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ABSTRACT

Lifetime Consequences of Lost Instructional Time in the Classroom: Evidence from Shortened School Years*

This study estimates the lifetime effects of lost classroom instruction on labor market performance. For identification, I use historical shifts in the school year schedule in Germany, which substantially shortened the duration of the affected school years without adjusting the core curriculum. The loss of classroom instruction was mainly compensated for by assigning additional homework. Applying a difference-in-differences design to social security records, I find adverse effects of the policy on earnings and employment over almost the entire occupational career. Plausible mechanisms behind the deteriorated labor market outcomes include unfavorable effects on human capital and a differential occupational sorting.

JEL Classification: I21, I26, J24, J17

Keywords: instructional time, education, earnings, skills, Germany

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1 Introduction

A substantial portion of public investment in education is devoted to the provision of classroom instruction. Across OECD countries, students receive, on average, more than 7,500 hours of classroom instruction during their compulsory education (OECD, 2023b). However, there is considerable variation across countries, ranging from less than 5,000 hours in Bulgaria and Croatia to twice that in Australia and Denmark.[1] Extensive research has linked the amount of instruction to learning outcomes and cognitive skills.[2] This is consistent with theoretical models assuming that time investments in education increase human capital (Becker, 1962; Ben-Porath, 1967). However, while theory further predicts that employers will value better skills (or credentials), empirical evidence on the impact of time spent in school on labor market outcomes is mixed (e.g., Pischke, 2007; Parinduri, 2014; Stephens and Yang, 2014; Bhuller et al., 2017; Fischer et al., 2020; Dominguez and Ruffini, 2023).[3] Moreover, while existing research has focused on the consequences of exposure to additional instructional time (e.g., through extended compulsory schooling, longer school terms or school days), much less is known about the labor market effects of lost classroom instruction.

This paper provides the first comprehensive evidence on the impact of lost instructional time on labor market outcomes from a life-cycle perspective. To do so, I combine a unique quasi-experiment that occurred nearly six decades ago with high-quality administrative data on labor market biographies. Specifically, I exploit two shortened school years in Germany in the 1960s that resulted from a shift in the start of the school year from spring to fall. Each short school year compressed the amount of classroom instruction by one-third of a regular school year (Pischke, 2007; Koebe and Marcus, 2022). All students who attended school during the transition eventually graduated earlier because their total schooling duration (measured in calendar time, not in grades completed) was shorter than usual. Despite the sizable reduction in instructional time, great emphasis was placed on maintaining the core curriculum. The main compensatory measures included additional homework assignments and some reduction of instruction in non-core subjects such as music and physical education.

The media described the short school years as a "large-scale experiment at the expense of students" (Landesarchiv, 2020). However, early studies by psychologists and educational researchers found mixed effects on cognitive skills by comparing relatively small samples of exposed and unexposed students, either across states or across cohorts (Kornadt and Meister,

[1] This partly reflects differences in the length of compulsory schooling (see Appendix A, Figure A.1).

[2] See e.g., Bellei (2009); Carlsson et al. (2015); Lavy (2015); Rivkin and Schiman (2015); Huebener et al. (2017); Lavy (2020). For a recent review, see Barrios-Fernández (2023).

[3] For a recent summary of empirical research testing human capital theory versus the signaling model (Spence, 1973), see Aryal et al. (2022). In addition to instructional time, the education production function depends on several other inputs (for details, see, e.g., Hanushek, 2020).

1970; Meister, 1972; Thiel, 1973). If anything, the immediate effects on learning seemed small, and no remedial efforts were undertaken. This paper examines the long-run effects using a difference-in-differences (DiD) design that exploits the quasi-random variation in the exposure to the policy across the federal states and birth cohorts. For this purpose, I link individual social security records to rich data on the institutional setting hand-collected from archival sources.

I find that exposure to the short school years led to adverse labor market effects over almost the entire career. My estimates imply that one year of lost classroom instruction reduces lifetime earnings by almost 3% on average. The earnings losses were partly driven by lower employment during the prime working ages (a 2% reduction in days worked). The results are robust to changes in model specification, sample restrictions, and accounting for potential treatment effect heterogeneity. The lifetime losses contrast sharply with a purely mechanical effect of the policy (i.e., earlier graduation due to effectively shorter schooling), which would imply more time in the labor market and, thus, higher earnings accumulated over the life cycle. Indeed, I show that treated students experienced an initial earnings advantage due to accelerated labor market entry, which was substantial but quickly disappeared. In contrast, the subsequent negative effects were less pronounced but persisted until retirement.

This paper is closely related to earlier work on the wage effects of this policy by Pischke (2007), who applied a similar DiD design to cross-sectional survey data.[4] He found no significant wage losses despite some worse educational outcomes. A distinctive aspect of my paper is that I evaluate the labor market effects over the entire career using administrative data on earnings biographies. The life-cycle perspective provides novel evidence that the policy generated large but short-lived benefits and moderate but persistent disadvantages during the prime working ages. I show that the opposite direction of the short- and long-run effects explains why Pischke (2007) found no effects using pooled cross-sectional regressions that averaged the effects over the first two-thirds of the career. Thus, my new life-cycle results complement and reconcile (rather than contradict) his findings. Finally, using career-long data up to retirement, I document an overall negative effect on lifetime earnings, which translates directly into lower pensions entitlements.

Regarding possible mechanisms behind the persistent earnings losses, I find no effects on secondary school qualifications. This could be due to the short-term increase in repetition rates (Pischke, 2007) and/or teachers allegedly being more lenient during the transition (Drewek, 2020). However, I show that the affected individuals (i) had lower postsecondary vocational education, (ii) entered the labor market in jobs requiring lower skills, and (iii) ended up in different

[4]For other outcomes, Hampf (2019) found long-lasting deficits in numeracy skills and Koebe and Marcus (2022) temporary effects on the timing of family formation. They also replicated Pischke's cross-sectional results for wages and employment. Related literature examines the skill and health effects of a German reform in the 2000s that compressed the duration of high school (e.g., Dahmann, 2017; Huebener et al., 2017; Marcus et al., 2020).

occupations. Complementary survey data confirm long-lasting cognitive deficits and reveal imprints on personality traits. The adverse effects on human capital and occupational sorting are plausible channels through which the policy may have affected labor market performance.

Decomposing the effects by time of exposure, I find the largest earnings losses from exposure in early elementary school or at the end of compulsory schooling. This is consistent with differences in achievement growth and productivity of investments in human capital at different stages of childhood (e.g., Cunha and Heckman, 2007; Bloom et al., 2008; Fischer et al., 2020; Carneiro et al., 2021). While the effects on school leavers could also reflect potential "scarring" from entering the labor market during the transition period, such effects typically dissipate over time and thus are unlikely to drive the flat pattern of earnings losses through retirement that I document.[5] The overall losses were borne entirely by men, likely due to more adverse effects on skills and unfavorable occupational sorting. For men, the policy also increased income dispersion because of the greater harm at the bottom of the income distribution.

This paper contributes to several strands of the literature. First, I build on broader research on the long-run returns to additional time spent in school. Most previous work estimates the labor market effects of extended compulsory schooling (e.g., Pischke and von Wachter, 2008; Stephens and Yang, 2014; Bhuller et al., 2017; Aryal et al., 2022) and longer school terms or days (e.g., Parinduri, 2014; Fischer et al., 2020; Dominguez and Ruffini, 2023). So far, the evidence is mixed.[6] I add to the literature by providing comprehensive evidence on the life-cycle effects of a substantial adverse shock to the length of schooling. Moreover, unlike in many contexts where additional instruction is accompanied by new curricula, the core curriculum remained unchanged during the short school years.

Second, my findings contribute to research on the consequences of situations that force students to temporarily interrupt schooling due to inclement weather conditions (e.g., Marcotte and Hemelt, 2008; Goodman, 2014), natural disasters (e.g., Sacerdote, 2012), teacher strikes (e.g., Belot and Webbink, 2010; Baker, 2013; Jaume and Willén, 2019), or extended summer breaks (e.g., Kuhfeld et al., 2020). Most papers find negative effects of such relatively short but mostly high-frequency interruptions on academic achievement. Little is known about their long-term effects. For Argentina, however, Jaume and Willén (2019) estimate that average exposure to teacher strikes during primary school reduces earnings by 3.2% for men and 1.9% for

[5]The "scarring" effect of entering the labor market during a recession is typically estimated to be 2%-6% of lost cumulative earnings in the first 10 years and decays to zero thereafter (e.g., Oreopoulos et al., 2012; Liu et al., 2016; Schwandt and Von Wachter, 2019; Rothstein, 2023).

[6]For example, Fischer et al. (2020) compare the earnings effects of two reforms that increased instructional time in Swedish primary schools by similar amounts. They find a 5% return to extended school term but a smaller (2%) return to extended compulsory schooling, which they attribute to a later exposure to additional instruction. However, a Norwegian compulsory schooling reform that affected even older children increased lifetime earnings by about 8% (Bhuller et al., 2017). At the other extreme, several studies find no significant returns to additional schooling (e.g., Pischke and von Wachter, 2008; Stephens and Yang, 2014).

women, largely due to lower educational attainment. I complement this literature by analyzing the effects of a large, one-time loss of classroom instruction.[7]

Third, my paper also connects to the literature on school closures in response to the spread of infectious diseases, which has recently gained prominence due to COVID-19.[8] Globally, schools were completely closed (on average) for nearly 5.5 months, or two-thirds of an academic year, with considerable variation between and within countries (UNESCO, 2021, 2023).[9] Although remote instruction was quickly adopted in many places, its actual implementation and effectiveness have often been questioned.[10] Eventually, students made little progress while learning at home (e.g., Andrew et al., 2020; Anger et al., 2020; Wößmann et al., 2020; Bansak and Starr, 2021; Grewenig et al., 2021; Engzell et al., 2021; Maldonado and De Witte, 2022).[11] International data reveal an average decline in student achievement equivalent to at least six months of learning (Betthäuser et al., 2023; Jakubowski et al., 2024). It is generally difficult to extrapolate from a historical context to modern settings, and the COVID-19 pandemic was arguably unprecedented.[12] Nevertheless, assuming similar dynamics in the transmission of cognitive deficits to adult outcomes, we might expect long-lasting consequences of recent school closures for future labor markets.

Finally, the natural experiment that I exploit contributes to the literature evaluating school scheduling policies such as year-round school calendars, four-day school weeks, or multishift schedules (e.g., Lusher and Yasenov, 2016; Thompson, 2021; Landon and Pope, 2023). They typically aim to allocate mandated instructional time more efficiently, but may also be designed to achieve cost savings through reduced in-school instruction (e.g., Bray, 1990). So far, this research has focused on the effects on student achievement. I extend this evidence by showing that a shift in the school year schedule that results in reduced instructional time can have adverse consequences that go beyond learning outcomes. By showing persistent effects on skills, postsecondary education, and job choice, this paper also helps us better understand the mechanisms behind such unintended effects and provides important lessons for thinking about school scheduling decisions today.

[7] The average exposure to teacher strikes during a seven-year primary school in Argentina was 88 days. During the two short school years studied here, most students lost about 120 days of instruction.

[8] Prior to COVID-19, disease outbreaks (including flu) accounted for 1% of all unexpected school closures in the US (Wong et al., 2014). The leading causes were severe weather (79%) and natural disasters (14%).

[9] For another five months, schools were only partially open (i.e., for certain age groups or with reduced in-person instruction). Some countries (e.g., Iceland, Greenland) reopened fully after one month, while others (e.g., Germany, Italy) took ten months. Partial school closures in some US regions lasted nearly 20 months.

[10] See e.g., Huebener et al. (2020); Bacher-Hicks et al. (2021); UNICEF (2021); Jack et al. (2023).

[11] The learning deficits appear to persist over time and are largest for the most disadvantaged groups. For reviews, see Hammerstein et al. (2021); Werner and Woessmann (2023); Jack and Oster (2023).

[12] Much shorter school closures during the 1918 flu pandemic had no wage effects, but also no effects on human capital (Ager et al., 2024; Dahl et al., 2023). These studies examine closures of 36 days (mean) in the US and 16 days (median) in Sweden.

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

2 Institutional background

In Germany, the responsibility for educational policy lies with the federal states (see, e.g., Helbig and Nikolai, 2015). Historically, the school year in all states began after the Easter break (see Appendix A, Table A.1), which was centrally moved to the fall by the Nazi regime. After World War II, several states gradually restored the historical Easter rule by shortening one school year that began in the fall but ended before Easter. In 1955, within the so-called *Düsseldorf Accord*, the Ministers of Education of all states agreed on April 1st as the starting date of the school year, which was never implemented in Bavaria (Koebe and Marcus, 2022). However, as part of the *Hamburg Accord* in 1964, Germany decided to follow the practice of most European countries and start the school year in the fall (DIE ZEIT, 1966).[13]

Most states shifted the start of the school year from spring to fall (i.e., 8 months backward) within two shortened school years, running from April to November 1966 and from December 1966 to July 1967 (Pischke, 2007). Thus, each short school year effectively reduced the length of instruction by one-third of a regular school year. This applied to all children attending school during the transition period, except for students in Bavaria, Hamburg, and Lower Saxony. Bavaria was not affected because it already started the school year in the fall. Hamburg made the change by extending one school year, which actually counted as two grades, but meant no enrollments and no graduations between April 1966 and August 1967. Moreover, the lost time lost was added to the students' final grade, so that the change did not affect their effective length of schooling. The same applied to most students in Lower Saxony.[14]

Most affected students experienced two short school years, with two exceptions: graduating classes in the first short school year and school beginners in the second short school year were affected in only one grade (see Appendix A, Figure A.2). Thus, despite being a one-time change, the policy affected different birth cohorts differently and had implications for millions of students who entered primary school well before 1966. Moreover, during the relevant period, the cutoff dates for school enrollment were state-specific. Thus, the exposure to the policy was largely determined by an individual's date of birth and the state of school attendance. Figure 1

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

[14]There, the final grade was extended in the lowest secondary track, which was the most common track, and ended up taking a full nine years as usual. Students in more advanced tracks, however, experienced shorter schooling (Koebe and Marcus, 2022). In the main analysis, I consider Lower Saxony as an untreated state, but my results remain unchanged when it is excluded from the analysis (see Appendix F, Figure F.3).

summarizes the state-specific exposure at the monthly level for individuals born between 1944 and 1963 (i.e., enrolled between 1950 and 1970).

Specifically, Figure 1 shows the effective duration of compulsory education as a function of exposure to short school years and the state-specific compulsory schooling requirements (eight or nine years).[15] For example, Schleswig-Holstein and Hamburg mandated nine years of compulsory schooling, but Schleswig-Holstein's students born between April 1951 and November 1960 experienced a compression to 8.66 (or 8.33) years due to exposure to one (or two) short school year(s). A similar pattern applies to Bremen, which, however, extended compulsory schooling from eight to nine years at the beginning of the period. The details of the parallel expansion of compulsory education in several states are described in Appendix B. Note that North Rhine-Westphalia, Hesse, Rhineland-Palatinate, and Baden-Wuerttemberg used the short school years in 1966/67 to introduce the compulsory ninth grade.

I focus on exposure to short school years during compulsory education (i.e., up to grade nine), because in the 1960s the vast majority of students left school after that. Typically, after four years of primary school, German students are sorted into different secondary school tracks, which differ in duration and curriculum.[16] During the relevant period, about 70% of fifth graders attended the basic track (see Appendix A, Figure A.3). Its duration corresponded to the compulsory schooling requirement. The middle track and the academic track (high school) lasted until grade ten and 13, respectively. Students in these more advanced tracks may have been affected by the short school years beyond grade nine. However, the dropout rate from high school was high (Van De Graaff, 1967), so that less than 10% of a given enrollment cohort actually continued beyond grade ten (see Appendix A, Figure A.4).

Although the short school years substantially compressed the amount of in-school instruction, much emphasis was placed on maintaining the core curriculum (Landesarchiv, 2020).[17] Priority was given to mathematics, German, and modern foreign languages (mostly English). The weekly hours of instruction and the homework load in these subjects increased.[18] In contrast, the instructional hours in non-core subjects, especially in music, arts, physical education were reduced. Beyond the mandatory curriculum, German schools play only a minor role in offering extracurricular activities, which are mostly offered in non-school-based clubs (Felfe et al., 2016). However, during the transition of 1966/67, traditional school events such as Christmas pageants and school trips were canceled. Many states also reduced the number of in-class tests

[15]In Germany, compulsory schooling laws are grade-based, i.e., they require a minimum number of completed years of education, independent of school starting age and the age upon completion.

[16]For details, see e.g., Lüdemann and Schwerdt (2013).

[17]I refer here primarily to contemporary sources such as newspaper articles and historical documents (about 400 pages) that I obtained from the State Archives of Baden-Württemberg.

[18]Teachers giving after-school math and writing lessons was an exception (Thiel, 1973). Minor compensatory measures taken by the states included radio broadcasts of English lessons (Landesarchiv, 2020).

and the requirements for final examinations. Contemporary witnesses also reported that teachers often overlooked knowledge deficits when making decisions about grade progression and tracking recommendations (Drewek, 2020). The potential impact of the policy on academic performance was a much-discussed topic in the press.

3 Data

3.1 Social security records (SIAB 1975-2017)

For the main analysis, I use register records from the Sample of Integrated Labor Market Biographies (Antoni et al., 2019).[19] The SIAB is a 2% sample of the population covered by the German social security system at least once between 1975 and 2017, mainly due to employment or receipt of unemployment benefits. The data represent approximately 80% of the total workforce in Germany because civil servants and the self-employed are not subject to social security. The main advantages of the data are the large sample size and the accuracy of information on employment biographies, earnings, and birthdates.

I consider German citizens born between 1944 and 1963 to ensure long earnings histories. The original data is organized by spells, and earnings are reported as gross daily pay in EUR, which I deflate to 2015 prices using the consumer price index (OECD, 2020). The payroll information on earnings is only included up to the statutory social security contribution ceiling, which is relevant for the calculation of unemployment insurance benefits and old-age pensions. All earnings above are top-coded (i.e., about 5% of all spells). I impute top-coded earnings using the two-step procedure of Dauth and Eppelsheimer (2020), but my main results change little when I use the top-coded values.

From the spell data, I construct an annual panel of total earnings and days worked for each individual between the ages of 20 and 64, which covers the potential working life. The SIAB's time frame (1975-2017) implies that the panel is unbalanced, as 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 though I miss the earliest or the latest career years for some cohorts. However, I also show the results for prime-age outcomes, defined as sums over ages 31-54, where the panel is balanced in cohorts. For comparability, I restrict the estimation samples to individuals whom I observe at least once (in employment or unemployment) between ages 31 and 54.

There is no detailed information on educational trajectories in social security records, so I do not observe the actual exposure to short school years. However, I can infer potential exposure to

[19] Specifically, I used 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.

the policy from an individual's date of birth and state-specific cutoffs for school enrollment.[20] For this purpose, I collected data on the relevant institutional details, which I describe below. To link the data, I use the first state of residence or employment ever reported for a given individual to social security as a proxy for state of schooling, since I do not directly observe it (nor place of birth). The resulting measurement error yields a small attenuation bias because, for the cohorts studied, cross-state mobility was low and uncorrelated with the exposure to short school years.[21]

In general, education variables in the SIAB are a by-product of reported employment or unemployment spells, and the focus is mainly on postsecondary credentials (Fitzenberger et al., 2006). Thus, I am not able to measure years of schooling. However, to examine the potential effects on educational attainment, I construct indicators for a high school diploma, a college degree (incl. university), and any vocational credential. The final sample comprises nearly 7.7 million annual observations on 278,797 individuals. Table A.2 in Appendix A presents the descriptive statistics.

3.2 Database with policy variables

I merge the SIAB with rich data on institutional details compiled from primary sources.[22] Specifically, for each combination of year and month of birth between January 1944 and December 1963, I collected information on the expected date of school entry (according to the state-specific cutoffs) and the date of the earliest possible school exit (from compulsory schooling laws). The difference between the two dates corresponds to the state- and cohort-specific duration of compulsory schooling. A duration of less than the mandated eight or nine years indicates exposure to short school years (see Figure 1). Specifically, downward deviations of one-third and two-thirds of a year correspond to one and two short school years, respectively. Thus, my treatment variable equals $1/3$ or $2/3$ for cohorts exposed to the policy and zero otherwise, thereby measuring the amount of lost in-school instruction (in years).

As additional state- and cohort-specific variables, I include the expected age of school entry and the size of the enrollment cohort, as all states affected by short school years adjusted their enrollment cutoffs to the new school year schedule.[23] I augment the data with information

[20] Grade retention was rare. In 1965, only 0.4% of first graders were retained, and the number was even lower in higher grades (DESTATIS, 1965).

[21] Appendix C provides supportive evidence on these issues from the National Educational Panel Study (NEPS; see Blossfeld and Roßbach, 2019).

[22] I obtained the data from public records such as original state laws (Makrolog, 2019), school vacation dates (KMK, 2020), aggregate administrative statistics on new school entrants, ninth grade attendance, school leavers (DESTATIS, 2021), and historical newspaper articles and policy documents (Landesarchiv, 2020).

