Does Schooling Affect Socioeconomic Inequalities in Educational Attainment?
Evidence from a Natural Experiment in Germany

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Abstract

Critical theories of education and the dynamics of skill formation model predict that the education system reproduces socioeconomic inequalities in educational attainment. Previous empirical studies comparing changes in socioeconomic inequalities in academic performance over the summer to changes in these inequalities during the school year argue, however, that schooling reduces inequalities in educational performance. The present study sheds light on the question of whether schooling affects socioeconomic inequalities in educational attainment by analyzing a natural experiment that induces exogenous variation in the length of schooling and allows me to investigate the causal, long-term effects of the length of schooling on inequalities in educational attainment. Some German states moved the school start from spring to summer in 1966/ 1967 and introduced two short school years, each of which was three months shorter than a regular school year. I use variation in the short school years across cohorts and states to estimate the causal effects of the length of schooling on socioeconomic inequalities in educational attainment based on two German panel surveys. Less schooling due to the short school years did not affect inequalities in educational attainment. This finding runs counter to the results from the summer learning literature and to the predictions of the dynamics of skill formation model and critical theories of education. I conclude by discussing the implications of this finding for our understanding of socioeconomic inequalities in educational attainment.

Keywords

education, inequality in educational attainment, natural experiment, schooling

Introduction

In modern societies, education in formal institutions has become a fundamental part of the human life course. Liberal thinkers such as John Dewey (1966) and Ralf Dahrendorf (1965) emphasized the equalizing role of the education system, arguing that equality of educational opportunity can be achieved only through schooling. The role of schooling has, however, not been uncontroversial. Several sociological theories argue that the education system reproduces educational inequalities. For example, cultural capital theory is developed around the claim that the education system rewards children for their cultural capital, which is transmitted to them from their parents and does not increase their educational performance (Bourdieu 1979; Bourdieu and Passeron 1966, 1970). Further support for this perspective comes from Willis' (1978) ethnographic study of English working-class children who develop a "counterschool culture" that does not allow them to profit from schooling. In the United States, Bowles and Gintis (1976, 2002) argued that the American school system reproduces social hierarchies because it prepares working-class children for working-class jobs by forcing them to accept the hierarchy in the education system, which resembles the hierarchy in the capitalist economy.

Following these theoretical and, on an empirical level, descriptive investigations from the 1970s, a number of more recent sociological studies employed natural experiments to identify the causal effects of schooling on the reproduction of educational inequalities. The evidence of these studies, as reviewed by Raudenbush and Eschmann (2015), provides no support for the theories developed by Bourdieu, Passeron, Willis, Bowles, and Gintis. On the contrary, some of these natural experiments have suggested that schooling reduces socioeconomic inequalities in educational performance. In particular, a number of empirical studies have shown that socioeconomic differences in skills grow more during the summer, when no schooling takes place, than during the school year (Alexander, Entwisle, and Olson 2001, 2007; Burkam et al.

2004; Downey, von Hippel, and Broh 2004; Entwisle and Alexander 1992; Heyns 1978, 1987; Holtmann and Bernardi 2019; Verachtert et al. 2009).

More recently, however, methodological critiques of these studies have emerged, arguing that after measurement error is taken into account, the increases in socioeconomic inequalities in skills during the summer are smaller than was previously assumed and of little substantive importance (von Hippel and Hamrock 2019; von Hippel, Workman, and Downey 2018). In addition, evidence from other studies does not find that the benefits of more schooling for skill development vary by family socioeconomic background (Carlsson et al. 2015; Passaretta and Skopek 2021). In economics, the dynamics of skill formation model also predicts that “skills beget skills and abilities beget abilities” (Cunha and Heckman 2007:10). Therefore, children from socioeconomically advantaged families should profit more from schooling than children from socioeconomically disadvantaged families (Raudenbush and Eschmann 2015). Finally, recent ethnographic evidence suggests that, in line with Willis’ (1978) study of English schoolchildren, for instance, universities can increase the intergenerational transmission of educational advantage (Armstrong and Hamilton 2015). Thus, evidence on the effects of schooling on the intergenerational transmission of education is mixed, and new studies are needed. These studies should employ research designs that allow researchers to estimate causal effects of the length of schooling. Natural experiments provide exogenous variation in the length of schooling and can be employed for such a purpose.

Previous empirical studies have estimated the short-term effects of schooling on socioeconomic differences in educational performance measured through cognitive skills and test scores. The present study investigates the long-term effects of the length of schooling on the intergenerational transmission of educational attainment. Such an investigation is important, as short-term effects may not necessarily translate into long-term effects.

In the present study, I estimate the effects of the length of schooling on the intergenerational transmission of educational attainment by exploiting the natural experiment of the short school years that occurred in Germany in 1966 and 1967. Several German states decided in 1964 to move the start of the school year from after Easter (April) to late summer (August/ September). To achieve this change, these states introduced two short school years in 1966/ 1967 (Helbig and Nikolai 2015). These short school years ran for 9 months instead of 12. For this reason, students in school during both short school years were exposed to up to six months less schooling than their counterparts who were in school after or before the short school years took place. The identification of the effect of the short school years on the intergenerational transmission of educational attainment is facilitated by the fact that some German states did not implement the short school years and can therefore be used as a control group. The identification strategy therefore closely resembles a difference-in-differences (DiD) design with the additional advantage that there are before-treatment, treatment, *and* after-treatment groups.

