similar to social security records, I also consider indicators for having a college degree (incl. universities) and any vocational degree as additional outcomes. The final sample comprises approximately 370,000 individuals. Unfortunately, the information on net income is missing for 5.1% of the sample because this question is exempt from the law mandating the survey participation. Nevertheless, these non-responses do not seem to be significantly correlated with the exposure to the short school years so that endogenous sample selection should not be a relevant issue.[45] Table C.3 below describes the full sample.

[45]Using a similar model specification as in equation (1), I regressed an dummy for missing income on the exposure to the short school years. The estimate on SSY was -0.005 with a standard error of 0.003.

Table C.3: Sample means - Micro Census

Variable	Mean (Std. Dev.)
Outcomes	
Net income (in 2015 EUR)	1826.601 (1751.549)
Highest school degree: basic (0/1)	0.502
Highest school degree: middle (0/1)	0.244
Highest school degree: high school (0/1)	0.254
Years of schooling	10.073 (1.829)
College/university degree (0/1)	0.177
Vocational degree (0/1)	0.682
Any post-secondary degree (0/1)	0.86
Self-employed (0/1)	0.125
Public servant (0/1)	0.078
Basic characteristics	
Year of birth	1954.449 (5.674)
Month of birth	6.401 (3.428)
Female	0.506
Age	57.097 (6.511)
Schleswig-Holstein	0.049
Hamburg	0.021
Lower Saxony	0.128
Bremen	0.009
North Rhine-Westphalia	0.262
Hesse	0.090
Rhineland-Palatinate	0.068
Baden-Wurttemberg	0.150
Bavaria	0.204
Saarland	0.018
Policy variables	
Exposure to short school years (in years)	0.194 (0.294)
Exposure to short school years (0/1)	0.313
Nine years of compulsory schooling (0/1)	0.694
Statutory age at school entry (in years)	6.477 (0.327)
Size of enrollment cohort (in months)	11.691 (1.360)
Observations	370,223

Notes: Sample restricted to (West-)German citizens born 1944-1963. Standard deviations in parentheses.
Source: Micro Census 2008, 2012, 2016; own calculations.

The German Socio-Economic Panel (SOEP 1984-2019)

Conducted annually since 1984, the Socio-Economic Panel (SOEP) is the longest-running representative longitudinal survey of private households in Germany (Goebel et al., 2019). The data is provided by the Research Data Center of the Socio-Economic Panel (FDZ SOEP) at the German Institute for Economic Research (DIW Berlin). In addition to a relatively stable set of core socio-demographic characteristics collected annually, each year, the questionnaire includes additional modules asking in-depth questions on specific topics. Of my main interest are several measures of cognitive ability and personality traits (for details, see, e.g., Heineck and Anger, 2010), which are not available in social security records.

Specifically, survey years survey years 2006, 2012, and 2016 provide scores from two cognitive measures assessed on a subsample of approximately one-third of all respondents (for details, see, e.g., Anger and Heineck, 2008). First, in a symbol correspondence test, they were asked to match as many numbers and symbols as possible within 90 seconds according to a given correspondence list. Second, in a word fluency test, they were suppose to name as many different animals as possible within 90 seconds. While the first test measures the speed of cognition and performance in solving tasks related to new material, the word fluency test reflects more the pragmatics of cognition and working memory. To measure personality traits, I focus mainly on the Big Five inventory comprising openness to experience, conscientiousness, extroversion, agreeableness, and neuroticism collected in 2005, 2009, 2012, 2013, 2017, and 2019 (for details, see, e.g., Heineck and Anger, 2010). For completeness, I also consider the locus of control, reciprocity, self-esteem, risk aversion, and trust, which are available irregularly in various waves.

Similar to my main analysis on labor market outcomes, I focus on German citizens born between 1944 and 1963. I exclude individuals who lived in East German states in 1989 because they potentially attended school in the former GDR. Unfortunately, for the relevant birth cohorts, there is no direct information on the state of schooling in the SOEP. Thus, I construct a proxy by using the available information on the state of birth (30% of the sample) and the state of residence in the childhood (21%). For the rest (49%), I use the first state of residence ever observed for a given individual in the SOEP. My results are robust to various alternative approaches to approximate the state of schooling. This is not surprising given that there is a substantial match between the different regional variables.[46] Given that the outcomes of interest were collected only in selected survey years, the size of my estimation sample varies depending on the outcome. To avoid repeated observations per person, I use the first value of an outcome ever observed for a given individual in the panel. The results change little if I alternatively use the last observation or pool the data likely because cognitive skills and personality traits remain relative stable late in life. Table C.4 reports descriptive statistics. Before running the regressions, I standardize the outcomes to ease the interpretation.

[46]For example, conditional on available information on the state of birth, 77% of respondents still live in the same state at the time of their first interview. There is a match of 92% between the state of birth and childhood in a subsample with available information on both.

Table C.4: Sample means - SOEP

Variable	Sample depending on the outcome: Symbol correspond. test	Word fluency test	Big Five personality traits
Outcomes			
Symbol correspondence score 30s	8.32 (3.73)		
Symbol correspondence score 60s	17.97 (6.30)		
Symbol correspondence score 90s	27.32 (8.36)		
Word fluency score 30s		12.77 (5.97)	
Word fluency score 60s		20.46 (8.66)	
Word fluency score 90s		26.00 (11.29)	
Openness to experience			14.04 (3.60)
Conscientiousness			17.85 (2.70)
Extroversion			14.61 (3.33)
Agreeableness			16.25 (2.96)
Neuroticism			11.26 (3.81)
Basic characteristics			
Year of birth	1,954.27 (5.68)	1,954.36 (5.71)	1,954.90 (5.64)
Month of birth	6.41 (3.43)	6.40 (3.45)	6.40 (3.45)
Female	0.53	0.54	0.50
Age	55.30 (6.89)	50.96 (5.72)	54.19 (7.32)
Schleswig-Holstein	0.05	0.04	0.05
Hamburg	0.03	0.03	0.02
Lower Saxony	0.14	0.17	0.13
Bremen	0.01	0.01	0.01
North Rhine-Westphalia	0.26	0.27	0.27
Hesse	0.10	0.12	0.09
Rhineland-Palatinate	0.07	0.08	0.07
Baden-Wurttemberg	0.14	0.11	0.14
Bavaria	0.19	0.16	0.20
Saarland	0.02	0.01	0.02
Policy variables			
Exposure to short school years (in years)	0.19 (0.29)	0.19 (0.29)	0.20 (0.30)
Exposure to short school years (0/1)	0.30	0.30	0.31
Nine years of compulsory schooling (0/1)	0.69	0.71	0.73
Statutory age at school entry (in years)	6.47 (0.33)	6.46 (0.32)	6.49 (0.32)
Size of enrollment cohort (in months)	11.68 (1.40)	11.66 (1.39)	11.67 (1.36)
Observations	2,930	1,252	8,651

Notes: Sample restricted to (West-)German citizens born 1944-1963. Standard deviations in parentheses.
Source: SOEP 1984-2019 (v36); own calculations.