[23] Expected age at enrollment is the scheduled enrollment date minus birthdate, which ranges from 5.6 to 7.6. The size of the enrollment cohort is measured as the number of birth months simultaneously scheduled for enrollment. While a typical school cohort consists of children born over 12 months, any shift in the cutoff date results in a one-time change in the number of birth months scheduled for enrollment. Using the number of individuals simultaneously scheduled for enrollment yields identical results.

on time-varying state-specific student-to-teacher ratios from statistical yearbooks (DESTATIS, 2021) as a proxy for schooling quality. I link all policy variables to the individual-level data from SIAB based on birthdate and state. Nearly one-third of the individuals in my sample were exposed to at least one short school year (see bottom panel of Table A.2 in Appendix A).

3.3 Complementary datasets

To address some limitations of the SIAB, I use three complementary datasets for auxiliary analyses. First, I use administrative records from the German Statutory Pension Insurance. In contrast to the SIAB starting in 1975, the pension data include all pension-relevant events (e.g., employment) from the age of 14 onwards (regardless of the calendar year), which allows me to estimate the effects of short school years directly upon labor market entry. These records do not directly document earnings, but statutory "pension points" from labor income are a close proxy.

Second, I use cross-sectional data from the German Micro Census, which is the largest national household survey. In contrast to the SIAB, the Micro Census includes civil servants and the self-employed who are not subject to social security contributions. Respondents report their net monthly income, including all sources of income such as wages, salaries, pensions, and public transfers. The survey provides additional measures of educational attainment, which allow me to examine the effects on completed school degrees and years of schooling.

Finally, I complement my results for labor market outcomes and educational attainment by examining other domains of human capital such as cognitive skills, the Big Five Inventory of personality traits, and other socioemotional skills in the German Socio-Economic Panel (SOEP) (Goebel et al., 2019). Because these outcomes were only collected in selected years, the available sample sizes are relatively small and depend on the outcome. Appendix D provides details and summary statistics for these additional datasets.

4 Estimation strategy

My empirical approach exploits the variation in the exposure to short school years across states and birthdates. 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 at monthly level (i.e., year and month of birth). My main outcomes are lifetime earnings (in 2015 EUR or in logs) and days of employment, but I also estimate age-specific regressions where earnings and employment are measured annually at a given age. The treatment variable is SSY, which measures the amount of classroom instruction (in years) lost due to exposure to short school years. Thus, α is a dosage parameter that accounts for treatment intensity. All regressions include state (θ_s) and birthdate (θ_t) fixed effects and a gender dummy (θ_f).[24] The vector of additional policy variables Z is described 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 birthdates. The state-fixed effects capture any unobserved determinants of the outcomes that differ across the states and are sufficiently constant over time such as the quality of teachers' education, the effectiveness of educational administration, labor market structure. The time fixed effects flexibly account for any common changes that took place over time such as general demographic trends and increasing educational attainment. 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 the compulsory schooling requirements, which potentially affected the outcomes in the opposite direction and may bias the estimated effect of short school years toward zero. Therefore, similar to Pischke (2007), I include an indicator for this parallel reform in Z. Appendix B provides a graphical illustration of the underlying variation that my DiD regressions use for identification conditional on the compulsory schooling regime.

Another challenge is that moving the start date of the school year from spring to the fall automatically changed the timing of school entry for newly enrolled students and many states adjusted their enrollment cutoffs to the new schedule. Thus, I also include the expected age at school entry and the expected size of a given enrollment cohort in Z. To mitigate any concerns that there could still have been other factors that disproportionately affected the states over time, Appendix F shows that my main results hold after including further regional controls.

Given the reduced-form nature of equation 1, the estimate of α reflects an intention to treat effect of the exposure to the short school years. Thus, I do not control for any factors that could have been endogenously affected by the treatment such as educational attainment, occupational status, and demographic outcomes. These are potential mediators and including them as (bad) controls could bias the estimate of interest.

The estimate on SSY is mainly identified from the one-time introduction in 1966/67.[25] Nevertheless, I exploit the staggered exposure to the treatment across birthdates that arises due to state-specific cutoffs for school enrollment. Even when parallel trends hold, a conventional DiD estimation might be biased if there is treatment effect heterogeneity due to a staggered treatment (Goodman-Bacon, 2021). Thus, I also apply an imputation estimator proposed by

[24] The time fixed effects θ_t include 239 indicators for each unique combination of year and month of birth between February 1944 and December 1963 (= 20 years x 12 months -1). January 1944 is the omitted reference category.

[25] The earlier occurrences of short school years in Baden-Wuerttemberg and Saarland (see Figure 1 and Appendix B) apply to a small percentage of my sample. My results remain unchanged if I exclude them (see Table 1).

Borusyak et al. (2024), which possesses attractive efficiency properties and allows for an analytic computation of conservative standard errors.[26]

Regarding inference, in my main specification, the standard errors are clustered at the state level, which is a common practice in DiD designs using variation across regions and over time. However, given the small number of states in Germany, clustering at the state level may be subject to finite sample problems (Cameron and Miller, 2015) and require the Wild cluster bootstrap (Cameron et al., 2008). Recently, Abadie et al. (2023) have argued that state-level clustering may be unnecessarily conservative, possibly by a large margin. Given the ongoing debate, Appendix F reports the results of several plausible inference methods.

5 Results

5.1 Effects on labor market performance

I begin by examining the effects on age-earnings and age-employment profiles in Figure 2. Following the literature on the life-cycle effects of other policies (e.g., Fredriksson and Öckert, 2014; Bhuller et al., 2017), the age-specific earnings include zeros. Thus, the estimates are not biased by potentially selective sorting into employment and capture both labor supply and wage responses to short school years. Each estimate comes from a separate age-specific regression using my main model specification. The vertical dashed lines mark the prime-age interval for which the panel is balanced in birth cohorts. Estimates outside of this range may reflect a different sample composition and should therefore be treated with some caution.

Figure 2a suggests that the policy led to earnings losses over almost the entire career. Only the point estimates at ages 20 and 21 are positive, possibly reflecting a mechanical effect of an earlier graduation due to shortened schooling. Indeed, repeating the analysis in the pension data yields positive premiums between ages 15 and 20 of up to 20% relative to the age-specific mean (see Appendix A, Figure A.5a). This confirms the expected acceleration of labor market entry.[27] Most of the estimated effects later in life are negative and statistically significant but moderate in magnitude; in the thirties and forties, the relative earnings losses are of 2% to 4% per year. The pattern remains remarkably flat through the prime ages and appears to extend beyond.[28] Figure 2b shows the corresponding effects on age-employment profiles. It confirms

[26]Moreover, unlike most robust estimators recently proposed in the literature (for reviews, see, De Chaisemartin and d'Haultfoeuille, 2023; Roth et al., 2023), the imputation estimator can be implemented when the treatment is nonbinary and non-absorbing. However, Appendix F shows that my conclusions hold when I use a binary treatment definition (i.e., ignoring exposure to one versus two short school years) and restrict my sample so that the treatment is an absorbing state (i.e., does not turn off). I also show the diagnostics suggested in de Chaisemartin and D'Haultfoeuille (2020).

[27]Using the pension data, I estimate that, on average, the affected individuals were almost half a year younger at labor market entry (see Table 2).

[28]The estimates for ages near retirement could be biased if the short school years affected retirement behavior or

the expected increase in labor supply at the beginning of the career and a persistent, but rather small, decline later in life.[29]

The opposite direction of the effects early and later in the career may explain why Pischke (2007) and its recent replication in Koebe and Marcus (2022) found no labor market effects using pooled cross-sectional data collected up to the early 2000s. In particular, their estimates reflect an average effect over the first two-thirds of the career, when the positive and negative effects potentially cancel each other out. I illustrate this issue in Appendix E. Specifically, cross-sectional regressions that pool the annual outcomes over all ages and calendar years included in Figure 2 confirm the overall negative effect of the policy. However, the estimates vanish to zero when I restrict the sample to closely resemble the younger age structure in previous research.[30] In general, Figure 2 reveals that it is crucial when we measure the effects. Thus, my results do not necessarily contradict previous findings, but rather complement them with new insights from a life-cycle perspective.

Table 1 quantifies the lifetime consequences of the short school years. The outcomes correspond to the sum of earnings or days worked between the ages of 20 and 64, but using only the prime ages of 31 to 54 leads to similar conclusions (see Appendix A, Table A.3). Column 1 begins with a simplified specification of Equation 1, which I extend through column 4 to document how other policy changes affect the results. In column 5, I restrict the sample to omit a small number of observations affected by earlier short school years that occurred in Baden-Wuerttemberg and Saarland before 1966/67 (see Appendix B for details). Finally, using the restricted sample, I apply the imputation estimator of Borusyak et al. (2024), which accounts for a potential bias from treatment effect heterogeneity.

Panel A documents the effect on lifetime earnings. The estimate in column 1 is negative but small and insignificant, amounting to a loss of 0.5% relative to the sample mean. In column 2, I control for the parallel expansion of compulsory schooling in some states. This changes the estimate substantially and it now implies that one lost year of in-school instruction reduces lifetime earnings by 2.8% relative to the sample mean.[31] If anything, including the expected age at school entry (column 3) and the size of the enrollment cohort (column 4) makes the results slightly more conservative. I therefore leave them in the main specification. The results are similar in the restricted sample (column 5), where identification comes only from the 1966/67 change. Finally, the imputation estimator in column 6 yields an even slightly stronger effect

mortality. However, I find no effects on the probability of early retirement or death (see Appendix A, Table A.5).

[29]The patterns for log earnings and employment probability are similar but not identical (see Appendix A, Figure A.6). Thus, the effects operate to some extent along the extensive margin. There is no information on hours worked in the SIAB to examine the intensive margin in more detail.

[30]My data also allow for more accurate treatment assignment due to the availability of information on the exact month of birth, which is essential to split students born within a given calendar year into school cohorts. Appendix E shows that the measurement error further attenuated Pischke's estimates towards zero.

[31]A graphical illustration of the identifying variation behind this result is provided in Appendix B.

than the conventional DiD model.

Panel B focuses on the natural logarithm of earnings, which excludes zero earners. Again, the estimate in column 1 is close to zero but becomes negative and significant when I control for other policy changes. Columns 2 through 4 imply that, conditional on employment, exposure to short school years reduced lifetime earnings by 3% on average. Again, the restricted sample and the imputation procedure yield somewhat larger effects. In Panel C, I use the logarithm of earnings plus one to deal with both zero and very high earnings levels. In most cases, the point estimates almost double, suggesting that the policy affected earnings not only at the intensive but also at the extensive margin. The estimated employment effects in Panel D mirror the evidence for earnings; my preferred specification in column 4 implies a significant decline in labor supply between ages 20 and 64 of about 2% relative to the sample mean.

Given that the timing and the intensity of the treatment depended on the grade attended in 1966/67, the question arises whether the lifetime effects vary by grade. Figure 3 decomposes the average earnings and employment effects (the dark bars) into grade-specific effects (the light bars). The treatment is defined as binary, and the estimates are related to the sample mean. Figure 3a suggests that earnings losses are mostly due to exposure in elementary school (up to grade 4) and in the final grades (8−9).[32] This is consistent with differences in achievement growth and productivity of investment in human capital at different stages of childhood (e.g., Cunha and Heckman, 2007; Bloom et al., 2008; Carneiro et al., 2021). Generally, the exposure to only one versus two short school years yielded similar effects. Similar but less pronounced are the grade-specific employment effects in Figure 3b.

Finally, Table 2 reassesses the lifetime effects when accumulated from age 14 in the pension data. The negative effect on the age of labor market entry (column 1) is consistent with shorter schooling and reveals a significant "first stage" effect of the policy. The results for pension-related points from earnings (columns 2 and 4) confirm a monetary lifetime loss of about 3%. For employment (columns 3 and 5), however, there is no net reduction when accumulated over the entire career. This is due to the offsetting effects of the large employment surplus between the ages of 15 and 20 (see Appendix A, Figure A.5b).

Taken together, I consistently find that, despite earlier graduation and theoretically longer careers, individuals exposed to the short school years did not accumulate significantly more labor market experience over the life cycle. The initial advantage of earlier labor market entry was almost entirely offset by a lower labor supply later in life. Nevertheless, they experienced significant monetary losses, as the persistently lower earnings during the prime ages eventually exceeded the initial gains. Given the direct link between earnings and pension entitlements, the

[32]The last bar shows that a potential exposure beyond compulsory schooling (i.e., in higher grades of secondary school) did not cause any harm. Figure A.7 in Appendix A aggregates the results for elementary school, middle grades, and the graduating classes to gain precision.

negative consequences extend beyond the working life.

The results pass a variety of validity checks and are robust to changes in model specification and sample restrictions (see Appendix F for details). For example, I find no significant pre-trends in the corresponding event studies, supporting the parallel trends assumption. Using the diagnostics suggested in de Chaisemartin and D'Haultfoeuille (2020), I also find that negative weighting issues are not a serious concern. This confirms that my DiD estimates are robust to heterogeneous treatment effects. Extended model specifications confirm that my estimates are not driven by different developments of contemporaneous trends across states. Results from the Micro Census and alternative inference methods also support my main conclusions.

5.2 Potential mechanisms

A natural starting point for investigating the mechanisms behind the earnings losses is the potential impact on educational attainment. The short school years may have affected a student's education production function (Hanushek, 2020) mainly by reducing school inputs (e.g., teachers' time and attention, curriculum, extracurricular activities). However, educational output also depends on family inputs (e.g., parental time and support) and student inputs (e.g., motivation and effort). While parents may have filled some gaps left by schools, contemporary surveys provide no consistent evidence that parental involvement increased (Meister, 1972; Thiel, 1973). There is also no evidence on how well students coped with the necessary changes, other than newspapers suggesting increased stress and anxiety (Landesarchiv, 2020).

Table 3 documents the estimated effects on educational attainment in the SIAB. Educational outcomes are missing for approximately 2% of my sample, but the missing data are not correlated with the treatment (column 1), so endogenous sample selection is not an issue. I find no effects on high school completion and college graduation (columns 2 and 3). However, the policy did prevent some students from successfully completing other types of vocational education or training (columns 4 and 5). The Micro Census shows similar patterns (see Appendix A, Table A.6). It also reveals no effects on the completion of other school tracks and years of schooling, which is broadly consistent with earlier findings.[33]

The lack of effects on secondary school credentials may simply reflect more lenient practices in grading and tracking recommendations during the transition period (Drewek, 2020). However, the poorer postsecondary outcomes suggest that the short school years may still have affected the acquisition of important skills. There is no information on the cognitive development of the relevant cohorts during the critical period, but the SOEP assesses their cognition much later in life (on average, in the 50s). Indeed, the affected cohorts perform significantly

[33]Pischke (2007) found increased repetition rates and a lower probability of attending the middle track, but he argues that the effects were temporary. Grätz (2023) confirms no effects on high school completion, regardless of socioeconomic background.

worse in a symbol correspondence test (see Appendix A, Table A.7). The effect of about 0.25 standard deviation (SD) is substantial compared to an average learning gain over a school year of about a quarter to a third of an SD (Werner and Woessmann, 2023). There is no effect on word fluency, which confirms earlier evidence by Hampf (2019).[34] In contrast to verbal fluency, the symbol correspondence test is positively related to earnings (Anger and Heineck, 2008). Thus, cognitive deficits may be one channel through which the policy has affected postsecondary education and labor market outcomes.

The short school years may also have affected socioemotional development (e.g., through more homework-oriented learning, less peer interaction, less emphasis on extracurricular activities). As for the Big Five personality traits, the SOEP shows a significant decrease in extraversion (i.e., increased introversion) and an increase in neuroticism (see Appendix A, Table A.8). According to APA (2021), introversion refers to a focus on one's inner thoughts, ideas, and feelings rather than on what is happening outside. Neuroticism describes the tendency to react poorly to negative experiences and psychological distress. Both traits are negatively related to wages in the SOEP (Heineck and Anger, 2010; Collischon, 2020). I find no significant effects on other personality traits and socio-emotional skills, but the estimates are relatively imprecise.

The negative effects on skill formation suggest that the affected students may have ended up in jobs with different skill requirements. Table 4 shows the estimated impact on the skill level required for a given job as reported by the employer to social security (in four levels). All estimates are negative. However, the statistically significant effect on the skill requirements in the first job (column 1) is relatively small and diminishes later in the career (columns 2 and 3). A binary measure of job complexity (levels 3 and 4) shows similar patterns, but the initial effect (column 4) is larger and does not disappear completely in the long run (columns 5 and 6). Eventually, affected individuals worked significantly fewer days in relatively complex jobs throughout their careers (2-3% compared to the mean).

These results are consistent with less time spent in school hindering human capital formation. Nevertheless, the initial effects on the first job could also point to some "scarring" effects from unfavorable labor market entry conditions. Indeed, the policy temporarily increased the supply of high school graduates by shortening the interval between the 1966 and 1967 graduating classes. However, such "scarring" would only affect students exposed to the policy shortly before graduation, and the earnings losses are just as large for students exposed in their early grades (see Figure 3). Moreover, "scarring" effects typically fade over time (e.g., Rothstein, 2023) and thus cannot explain the flat pattern of long-run effects in Figure 2.

Given the negative effect on vocational training, we might also expect that the short school years led to a different occupational sorting. I examine this issue using employer-reported

[34]Using a much smaller sample (about 300 individuals) from the Survey of Adult Skills (PIAAC), Hampf (2019) found negative effects on numeracy, but no effects on literacy measured at similar ages.

information on the German Classification of Occupations (KldB). Figure 4 plots the estimated effects on an individual's occupation in the first job, the main occupation throughout the career (i.e. with the most days worked), and the share of days worked in a given occupation in total employment. The results consistently show a significant shift away from service occupations and toward jobs in agriculture and manufacturing.

The long-run effects on educational and labor market outcomes cannot be driven by a different amount of instruction in core subjects because the core curriculum remained unchanged.[35] However, the learning process was partially shifted from classroom instruction to additional homework. Thus, a different mode of instruction may have been important, as suggested by recent evidence from the COVID-19 pandemic (e.g., Engzell et al., 2021; Jack et al., 2023). The negative effects may also result from less emphasis on non-core subjects (e.g., physical education, music, and art) and less engagement in extracurricular activities, as these affect skill development and adult outcomes (Stevenson, 2010; Hille and Schupp, 2015; Knaus et al., 2020). Unfortunately, there are no data to shed more light on these issues. My intention-to-treat analysis captures the overall effect of the multifaceted treatment.

Overall, the results suggest that the short school years did not impede the acquisition of school credentials, but they did affect the subsequent educational pathways and occupational choices. They also left long-lasting imprints on cognitive skills and some labor market-relevant personality traits. These adverse effects on human capital and different occupational sorting are plausible channels for the persistently worse labor market performance during the prime ages.

5.3 Heterogeneities

For the generation under study, educational and labor market choices differed fundamentally by gender. While describing the striking underrepresentation of women among high school graduates and university entrants in West Germany in the 1960s, Van De Graaff (1967) points to traditional social roles as inhibiting women's academic and professional ambitions. Thus, the short school years may have led to different responses by gender. Indeed, Table 5 shows that the negative labor market effects are entirely driven by men. For women, the point estimates are even positive but insignificant.[36] Gender-specific estimates for educational attainment generally confirm the negative impact on post-secondary education (see Appendix A, Table A.10). The point estimate is statistically significant only for men, but even slightly larger for women.[37]

That the labor market responses are driven by men, despite similar effects on educational

[35] For evidence on the medium- and long-run effects of instructional time in core subjects, see, e.g., Joensen and Nielsen (2009); Rivkin and Schiman (2015); Lavy (2015); Goodman (2019); Lavy (2020).

[36] The pension data and the Micro Census show the same patterns (see Appendix A, Table A.9). Gender-specific estimates of the life-cycle profiles also lead to similar conclusions (see Appendix A, Figure A.8).

[37] The Micro Census supports this conclusion (see Appendix A, Table A.11).

attainment, is reassuring because men are less likely to be affected by selective labor force participation or any endogenous effects on fertility or marriage. However, such differences are unlikely to explain the heterogeneous earnings effects because the short school years only affected the timing of family formation, not the probability of ever marrying or becoming a parent, and the effects were similar by gender (Koebe and Marcus, 2022).[38]

An alternative explanation could be that West German women of the generation studied were weakly attached to the labor market. Presumably because of prevailing social norms, women tended to work part-time and for few hours. Social security data do not include hours worked, but the gender differences in mean outcomes (see Table 5) provide suggestive evidence of the extent of this phenomenon. Specifically, men's lifetime earnings are more than twice as high as women's, which cannot be explained by a lower female participation rate alone, as women worked on average "only" one-fifth fewer days than men. This suggests considerable part-time work among women.[39] Given the relatively weak attachment, women may not have been penalized as much as men in the labor market for lower educational attainment due to short school years.

Nevertheless, the heterogeneous earnings responses could also be due to gender-specific effects on skills. Indeed, the SOEP data suggest that the cognitive decline and the increase in neuroticism were greater for men (see Appendix A, Table A.12). In contrast, affected women have developed higher conscientiousness, which is positively related to lifetime earnings (Gensowski, 2018). These results should be interpreted with caution due to small samples, but the negative effects on job skill requirements in the SIAB are also larger and more persistent for men (see Appendix A, Table A.13). Affected men are also more likely to end up in agricultural occupations, which seems to be driven by a shift away from technical occupations (see Appendix A, Figure A.9). For women, on the other hand, I find a significant shift from (female-dominated) service occupations to (more male-dominated) manufacturing, which may have offset the potential earnings losses from lower education.[40]

Regardless of the precise mechanism, the heterogeneous earnings effects for men and women suggest that overall gender inequality shrank in the affected cohorts. Indeed, I find that the male premium in lifetime earnings declined by almost 11% relative to the sample mean (see Ap-

[38]Consistent with this argument, Table F.6 in Appendix F shows that the estimated income effects are robust to controlling for marital status and number of children.

