Does Shorter Schooling Hurt Student Performance and Earnings? Evidence from the German Short School Years

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Abstract

Could students go to school less, by having longer vacations or graduating earlier? There is little empirical evidence on how changing the length of school or term length, leaving the basic curriculum unchanged, affects learning and subsequent earnings. This paper investigates this issue using variation introduced by the West-German short school years in 1966-67. I show that the short school years led indeed to shorter schooling for affected students. Using comparisons across cohorts, states, and secondary school tracks, I find that the short school years increased grade repetition in primary school, but had no adverse effect on the number of students attending the highest secondary school track or earnings later in life.

JEL Classification J24, J31

Keywords Human capital, returns to schooling, length of school year, term length, grade repetition, tracking

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1 Introduction

The quality of public schools is important to parents and they are willing to pay for better schools in terms of housing costs (Black, 1999). Education researchers agree that schools clearly matter, but there is little agreement on what specific measures to take to improve learning (Burtless, 1996; Hanushek, Kain, and Rivkin, 1998). One dimension of school quality is the length of the school year. School administrators like to reduce the length of school terms in order to save scarce money, often facing opposition from parents, who fear about the educational quality for their children. But there is little evidence to what degree term-length matters for academic achievement and later earnings of students. In this paper, I study the impact of a reform in the West-German school system in 1966-67 which dramatically changed the amount of instructional time for some students in school at the time without directly affecting the highest grade completed or secondary school degree received by these students. I use this as a natural experiment to study the effects of time spent in school on grade repetition, the choice of the secondary school track attended, and on later earnings.

Until the 1960s, all German states except Bavaria started the school year in spring. Politicians felt at the time that it was more sensible to start the school year after summer vacation, and they wanted to achieve uniformity in this policy across states. The transition to a fall start of the school year was achieved in most states through two short school years with 24 instead of the regular 37 weeks of instruction each. Students in school during this time therefore lost a total of 26 weeks of instruction, about two thirds of a school year. The city states of West-Berlin and Hamburg opted for a single long school year instead. The state of Niedersachsen, although introducing the short school years, added extra time to graduating classes, so that many students in this state did not loose any time in school, even though they participated in the short school years. This means that there is substantial heterogeneity across birth cohorts and states in who was exposed to less schooling because of the short school years.

I use variation across cohorts, states, and the secondary school track attended by a student to identify the effect of participating in the short school years on a variety of outcomes. While the short school years nominally eliminated about two thirds of a year, I show that the total time affected students spent in school has actually been reduced by much less. This implies that some students may have stayed in school longer, for example because of grade repetition. This is an important aspect, which has to be kept in mind when assessing policies which try to reduce term length in order to save resources. I analyze grade repetition among primary school students directly and show that the short school years did indeed have the effect that more students were held back. However, these effects are not quite large enough to fully explain the discrepancy between the nominal reduction in school length due to the short school years and the actual reduction of time in school. Unlike grade repetition, which is a relevant outcome only for weaker students, the short school years did not seem to have had a negative effect on the proportion of students entering the highest secondary school track. Finally, I also fail to find negative effects on earnings later in life.

These results may seem surprising in light of the evidence showing that returns to schooling are quite substantial.[1] The association between earnings and schooling may not be causal, of course, because individuals select the amount of schooling they obtain partly on the basis of unobserved characteristics, which also affect earnings. To overcome this problem, many recent studies have used instrumental variables to estimate the returns to schooling, exploiting compulsory schooling laws or differences in the costs of schooling for particular individuals. While these studies should be free of ability bias, they have typically found even larger returns than the OLS estimates. In the US these estimates are clustered around 10 percent; see Card (1999) for a survey of this literature.