Meister (1972) and Thiel (1973) estimated the short-term effects of the short school years on pupils' test scores while they were still in school.¹ Pischke (2007) investigated the effects of the short school years on employment and earnings. These studies found no effect of the short school years on educational performance, educational attainment, earnings, and employment (Meister 1972; Pischke 2007; Thiel 1973). With respect to educational attainment, such a finding is not surprising, as teachers may have had certain quotas of how many students they sent to the upper track in secondary school independent of the length of the school year. The present study, however, investigates the long-term consequences of the short school years in 1966/ 1967 in Germany for socioeconomic differences in educational attainment. Even if the number of students promoted to the upper track in secondary school did not change due to the

¹ These two studies did, however, report only a before-after comparison, did not employ a control group, and used each only data on one of the eight treatment states in West Germany.

short school years (as demonstrated by previous research), the composition of those who were promoted by social origin could have changed due to the length of schooling. Such a change is precisely what the theories motivating this study predict. These theories are discussed in detail in the next section.

Does Schooling Increase or Decrease Socioeconomic Inequalities in Educational Attainment?

The function of schooling in a society is a debated issue. Economists often see the primary purpose of the education system as improving children's skill development (Bradbury et al. 2015; Cunha and Heckman 2007; Hanushek 1979; Heckman 2000). Functional theories developed in sociology attribute to education the main function of matching students to labor market positions (Davis and Moore 1945; Parsons 1959; Sorokin 1959).

While the role of schooling is perceived as positive in functional theories, it is criticized from a Marxist perspective. Arguing that the school system prepares children to accept the hierarchies in the capitalist economy, Bowles and Gintis (1976:11) wrote that schools "create and reinforce patterns of social class, racial and sexual identification among students which allow them to relate 'properly' to their eventual standing in the hierarchy of authority and status in the production process". If children spend more time in school, there is more time for this process to play out. Therefore, Bowles and Gintis' (1976) theory expects more schooling to increase the intergenerational transmission of educational attainment.

Cultural capital theory, as developed by Bourdieu and Passeron (1966, 1970) and Bourdieu (1979), also argues that the school system reproduces inequalities in educational attainment. This theory is based on the claim that the education system values cultural capital, which is transmitted from parents to their offspring. Importantly, this is due not to cultural capital increasing children's educational performance but to teachers' positive view of the

cultural capital of the socioeconomically advantaged social classes.² If children spend more time in school, those with higher levels of cultural capital have more time to make their teachers aware of their cultural capital. Cultural capital theory therefore also predicts that more schooling results in a higher transmission of educational attainment across generations.

Further empirical support for the notion that schooling increases inequality in educational attainment comes from an ethnography of pupils from working-class families in the United Kingdom (Willis 1978). The working-class pupils observed in this ethnography developed a counterschool culture. More schooling did not change the counterschool culture and therefore did not benefit the offspring of working-class families.³ Therefore, only children from socioeconomically advantaged families will profit from more schooling. This perspective leads us to expect an increase in the intergenerational transmission of educational attainment as a consequence of an increase in the length of schooling.

A further reason why schooling may reproduce socioeconomic inequalities in educational attainment is ability grouping and tracking within schools (Oakes 2005). In addition, the quality of schools that children from socioeconomically advantaged families attend may be better than the quality of schools that children from socioeconomically disadvantaged families attend. Within the same schools, children from socioeconomically advantaged families may ask for and receive more support from teachers than children from socioeconomically disadvantaged families (Calarco 2018).

Finally, the dynamics of skill formation model in economics predicts that students from socioeconomically advantaged families will profit more from schooling than children from socioeconomically disadvantaged families (Cunha and Heckman 2007; Heckman 2000;

² For a formal model of cultural capital theory, see Jæger and Breen (2016).

³ The cohort studied by Willis (1978) was affected by the raise in the school leaving age (RSLA) in England, a 1972 educational reform that increased the minimum school leaving age from 15 to 16. Sturgis and Buscha (2015) showed that this reform had largely no effects on the intergenerational transmission of advantage, confirming Willis' (1978) ethnographic observations.

Raudenbush and Eschmann 2015). The model is motivated by the central idea that “skills beget skills and abilities beget abilities” (Cunha and Heckman 2007:10). Children from socioeconomically advantaged families have more skills at the start of school. According to the dynamics of skill formation model, these children therefore profit more from schooling than children from socioeconomically disadvantaged families. Therefore, the dynamics of skill formation model makes the same prediction as critical theories of education: More schooling will increase socioeconomic inequalities in educational attainment.

Contrary to these sociological and economic theories, liberal philosophers have argued that schooling has an equalizing function (Dahrendorf 1965; Dewey 1966). Differences in the learning environment at home between socioeconomic groups may be larger than differences in the school environment (Raudenbush and Eschmann 2015). If that is the case, schooling does reduce socioeconomic inequalities in educational attainment. Lareau’s (2011) ethnographic work found that children from socioeconomically advantaged families participated in educationally oriented activities while out of school. In contrast, children from socioeconomically disadvantaged families had more free time and liberty to do what they liked. As a result, the “schools as equalizer” (Downey et al. 2004) hypothesis argues that reducing the length of schooling increases the intergenerational transmission of educational attainment.