[39]Although the female participation rate in West Germany rose from 47% to almost 75% between the 1970s and the 2000s, the part-time rate remained unchanged at 35-40% (OECD, 2023a). Over the same period, the female part-time rate in the US was 20% although participation was generally higher. The male participation rate in both countries was consistently close to 95%, with part-time rates of less than 3%. Female part-time work is still common in West Germany, not only among new mothers. In the 2000s, less than 15% (25%) of mothers worked full time when their youngest child was 7-9 (12-15) years old (Dehos and Paul, 2023).

[40]Most women in my sample (85%) work in services (followed by 10% in manufacturing). Among men, 45% work in services, 40% in manufacturing, and 10% in technical occupations.

pendix A, Table A.14).[41] The corresponding effect on the employment gap is 21%. The results for prime-age outcomes are almost identical. However, it is important to emphasize that the narrowing of the overall gender gaps was entirely to the detriment of men (who experienced large earnings losses) and not to the benefit of women (who experienced zero effects).

Another important source of heterogeneity could be the socioeconomic background of the students if, e.g., better-educated parents were more likely or able to compensate for lost classroom instruction at home. Unfortunately, data limitations preclude any direct evidence on this issue.[42] Nevertheless, given the high intergenerational transmission of socioeconomic status in Germany (e.g., OECD, 2018), differential effects at the lower and upper tails of the income distribution may suggest potentially different responses of the most disadvantaged and privileged students. Indeed, quantile regressions show that low-income men suffered the largest income losses, while the policy generally did not hurt women and high-income men (see Appendix A, Figure A.11).[43] This suggests broader implications for income inequality. Estimated effects on basic measures of income dispersion confirm that the policy increased earnings dispersion for men but not for women (see Appendix A, Table A.15).[44]

Overall, the negative labor market effects are driven entirely by men. For them, the short school years also increased income dispersion due to larger harm at the bottom of the income distribution. More adverse effects on human capital and unfavorable occupational sorting are plausible (though not necessarily exclusive) mechanisms behind the large losses for men. Given the gender-specific effects, the policy temporarily narrowed gender gaps in the labor markets (see Appendix A, Figure A.10).[45] More broadly, the relatively greater disadvantage for low-wage earners suggests that less classroom instruction may exacerbate income inequality.

6 Conclusions

This paper investigates the lifetime effects of lost instructional time on earnings and employment. Specifically, I evaluate a German policy that substantially reduced the duration of two school years in the 1960s while leaving the core curriculum unaffected. The lost classroom instruction was mainly compensated for by assigning additional homework in core subjects and

[41] For this analysis, I collapsed the data by birthdate and state and calculated the relative male premiums in the average outcomes within each cell. The average male premium in lifetime earnings of 1.237 implies that men accumulated nearly 125% more earnings than women (i.e., more than twice as much).

[42] Neither the SIAB nor the Micro Census provides information on parental background. In the SOEP, parental education is missing for almost 15% of the relevant cohorts, and among the valid responses, most parents (about 80%) completed the basic track. The limited variation and the small sample sizes preclude any reliable analysis using sample splits.

[43] This evidence is suggestive rather than causal, as the policy may have induced some individuals to move across deciles, which would bias the estimates.

[44] These regressions are run on data aggregated by birthdate, state, and gender. The outcomes are standardized.

[45] The figure illustrates the policy contribution to the overall trend in the gender gap in prime-age earnings.

a shift in emphasis away from other subjects. So far, evidence on the long-term effects of this policy has been scarce and somewhat inconclusive: while Pischke (2007) found negative effects on some educational outcomes but no effects on wages, Hampf (2019) documented long-lasting deficits in numeracy skills. The affected students are currently nearing retirement, which allows me to study their labor market responses from a life-cycle perspective.

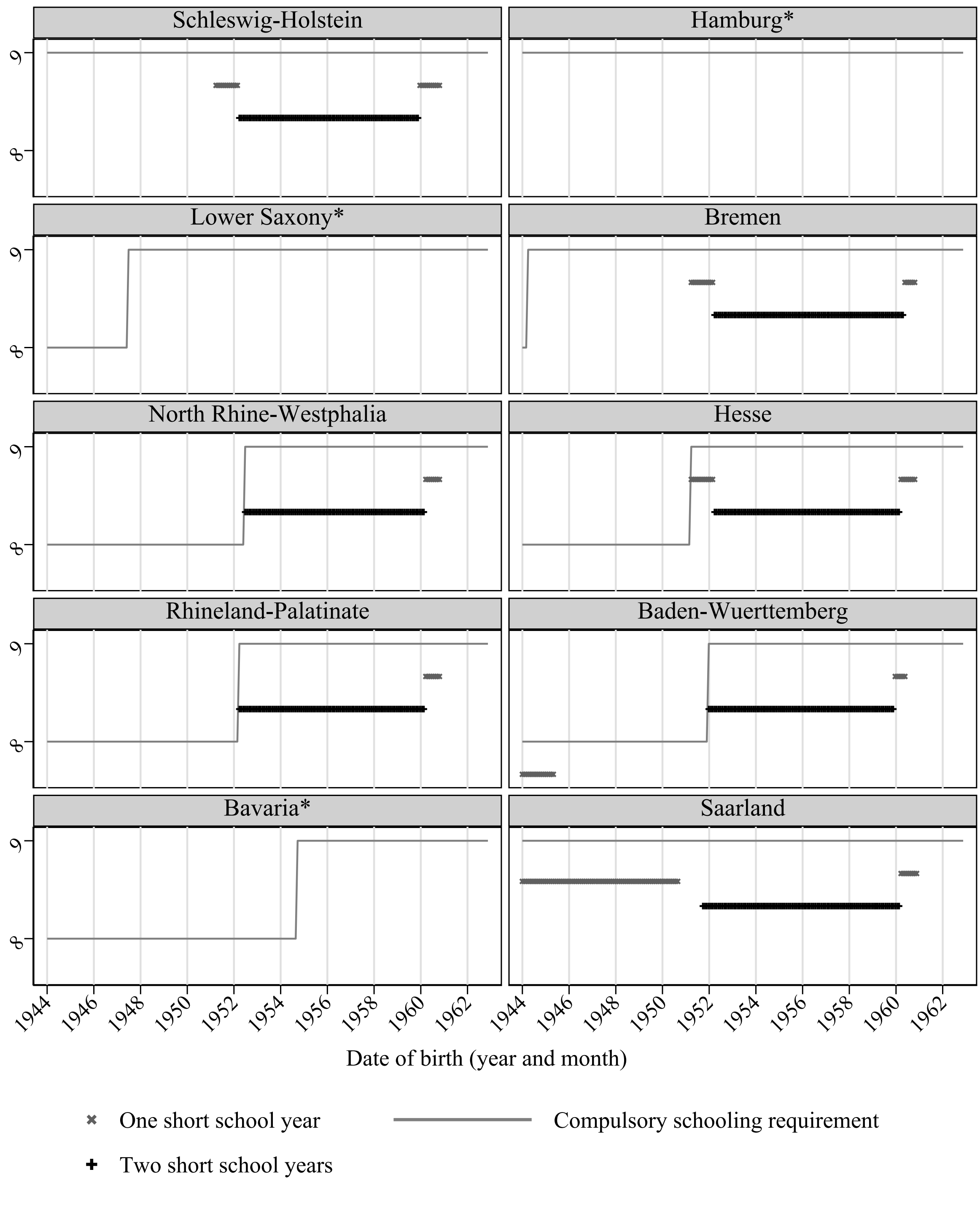
Using social security records with detailed employment biographies, I find that one year of lost classroom instruction reduces lifetime earnings by nearly 3% on average. Thus, for a typical school year of 38 weeks (OECD, 2023b), each lost month translates into a 0.3% loss in lifetime earnings. This is not negligible given the strong emphasis on maintaining the usual curriculum during the short school years. In terms of possible mechanisms, I document detrimental consequences for post-secondary vocational education and occupational sorting. Survey data also show that four to five decades after the reform, affected students score lower on cognitive tests and are more likely to be introverted and neurotic. Taken together, I find consistent evidence of negative consequences for human capital formation. Interestingly, the overall earnings losses are driven by men, likely due to more adverse effects on skills and unfavorable occupational sorting. For men, the policy also increased income inequality, as the largest losses occurred at the bottom of the earnings distribution. This suggests that the impact of lost instruction is more severe for the most disadvantaged students.

The short school years led to turbulent changes in the course of instruction and students' learning routines (Landesarchiv, 2020), and my results suggest their lasting imprints on important skills and labor market performance. Some circumstances resembled the recent school closures during the COVID-19 pandemic (Drewek, 2020; Wößmann, 2020). For example, in both cases, the lost classroom instruction was largely replaced by self-study at home using homework assignments, curriculum materials, and family support.[46] Extensive research documents that students made little progress while learning at home during COVID-19, and many countries have faced historic declines in test scores since (Jakubowski et al., 2024). Assuming a similar dynamic for the transmission of student cognitive deficits to adult outcomes as in the 1960s, we might expect lasting consequences of recent school closures for future labor markets and economies in general (Hanushek and Woessmann, 2020). However, important differences challenge the extrapolation of my findings to the recent context. Unarguably, the pandemic imposed additional costs on children such as a tangible health threat, social isolation, and economic insecurity (Jack and Oster, 2023). Also, many governments have undertaken some remedial efforts, so their effectiveness seems crucial to the question of how current learning deficits ultimately translate into long-term effects.

[46]During COVID-19, digital tools were mostly used to provide students with self-learning resources rather than synchronous instruction with a teacher (UNICEF, 2021). The most technologically advanced format of educational delivery during the short school years was radio broadcasting (Landesarchiv, 2020).

More broadly, my findings are consistent with the need for immediate interventions to address the human capital losses that occur when students lose classroom instruction. By showing direct effects on postsecondary education and occupational choices, this paper also opens a new avenue of research that helps us better understand the mechanisms through which lost instructional time can be detrimental in the long run.

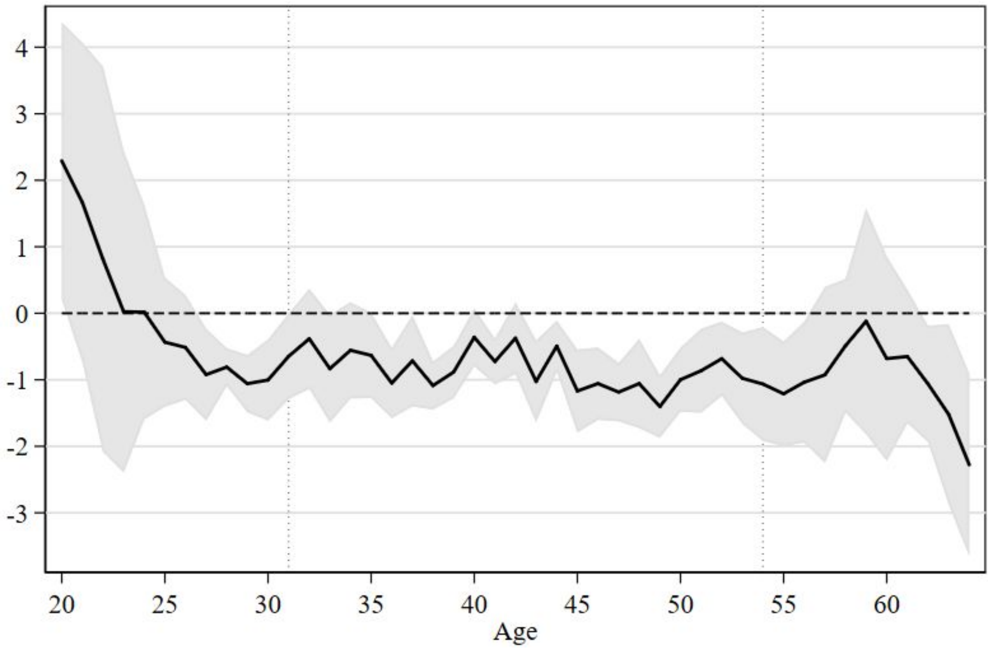
Figure 1: Exposure to short school years for children enrolled between 1950 and 1970



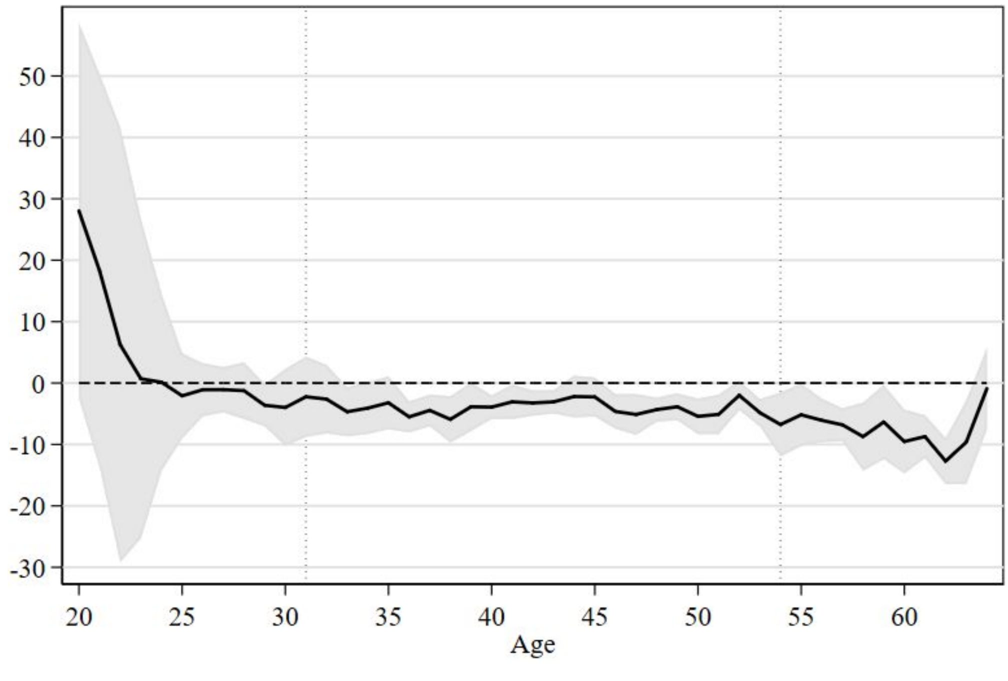
Note: The figure shows the required and effective duration of compulsory schooling depending on exposure to the short school years. The stars mark the control states. In the treated states, the effective schooling duration for birth cohorts exposed to one (two) short school year(s), was by one-third (two-thirds) of a regular school year shorter than the compulsory schooling requirement.
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 2: Effects of the short school years over the live cycle

(a) Effects on annual earnings (in 1,000 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 and birthdate 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. The vertical dashed lines mark the prime-age range of 31-54, for which, the estimation samples are balanced in birth cohorts (1944-1963). Outside this age range, the panel is not balanced due to the time frame of the data.
Source: SIAB 1975-2017; own calculations.

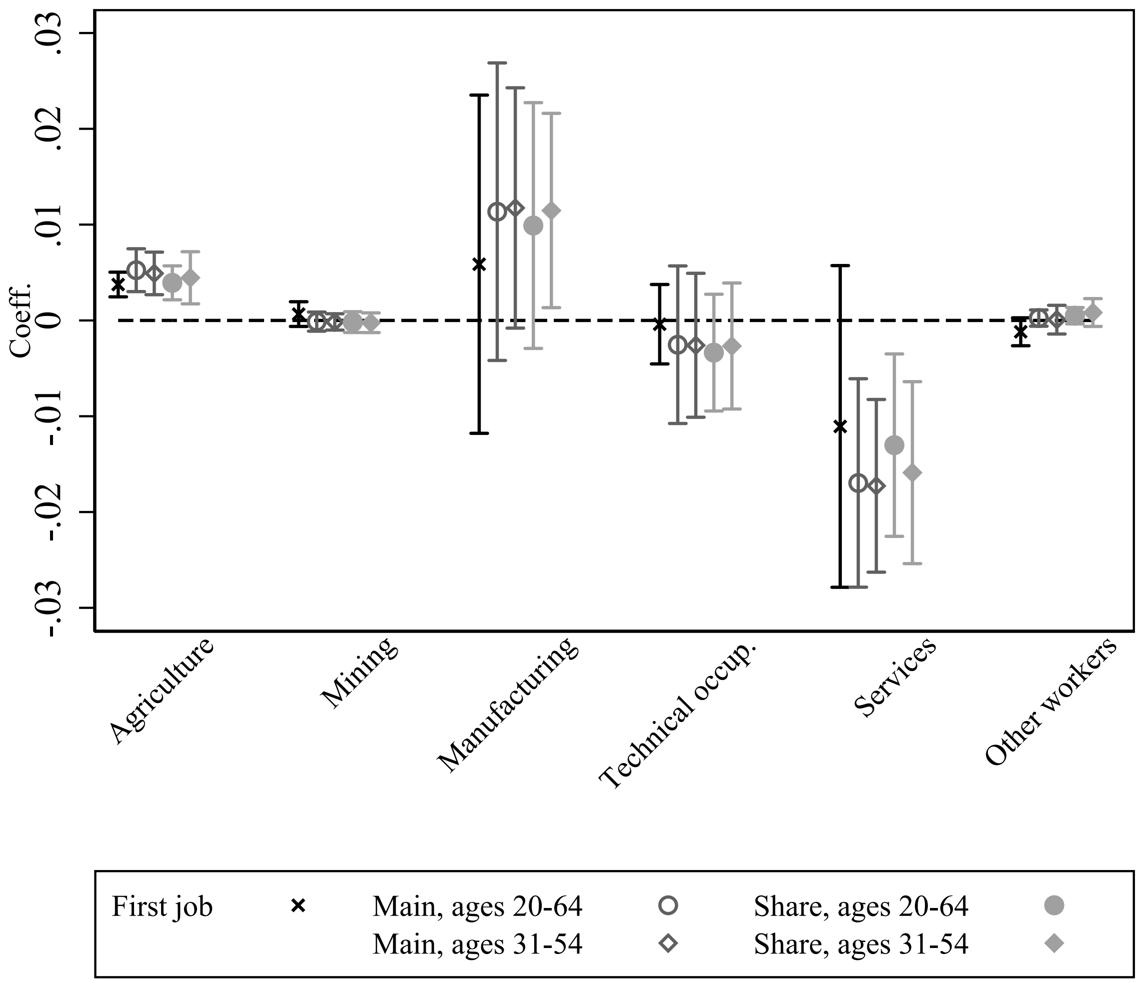
Figure 3: Relative effects on lifetime outcomes (ages 20-64) depending on treatment timing

(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 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) in which SSY is a dummy variable. The brighter bars are based on a separate linear regression of Equation (1) in which SSY is split into eleven dummy variables indicating the expected grade attended at the time of the treatment. Only the school starters of December 1966 and graduating classes of the fall 1966 experienced one short school year. Otherwise, the exposure spanned two consecutive grades. All regressions include state and birthdate 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 used to construct the figures are reported in Appendix A, Table A.4.
Source: SIAB 1975-2017; own calculations.

Figure 4: Effects on occupational area



Note: The six occupational areas (*Berufsbereiche*) correspond to the highest level of aggregation of the German Classification of Occupations (KldB) in its 1988 version. The detailed (three-digit) code of the KldB 1988 contains about 330 values. Since December 2011, employers use a new code (KldB 2010) for declarations, which is recoded in the SIAB according to the KldB 1988. This introduces some measurement error, but it should be negligible at the level of *Berufsbereiche*. Main occupation is defined as the occupation with the most days of employment (in the corresponding age interval). Shares correspond to the ratio of employment days spent in a given occupational to total employment days (in the corresponding age interval). The figure plots the estimated coefficient on SSY in Equation (1). Each estimate comes from a separate linear regression of the respective outcome (dummy or share) on state and birthdate 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.

Table 1: 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: Log (earnings + 1)						
SSY	-0.024	-0.062	-0.063	-0.063	-0.068	-0.066
	(0.016)	(0.022)	(0.021)	(0.022)	(0.025)	(0.008)
Mean dep.	13.051				13.066	
Obs.	278,797				255,298	
Panel D: 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 and birthdate 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 suggested by Borusyak et al. (2024).
Source: SIAB 1975-2017; own calculations.