These estimates of returns to schooling may not be the correct compari-

[1] Acemoglu and Pischke (1999) report OLS returns to schooling of 7 to 8 percent for Germany during the 1980s. US returns were slightly lower than that at the beginning of the decade and higher at the end.

son when trying to interpret the impact of reducing term length on student achievement and earnings. Most importantly, the variation underlying the results on returns to schooling comes from the highest grade completed or degree obtained. The short school years, on the other hand, affected the length of schooling obtained without affecting secondary degrees obtained directly. One plausible explanation for the differing results would therefore be that returns to schooling estimated previously reflect mostly the signalling value of schooling, which is tied to degrees, rather than actual human capital accumulation, which is related to the time spent in school. The short school years had the same impact on the time in school for all affected students, therefore not altering the relative costs of different degrees or their signalling value. If this interpretation is correct, the length of the school year might easily be reduced in many advanced countries where the minimum level of schooling obtained by all students is high.[2]

However, the results may also be consistent with schooling reflecting mostly human capital accumulation. It has to be kept in mind that the nominal curriculum did not change for students exposed to the short school years. Teachers might have been able to actually teach all the relevant material in a reduced amount of time. Universities and post-secondary vocational schools might have compensated for material that had been missed in school. Individuals exposed to the short school years spent more time in the labor market. The increased incidence of grade repetition might indicate that slower students were not able to cope with the increased pace during the short school years. Grade repetition might have been a mechanism that insured that some marginal students learned the same amount. Nevertheless, this interpretation would also suggest that reducing time students spent in school would be socially beneficial, since overall resources are being saved without adverse effects on the labor market performance of students. My results would fail to carry over to broader policies if the short school years

[2]Note that changing the length of the school year for a given level of compulsory schooling has different implications in the signalling model than changing the compolsory schooling age. See Lang and Kropp (1986) for evidence from compulsory schooling laws on the signalling hypothesis.

brought about particular effort from educators and students, which was specific to this episode and could not be sustained in a more normal setting.

There are few previous results on the effects of term length on student achievement and earnings. Various studies on school quality in the US include term length at the school level as a regressor (for example, Grogger, 1997, Eide and Showalter, 1998). These studies typically find insignificant effects of term length on achievement and earnings but term length may proxy for other school attributes, which are unobserved in these equations. Rizzuto and Wachtel (1980), Card and Krueger (1992), and Betts and Johnson (1998) examined the effect of state level policies, where this type of unobserved heterogeneity may be less of an issue. All three studies found positive and significant effects of term length on later earnings when state effects are not controlled for. Card and Krueger also present results controlling for state effects. The positive effect of term length vanishes within states and conditional on other school quality variables. While some of the findings by Card and Krueger have been challenged by Heckman, Layne-Farrar, and Todd (1996), the zero term length result is relatively robust in their re-estimations. These previous results therefore largely agree with my findings on the German short school years. None of these previous studies exploits policy induced variation in the length of the school year of the magnitude which I study here, which makes the German experience one of particular interest.

The remainder of the paper is organized as follows. Section 2 starts by laying out some background about the German school system and the short school years, and discusses what type of variation is used for identification of the short school year effects. It also discusses the measurement framework used to obtain the empirical results in Section 4, while Section 3 describes the data sources. Conclusions are drawn in Section 5.

2 Institutions and Empirical Framework

2.1 Background on the German School System and Identification

In the 1930s, the German school year started uniformly after Easter. When new territories were integrated into the country due to Nazi expansion after 1937, heterogeneity arose because some of these areas started their school year in summer. The Ministry for Science and Education therefore decreed in 1941 to move the beginning of the school year to summer for all of Germany.

Education has been in the political domain of the federal states in post-war West-Germany. After the war, all states except Bavaria returned to the pre-war custom of starting school in spring. This heterogeneity caused frictions, for example, when families moved across state borders and children had to switch schools. Therefore, the prime ministers of the states signed an Agreement on the Unification of the School System in 1964, the so called Hamburg Accord (Hamburger Abkommen). Among other provisions, the agreement stipulated to move the start of the school year uniformly to summer again, so that the new school year would commence after the summer vacation.[3] The accord was to be implemented by the beginning of the 1967 school year.

A heated debate ensued on how to accomplish the transition from a start of the school year after Easter to the new date in summer. An early consensus emerged among the states, which was based on a prolonged school year, lasting from April 1966 to summer of 1967. This solution was supposed to avoid that children in school during this time would graduate with having attended for a shorter period than what is required by law. However, the Hamburg Accord had also stipulated that schooling is compulsory up to at least grade 9. Some, predominantly southern, states had only required 8 grades in the basic secondary school track, while 9 years were already common

[3] Summer vacations are staggered across German states, so that the beginning of the new school year moves around from year to year and can be anywhere from beginning of August until middle of September.

in the northern states. Various of these states, for example Rheinland-Pfalz, decided to use the 1966-67 transition period to introduce the 9th grade as well. To do this, they planned to split the April 1966 to summer 1967 period into two short school years. This way, the cohort of students entering 7th grade in April 1966 and not attending higher secondary schools, could graduate after nominally attending nine grades by summer 1967, even though they only spent 8 years and four months in school.