In summary, there are sociological theories arguing that increasing the length of schooling strengthens the intergenerational transmission of educational attainment as well as an opposing theoretical perspective according to which more schooling leads to more socioeconomic inequalities in educational attainment. Solving the question of whether schooling increases or decreases the intergenerational transmission of educational attainment is therefore an empirical task. Most empirical tests of the effects of the length of schooling on socioeconomic inequalities in education thus far have focused on the question of whether schooling reduces or increases socioeconomic inequalities in educational performance. In contrast, my study estimates the

long-term effects of the length of schooling on socioeconomic inequalities in educational attainment by measuring educational attainment through the highest educational degree completed.

Raudenbush and Eschmann (2015) provided an overview of the literature using different natural experiments to estimate the effects of schooling on socioeconomic inequalities in children's skills. They reviewed studies estimating the effects of free and universal education in early childhood, summer vacation, the extension of the school day, and changes in the length of compulsory schooling on socioeconomic inequalities in educational performance.

The arguably best known of these studies compared differences in learning rates by family socioeconomic background during the school year to differences in learning rates by family socioeconomic background during the summer, when no schooling takes place. Alexander et al. (2001, 2007) demonstrated that in a sample of students from Baltimore, socioeconomic differences in learning grew more over the summer than over the school year. Downey et al. (2004) replicated these results for a sample representative of the United States. Condrón, Downey, and Kuhfeld (2021) provided evidence that schooling reduces also overall inequality in skills between students. Most research on the effects of summer vacation on learning inequalities used data from the United States, but Holtmann and Bernardi (2019) and Verachtert et al. (2009) found evidence of this dynamic also in Europe.

Von Hippel and Hamrock (2019) provided a critique of the research comparing inequalities in learning over the summer to inequalities in learning during the school year. They argued that most previous results in the literature in this area were affected by two sources of measurement error. First, the results were influenced by the scaling method used to evaluate children's performance. Second, changes in the type of test administered before and after the summer break influenced the results. Correcting in their own empirical analysis for these types of measurement error led von Hippel and Hamrock (2019) to conclude that socioeconomic

differences in children's educational performance were already in place before formal schooling started and did not systematically grow or shrink more in summer than during the school year.

Passaretta and Skopek (2021) pointed out another problem in the research design employed in the summer learning literature. The research design has to assume that any differences in learning rates between the summer and the school year are due to the absence of schooling. However, there may be factors related to summer other than the absence of schooling that affect socioeconomic inequalities in educational performance. For instance, the weather in Europe and in the United States (the countries on which the summer learning literature has focused) is warmer during the summer, and there is more daylight. Therefore, there are more opportunities to spend time outside. This may negatively affect learning, and the effect may be greater in socioeconomically disadvantaged than in socioeconomically advantaged families.

Comparing socioeconomic differences in skill development over the summer to socioeconomic differences in skill development during the school year is not the only research design that has been used to estimate the effects of schooling on socioeconomic inequalities in educational performance. Passaretta and Skopek (2021) used differences in age at testing to estimate the effects of days in the first grade in primary school on socioeconomic inequalities in educational performance in Germany. They found that schooling increased skills but that these skill returns to schooling did not vary by family socioeconomic background. In a similar analysis, using measures of intelligence from military conscription tests among male adolescents in Sweden, Carlsson et al. (2015) found no socioeconomic differences in the effects of the number of school days spent prior to the test on intelligence by parental education and father's earnings.

In contrast to these previous studies, which focused on the short-term effects of the length of schooling on socioeconomic differences in academic performance, the present study

investigates the long-term consequences of the amount of schooling on socioeconomic inequalities in educational attainment. Such a shift in focus is important because currently, we do not know whether short-term effects of the quantity of schooling on socioeconomic differences in academic performance have long-lasting consequences for socioeconomic inequalities in educational attainment.

The Natural Experiment of the German Short School Years

My study estimates whether schooling increases or decreases socioeconomic inequalities in educational attainment by exploiting the German short school years, which reduced the length of schooling for some students by approximately six months, as a natural experiment. Pischke (2007) analyzed the effects of the German short school years on student performance and on earnings, which was his main focus. Earlier studies also estimated the effects of the short school years on student performance (Meister 1972; Thiel 1973). However, none of these studies investigated the effects of the short school years on socioeconomic inequalities in educational attainment, which is the focus of the present study.

The short school years were the result of a change in the month in which the school year started. Before the reform, the school year started in all (West) German states, with the exception of Bavaria, after the Easter holidays, i.e., in April. Bavaria, along with other European countries, started the school year in August. In 1964, the German state governments decided collectively to move the school start in all states to the summer (August or September) (Helbig and Nikolai 2015; Pischke 2007). Initially, the agreement was to accomplish this change by introducing one long school year that would run from April to August of the next year. However, only the state of Hamburg ended up implementing this solution. Bavaria, as mentioned above, did not have to change the start of the school year, as it already started in

August before the reform. For these reasons, Hamburg and Bavaria are used as the control group in the empirical analysis.

Eight West German states (Baden-Württemberg, Bremen, Hesse, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate, Saarland, and Schleswig-Holstein) accomplished the change in the start of the school year by introducing two short school years that ran from April 1966 until November 1966 and from December 1966 until July 1967. In other words, pupils who attended school during these years completed two grades with a reduced length of instruction from 12 to 9 months (not counting the holidays, the length of which were not affected by the reform). As a result, students exposed to the full two short school years in primary school spent approximately six months less in primary school than the cohorts not affected by the short school years.