Table 2: Lifetime effects in Pension Insurance records (from age 14)

	(1) Age at labor market entry	(2) Pension points	(3) Employment	(4) Pension points	(5) Employment
		(total sum over ages 14 - 64)		(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 birthdate 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 3: 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 birthdate 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 4: Effects on skill requirement for a given job

	(1)	(2)	(3)	(4)	(5)	(6)
	Skill level (scale from 1 to 4)			Complex tasks (0/1)		
	first job	highest age 20-64	highest age 31-54	first job	share age 20-64	share age 31-54
SSY	-0.022	-0.010	-0.008	-0.011	-0.006	-0.004
	(0.007)	(0.006)	(0.007)	(0.003)	(0.002)	(0.002)
	[-1.1%]	[-0.4%]	[-0.3%]	[-8.9%]	[-3.1%]	[-2.1%]
Mean dep.	2.119	2.552	2.426	0.124	0.193	0.200
Obs.			278,797			

Note: See Table 3. The skill requirement for a given job is reported by the employer and includes four levels ranging from unskilled (1), skilled (2), complex (3) to highly complex task (4). The complex task dummy indicates jobs with levels 3 or 4. The dependent variable in columns 5 and 6 corresponds to a ratio of days spent in jobs with complex requirements (levels 3 or 4) over total employment days. The estimated effect relative to the respective sample mean of the outcome is reported in brackets.
Source: SIAB 1975-2017; own calculations.

Table 5: Gender-specific effects on labor market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	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 birthdate 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.

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Lifetime Consequences of Lost Instructional Time in the Classroom: Evidence from Shortened School Years

– Appendix –

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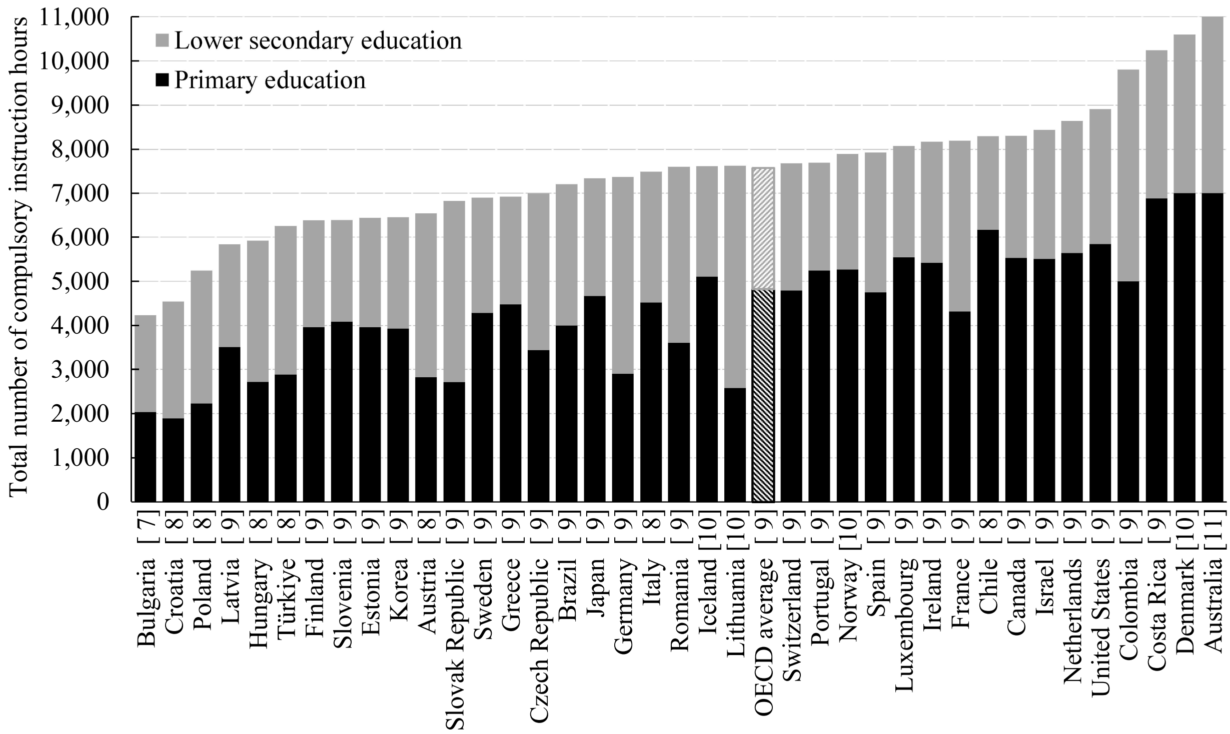
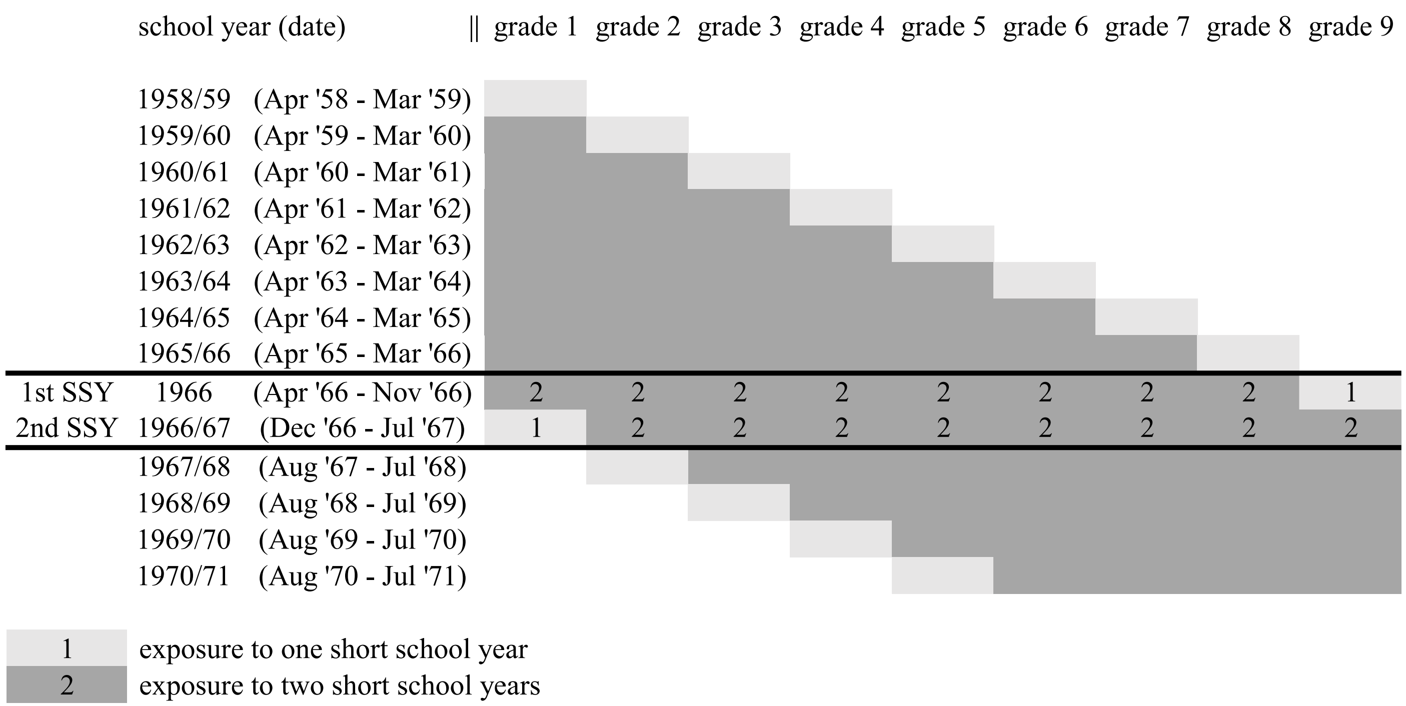
Appendix A Additional figures and tables

Figure A.1: Compulsory instruction time in general education across OECD countries



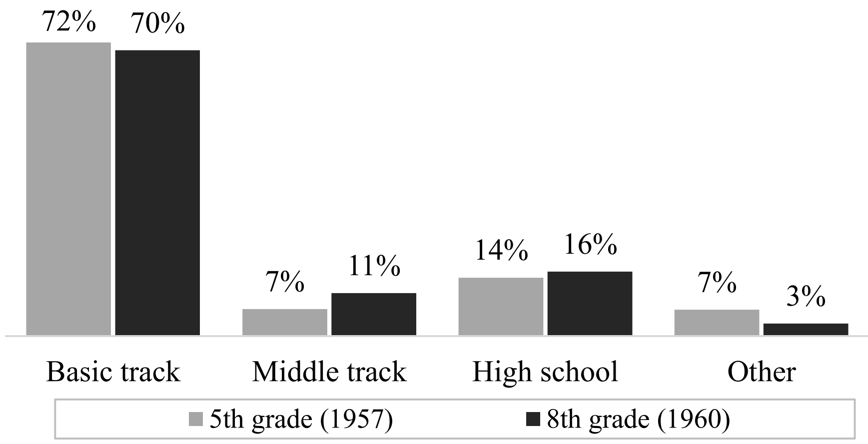
Note: The figure shows the total number of compulsory hours of instruction in public institutions of primary and lower secondary education in 2023 (2022 for Germany). The numbers in square brackets report the country-specific number of years of compulsory education.
Source: OECD (2023b), Table D1.1.; own illustration.

Figure A.2: Exposure to the short school years 1966/67 during compulsory schooling



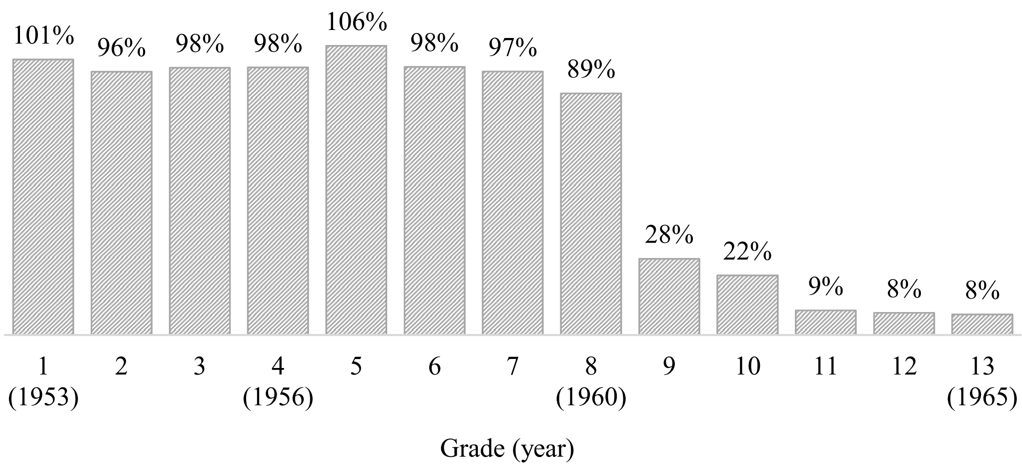
Note: Own stylized illustration.

Figure A.3: 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).

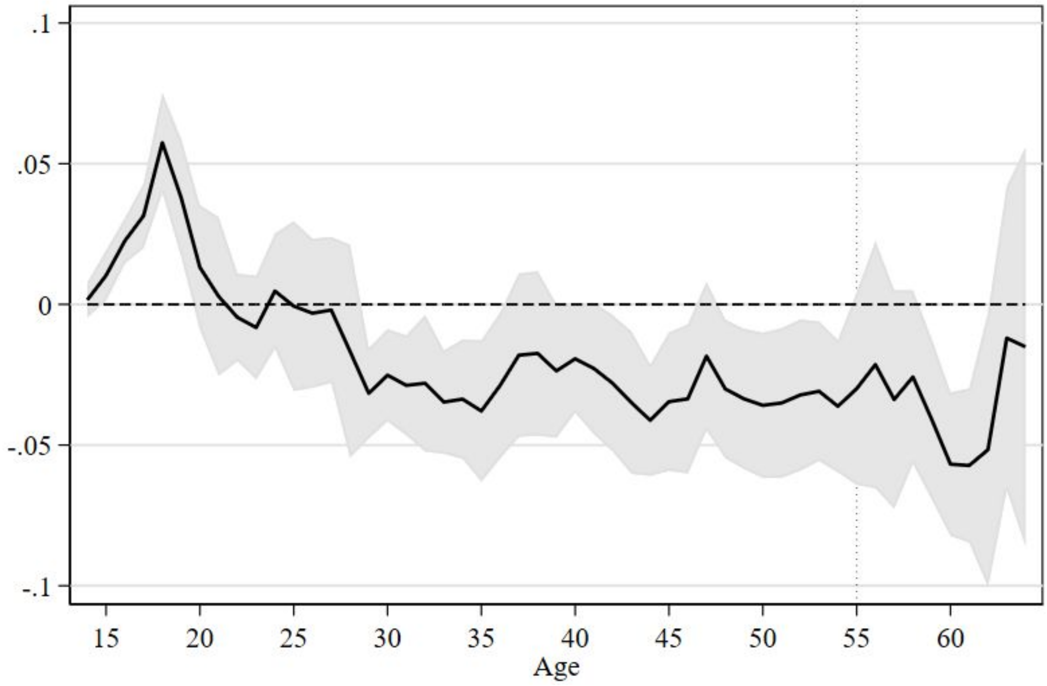
Figure A.4: Grade progression for enrollment cohort 1953



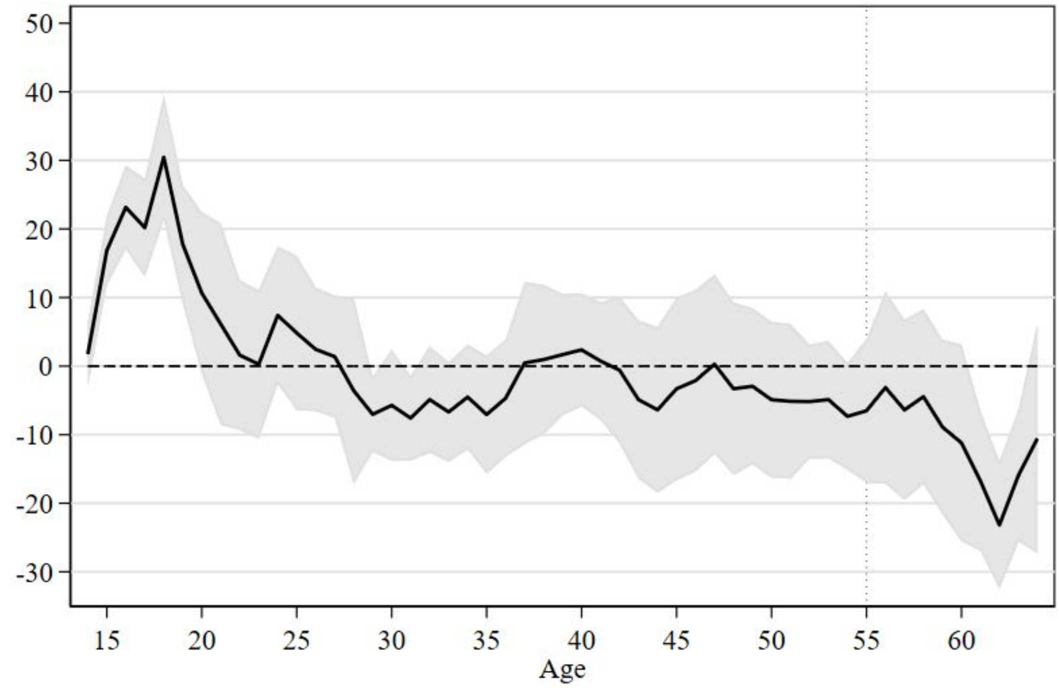
Note: The figure shows the raw number of students in a particular grade (and the relevant calendar year in parenthesis) relative to the number of students enrolled in 1953. Grade 4 corresponds to the final year in primary school. Grade 8 (9 in Schleswig- Holstein, Hamburg, and Bremen) marks the end of 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.5: Effects of the short school years over the live cycle

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



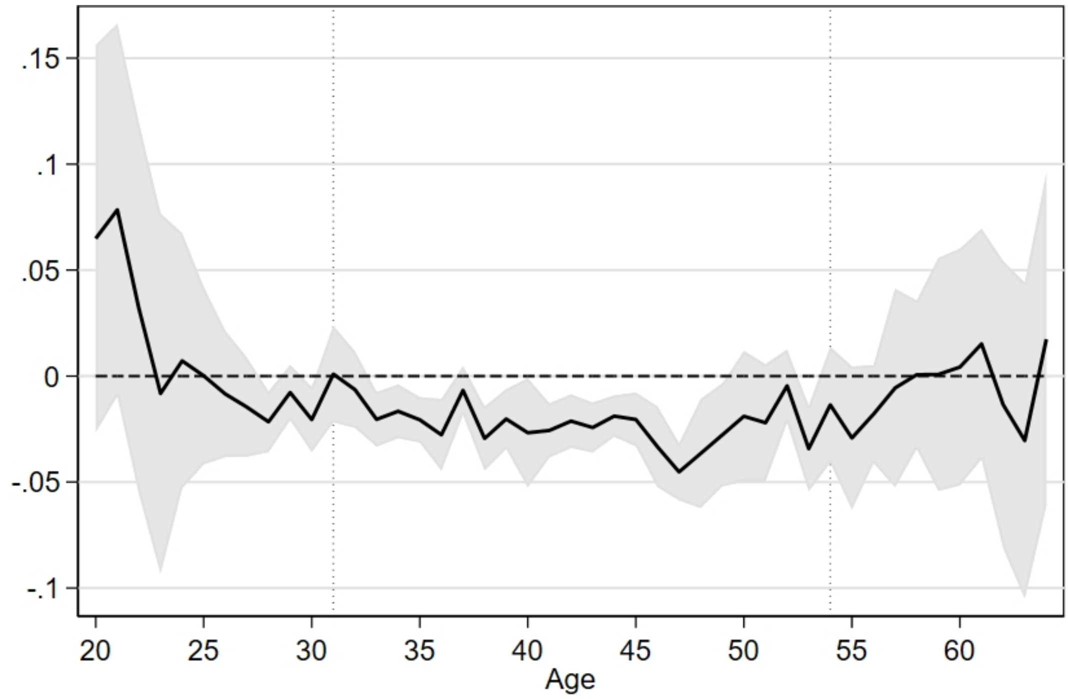
(b) Effects on the annual number of days in employment



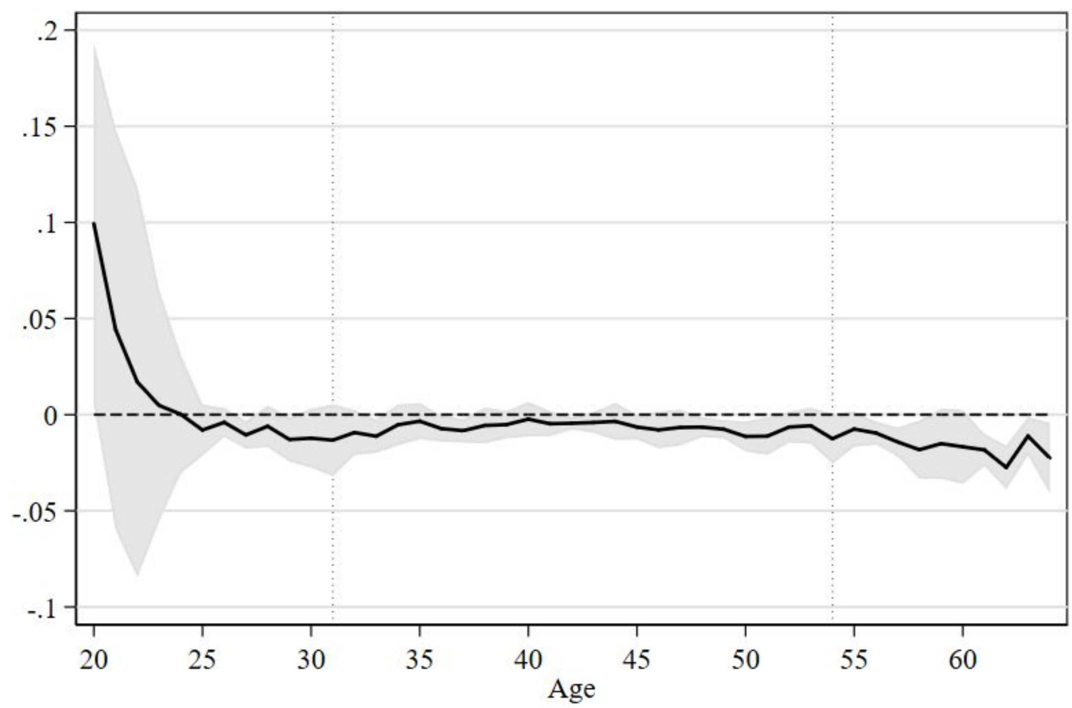
Note: Pension points refer to the statutory points stemming from labor market earnings that determine future pension entitlements. The figures plot the age-specific estimates on SSY in Equation (1). Each estimate is from a separate linear regression of the outcome at a given age on state and birthdate 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. The vertical dashed line marks the age 55, after which the panel is no more balanced in birth cohorts.
Source: VSKT-SUFs 2004-2018; own calculations.