The early consensus of a long school year unraveled as more and more states decided to opt for the short school years. Eight states carried out the transition by having a short school year starting April 1, 1966 and ending November 30, 1966, and a second short school year starting December 1, 1966 and ending July 31, 1967.[4] The two city states of West-Berlin and Hamburg stuck to the solution with a single long school year. Starting in 1967, the school year would begin in August and end in July in these states. Graduating classes which participated in the long school year, however, would graduate at the end of March after a shortened final year. Hence, everybody in Hamburg and Berlin attended school for the regular amount of time despite the transition. Bavaria, which already started in summer, had a regular length school year during the transition period. Finally, Niedersachsen adopted the short school years during 1966-67 but added additional school periods in subsequent years for some types of schools (see below for details).

The mechanics of the transition lead to variation in the length of schooling along a variety of dimensions, which can be used for identification. Since the two short school years involved 24 instead of the regular 37 weeks of instruction, students in school during 1966-67 lost a total of 26 weeks in class, and therefore graduated after having attended school for about two thirds of a year less than other students who either completed their schooling by 1966 or began school in 1967 or later. Hence, cohorts which graduated before 1966 or which entered after 1967 went to school longer than cohorts in school

[4]These are the nominal starting and ending dates of the school years. The second short school year effectively ended with the beginning of summer vacation at varying dates across states.

during 1966-67. Throughout the analysis, I will not use this variation alone, because I want to control for cohort main effects.

The second dimension is due to the fact that students in Germany attend one of three secondary school tracks, each of which is of a different length. The lowest track is basic school (Volksschule or later Hauptschule), which ended with the end of compulsory schooling after 8 or 9 grades.[5] The second track, middle school (Realschule), ends after grade 10, and the highest track, Gymnasium, leads to graduation after 13 grades. This means that some students, who were born in the late 1940s and were close to graduation by the mid-60s, will have been affected by the short school years and not others, depending on which track of secondary school they attended. For example, consider someone born in 1949 and entering school in 1956. This person will have graduated by spring 1966 if she went to basic or middle school but will have been in school during both short school years if she went to Gymnasium. The interaction of cohort and track helps identify the effects of the short school year, and is used in my analysis.

The third dimension is the contrasts across states. This makes use of the fact that Bavaria, Hamburg, and Berlin did not have short school years. The state of Niedersachsen provides an additional source of variation. Niedersachsen decided not to have students enter 1st grade for the school year starting December 1966, but only in August 1967. This decision freed up resources (class rooms and teachers) which were used to lengthen the final school year for students attending basic and middle school in the subsequent years. Every basic school cohort entering 9th grade between 1966 and 1974, had an additional 8 month period added to their last school year. For example, the cohort, which entered 9th grade in April 1966 (the first short school year), did not graduate until March 1967. The next cohort, entering 9th grade in December 1966, graduated in March 1968 and so on. Thus, all basic school students attended school for 9 years, even those who were in school during the short school years.

Things were slightly more complicated for middle school students. The

[5] States only started introducing 10th grades in basic school in later periods.

students entering 10th grade in April 1966 graduated in November 1966 after 9 years and 8 months. The next three cohorts, entering 10th grade between December 1966 and August 1968, graduated after 9 years and 4 months of school. These cohorts were affected by the short school years just like their peers in other states. The next six cohorts, entering 10th grade between August 1969 to August 1974, graduated from March 1971 to March 1976 after a total of 10 years in school. Hence, the total schooling of these cohorts was unaffected by the short school years. Students attending Gymnasium were fully affected by the short school years. The length of their schooling was not extended for any cohorts. Obviously, the variation introduced by the Niedersachsen rules can only be exploited together with the variation across tracks and cohorts. I will use both the full interactions of cohort, track, and state, as well as cohort and state differences only (for states outside Niedersachsen) to identify the effect of the short school years, while controlling for main effects of each of these.