Importantly, the policy change in shortening the school year by $\frac{1}{4}$ was not accompanied by a change in the curriculum (Pischke 2007). In addition, teachers did not compensate for the loss in instruction time over the year by increasing the number of class hours per week. Based on a survey of teachers, Thiel (1973) concluded that most teachers did not offer additional classes, and those who did provided at most one hour more of teaching per week. Pischke (2007:1226) also showed that teachers were not ill more often during the short school years than before or after them. In sum, there is no evidence that strong compensatory measures were taken to counteract the reduction in instruction time that resulted from shortening the school year. The natural experiment of the short school years in West Germany in 1966/ 1967 therefore has general implications for reductions in instruction time.

In the empirical analysis, I focus on the effects of the short school years that respondents experienced while they were in primary school. The reason for this focus is the central role of early tracking, which occurs in Germany after four years of primary school (Henninges, Traini, and Kleinert 2019; Hillmert and Jacob 2010). In particular, attending university requires the

completion of the highest level of upper secondary education (*Gymnasium*). The transition to *Gymnasium* is made after four years of primary school at approximately age 10. Therefore, exposure to the short school years in primary school is most relevant for final educational attainment.

It is important to understand how the natural experiment analyzed in the present study differs from the natural experiments employed by previous research to study the effects of schooling on inequalities in educational performance. In contrast to the literature on summer learning, the focus of the present study is on the consequences of shortening instruction time. This shortening of instruction time by approximately six months is particularly crucial in the German education system, as students make the transition to secondary school in Germany after only four years of primary school. I estimate how the transition to different types of secondary school is affected by a considerable reduction ($6 \text{ months} / 48 \text{ months} = 12.5\%$) in the time that students are in school before this transition is made.

Of the three previous studies analyzing the effects of the short school years on educational attainment (Meister 1972; Pischke 2007; Thiel 1973), only Meister (1972:115) reported results stratified by family socioeconomic background. He distinguished between two socioeconomic classes based on father's occupation, referred to as working and middle class, and showed that the increase in performance in reading and math among pupils who experienced the short school years was more pronounced in working-class than in middle-class families. In other words, Meister's (1972) results, which were based on the before-and-after comparison of pupils attending schools in one city in one of the eight states that implemented the short school years⁴, suggested that both average academic performance and equality in academic performance increased through the two short school years (in line with critical theories of education and the dynamics of skill formation model). The present study provides the first analysis of the long-

⁴ The city was Saarbrücken, which lies in the state of Saarland.

term consequences of the German short school years on socioeconomic inequalities in educational attainment. It also uses a more robust research design exploiting variation across both states and cohorts to estimate the causal effects of the length of schooling on the intergenerational transmission of educational attainment, and it employs data that are nationally representative of (West) Germany.

Data, Measures, and Analytic Strategy

Data

The empirical analysis employs two nationally representative survey data sets from Germany. The data sources are the German Socio-Economic Panel Study (SOEP; Goebel et al. 2019) and Starting Cohort 6 of the National Educational Panel Study (NEPS; Blossfeld, Roßbach, and von Maurice 2011). I harmonize the variables across the two data sets and pool both data sets to increase the precision of point estimates.⁵

The sample is restricted to women and men born between 1951 and 1965. This sample selection ensures that the birth years affected by the short school years in primary school (1956 to 1960) as well as some birth years before (1951 to 1955) and after (1961 to 1965) the reform are used. The sample is restricted to respondents who lived in the Federal Republic of Germany (“West Germany”) before 1989, as the reforms were implemented in this country. In other words, respondents who lived abroad or in the German Democratic Republic (“East Germany”) are excluded from the analysis. I also exclude respondents from Berlin from the analysis sample, as this was the only state in West Germany in which primary school was six years long and therefore two years longer than in all other states. As a result, the respondents in the analysis sample come from ten states (Baden-Württemberg, Bavaria, Bremen, Hamburg, Hesse, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate, Saarland, and Schleswig-Holstein).

⁵ By doing so, I follow the approach of Skopek and Leopold (2020).

Eight of these states belong to the treatment group, as they implemented the short school years; two (Bavaria and Hamburg) did not and are therefore part of the control group.⁶

Measures

Educational Attainment. The main outcome of the analysis is the completion of upper secondary education (level 3 of the International Standard Classification of Education [ISCED]). In both data sets, a respondent's highest level of education is observed. This information is recoded in a binary variable that is set to 1 for women and men who completed upper secondary education. This level of education at the time of the reform was the major dividing line in the German education system (Dahrendorf 1965) and still is today, as Germany has, compared to other countries, a rather low level of university education (Helbig and Nikolai 2015). As a robustness check, I also report results using (a) years of schooling as a continuous indicator of educational attainment and (b) a binary variable for university education. The variable measuring the years of schooling corresponding to the highest educational degree obtained by the respondent.

Household Income. In addition to educational attainment, I investigate the consequences of the German short school years for respondents' current household income. Because this variable is measured in two different ways in the two data sets, I standardize the variable within each data set. The results should therefore be interpreted in terms of standard deviation. Using household income allows me to investigate the effects of the short school years not only on socioeconomic differences in educational attainment but also on socioeconomic differences in labor market outcomes.