Figure A.6: Effects on alternative labor market outcomes over the life cycle

(a) Age-specific effects on log annual earnings

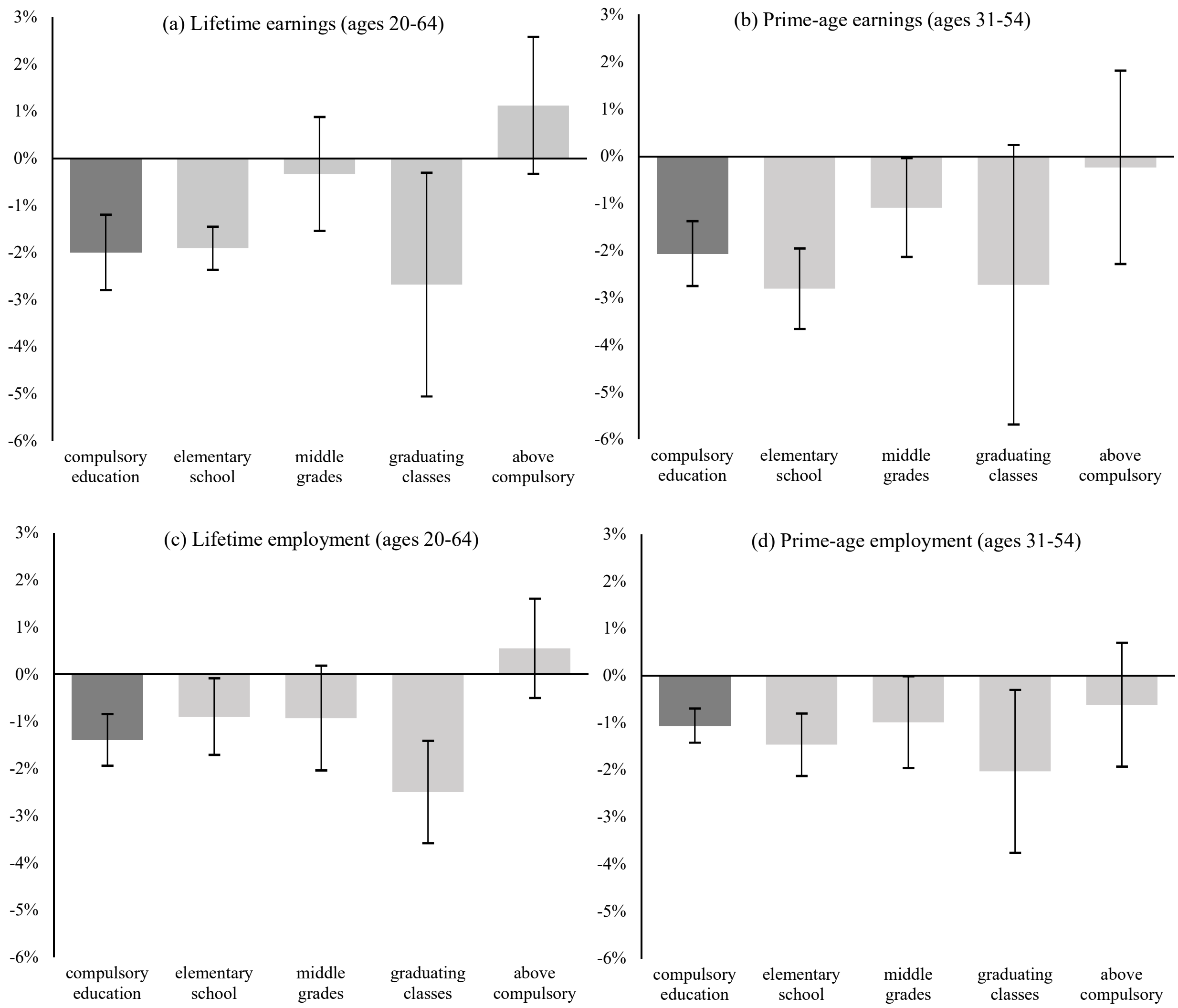


(b) Age-specific effects on employment probability



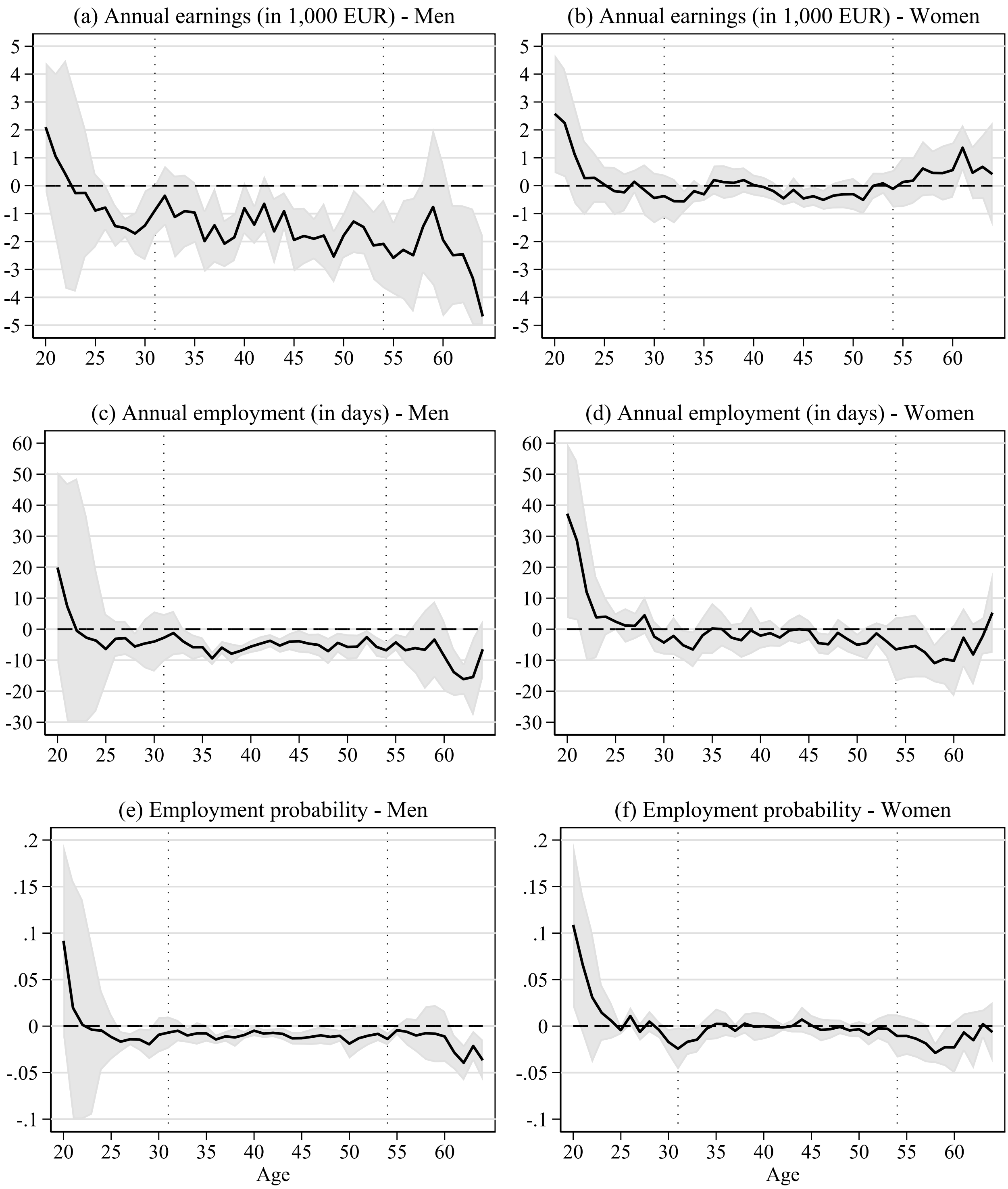
Note: The figure plots the age-specific estimates on SSY in Equation (1). Each estimate is from a separate linear regression of the outcome at a given age on state and birthdate fixed effects, a gender dummy, an indicator for nine years of compulsory schooling, statutory age at school entry, and the size of the enrollment cohort (in months). Shaded areas show 95% confidence intervals based on standard errors clustered at the state level. The vertical dashed lines mark the prime-age range of 31-54, for which, the estimation samples are balanced in birth cohorts (1944-1963). Outside this age range, the panel is not balanced due to the time frame of the data.
Source: SIAB 1975-2017; own calculations.

Figure A.7: Relative effects on depending on the timing of the exposure



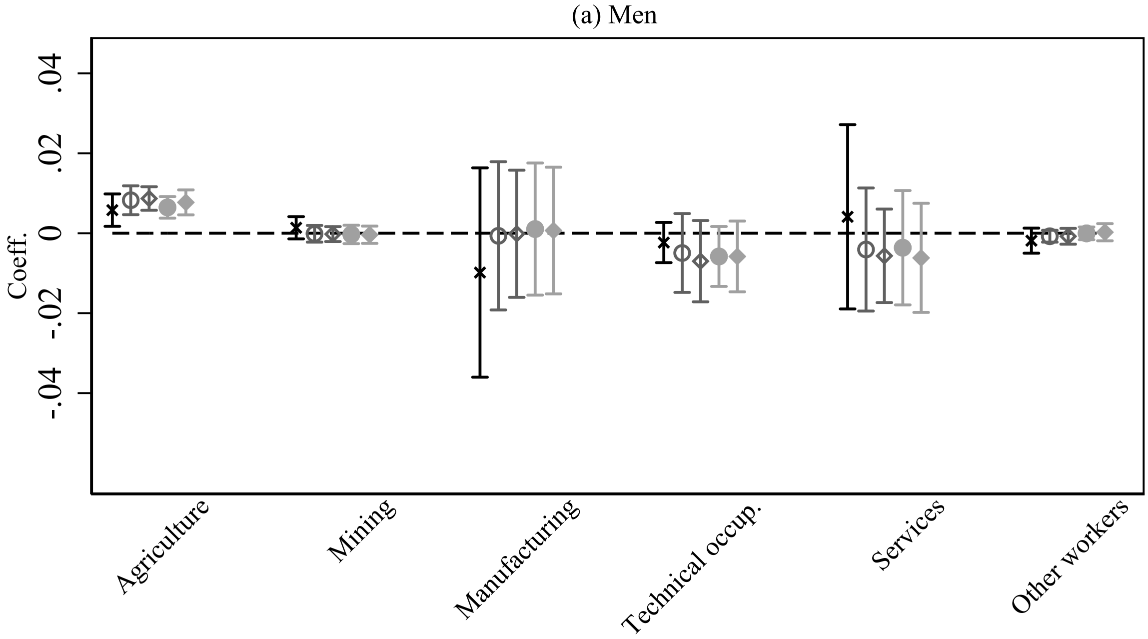
Note: The bars represent the estimated effects of the exposure to the short school years (defined as a binary treatment) relative to the mean of the outcome. The darkest bar is based on a linear regression of Equation (1) where SSY is a dummy variable. The brighter bars are a based on a separate linear regression of Equation (1) where SSY is split into four dummy variables indicating the expected grade attended at the time of the treatment. Elementary school comprises grades $1-4$. Middle grades refer to grades $5-7$ and graduating classes to grades $8-9$. All regressions include state and birthdate 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.
Source: SIAB 1975-2017; own calculations.

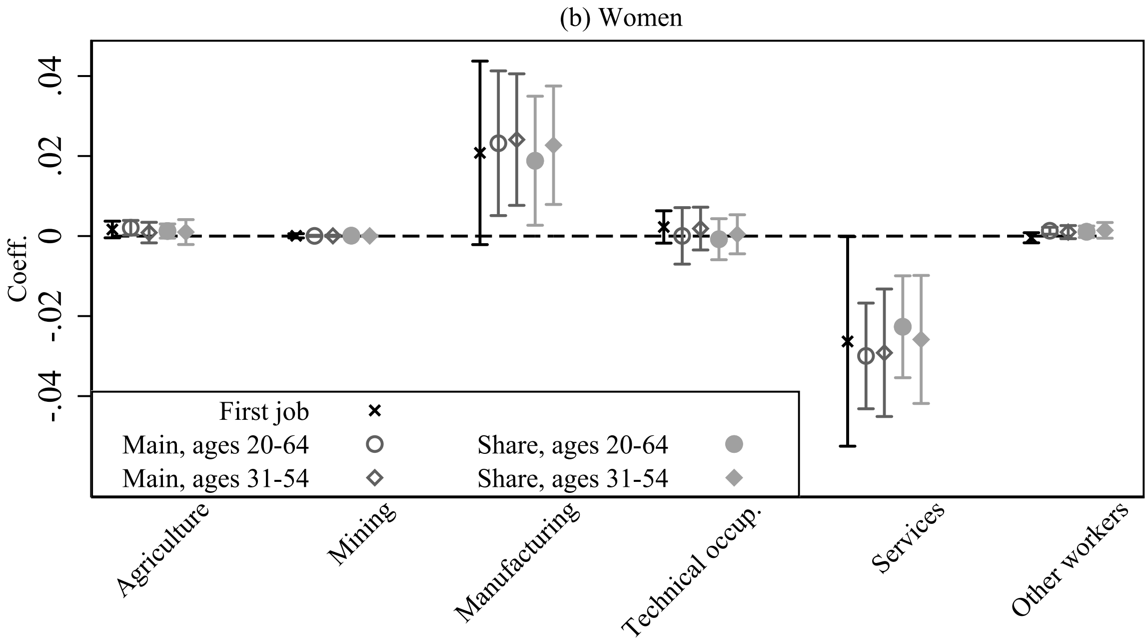
Figure A.8: Life-cycle effects on labor market outcomes by gender



Note: The figure plots the age-specific estimates on SSY in Equation (1) by gender. Each estimate is from a separate linear regression of the outcome at a given age on state and birthdate fixed effects, 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. The vertical dashed lines mark the prime-age range of 31-54, for which, the estimation samples are balanced in birth cohorts (1944-1963). Outside this age range, the panel is not balanced due to the time frame of the data.
Source: SIAB 1975-2017; own calculations.

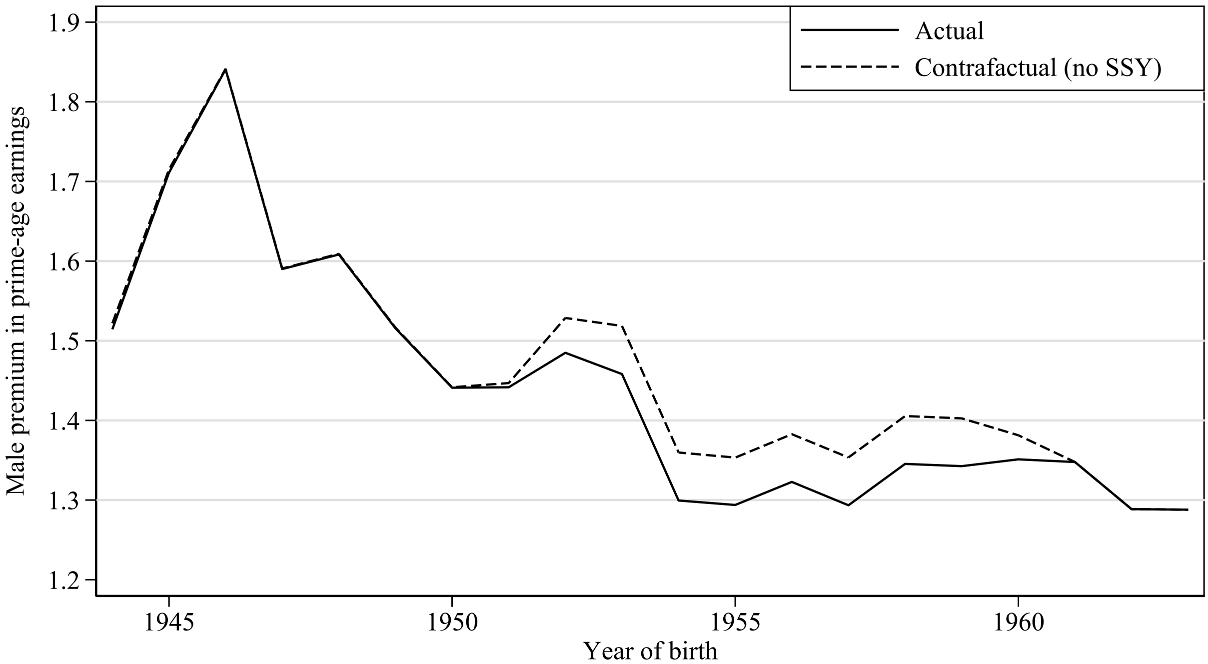
Figure A.9: Gender-specific effects on occupational area





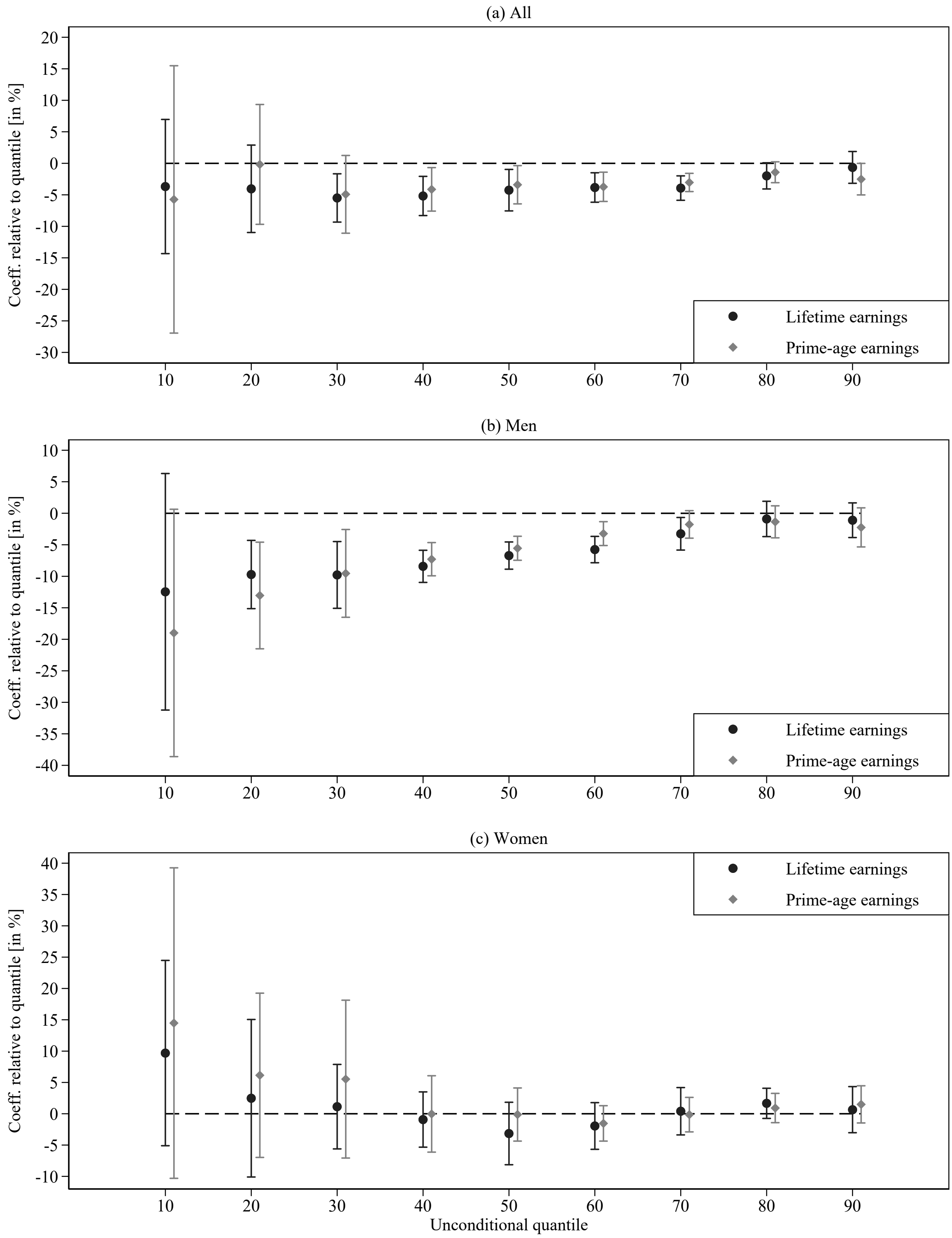
Note: Main occupation is defined as occupational area with the most employment days (in the corresponding age interval). Share corresponds to the ratio of employment days spent in a given occupational area over total employment days (in the corresponding age interval). The figure plots the estimated coefficient on SSY in Equation (1). Each estimate is from a separate linear regression of the respective outcome (dummy or share) on state and birthdate 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.10: Gender gap in prime-age earnings by birth year



Note: The figure plots the male premium in total prime-age (31-54) earnings by birth year. The male premium was calculated as a ratio of the average male earnings within a given birthdate-state cell over the corresponding mean for women minus one. The contrafactual trend was predicted using the regression results in column 3 of Table A.14 in Appendix A under the assumption of no short school years (i.e., $SSY = 0$).
Source: SIAB 1975-2017; own calculations.

Figure A.11: 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 decile. Each estimate is from a separate unconditional quantile regression of the outcome on state and birthdate 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.

Table A.1: Starting 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 A.2: 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)
Annual employment probability (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 postsecondary 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.3: 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: Log (earnings + 1)						
SSY	-0.015	-0.052	-0.056	-0.056	-0.063	-0.055
	(0.028)	(0.028)	(0.028)	(0.028)	(0.025)	(0.016)
Mean dep.	12.448				12.441	
Obs.	278,797				255,298	
Panel D: 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 birthdate 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 suggested by Borusyak et al. (2024).
Source: SIAB 1975-2017; own calculations.

Table A.4: 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 birthdate 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.5: 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 postsecondary 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 birthdate 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 suggested by Borusyak et al. (2024).
Source: SIAB 1975-2017; own calculations.