The short school year might have affected students in a variety of ways. Instructional time was obviously reduced for these students, not necessarily only during the short school years but even in later years as curricula were adapted for the affected cohorts. For example, the education minister of the state of Schleswig-Holstein decreed that the curricula for four years were to be taught during the two short school years and the subsequent two regular school years. Thus, the available time for each one year curriculum was only reduced by one sixth. In addition, some requirements were reduced for the students exposed to the short school years.[6] Nevertheless, some students may not have been able to cope with the necessary acceleration in pace, resulting in students repeating a grade. The short school years will have lengthened the time these students actually ended up spending in school. Furthermore, students who were in primary school during the short school years may have ended up choosing a different secondary school track. I will analyze grade

[6]For example, the state of Schleswig-Holstein usually required the reading of three authors for the Great Latin Exam (Grosses Latinum), but reduced the number to two during the 1966 short school year.

repetition and attendance of the highest track (Gymnasium) as outcomes directly below. These behaviors, grade repetition and track choice, will also affect the interpretation of the results on earnings. The short school year experiment does not manipulate the total amount of time spent in school directly but rather the length of the instructional period in a certain set of grades.

Test scores on a standardized test would be the preferred choice to assess the effects on student achievement and learning. Since there are no uniform standardized tests available in Germany, I analyze grade repetition in primary school and secondary track choice. In order to understand these outcomes, it is important to note that grades and therefore academic achievement in primary school are a major determinant of both. Unlike in the United States, whether a student repeats a grade is determined by the teacher and school without input from the parents. In principle, there is a set rule, and if certain grades of a student drop below a cutoff, the student is required to repeat a grade. In practice, there is some teacher discretion involved. A single teacher is typically responsible for most subjects of a class in primary school, and there is a subjective component to grades (like class participation), so that the teacher can influence promotion. Teacher discretion is larger in 1st grade, when no numerical grades are given, than in later grades. Nevertheless, grade repetition should largely reflect academic achievement, especially in grades 2 to 4.

The same is true for the choice of the secondary school track after grade 4. In the 1960s, all states except Berlin started Gymnasium, the highest track, with grade 5, while middle school started in many states only with grade 7.[7] I therefore concentrate on the decision to enroll in Gymnasium. At the end of grade 4, the primary school will make a recommendation based on grades, possibly specific exams, and teacher assessment, whether a student should attend Gymnasium. Independent of this recommendation, parents can choose to have their child apply to Gymnasium. In case of a negative

[7]Some states treat grades 5 and 6 as an orientation phase, and allow entry into Gymnasium in grade 5 as well as in grade 7.

primary school recommendation, the student will have to take an admissions exam, which determines whether the Gymnasium will admit the student. Whether a student enrolls in Gymnasium therefore depends both on parental choice and on the academic performance of the student. Since low achieving students cannot enter Gymnasium, even if parents so desire, track choice is a useful measure of student achievement.

After the initial choice of a secondary track is made, switching tracks, while possible in principle, is rare. For example, in 1966, before the first short school year, 13579 students switched into Gymnasium from basic or middle school, compared to 174828 students entering the first grade of Gymnasium from primary school. Thus, switchers are only about 7 percent of total accessions into Gymnasium in that year. Most of this lateral movement takes place by grade 7.

2.2 Measurement Framework

In order to evaluate the effect of the short school years on various outcomes, I construct variables D_i, indicating whether an individual participated in the short school years. These indicators are constructed based on an individual's year of birth, state, and secondary school track or graduation year as described in detail below. I then estimate equations of the form

$$y_i = \alpha + \beta D_i + \gamma_s + \delta_g + \lambda_c + \theta_a + \phi_t + \mu_f + \varepsilon_i \tag{1}$$

where y_i is an outcome, like the log of earnings or wages, γ_s is a set of state effects, δ_g is a set of secondary school track effects, λ_c is a set of year of birth or cohort effects, θ_a is a set of age effects, ϕ_t is a set of time effects, and μ_f is a gender effect.

The regressor of interest, D_i, varies at the state, year of birth, and secondary school track level. Because state, cohort, and secondary school track are likely to influence wages independently of the length of school, it is important to include these control variables in the regression. The implicit assumption is that D_i, conditional on state, year of birth, and secondary school track is as good as randomly assigned. The state where an individual went to school and track are variables which are (at least partly) under the control of individuals. A possible concern is that parents moved across states or decided to send their child to a different secondary school track in response to a state's decision to introduce the short school years. This is unlikely to be the case. The ultimate decisions of the states whether to introduce the short school years were only made at the beginning of 1966. This left little time for parents to move in order to have their children attend school in a different state. The only students possibly affected were therefore those living near the border of one of the states without the short school years (Hamburg and Bavaria, since West-Berlin has no borders with other West-German states) who could possibly send their children to a school in the neighboring state. This should be a very small proportion of students.