Social Origin. Social origin is measured in three ways. First, I use information on mother's and father's education. A binary variable is constructed that is set to 1 if either parent

⁶ As Hamburg experienced the long short school years, I also estimated models which drop Hamburg from the analysis and therefore only include students from Bavaria in the control group. These models are reported in Table S4 in the *Online Supplement* and report virtually identical results to those which include students from Hamburg.

completed the upper track in the German education system (*Gymnasium*).⁷ This corresponds to a high level of parental education. Second, as a robustness check, I employ a measure of parental years of education. Third, as a further robustness check, I report results using measures of father's and mother's occupational status based on the ISEI (International Socio-Economic Index of Occupational Status) (Ganzeboom and Treiman 1996). The results are virtually identical across all three measures of social origin.

Short School Years. The main independent variable is the number of short school years that a respondent experienced while in primary school. This variable is continuous and includes values of 0, 1, and 2.⁸ Given that primary school in Germany is four years long, it means that those respondents who were fully exposed to the short school years spent half of their time in primary school in short and the other half in regular (i.e., long) school years. In Germany, students in regular school years enter primary school in the first school year after they turned six relative to a cut-off date. This cut-off date was on December 31 for the cohorts born 1959 and earlier. It was moved to June 30 for the cohorts born 1960 and later. The short school years occurred in April to November 1966 and December to July 1967 (Pischke 2007; Helbig and Nikolai 2015). Pupils born in 1959 (independent of their month of birth) entered school in April 1966, while students born in 1958 (independent of their month of birth) entered school in April 1965. Students born in 1957 entered school in April 1964. They all experienced two short school years while in primary school. Students born in 1956 entered school in 1963 and had already completed three full years in primary school when the short school years were introduced. They were therefore exposed to the first short school year, running from April to

⁷ Table S1 in the *Online Supplement* demonstrates that the findings do not differ if, instead of looking at the highest level of parental education, I look at maternal or paternal education.

⁸ The results are robust to operationalizing the short school years through two dummy variables, one of which is set to one for students who experienced one short school year in primary school and one of which is set to one for students who experienced two short school years in primary school. This specification, which is reported in Table S2 in the *Online Supplement*, leads to results that are virtually identical to those for the specification using a continuous variable reported in the main text.

November 1966 in their last year of primary school, and transitioned to secondary school in December 1967.⁹ Pupils who were born in the first six months of 1960 entered school in December 1966 and therefore experienced one short school year.¹⁰ Students born in the second six months of 1960 and the first six months of 1961 entered school in August 1967 and therefore experienced no short school years. Students born in 1955 and earlier were already in secondary school or had already completed their schooling before the short school years occurred. They are therefore assigned a value of zero for the short school years variable. In addition, students from Hamburg and Bayern are assigned a zero for this variable independent of their year of birth, as these states did not have short school years.

The coding of the short school years variable is also summarized in Table 1.

[INSERT TABLE 1 ABOUT HERE]

A potential source of bias could be that the data provide information only on the current state of residence at the time of the interview and not of the state where a respondent was born or, which would make the analysis most reliable, where he or she went to school. It is, however, quite uncommon to move between states in Germany, especially among the cohorts I study (Pischke 2007). For instance, all states in Germany have universities, and students usually attend university in the same state in which they went to school. Therefore, the bias introduced by this data limitation is likely to be small. If there is any remaining bias, it should be random

⁹ Students in secondary school were also affected by the short school years. If focus, however, on primary school, as I am interested in how the short school years affected the transition to secondary school. After the transition is made, there is little movement between secondary school tracks in Germany, in particular among the cohorts I examine (Henninges, Traini, and Kleinert 2019; Hillmert and Jacob 2010). As a robustness check, I also estimated models without the oldest cohort, who was affected by the short school years in secondary school. These models, which are reported in Table S3 in the *Online Supplement*, lead to virtually identical results than those which include this cohort.

¹⁰ An exception is Lower Saxony, a state in which no students entered primary school in first grade in December 1966 (Pischke 2007). The students born in January to June 1960 in Lower Saxony entered school in August 1967 and experienced no short school year.

measurement error, as it is unlikely that residential changes are connected to the short school years (Pischke 2007).

Control Variables. All models control for sex via a dummy variable that is set to 1 for male respondents. In addition, all models control for a dummy for the survey in which a respondent participated (SOEP or NEPS).

Descriptive statistics on all variables used in the analysis are provided in Table 2.

[INSERT TABLE 2 ABOUT HERE]

Hypotheses and Analytic Strategy

Critical theories of schooling and the dynamics of skill formation model predict that shortening the length of schooling will decrease the intergenerational transmission of education. In contrast, the “schools as equalizer” (Downey et al. 2004) hypothesis argues that if children spend less time in school, the intergenerational transmission of education should increase. To test these opposing hypotheses against each other, I exploit variation in the implementation of the short school years in Germany across cohorts and states. This variation allows me to identify the causal effect of the length of schooling on socioeconomic inequalities in educational attainment.

The identification strategy of my study is motivated by the main analysis reported by Pischke (2007). However, it differs in crucial aspects, as Pischke (2007), in his main analysis, did not focus on exposure to the short school years during primary school.¹¹ I estimate a difference-in-differences (DiD) model with the modification that there are before-treatment, treatment, *and* after-treatment groups with respect to the time dimension. Because of this setup,

¹¹ In one part of his analysis, Pischke (2007) isolated the effects of the short school years in primary school. This part of his analysis is more closely related to ours. However, Pischke (2007) did not investigate socioeconomic differences in the effects of the short school years, which are the central focus of my study.

other time-confounding variables, such as educational reforms, are unlikely to bias the results, as these would usually be implemented at one point in time and affect all following cohorts (Pischke 2007). In contrast, the short school years were not a long-lasting policy change but an event that affected only one cohort.