Table A.6: Comparison with the Micro Census: Effects on educational attainment

	(1) Basic degree	(2) Middle degree	(3) High school	(4) Years of schooling	(5) Univ./ College	(6) Vocational degree	(7) Any post-secondary
Social security records (SIAB)							
SSY	n.a.	n.a.	0.009	n.a.	0.004	-0.014	-0.011
			(0.006)		(0.005)	(0.008)	(0.005)
Mean dep.			0.236		0.150	0.775	0.925
Obs.			271,496		271,496	271,496	271,496
Micro Census - all							
SSY	-0.009	0.003	0.005	0.014	-0.002	-0.020	-0.022
	(0.009)	(0.013)	(0.006)	(0.019)	(0.004)	(0.012)	(0.009)
Mean dep.	0.502	0.244	0.254	10.073	0.177	0.682	0.859
Obs.	370,223	370,223	370,223	370,223	370,223	370,223	370,223
Micro Census after excl. self-employed & public servants							
SSY	-0.001	0.004	0.006	0.021	-0.002	-0.020	-0.021
	(0.010)	(0.012)	(0.004)	(0.019)	(0.003)	(0.012)	(0.010)
Mean dep.	0.559	0.250	0.190	9.806	0.115	0.724	0.839
Obs.	295,173	295,173	295,173	295,173	295,173	295,173	295,173

Note: Each cell is based on a separate linear regression of Equation (1). All regressions include state and birthdate 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. SSY = short school year.
Source: SIAB 1975-2017, German Micro Census 2008, 2012, 2016; own calculations.

Table A.7: 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 birthdate 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 A.8: Effects on socioemotional skills

	Big Five: Openness	Big Five: Conscien-tiousness	Big Five: Extra-version	Big Five: Agree-ableness	Big Five 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		
	Self-esteem	**External LOC**	**Patience**	**Risk aversion**	**Trust**
SSY	-0.032	0.064	0.020	-0.047	-0.018
	(0.074)	(0.065)	(0.085)	(0.031)	(0.113)
Mean age	56.9	53.5	56.3	53.8	52.7
Obs.	5,533	6,631	6,565	9,855	7,984

Note: See Table A.7. LOC=Locus of Control
Source: SOEP 1984-2019 (v36); own calculations.

Table A.9: Gender-specific effects on labor market outcomes: alternative datasets

	(1)	(2)	(3)	(4)
	Micro Census		Pension Insurance records	
	Net income (in EUR)	Employment probability	Pension points (ages 14-64)	Pension points (ages 14-55)
Men	-81.583	-0.021	-1.299	-1.294
	(32.33)	(0.010)	(0.591)	(0.535)
	[-3.3%]	[-3.2%]	[-4.2%]	[-4.4%]
Mean dep.	2501.308	0.664	30.942	29.082
Obs.	172,039	182,913	25,225	25,225
Women	1.700	-0.010	0.357	0.310
	(11.089)	(0.008)	(0.496)	(0.460)
	[0.1%]	[-1.8%]	[2.5%]	[2.3%]
Mean dep.	1179.867	0.554	14.177	13.340
Obs.	179,480	187,310	27,745	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 birthdate 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.10: 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 birthdate 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 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 birthdate 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: Gender-specific effects on cognitive skills and personality traits

	(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: See Table A.7.
Source: SOEP 1984-2019 (v36); own calculations.

Table A.13: Gender-specific effects on skill requirement for a given job

	(1)	(2)	(3)	(4)	(5)	(6)
	Skill level (scale from 1 to 4)			Complex tasks (0/1)		
	first job	highest age 20-64	highest age 31-54	first job	share age 20-64	share age 31-54
Men	-0.028	-0.020	-0.029	-0.011	-0.009	-0.009
	(0.011)	(0.009)	(0.007)	(0.006)	(0.005)	(0.004)
	[-1.3%]	[-0.8%]	[-1.1%]	[-7.3%]	[-3.7%]	[-3.5%]
Mean dep.	2.187	2.661	2.552	0.150	0.251	0.262
Obs.	142,996					
Women	-0.014	0.002	0.015	-0.011	-0.002	0.001
	(0.009)	(0.008)	(0.012)	(0.003)	(0.003)	(0.005)
	[-0.7%]	[0.1%]	[0.7%]	[-11.3%]	[-1.5%]	[1.5%]
Mean dep.	2.047	2.437	2.293	0.097	0.132	0.135
Obs.	135,801					

Note: The dependent variable in columns 5 and 6 corresponds to a ratio of days spent in jobs with complex requirements (level 3 or 4) over total employment days. Each cell is based on a separate linear regression and shows the estimate on SSY in Equation (1). All regressions include state and birthdate 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 A.14: Effects on gender gaps

	(1)	(2)	(3)	(4)
	Male premium in lifetime		Male premium in prime-age	
	earnings (in 1,000 EUR)	employment (in days)	earnings (in 1,000 EUR)	employment (in days)
SSY	-0.132	-0.038	-0.135	-0.043
	(0.035)	(0.015)	(0.050)	(0.014)
	[-10.7%]	[-21.0%]	[-9.5%]	[-18.8%]
Mean dep.	1.237	0.183	1.417	0.227
Obs.	2,396	2,396	2,395	2,395

Note: The data is aggregated to birthdate-state cells. The dependent variable is the male premium in a specific outcome within a given cell. The male premium was calculated as a ratio of the average male outcome over the corresponding mean outcome for women minus one. Each estimate comes from a separate linear regression of Equation (1). All regressions include state and birthdate 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 regressions are reweighed using the number of individuals in each cell. The estimated effect relative to the respective sample mean of the outcome is reported in brackets.
Source: SIAB 1975-2017; own calculations.

Table A.15: Effects on earnings dispersion

	(1)	(2)	(3)	(4)	(5)	(6)
	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 outcomes are standardized. The data is aggregated to birthdate-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 dividing the standard deviation of earnings by the corresponding mean value. Each estimate comes from a separate linear regression of Equation (1). All regressions include state and birthdate 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 Parallel extensions of compulsory education and identifying variation

As mentioned in section 2, several states extended compulsory schooling from eight to nine years during the analysis period (see Figure 1).[1] In Lower Saxony and Bremen the extensions were independent of the short school years in 1966/67. However, North Rhine-Westphalia, Hesse, Rhineland-Palatinate, and Baden-Wuerttemberg used the transition in 1966/67 to introduce the ninth grade. The patterns are not identical because Hesse introduced the ninth grade in the first short school year, while the other three states waited until the second year.

In addition, my main sample also includes some short school years that occurred before 1966/67. Specifically, in Baden-Wuerttemberg, students born before June 1945 experienced a shift in the start of school from fall to spring in 1952. Saarland is a special case because it joined the Federal Republic of Germany in January 1957. Immediately thereafter, the school year was changed to spring and had to be changed back to fall during the transition in 1966/67. Thus, most of the cohorts considered lost some instructional time due to the shorter school year in this state. However, the earlier events in Baden-Wuerttemberg and Saarland apply to a small percentage of my sample, and the results remain unchanged if I exclude them from the analysis (see table 1). Thus, my estimates are mainly identified from the one-time change in 1966/67.

However, the parallel expansions of compulsory schooling in North Rhine-Westphalia, Hesse, Rhineland-Palatinate, and Baden-Wuerttemberg make it challenging to identify the effect of short school years, as the simultaneous compulsory schooling reform likely affected labor market outcomes in the opposite direction. Thus, not accounting for the potentially confounding effects of the simultaneous reform may bias the estimated effect of the short school year toward zero. Therefore, similar to Pischke (2007), in my main specification (see Section 4), I parametrically control for the compulsory schooling reform. Specifically, I include an indicator for individuals exposed to the nine instead of eight years of compulsory schooling.[2]

Figure B.1 illustrates the identifying variation behind the difference-in-differences regressions. For this purpose, I distinguish between three groups of states, depending on their treat-

[1] There are some inconsistencies in the literature about the exact timing of the German extension of compulsory schooling from eight to nine years (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). In addition, I compared and discussed the results of my background research with Josefine Koebe, who simultaneously and independently conducted institutional research focused on this period (Koebe and Marcus, 2022). Therefore, I believe that the information provided here is very accurate.

[2] The estimate on this indicator from my main model specification is 40.012 (with a standard error of 8.806), suggesting a significant return to extended compulsory schooling of 4.5% (relative to the sample mean). Previous estimates of the monetary returns to this reform from survey data are largely imprecise and inconclusive. Pischke and von Wachter (2008) found no statistically significant effect, which was both confirmed (Kamhöfer and Schmitz, 2016) and challenged (Cygan-Rehm, 2022). The most recent study finds a return of about 8% per year of compulsory schooling, which is consistent with the reduced-form effect estimated here in the SIAB data.

ment status: (A) states affected only by shortened school years (Schleswig-Holstein, Bremen, and Saarland)[3], (B) states affected by shorter school years and a parallel increase in compulsory schooling (North Rhine-Westphalia, Hesse, Rhineland-Palatinate, and Baden-Wurttemberg), and (C) the remaining states not affected by short school years. In the last group, Hamburg did not experience any schooling reform during the relevant period while Lower Saxony and Bavaria extended compulsory schooling in two different years.

The subfigures (A) through (C) show trends in average adult earnings by date of birth for the three groups. Because age-specific earnings by birth month and state (group) are typically relatively noisy, the y-axis shows the average earnings at age 45 and two adjacent years (i.e., at age 45 -/+2).[4] For data confidentiality reasons, I aggregate birthdates into quarters for the x-axis. Note that in the regression analysis, I include fixed effects for birthdates at the monthly level, as this is the precise level of identifying variation over time.

The treated group A provides arguably the cleanest before-after comparison to infer the effect of the short school years from pure time series. The vertical shaded areas indicate the range of the first and the last birth cohorts affected by the short school years, respectively. Although this was a one-time policy, there is some variation in the treatment status by birthdate due to state-specific cutoff rules for school enrollment. Given that the three states are relatively small, there means are somewhat noisy. Nevertheless, we observe a slightly declining trend in earnings over time, with a visible discontinuity after the introduction of short school years, suggesting a decline in earnings. I cannot use only these three states to identify the effect of interest because they account for less than 8% of the total sample (see, Appendix A, Table A.2).

For the treated group B, there is also a declining pre-trend and a discontinuity for the first cohorts affected by the short school years. However, the average level of earnings after the introduction of the policy is generally higher than in group A during the same period. This is consistent with a potential positive effect of the parallel increase in compulsory schooling.

That extended compulsory schooling shifts the level of earnings upwards can be illustrated by looking at group C, which was not affected by short school years. However, Lower Saxony and Bavaria introduced the compulsory schooling reform, albeit in different years. The vertical lines show the first birth cohorts with extended compulsory schooling in these two states.

[3] Almost all of the Bremen students in my sample attended school under the nine-year compulsory schooling regime (see Figure 1). Therefore, I assigned Bremen to group A. However, several birth months in 1944 were still bound to the eight-year regime. For this reason, in Figure (A) below, I excluded the first three birth quarters from the estimation of the linear trend. As explained above, in Saarland, students born before 1951 were affected by earlier short school years, which were for other reasons. For illustrative purposes, I coded them here as non-treated, but Figure (A) looks very similar when I exclude Saarland entirely.

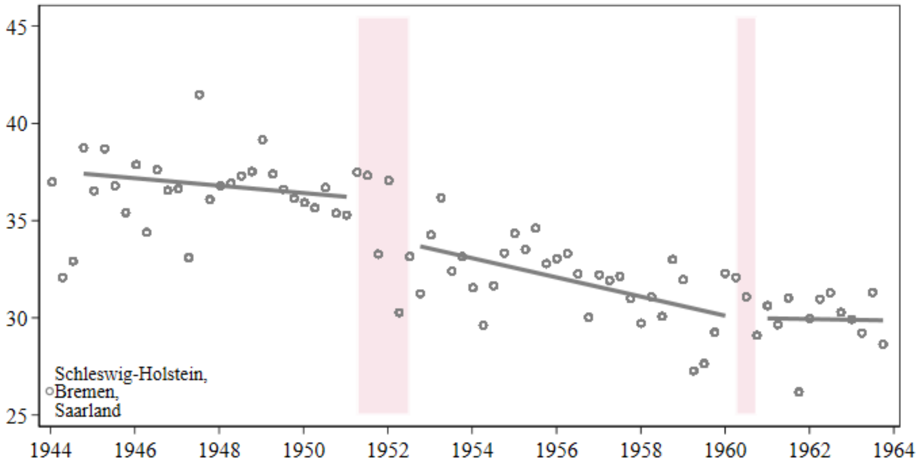
[4] The figures look very similar when earnings are measured exactly at the age of 45 (or 45 -/+1), but the means are noisier so the scale on the x-axis would be wider. The figures are also similar if I measure earnings at earlier or later ages (e.g., 35, 40, or 50). The calculation of average earnings includes zero earnings, but the figures are nearly identical when the no-earners are excluded.

For them, we observe an upward shift in the trend after the extensions. Note that the trends in subfigures (A) to (C) are very similar until mid-1947, which supports the parallel trends assumption. Intuitively, my difference-in-differences regressions use Lower Saxony and Bavaria to offset the opposite effect of the coincidental compulsory schooling extensions for group B. Visually, the two states shift the earnings level in the middle of subfigure B downward.

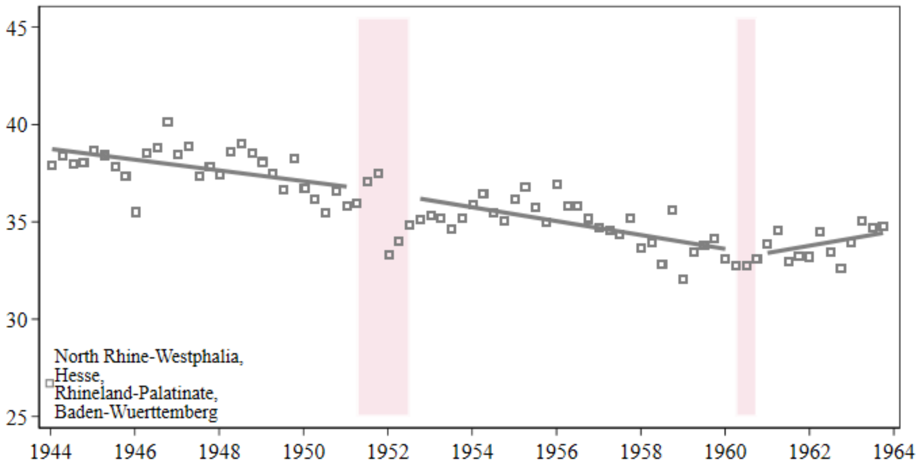
The regression results in Section 5.1 are consistent with an attenuation bias. Specifically, the estimated effect of short school years is negative but small in magnitude and statistically insignificant when no control variable is included (column 1 of Table 1). However, the point estimate increases in magnitude and becomes significant after controlling for compulsory schooling regime (column 2 of Table 1). In Appendix F, I test the robustness of my main results when the compulsory schooling reform is accounted for in a non-parametric way.

Figure B.1: Time trends in adult earnings by birthdate and treatment status

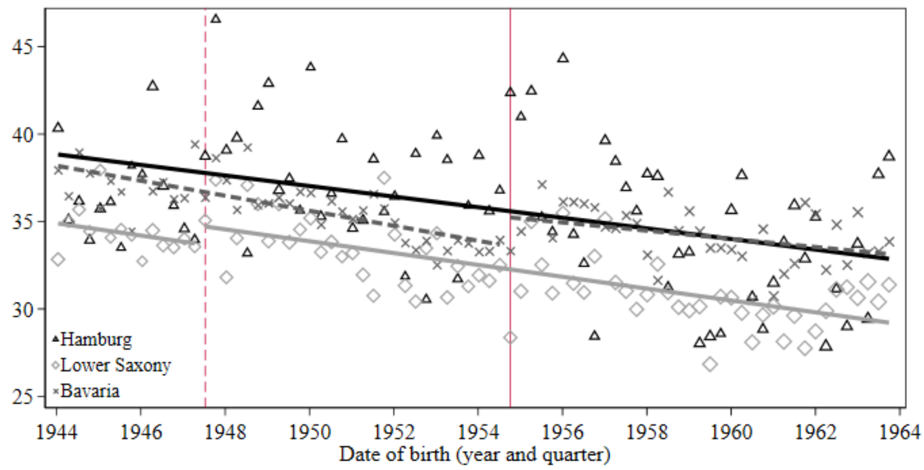
(a) States affected only by short school years



(b) States affected by short school years and parallel increase in compulsory schooling



(c) States not affected by short school years



Note: Each dot shows the average annual earnings (in 1,000 EUR) measured at the age of 45−/+2 years by birthdate (in quarters). The vertical shaded areas in panels (A) and (B) mark the range of the first and the last birth cohorts affected by the short school years, respectively. The ranges are a result of state-specific cutoff rules for school enrollment. The vertical lines in panel (C) show the first birth cohorts with extended compulsory schooling in Lower Saxony and Bavaria, respectively.
Source: SIAB 1975-2017; own calculations.

Appendix C Measurement error from regional mobility

As explained in Section 3.1, 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 introduces a measurement error in the treatment variable.[5] This appendix uses auxiliary survey data to provide evidence on the magnitude of the measurement error and it's potential threat to the internal validity of my main results.

I use here 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. I draw on the Starting Cohort Adults (NEPS-SC6), which includes self-reported information on both the state of schooling and later residence for the relevant birth cohorts. The NEPS-SC6 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 was expanded to include the 1944-1955 birth cohorts. Since then, the survey has been conducted annually. I use the 2007/8-2018/19 surveys.

During the first interview, the NEPS respondents provide retrospective information on their educational careers including the location of each educational institution that they have ever attended. I focus on the state of school enrollment. Each survey also reports the respondent's 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 with information collected in subsequent surveys.[6]

Taken together, the NEPS allows me to study the extent to which an individual's state of schooling matches his/her state of residence or the state of employment later in life. 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). This yields a sample of 6,131 individuals, who were between 43 and 68 years old at the time of the first interview. Table C.1 below provides descriptive statistics.

I find that almost 80% of individuals from the relevant cohorts 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

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

[6]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 measure in the NEPS data.

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 C.2 below investigates this issue using my DiD design. Similarly to the main analysis for labor market outcomes (see Table 1), 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 (columns 5 and 6). The vast majority of the point estimates in Table C.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 on regional mobility. Thus, if anything, my main results from social security records are attenuated due to a measurement error in the treatment variable.[7]

To quantify the attenuation bias, I follow Pischke (2003) who suggests regressing the treatment status assigned using the actual state of schooling on the less accurate treatment status constructed using alternative regional information.[8] Table C.3 shows the results of this analysis for various regional proxies and model specifications. Reassuringly, the estimates are largest in Panel C, where I use the state of the first employment as a proxy for the state of schooling. This is most similar to what I use in social security records for my main analysis. The attenuation factor estimated from my preferred model specification (column 4) is 0.826. This implies that my main estimates from the social security records should be inflated by the factor $1/0.826 = 1.2$. Thus, the attenuation bias seems rather small.

[7] I provide additional evidence for this argument in Appendix F where I alternatively use the last state observed in social security records for a given individual as a proxy for his or her state of schooling (see Table F.3). This approach arguably increases the measurement error, which unsurprisingly attenuates the estimates even more but does not invalidate the paper's main conclusions.

[8] I thank Steve Pischke for drawing my attention to this approach. In Pischke (2003), he used the German General Social Survey (ALLBUS), which allows comparisons between the current state of residence and the state of birth. Unfortunately, ALLBUS does not include state of schooling. Nevertheless, my estimates are very similar.

Table C.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 C.2: The effect of exposure to short school years on interstate immobility

	(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 and birthdate 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 suggested by Borusyak et al. (2024).
Source: NEPS-SC6:11.1.0; own calculations.

Table C.3: Quantifying the attenuation bias

	(1)	(2)	(3)	(4)	(5)	(6)
	Full sample				Restricted sample	
Panel A: Treatment based on the state of residence at the first interview						
SSY	0.763	0.728	0.728	0.728	0.726	0.726
	(0.017)	(0.014)	(0.014)	(0.014)	(0.014)	(0.016
Panel B: Treatment based on the state of residence at the last interview						
SSY	0.764	0.730	0.729	0.729	0.726	0.728
	(0.016)	(0.013)	(0.014)	(0.014)	(0.014)	(0.016)
Panel C: Treatment based on the state of the first employment						
SSY	0.847	0.825	0.825	0.826	0.827	0.832
	(0.030)	(0.033)	(0.032)	(0.031)	(0.030)	(0.015)
Panel D: Treatment based on the state of the last employment						
SSY	0.761	0.728	0.727	0.728	0.725	0.728
	(0.023)	(0.022)	(0.021)	(0.020)	(0.021)	(0.016)
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: The dependent variable is exposure to the short school years based on the actual state of school attendance. Each cell is based on a separate linear regression of Equation (1) where SSY is constructed using a proxy for the state of schooling. All regressions include state and birthdate 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 suggested by Borusyak et al. (2024).
Source: NEPS-SC6:11.1.0; own calculations.