To be precise, i estimate the following types of models:

$$Y_{ist} = \alpha_0 + \alpha_1 O_{ist} + \alpha_2 S_{ist} + \alpha_3 O_{ist} \times S_{ist} + \alpha_4 M_{ist} + \alpha_5 N_{ist} + \zeta_s + \eta_t + \varepsilon_{ist} \quad (1)$$

where y is the outcome of interest (the completion of upper secondary education, years of education, and household income) for individual i in state s from birth cohort t , O is the measure of social origin, and S is the continuous variable indicating the number of short school years to which a respondent was exposed (as indicated by Table 1). The interaction $S_{ist} \times O_{ist}$ estimates whether the effects of the short school years vary by social origin. This interaction is what the analysis focuses upon, as it measures whether the intergenerational transmission of education is affected by the number of short school years.

The analysis includes a control for male (M), survey (N) and both state (s) and cohort (t) fixed effects (through dummy variables for each state and each year of birth). These controls ensure that the analysis isolates the causal effects of the short school years on the intergenerational transmission of educational attainment. The fixed effects ensure that the interaction between social origin and school years can be interpreted in a causal sense. They control for unobserved variables related to the birth year and the state of residence.¹²

¹² I conducted also a robustness check which did not include year of birth fixed effects but a linear trend across cohorts. The results of this alternative and less restrictive model are reported in Table S5 in the *Online Supplement*. They are fully in line with the results reported in the main specification which uses year of birth fixed effects.

Results

Main Analysis

The results of my preferred specification are reported in Table 3. The table reports Linear Probability Models (LPMs) of the effects of the short school years on the completion of upper secondary education.¹³ The interaction between social origin and high parental education is the focus of the analysis, as this interaction indicates whether the change in the length of schooling affected the intergenerational transmission of educational attainment.

[INSERT TABLE 3 ABOUT HERE]

The results are unequivocal. The intergenerational transmission of educational attainment was not affected by the short school years. The interaction between social origin and the short school years is statistically insignificant and substantively small. The estimate in Model 2 suggests that each short school year increased the intergenerational transmission of educational attainment by 1.7 percentage points. Given that the main effect of high parental education is 33.1 percentage points, this is a substantively negligible change. Moreover, the estimate is statistically insignificant and therefore not generalizable to the population level.

Robustness Checks

I conduct a number of robustness checks to ensure that the finding of no effect of the short school years on the intergenerational transmission of educational attainment is not due to a coding decision. This section reports these robustness checks.

¹³ I report LPMs instead of logistic regression models because of the more straightforward interpretation of the coefficients of the former and because of their stronger robustness to the inclusion of interaction and fixed effects, which are both necessary for the present analysis (Ai and Norton 2003; Angrist and Pischke 2009; Gomila 2021; Mood 2010).

[INSERT TABLE 4 ABOUT HERE]

First, Table 4 reports OLS regression models estimating the effects of the short school years on years of education. Again, the interaction between parental education and the short school years is both statistically insignificant and substantively small. Hence, using years of education as an outcome supports the conclusion that the short school years did not affect the intergenerational transmission of educational attainment.¹⁴

[INSERT TABLE 5 ABOUT HERE]

Second, I investigated the effects of the short school years not only on socioeconomic differences in educational attainment but also on socioeconomic differences in income. I used household income, standardized within each survey to account for the variation across surveys, and estimate OLS regression models. These models, reported in Table 5, show that the association between parental education and offspring income was not affected by the short school years.

[INSERT TABLE 6 ABOUT HERE]

Third, instead of a dummy variable indicating a high level of parental education, I also estimated models using parental years of education as a measure of social origin. These models are reported in Table 6. The estimates of these models fully support the main conclusion of the analysis that the short school years had no effect on the intergenerational transmission of

¹⁴ Table S6 in the *Online Supplement* reports a robustness check using the completion of university education as an outcome. These models find no evidence for a change in the intergenerational transmission of education due to the short school years either.

educational attainment. The interaction between parental years of education and the short school years is zero and statistically insignificant.

[INSERT TABLE 7 ABOUT HERE]

Fourth, I used parental occupational status (ISEI) instead of parental education as a measure of social origin. These models, which are reported in Table 7, demonstrate that the association between parental occupation and child education was also not affected by the short school years.

Conclusion and Discussion

How does more schooling affect the intergenerational transmission of educational attainment? This study reports results from a natural experiment to answer this important question. The findings are robust across different specifications and demonstrate that the short school years in Germany in 1966/ 1967 neither increased nor reduced socioeconomic inequalities in educational attainment.

These findings are at odds with two opposing theoretical perspectives motivating this type of research. On the one hand, the findings do not confirm the predictions of the dynamics of skill formation model (Cunha and Heckman 2007) and of critical theories of education (Bourdieu and Passeron 1966, 1970; Bourdieu 1979; Bowles and Gintis 1976, 2002; Willis 1979), which argue that more schooling increases the intergenerational transmission of educational attainment. On the other hand, these findings also do not confirm the more recent notion that more schooling equalizes educational outcomes among children from different socioeconomic backgrounds (Downey et al. 2004; Raudenbush and Eschmann 2015).¹⁵

¹⁵ Raudenbush and Eschmann (2015) argued that the equalizing effect of schools on socioeconomic inequalities in educational performance should vary by child age. The present study investigates the consequences of the length

While the results of the present study are at odds with these theories, they are actually in line with a number of classical sociological studies that have suggested that the intergenerational transmission of education is due mainly to factors operating outside (and not within) schools (Coleman et al. 1966; Jencks et al. 1972). In addition, various other recent empirical studies have also suggested that schooling neither reduces nor increases socioeconomic inequalities in education. For instance, with respect to a much more recent cohort of German schoolchildren than the one analyzed in the present study, Skopek and Passaretta (2021) found that socioeconomic inequalities in educational performance emerge before formal schooling takes place and are largely constant during the school career. These findings are mirrored by results for the United States according to which socioeconomic inequalities in educational performance emerge before school starts and vary little over the school career (von Hippel and Hamrock 2019).

In terms of more causally oriented designs, other natural experiments have also found that schooling does not affect socioeconomic inequalities in educational performance. Passaretta and Skopek (2021) exploited variation in the age at school entry and the age at testing in the first year in primary school to estimate the effects of the length of schooling on socioeconomic inequalities in educational performance in Germany. They found no such effects, in line with the results of the present study. Similar results were obtained by Carlsson et al. (2015), who analyzed intelligence scores from male military conscription tests in Sweden. The findings of both of these studies also match the results reported by von Hippel and Hamrock (2019), according to which the evidence regarding whether socioeconomic differences in test scores grow more during the school year or during the summer holidays in the United States is inconclusive.

of schooling in primary school at age 6–10. It could be that the equalizing effect of schooling is only found for *Kindergarten* and other forms of child care before school starts.

Finally, suggestive support for the idea that schooling plays a rather limited role in the intergenerational transmission of education comes from research comparing socioeconomic inequalities in education across countries. This research has not managed to provide a consistent and robust ranking of countries in terms of such inequalities (Breen and Jonsson 2005; Grätz et al. 2021; Triventi et al. 2020). Therefore, it is unlikely that school characteristics, which vary across countries, have strong effects on socioeconomic inequalities in education. With respect to one such school characteristic, the age of allocation to school tracks, causal research found that reforms in the age at tracking affected the association between parental occupational class and child education but that the same reforms did not affect the association between parental education and child education (van de Werfhorst 2019). Triventi et al. (2020) summarized the results of a project comparing socioeconomic inequalities in education across 17 countries with different education systems and argued that socioeconomically advantaged families always find ways to transmit educational advantage to their offspring.

For all these reasons, the findings of the present study, while certainly arising in a specific context, are part of a more general pattern. As a consequence, I argue that social scientists interested in explaining the intergenerational transmission of educational advantage should change their focus of attention from school to nonschool factors (Coleman et al. 1966; Jencks et al. 1972). Further progress in this area requires a better understanding of which processes operating outside school, e.g., within families, neighborhoods, and among peer groups, affect socioeconomic inequalities in education.

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TABLES

Table 1. Exposure to the Short School Years by Year and Month of Birth in the Treatment States

Year of birth	Month of birth	School entry date	Number of short school years in primary school
1951	January – June	April 1958	0
1951	August – December	April 1958	0
1952	January – June	April 1959	0
1952	August – December	April 1959	0
1953	January – June	April 1960	0
1953	August – December	April 1960	0
1954	January – June	April 1961	0
1954	August – December	April 1961	0
1955	January – June	April 1962	0
1955	August – December	April 1962	0
1956	January – June	April 1963	1
1956	August – December	April 1963	1
1957	January – June	April 1964	2
1957	August – December	April 1964	2
1958	January – June	April 1965	2
1958	August – December	April 1965	2
1959	January – June	April 1966	2
1959	August – December	April 1966	2
1960 ¹	January – June	December 1966	1
1960	August – December	August 1967	0
1961	January – June	August 1967	0
1961	August – December	August 1968	0
1962	January – June	August 1968	0
1962	August – December	August 1969	0
1963	January – June	August 1969	0
1963	August – December	August 1970	0
1964	January – June	August 1970	0
1964	August – December	August 1971	0
1965	January – June	August 1971	0
1965	August – December	August 1972	0

Notes:

¹ Students from Lower Saxony born between January and June 1960 entered school only in August 1967 and experienced no short school year (see footnote 10 in the main text).

Table 2. Descriptive Statistics

	Mean	SD	N
Upper secondary education	0.42	0.49	15,931
Years of education	12.98	2.70	15,828
Household income (standardized)	0.00	1.00	15,876
High parental education	0.19	0.39	14,895
High maternal education	0.05	0.23	14,913
High paternal education	0.17	0.38	14,895
Parental years of education (highest)	10.92	2.71	15,690
Parental occupational status (ISEI)	42.22	16.01	13,081
Short school years	0.39	0.75	15,851
Male	0.49	0.50	15,964

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

Table 3. OLS Regression Models Estimating the Effects of the Short School Years on the Completion of Upper Secondary Education

	(1)	(2)
High parental education	0.324* (0.010)	0.331* (0.011)
Short school years	0.001 (0.011)	0.005 (0.011)
Male	0.114* (0.008)	0.114* (0.008)
High parental education X Short school years		-0.017 (0.013)
<i>N</i>	14,780	14,780