Appendix D Detailed description of auxiliary datasets

Pension Insurance records (VSKT-SUFs 2004-2018)

The Research Data Center of the German Federal Pension Insurance (*Deutsche Rentenversicherung*) administers a 1% random sample of persons aged 30-67 who have ever paid contributions to the statutory Pension Insurance (*Versicherungskontenstichprobe* - VSKT). The initial sample was drawn in 1983, but data have only been available to researchers since 2002. I start with 2004, which is the first wave with state information that I need for treatment assignment. In each subsequent calendar year, the VSKT excludes the oldest birth cohort (turning 68) and adds the youngest one (turning 30) to the sample. The most recent wave is currently 2018 and covers the 1950-1987 cohorts. Each wave provides basic demographic characteristics (e.g., gender, birthdate) and retrospective information on all pension-relevant events (e.g, (un)employment, vocational training, military service, parental leave, invalidity). Data is stored in monthly spells beginning in January of the calendar year in which a given individual turns 14.

I use the Scientific Use Files (SUFs) 2004-2018, each including a 25% subsample of the total VSKT for a given calendar year. The SUFs are newly drawn from the corresponding VSKT every year, which means that a given individual could randomly appear in the SUFs in different years. According to the Research Data Center, of all individuals drawn for the 2018 SUF, 25% were also included in 2018 and 10% in 2016. The Research Data Center does not provide personal identifiers that would allow tracking of individuals across 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 D.1. Specifically, for a given birth cohort, I pool three SUFs using every second wave. Thus, my estimation sample could include a given individual up to three times, although this is very unlikely. Several alternative sampling schemes produce very similar results.

I convert the monthly spells into an annual panel covering the calendar years 1958 to 2018. Thus, for the first (last) birth cohort considered, i.e. 1944 (1963), I obtain a balanced panel with ages ranging from 14 to 64 (55). Following my main sample restrictions, I focus on German citizens from West German states (excluding Berlin). Since the data do not contain information on school attendance, I also exclude individuals with pension entitlements earned in the former East Germany or with entitlements under the law on foreign pensions to exclude potential immigrants.

As the main outcome, I use the total number of pension points earned from employment spells subject to social security,[9] Pension points are a close proxy for earnings, as the average

[9] Self-employment is not subject to compulsory social security contributions. However, some self-employed pay voluntary pension contributions and are therefore included in the data. This is generally rare and possibly highly selective. Thus, I omit pension points from self-employment, but their inclusion leads to very similar results.

earners gain exactly one additional point each year, while lower or higher earnings contribute proportionately fewer or more points to an individual's pension account. The total number of points accumulated until retirement determines the final pension entitlement. I also examine the effects on the number of days worked and an individual's age at labor market entry. The latter is derived from the start date of the first employment or unemployment spell and allows me to test whether the exposure to short school years actually accelerated the labor force entry, which would be an expected "first stage" effect. Table D.2 shows summary statistics for my estimation sample of nearly 53,000 individuals.

Table D.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.

Table D.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. Thus, the data include civil servants and self-employed persons, who are not subject to social security contributions. As a part of the official statistics of the Federal Republic of Germany, participation in the Micro Census is compulsory by law. The data are provided by the Research Data Centers of the Statistical Offices of the Federation and the Federal States. The study is designed as a rotating panel, with a quarter of the sample being replaced each year. The data released to researchers do not contain personal identifiers that would allow tracking individuals over time. To avoid multiple occurrences, I use every fourth survey year starting with the most recent wave, i.e., 2016, 2012, and 2008, resulting in a pooled cross-sectional sample. I cannot include earlier waves 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). I omit 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%). Similar to the SIAB, 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.

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

The Micro Census collects information on the highest educational attainment. First, I construct three mutually exclusive indicators for the basic, middle, and high school degree. Second, I compute a proxy for years of schooling by assigning to each school degree the typical number of years required to obtain it (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 the SIAB, I also consider indicators for having a college degree (incl. universities) and any vocational degree. The final sample consists of about 370,000 individuals. Information on net income is missing for 5.1% of the sample because this question is exempted from the law requiring participation in the survey. However, these non-responses are not significantly correlated with exposure to the short school years, so endogenous sample selection is not a concern.[10] Table D.3 describes my estimation sample.

Table D.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 postsecondary 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.

[10] Using a model specification similar to Equation 1, I regressed a dummy for missing income on exposure to short school years. The estimate on SSY was -0.005 with a standard error of 0.003.

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

The German Socio-Economic Panel (SOEP), conducted annually since 1984, is the longest-running representative longitudinal survey of private households in Germany. The data are 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 stable set of socio-demographic characteristics collected annually, the questionnaire contains additional modules that ask in-depth questions on specific topics. I am particularly interested in cognitive and socio-emotional skills. The SOEP has been collecting information on these domains of human capital since the mid-2000s, which allows me to look at the effects from a long-term perspective (i.e., 40 to 50 years after exposure to short school years).

Specifically, for several survey years between 2006 and 2018, the SOEP provides results from two cognitive assessments for a subsample of about one-third of all respondents (for details, see Anger and Heineck, 2008). [11] First, in a symbol correspondence test (2006, 2012, 2014, 2016, and 2018), respondents 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 (2006, 2012, and 2016), they were asked to name as many different animals as possible in 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 socioemotional skills, I focus on the Big Five Inventory of personality traits, which includes openness to experience, conscientiousness, extroversion, agreeableness, and neuroticism. They were measured in 2005, 2009, 2012, 2013, 2017, and 2019 (for details, see Heineck and Anger, 2010). An extensive literature argues that personality traits and socioemotional skills respond to experiences during childhood and adolescence but remain relatively stable later in life.[12] For completeness, I also consider locus of control, self-esteem, risk aversion, and trust, which are irregularly available in different waves. In general, socioemotional skills are strong predictors of earnings and other adult outcomes.[13]

Similar to my main analysis, I focus on German citizens born between 1944 and 1963. I exclude individuals who lived in East Germany in 1989 because they may have attended school in the former GDR. As in the SIAB, there is no direct information on schooling in the SOEP for the relevant birth cohorts. Therefore, I construct a proxy using the available information on the state of birth (30% of the sample) and the state of residence in childhood (21%). For the

[11] Testing was possible only through computer-assisted personal interviews (CAPI), and the participation rate was about 75%. While selective participation could bias my estimates, in auxiliary regressions, I validated that it was not correlated with exposure to short school years.

[12] See e.g., Almlund et al. (2011); Cobb-Clark and Schurer (2012); Fletcher and Schurer (2017).

[13] See e.g., Bowles et al. (2001); Heineck and Anger (2010); Heckman and Kautz (2012); Cubel et al. (2016).

remainder (49%), I use the first state of residence ever observed for a given individual in the SOEP. My results are robust to alternative approaches to approximating the state of schooling, which is not surprising given the considerable overlap between different regional variables.[14]

Because skills were measured in only selected SOEP waves, so the available sample sizes are relatively small and vary between approximately 1,300 and 8,700 individuals depending on the outcome. I avoid repeated observations per person by using the first value of a given skill ever observed for a given individual in the panel. The results change little when I alternatively use the last observation or pool the data, probably because cognitive abilities and personality traits remain relatively stable late in life. Table D.4 reports descriptive statistics.

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

Table D.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.

Appendix E Reconciliation with the cross-sectional perspective of Pischke (2007)

In Section 5.1, I argue that the opposite direction of large effects early in the career and moderate effects later in life (see Figure 2) may explain why earlier evidence has suggested no adverse labor market effects of short school years (see Pischke (2007) and its replication in Koebe and Marcus (2022)). These studies used pooled cross-sectional survey data collected in several years up to the early 2000s. Thus, their estimates reflect an averaged effect "only" over the first two-thirds of the occupational career, when positive and negative effects potentially cancel each other out when pooled.

Table E.1 illustrates this issue using pooled cross-sectional regressions for annual outcomes.[15] The estimates in the top panel can be interpreted as weighted averages of the age-specific effects in Figure 2, and they depend on the age structure in the data. The estimates imply a significant reduction in the annual sum of earnings and employment days, on average, by 2.3% and 1.1% (relative to the sample mean), respectively. The corresponding reduction in log earnings is by almost 0.02 log points and in the probability of employment by 0.6 percentage points. Previous research has focused on the last two columns. In general, these results largely confirm my main conclusions from a lifetime perspective (see Table 1), although the pooled cross-sectional regressions somewhat underestimate the lifetime effects.[16]

To reconcile these results with previous findings, in Panel A, I adjust the age restrictions to 15-57 to reflect the largest possible age range in the original study by Pischke (2007). As expected, the effects decrease in magnitude as the entire age distribution shifts to earlier career years. The effect on the probability of employment disappears in the last column, but the reduction in earnings of almost 2% is still significant. In Panel B, I further restrict the sample to calendar years 1979-2001, which mirrors the data availability in Pischke's study and substantially lowers the mean/median age in the data. The point estimates become weaker and lose statistical significance. These results are consistent with previous evidence of no effect.

Any remaining differences are potentially due to measurement error issues.[17] For example, administrative data arguably measure the outcomes more accurately than self-reported information from survey data. In addition, the social security records allow for a more thorough assign-

[15] In these regressions, standard errors are clustered at the individual level to account for repeated occurrences of individuals in the panel. However, clustering by state, as in my main analysis, or the Wild cluster bootstrap by state lead to the same conclusions.

[16] A similar pattern is found in Bhuller et al. (2017), who studied the life-cycle effects of extended compulsory schooling in Norway. They attribute the large downward bias in pooled cross-sectional regressions to the potential violation of the stationarity assumption underlying the Mincer earnings regression.

[17] Any differences in the exact outcome measures across data sources seem irrelevant to explaining the differences in results. While Pischke (2007) focuses on log hourly wages, he shows earlier in Pischke (2003) that using monthly earnings yields nearly identical results. There is no information on hours worked in social security records to compute hourly wages, but my main conclusions hold when I use hourly wages or monthly income as alternative outcomes in the Micro Census (see Appendix F, Table F.4).

ment of the exposure to short school years due to the availability of information on month of birth, which is essential to split students born within a given calendar year into school cohorts. In the absence of this information, Pischke (2007) assigned the treatment variable based only on the year of birth. I replicate his approach in Panel C, which further attenuates the point estimates toward zero. For completeness, Panel D reports the results after restricting the data to selected calendar years between 1979 and 2001, as in Pischke's samples from two different data sources (the Qualification and Career Survey and the Micro Census). This does not change the general conclusions.

Overall, this replication exercise strongly supports my argument that it is crucial when we measure the effects (as documented in Figure 2). Thus, my main findings on the negative lifetime consequences of short school years (see Table 1) do not necessarily contradict previous estimates from pooled cross-sectional data, but rather complement them with new insights from a life-cycle perspective. More broadly, my findings underscore the importance of a life-cycle perspective in policy evaluation.

Table E.1: Effects on labor market outcomes from pooled cross-sectional regressions

	Annual earnings (in 1,000 EUR)	Annual employment (in days)	Log annual earnings	Any employment in a given year
Baseline: Age 20-64, years 1975-2017				
	-0.744	-3.446	-0.018	-0.006
	(0.261)	(0.762)	(0.008)	(0.002)
	[-2.3%]	[-1.1%]		
Obs.	7,648,008	7,648,008	7,090,385	7,648,008
Mean/median age	41.6/42	41.6/42	41.6/42	41.6/42
Panel A: Age 15-57, years 1975-2017				
	-0.587	-1.304	-0.016	-0.000
	(0.249)	(0.738)	(0.008)	(0.001)
	[-1.8%]	[-0.4%]		
Obs.	7,253,819	7,253,819	6,625,721	7,253,819
Mean/median age	39.1/40	39.1/40	39.6/40	39.1/40
Panel B: Age 15-57, years 1979-2001				
	-0.265	-1.378	-0.009	-0.001
	(0.236)	(0.819)	(0.007)	(0.002)
	[-0.8%]	[-0.4%]		
Obs.	4,502,353	4,502,353	4,141,751	4,502,353
Mean/median age	35.7/36	35.7/36	36.1/36	35.7/36
Panel C: Age 15-57, years 1979-2001, treatment based on birth year				
	-0.000	-0.450	-0.001	0.001
	(0.237)	(0.819)	(0.007)	(0.002)
	[-0.0%]	[-0.1%]		
Obs.	4,502,353	4,502,353	4,141,751	4,502,353
Mean/median age	35.7/36	35.7/36	36.1/36	35.7/36
Panel D: Age 15-57, selected calendar years between 1979-2001, treatment based on birth year				
à QaC sample in Pischke (2007)	-0.002	0.350	-0.003	0.003
	(0.235)	(0.987)	(0.008)	(0.002)
	[-0.0%]	[0.1%]		
Obs.	763,286	763,286	692,399	763,286
Mean/median age	34.1/34	34.1/34	34.6/35	34.1/34
à Micro Census sample in Pischke (2007)	-0.180	-3.419	-0.009	-0.004
	(0.321)	(0.976)	(0.009)	(0.002)
	[-0.5%]	[-1.1%]		
Obs.	2,027,278	2,027,278	1,900,938	2,027,278
Mean/median age	41.4/41	41.4/41	41.4/41	41.4/41

Note: Each estimate is based on a separate linear regression of Equation (1) using person-year-level data (pooled cross-sections). All regressions include state, cohort, age, and calendar year 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 individual level to account for repeated occurrence of individuals in the panel. The estimated effect relative to the mean of the outcome is reported in brackets. QaC = Qualification and Career survey. In Pischke (2007), the QaC sample comprises survey years 1979, 1985, 1991 and 1998. His Micro Census sample includes years 1989, 1991, 1993, and 1995-2001.
Source: SIAB 1975-2017; own calculations.

Appendix F Validity and robustness tests

Overall, my results suggest that exposure to short school years had negative long-run consequences for earnings and employment. To support this conclusion, this appendix presents the results of various validity checks and robustness tests.

Figure F.1 shows the dynamics of the lifetime effects in event studies. I use the restricted sample as in column 5 of Table 1 to avoid complications in assigning a relative event time to birth cohorts affected by the pre-1966/67 changes in Saarland and Baden-Wuerttemberg (see Figure 1). The treatment is defined as binary. Thus, the graphs show the lifetime effects of exposure to at least one short school year across 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.[18] Thus, the estimates for earlier cohorts ($t \leq 0$) allow for a graphical inspection of the common trend assumption. Regardless of the outcome variable, we observe a slightly increasing pre-trend, but all estimates in the pre-treatment period are statistically insignificant. The estimates on the right-hand side are all negative, but more pronounced and statistically significant only for the first eight event periods ($1 \leq t \leq 8$). After that, the effects seem to disappear, which is consistent with the treatment being switched off for cohorts that started school after the transition. The patterns for the prime-age outcomes in Figure F.2 are very similar, although less precisely estimated. In general, the event studies confirm the negative effects for the exposed cohorts.

Using the imputation estimator of Borusyak et al. (2024), I show in Table 1 that my conventional DiD estimates are not driven by potential bias from treatment heterogeneity. Unlike most alternative estimators recently proposed in the DiD literature,[19] the imputation procedure is not restricted to binary and absorbing treatments. This is an advantage because in my setting, the treatment is multivalued (i.e., exposure to no, one, or two short school years) and non-absorbing (i.e., turns off after the transition period). However, this may pose additional challenges if responses differ across treatment intensities or over time (e.g., Callaway et al., 2024). Thus, Table F.1 shows that my conclusions hold when I assume a binary nature of the treatment (i.e., ignoring exposure to one versus two short school years) and further restrict the sample so that the treatment is an absorbing state (i.e., does not turn off). This applies to both the conventional DiD estimates (columns 1, 2, and 4) and the imputation procedure (columns 3 and 5).

To further test whether my conventional DiD estimates are robust to heterogeneous treatment effects, I apply the diagnostics suggested in de Chaisemartin and D'Haultfoeuille (2020). Table F.2 shows that for all outcomes, the sum of the negative weights attached to the treatment effect (ATT) across all treated states and time periods is small and does not exceed 0.010. The

[18]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 in calendar time (individuals born between January 1946 and December 1963).

[19]For reviews, see e.g., De Chaisemartin and d'Haultfoeuille (2023); Roth et al. (2023).

relative number of state×cohort cells with a negative weight is highest in the simplest specification (column 1) and lowest in the main specification applied to the restricted sample (column 5). In addition, the summary measures ($\underline{\hat{\sigma}}_{fe}$ and $\underline{\underline{\hat{\sigma}}}_{fe}$) are very large, suggesting that heterogeneity in the treatment effect is not a serious concern in my application.

Table F.3 assesses the robustness of my main findings to alternative model specifications and sample restrictions. For comparability, the top panel repeats the baseline results. I begin by providing additional evidence regarding the potential bias from effect heterogeneity across the two different doses of the treatment in my main specification (e.g., Callaway et al., 2024). For this purpose, the regressions in Panel A omit individuals who were exposed to only one short school year. Reassuringly, the estimates change little. The results alleviate concerns that the multivalued nature of the treatment might 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 seasonal 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 geographic regions, as suggested in Stephens and Yang (2014). Specifically, I distinguish between northern Germany (Schleswig-Holstein, Hamburg, Lower Saxony, and Bremen) and southern Germany (the remaining states).[20] In Panel D, I include state-specific student-to-teacher ratios to account for potentially different trends in schooling quality.[21] The extended specifications generally lead to similar conclusions. Thus, my main results are not primarily driven by unobserved state-specific factors or by differences in contemporaneous trends across states.

Next, I assess the robustness of my results to various changes in sample restrictions. In Panel E, I restrict the sample to individuals born in 1947 and later, excluding the first three birth years from my analysis. This sample omits the compulsory school reform in Lower Saxony and Bremen and the earlier short school years in Baden-Wuerttemberg (see Figure 1). In Panel F, I exclude the last three birth cohorts. This specification uses only the pre-treatment cohorts as a control group. The relative effect sizes are consistently comparable to the baseline results. I also exclude individual states from the analysis. Figure F.3 shows that the effects remain relatively stable and statistically significant across the different samples, suggesting no substantial heterogeneity in treatment effects across states.

The main results depend crucially on whether I control for the parallel extension of compulsory schooling in some states (see Table 1 and Appendix B). Thus, the estimate on *SSY*

[20] While the aggregation of the West German states into larger regions may seem arbitrary, this division into northern and southern states corresponds to two (to some extent competing) groups within the Standing Conference of the Ministers of Education during the 1950s and 1960s (see e.g., DER SPIEGEL, 1966).

[21] Student-to-teacher ratios are measured when an individual was in grade 1, 4, and 9 (i.e., at the time of school enrollment, shortly before tracking, and at the end of compulsory schooling).

could be susceptible to any bias in the estimation of compulsory schooling extensions.[22] To eliminate this possibility, in the next two panels I trim the estimation samples so that they do not include any changes in compulsory schooling. Specifically, Panel G excludes all individuals born before July 1952 and those from Bavaria, which was the last state to extend compulsory schooling to nine years (see Figure 1). Alternatively, Panel H includes only individuals born after June 1947 and from the states of Schleswig-Holstein, Hamburg, Lower Saxony, Bremen, and Saarland, which introduced the ninth compulsory year before the study period.

To reduce measurement error in treatment assignment due to limited geographic information, Panel J excludes individuals who entered the social security system after the fall of the Berlin Wall (November 9, 1989). I do this to exclude individuals who potentially attended school in the former GDR and moved to West Germany after 1989. The results are largely unchanged. In Panel K, I use the last (instead of the first) state of residence as a proxy for state of schooling. This increases the measurement error in the 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 are somewhat weaker, suggesting that the measurement error from interstate mobility leads to an attenuation bias in my baseline results.[23]

A remaining issue is that some students who did not experience short school years during compulsory schooling may still have been affected beyond the ninth grade. This applies to students who attended grades beyond nine in the middle track or high school during the transition. Unfortunately, I cannot identify students 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. However, I do observe those who eventually graduated from the highest track, which required up to four years of additional schooling. Thus, in Panel L, I exclude high school graduates[24] and obtain somewhat stronger results.

Table F.4 replicates my main results in the Micro Census. The income measures differ between the datasets, so the magnitudes of the coefficients are not directly comparable, but the effects are very similar when related to the respective sample means. Specifically, column 1 yields a 2.3% decrease in the monthly net income (measured, on average, at age 57). Column 2 shows that the relative effects on labor supply are also comparable across the data sets. Given that the Micro Census additionally includes information on working hours, I can also estimate the effects on hourly wages (column 3). The effect on log wages needs to be interpreted carefully given that it is conditional on employment and I find significant effect on the extensive margin. Indeed, the effect size increases after including non-working individuals in column 4.

[22]This effect is identified within a staggered DiD design and could suffer from a potential bias from treatment effect heterogeneity, which could carry over to the estimate on *SSY*.

[23]This is consistent with no effects of short school years on cross-state mobility (see Appendix C).

[24]This restriction could lead to endogenous sample selection if short school years affected high school graduation rates, which is apparently not the case (see Section 5.2 and Table 3).

The overall picture changes little after excluding self-employed individuals and civil servants, who are not subject to social security contributions. This is not surprising given that in last two columns, I do not find a different sorting into these occupations due to the policy.

Using the detailed information on educational attainment in the Micro Census, I can assign the potential exposure to short school years beyond the ninth grade. Table F.5 reveals that the refined treatment coding generates even larger effects on earnings, which is consistent with the results in Panel L of Table F.3. Unlike the social security records, the Micro Census also includes some demographic information on the respondents such as marital status, number of children (for women only), and disability. Table F.6 shows that including these variables as additional controls leaves the estimated effect of short school years on income largely unaffected. This is reassuring because, compared to birthdate fixed effects, these variables more flexibly capture potential differences in demographic and health trends across the states.[25] Overall, the alternative results support my main conclusions.

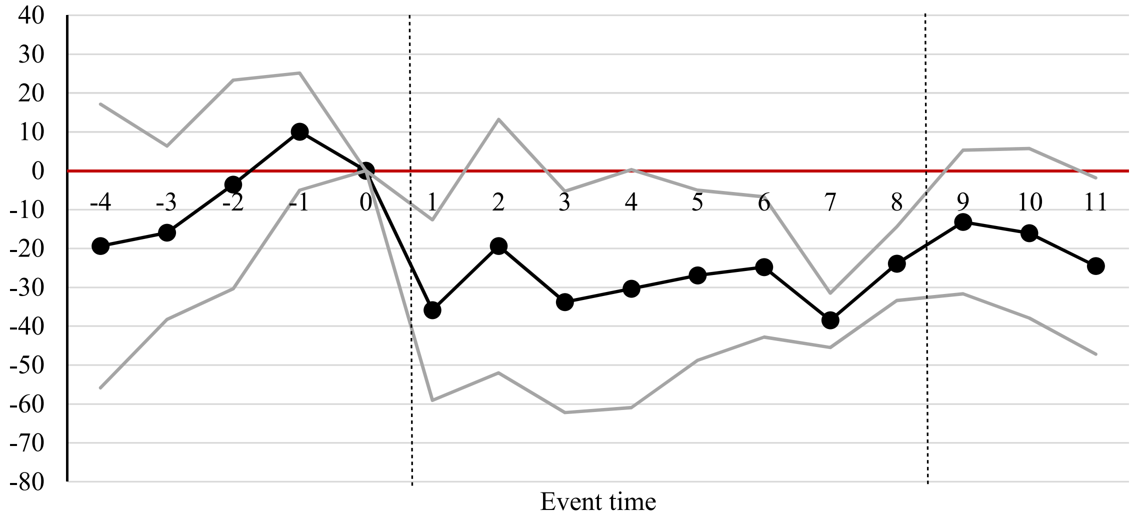
Finally, F.7 reports alternative inference results. In my main analysis, the standard errors are clustered at the state level, which has become common practice in DiD research since Bertrand et al. (2004). While this seems conservative, it is also subject to finite sample problems when the number of states is small (e.g., Cameron and Miller, 2015). Therefore, I alternatively use a conservative version of the Wild cluster bootstrap as recommended in Cameron et al. (2008) and Roodman et al. (2019). However, the appropriate level of clustering in DiD designs that rely on variation across regions and over time is not straightforward. A common alternative has been to assume that errors are correlated across individuals from the same state and cohort, implying clustering at the level of the identifying variation. This has been used in previous research on short school years (Pischke, 2007; Hampf, 2019; Koebe and Marcus, 2022). One could also argue that clustering should be both at the regional and time levels, leading to two-way clustering (Cameron et al., 2011). Recently, Abadie et al. (2023) show that clustering at the state level may be unnecessarily conservative, possibly by a large margin. They argue for a design-based approach to inference, i.e., clustering so that individuals in the same cluster have the same treatment assignment. This implies clustering at the state$\times$*SSY* level in my setting. Given the ongoing debate, the appropriate inference approach is an open question (De Chaisemartin and d'Haultfoeuille, 2023).[26] Reassuringly, the alternative results lead to similar conclusions.

[25] On the other hand, these factors could also represent outcomes that were endogenously affected by the policy itself (bad controls). That the estimated income effect remains nearly identical suggests that the policy had no substantial effect on marriage, fertility, and serious health problems. This is consistent with Koebe and Marcus (2022), who found some short-run effects on the timing of family formation but no long-run effects on the probability of ever marrying or becoming a parent.

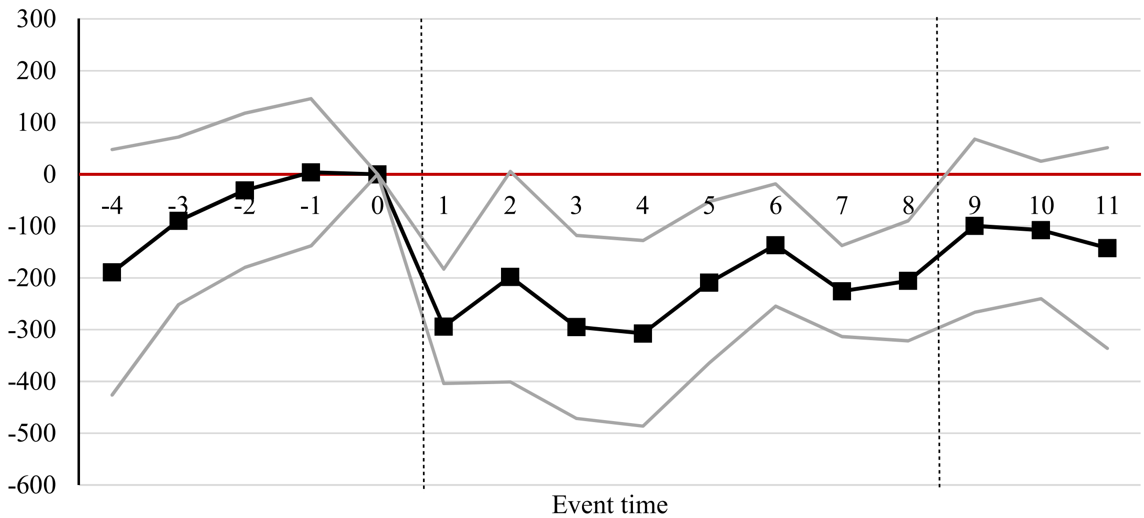
[26] For recent reviews, see e.g., Cameron and Miller (2022); Roth et al. (2023); MacKinnon et al. (2023).

Figure F.1: Event time studies for the effect on lifetime outcomes (ages 20-64)

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



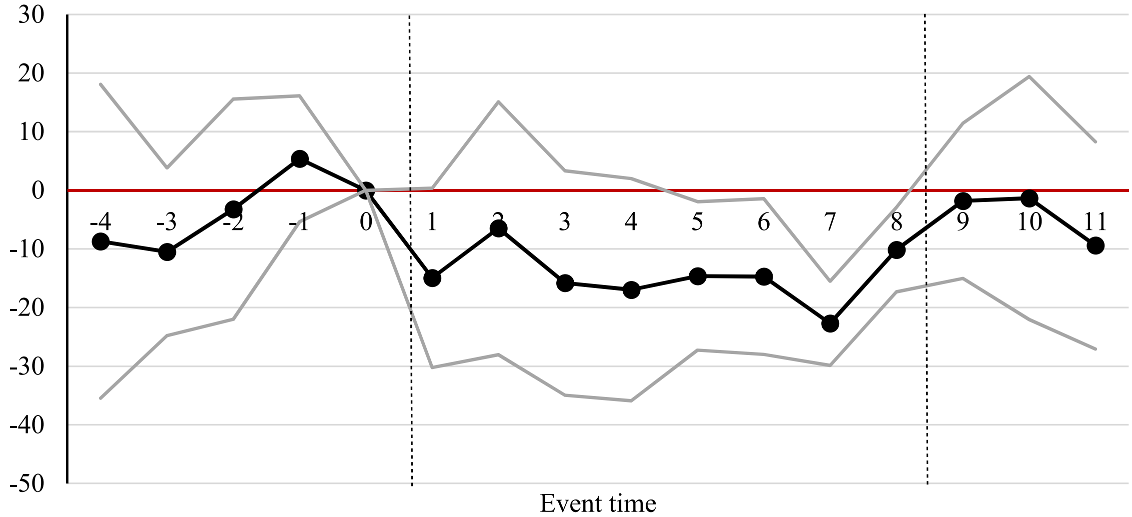
(B) Effects on the number of days in employment



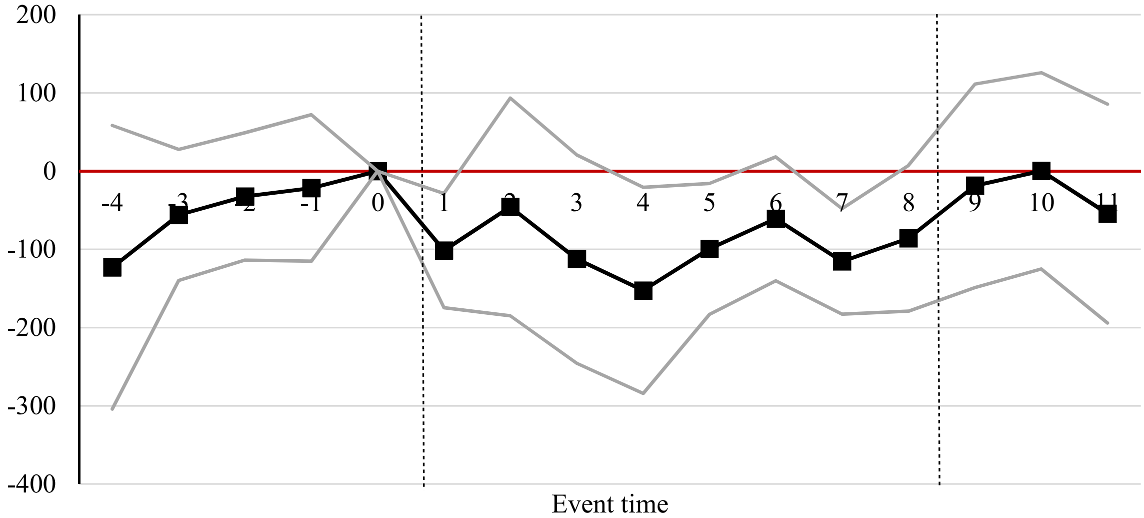
Note: The figures show the results from event time studies in which the event time (t) is measured in 12-month increments. The first 12 treated birth months in each affected state are assigned $t = 1$. The vertical dashed lines mark the range of birth cohorts affected by the policy. Each figure plots the event time estimates from a separate linear regression of the outcome on event time dummies, state and birthdate 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 F.2: Event time studies for the effect on prime-age outcomes (ages 31-54)

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

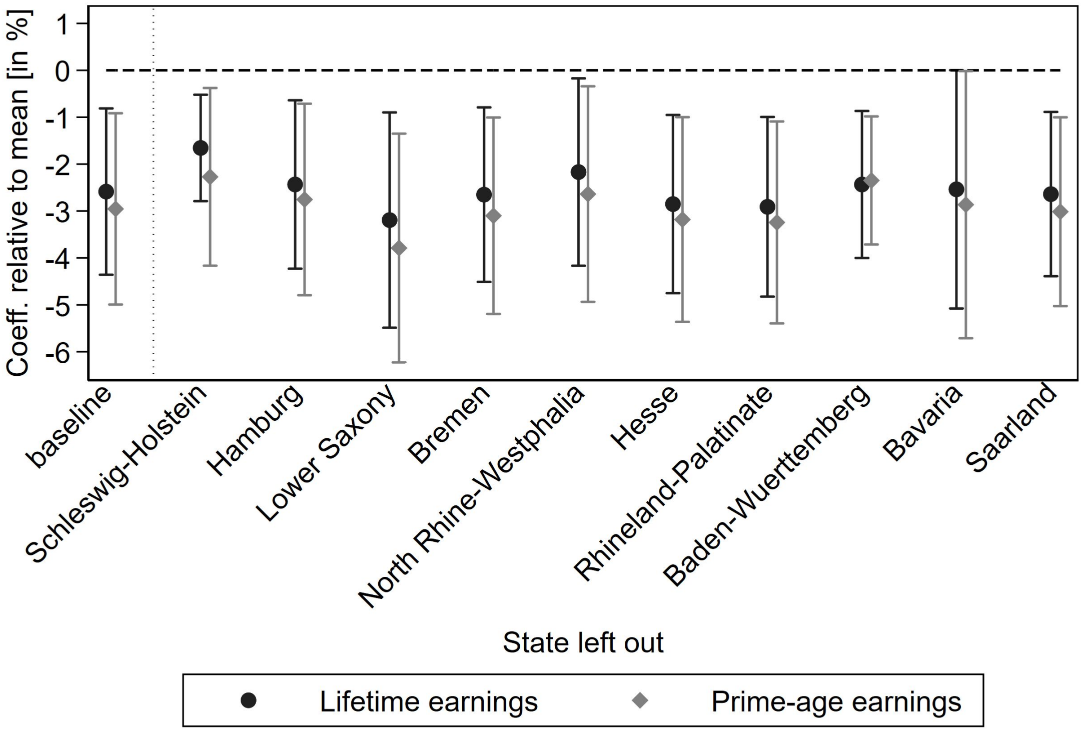


(B) Effects on the number of days in employment



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 $t = 1$. The vertical dashed lines mark the range of birth cohorts affected by the policy. Each figure plots the event time estimates from a linear regression of the outcome on event time dummies, state and birthdate 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 F.3: 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 birthdate 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.

Table F.1: 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: Log (earnings + 1)					
$SSY(0/1)$	-0.039	-0.043	-0.040	-0.054	-0.046
	(0.011)	(0.013)	(0.005)	(0.018)	(0.009)
Mean dep.	13.051	13.066		13.061	
Obs.	278,797	255,298		200,210	
Panel D: 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 birthdate 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 suggested by Borusyak et al. (2024). Last column estimated using the *did_imputation* Stata command.
Source: SIAB 1975-2017; own calculations.

Table F.2: 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: Log (earnings + 1)					
SSY	-0.024	-0.062	-0.063	-0.063	-0.068
$\underline{\hat{\sigma}}_{fe}$	0.061	0.143	0.144	0.146	0.155
$\underline{\underline{\hat{\sigma}}}_{fe}$	0.264	0.615	0.295	0.693	0.681
Panel D: 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 birthdate fixed effects and a gender dummy. Because the gender dummy varies within the state$\times$cohort cells, the command uses its average value at the state$\times$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 F.3: Sensitivity analysis

	Lifetime (ages 20-64) earnings	Lifetime (ages 20-64) employment	Prime-age (ages 31-54) earnings	Prime-age (ages 31-54) 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 birthdate 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 F.4: Comparison with the Micro Census: Effects on labor market outcomes

	(1) Income measure	(2) Employ-ment	(3) Log wage	(4) Log (wage+1)	(5) Self-employed	(6) Public servant
Social security records (SIAB)						
SSY	-24.304	-175.856	n.a.	n.a.	excl.	excl.
	(8.734)	(57.827)				
	[-2.7%]	[-2.1%]				
Mean dep.	888.496	8560.277				
Obs.	278,797	278,797				
Micro Census - all						
SSY	-41.242	-0.015	-0.012	-0.038	0.003	0.004
	(15.556)	(0.008)	(0.005)	(0.021)	(0.004)	(0.003)
	[-2.3%]	[-2.5%]				
Mean dep.	1826.601	0.609	2.562	1.613	0.125	0.078
Obs.	351,519	370,223	226,266	370,223	370,223	370,223
Micro Census after excl. self-employed & public servants						
SSY	-45.231	-0.023	-0.012	-0.061	excl.	excl.
	(22.939)	(0.012)	(0.005)	(0.033)		
	[-2.8%]	[-3.9%]				
Mean dep.	1598.293	0.596	2.522	1.556		
Obs.	283,690	295173	175990	295173		

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. The employment measure is the lifetime employment (in days) in the SIAB data and employment probability in the Micro Census. Hourly wages are computed using the information on monthly income and the usual number of working hours per week. 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 birthdate 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 F.5: 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 birthdate 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 F.6: Income effects after adding controls for demographic characteristics

	(1) Baseline	(2) Incl. marital status	(3) Incl. number of births	(4) Incl. disability status
Panel A: Full sample				
All	-41.242	-40.395		-41.982
	(15.556)	(15.334)		(15.678)
	[-2.3%]	[-2.2%]		[-2.3%]
Mean dep.	1826.601	1826.601		1826.601
Obs.	351,519	351,519		351,519
Men	-81.583	-77.772		-82.771
	(32.330)	(28.015)		(32.859)
	[-3.3%]	[-3.1%]		[-3.3%]
Mean dep.	2501.308	2501.308		2501.308
Obs.	172,039	172,039		172,039
Women	1.700	-6.210	-6.254	1.693
	(11.089)	(12.080)	(12.189)	(11.108)
	[0.1%]	[-0.5%]	[-0.5%]	[0.1%]
Mean dep.	1179.867	1179.867	1173.464	1179.867
Obs.	179,480	179,480	166,948	179,480
Panel B: After excl. self-employed & public servants				
All	-45.231	-44.960		-45.718
	(22.939)	(22.950)		(22.958)
	[-2.8%]	[-2.8%]		[-2.9%]
Mean dep.	1598.293	1598.293		1598.293
Obs.	283,690	283,690		283,690
Men	-93.100	-85.948		-94.483
	(49.808)	(45.955)		(49.743)
	[-4.1%]	[-3.8%]		[-4.2%]
Mean dep.	2248.862	2248.862		2248.862
Obs.	129,410	129,410		129,410
Women	-0.193	-10.070	-8.301	0.041
	(14.971)	(16.131)	(18.516)	(15.033)
	[0.0%]	[-1.0%]	[-0.8%]	[0.0%]
Mean dep.	1052.597	1052.597	1047.432	1052.597
Obs.	154,280	154,280	143,619	154,280

Note: The dependent variable is the current monthly net income (in EUR). Each cell is based on a separate linear regression and shows the estimate on SSY in Equation (1). All regressions include state and birthdate 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. Marital status is an indicator of being married at the time of the interview. Total number of children ever born is asked only for women. Disability status is an indicator for having an officially approved level of care (*Pflegestufe*). Standard errors in parentheses are clustered at the state level. German Micro Census 2008, 2012, 2016; own calculations.

Table F.7: Alternative inference results

	(1) Earnings (in 1,000 EUR)	(2) Log earnings	(3) Log (earnings+1)	(4) Employment (in days)
Point estimate on *SSY*	-24.304	-0.030	-0.063	-175.856
Panel A: *p*-values for the null hypothesis				
Baseline: Clustering by state (g=10)	0.005	0.041	0.004	0.002
Clustering by state×SSY cells (g=*24*)	0.001	0.012	0.017	0.000
Clustering by state×birth year cells (g=200)	0.006	0.047	0.001	0.001
Two-way clustering by state & birth year (g_1=10, g_2=20)	0.005	0.008	0.002	0.000
Wild cluster bootstrap (WCB) by state (g=*10*)	0.063	0.078	0.038	0.070
WCB by state×SSY cells (g=24)	0.035	0.088	0.075	0.079
WCB by state & birth year (g_1=10, g_2=20)	0.049	0.041	0.015	0.010
Panel B: 95% confidence intervals (CI)				
Baseline: Clustering by state (g=10)	[-41.4;-7.2]	[-0.060;-0.001]	[-0.105;-0.021]	[-289.2;-62.5]
Clustering by state×SSY cells (g=*24*)	[-38.8;-9.8]	[-0.054;-0.007]	[-0.096;-0.029]	[-267.4;-84.3]
Clustering by state×birth year cells (g=200)	[-41.6;-7.0]	[-0.060;-0.001]	[-0.101;-0.025]	[-275.7;-76.0]
Two-way clustering by state & birth year (g_1=10, g_2=20)	[-41.3;-7.3]	[-0.053;-0.008]	[-0.103;-0.023]	[-266.1;-85.6]
Wild cluster bootstrap (WCB) by state (g=*10*)*	[-73.2;1.6]	[-0.119;0.010]	[-0.169;-0.011]	[-402.3;45.6]
WCB by state×SSY cells (g=24)*	[-84.3;-1.9]	[-0.154;0.024]	[-0.202;0.028]	[-469.5;56.5]
WCB by state & birth year (g_1=10, g_2=20)*	[-54.7;-0.2]	[-0.073;-0.002]	[-0.121;-0.020]	[-320.9;-73.8]
Panel C: standard errors (SE)				
Baseline: Clustering by state (g=10)	(8.734)	(0.015)	(0.022)	(57.827)
Clustering by state×SSY cells (g=*24*)	(7.387)	(0.012)	(0.017)	(46.688)
Clustering by state×birth year cells (g=200)	(8.847)	(0.015)	(0.019)	(50.934)
Two-way clustering by state & birth year (g_1=10, g_2=20)	(8.691)	(0.012)	(0.020)	(46.039)
Wild cluster bootstrap (WCB) by state (g=*10*)**	(19.093)	(0.033)	(0.040)	(114.241)
WCB by state×SSY cells (g=24)**	(21.010)	(0.045)	(0.059)	(134.171)
WCB by state & birth year (g_1=10, g_2=20)**	(13.895)	(0.018)	(0.026)	(63.030)

Notes: g refers to the number of clusters. The Wild cluster bootstrap (WCB) uses the recommended procedure with 999 replications, imposing the null hypothesis, and using the Rademacher weights (Cameron et al., 2008; Roodman et al., 2019). Its immediate result is the p-value for the null hypothesis in Panel A.

* The WCB confidence intervals in Panel B are not symmetric.

** WCB does not produce a standard error (SE). A conservative estimate of the SE can be calculated as the width of the 95% confidence interval divided by 2×1.96 (Cameron and Miller, 2015), which assumes asymptotic normality of the bootstrap distribution of the point estimate. The WCB SE in Panel C should be treated with caution: they are not directly comparable to those from conventional inference methods and cannot be used to calculate symmetric confidence intervals.

Source: SIAB 1975-2017; own calculations.