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table 4. OLS Regression Models Estimating the Effects of the Short School Years on Years of Education

	(1)	(2)
High parental education	2.235* (0.053)	2.267* (0.060)
Short school years	0.035 (0.059)	0.050 (0.060)
Male	0.472* (0.041)	0.472* (0.041)
High parental education X Short school years		-0.077 (0.068)
<i>N</i>	14,690	14,690

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table 5. OLS Regression Models Estimating the Effects of the Short School Years on Household Income (standardized)

	(1)	(2)
High parental education	0.304* (0.022)	0.319* (0.025)
Short school years	0.030 (0.024)	0.037 (0.025)
Male	0.106* (0.017)	0.106* (0.017)
High parental education X Short school years		−0.035 (0.028)
<i>N</i>	14,738	14,738

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table 6. OLS Regression Models Estimating the Effects of the Short School Years on the Completion of Upper Secondary Education, Using Parental Years of Education to Measure Social Origin

	(1)	(2)
Parental years of education	0.056* (0.002)	0.056* (0.002)
Short school years	-0.002 (0.011)	0.001 (0.023)
Male	0.112* (0.007)	0.112* (0.007)
Parental years of education X Short school years		-0.000 (0.002)
<i>N</i>	15,545	15,545

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table 7. OLS Regression Models Estimating the Effects of the Short School Years on the Completion of Upper Secondary Education, Using Parental Occupational Status (ISEI) to Measure Social Origin

	(1)	(2)
Parental occupational status	0.009* (0.000)	0.009* (0.000)
Short school years	−0.001 (0.012)	0.010 (0.019)
Male	0.110* (0.008)	0.110* (0.008)
Parental occupational status X Short school years		−0.000 (0.000)
<i>N</i>	13,002	13,002

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

ONLINE SUPPLEMENT TO

Does Schooling Affect Socioeconomic Inequalities in Educational Attainment?

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Table S1. Linear Probability Models Estimating the Effects of the Short School Years on the Completion of Upper Secondary Education, Comparing Maternal and Paternal Education

	(1)	(2)
High maternal education	0.359*	
	(0.020)	
High paternal education		0.331*
		(0.012)
Short school years	0.002	0.003
	(0.011)	(0.011)
Male	0.114*	0.114*
	(0.008)	(0.008)
High maternal education X	0.000	
Short school years	(0.023)	
High paternal education X		−0.019
Short school years		(0.013)
<i>N</i>	14,798	14,780

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table S2. Linear Probability Models Estimating the Effects of the Short School Years on the Completion of Upper Secondary Education, Using Dummy Variables for the Short School Years

	(1)	(2)
High parental education	0.325*	0.332*
	(0.010)	(0.012)
One short school year	0.004	0.007
	(0.026)	(0.027)
Two short school years	0.003	0.010
	(0.024)	(0.024)
Male	0.114*	0.114*
	(0.008)	(0.008)
High parental education X One short school year		-0.011 (0.038)
High parental education X Two short school years		-0.036 (0.027)
<i>N</i>	14,864	14,864

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table S3. Linear Probability Models Estimating the Effects of the Short School Years on the Completion of Upper Secondary Education, Without the Cohort Affected by the Short School Years Whilst in Secondary School

	(1)	(2)
High parental education	0.314*	0.320*
	(0.013)	(0.017)
Short school years	0.000	0.002
	(0.013)	(0.013)
Male	0.137*	0.137*
	(0.010)	(0.010)
High parental education X		−0.010
Short school years		(0.015)
<i>N</i>	8,791	8,791

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table S4. Linear Probability Models Estimating the Effects of the Short School Years on the Completion of Upper Secondary Education, Without Hamburg

	(1)	(2)
High parental education	0.326*	0.333*
	(0.010)	(0.012)
Short school years	−0.004	0.000
	(0.012)	(0.012)
Male	0.118*	0.118*
	(0.008)	(0.008)
High parental education X		−0.018
Short school years		(0.013)
<i>N</i>	14,479	14,479

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table S5. Linear Probability Models Estimating the Effects of the Short School Years on the Completion of Upper Secondary Education, Controlling for a Linear Trend across Cohorts Instead of Cohort Fixed Effects

	(1)	(2)
High parental education	0.325*	0.332*
	(0.010)	(0.011)
Short school years	0.009	0.013*
	(0.005)	(0.006)
Male	0.114*	0.114*
	(0.008)	(0.008)
High parental education X		−0.017
Short school years		(0.013)
<i>N</i>	14,780	14,780

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP), state fixed effects, and a linear trend across year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)

Table S6. Linear Probability Models Estimating the Effects of the Short School Years on the Completion of University Education

	(1)	(2)
High parental education	0.357*	0.380*
	(0.040)	(0.046)
Short school years	−0.004	0.008
	(0.044)	(0.046)
Male	0.161*	0.161*
	(0.031)	(0.031)
High parental education X		−0.056
Short school years		(0.052)
<i>N</i>	14,780	14,780

Notes: Standard errors in parentheses. All models control for whether a respondent is male, the survey (NEPS or SOEP) as well as fixed effects for state and year of birth.

Sources: NEPS SC6, v11 and SOEP, v36 (pooled).

* $p < 0.05$ (two-tailed tests)