In a given state (outside Niedersachsen), the secondary school track only matters for the assignment of D_i for students who were going to be in grades 10 or higher at the time of the short school years. These students made their track choice many years earlier. By grade 9 it is relatively difficult to switch tracks. Nevertheless, students affected by the short school years in primary school may have ended up attending a different secondary school track than they would have otherwise. In this case, track would be an outcome variable of the treatment, and should therefore not be included as a control in regression (1).[8] I show below that the short school years did not actually have much of an impact on the choice of secondary track. If D_i does not have a causal effect on track, it is safe to include track as a control. In order to probe this issue, I estimate equation (1) only for students who were in grades 1 to 9 during the short school years. Track is not used in the construction of D_i for these students in states outside Niedersachsen, so that it can be omitted from the regression in this case when Niedersachsen is excluded.

One issue in controlling for track effects is how to account for the fact that the basic track was extended from 8 to 9 years in many states during

[8]See Angrist and Krueger (1999).

the 1960s as well. Instead of using dummies for three tracks, I divide basic track students into separate groups depending on whether they graduated after eight or nine years.[9] The other controls in equation (1), for age, year, and gender, are only included to help increase the precision of the estimates.

The validity of the indentification hinges on the assumption that interactions of state, year of birth, and track effects do not matter for the outcome variables except for the effects of the short school years. This assumption is more likely to be satisfied when fewer cohorts are used. I therefore start by presenting regressions using the cohorts born from 1943 to 1964. This includes the cohorts potentially exposed to the short school years, those born 1947 to 1960, as well as four adjacent cohorts. As an alternative, I also present regressions using all cohorts born after 1943. Adding additional cohorts helps estimating the large number of control variables and might therefore improve the precision of the estimates. Since the assumption of no interaction effects is less likely to be satisfied in these larger samples, there is a trade-off between efficiency and bias. I do not use respondents born in 1942 or earlier because schooling before and during the war may have been sufficiently different so that including these earlier cohorts would be even more problematic. There was another change in the school year in 1941 which would have to be accounted for. In addition, Ichino and Winter-Ebmer (1998) demonstrate that the war impacted the education and earnings of the cohorts in school at the time.

3 The Data

In order to study the impact of the short school years on student performance, I analyze data on grade retention and secondary track choice. The number of students repeating a grade and the total number of students enrolled in each grade are published annually by the Federal Statistical Office in

[9]In Niedersachsen, the first birth cohort attending 9 years of basic school is the 1946 cohort, in Nordrhein-Westfalen, Hessen, Rheinland-Pfalz, and Baden-Württemberg the 1952 cohort, in Bavaria the 1954 cohort, and in Saarland the 1948 cohort. In all other states, all birth cohorts in the sample attended 9 school years.

the serial *Fachserie A. Bevölkerung und Kultur*, Reihe 10, I, Allgemeines Bildungswesen. Thus, I have the population data on grade retention available. I measure enrollment in the highest secondary school track (Gymnasium) from the same data source, but it is harder to get a clean measure of this. The Federal Statistical Office does not report a consistent series of students leaving fourth grade to enter Gymnasium during the required time period. Instead, I use the number of students entering 5th grade of Gymnasium in a particular school year divided by the number of fourth graders in the state during the previous year, since this is the only measure that I can construct consistently. This measure is slightly problematic, because some students may move across state borders when they change school. In addition, some entry into 5th grade of Gymnasium is from other grades than grade 4 in primary school (e.g. from later grades in basic school). Finally, some states allow entry into Gymnasium after grade 6 in addition to grade 4. In order to minimize the impact of this, I limit the analysis to states other than Berlin and Bremen, where this is a particular problem.[10]

Earnings data are taken from two data sets, each with its own strength and weaknesses.[11] The first is the Qualification and Career Survey (QaC) collected by the Institut für Arbeitsmarkt- und Berufsforschung (IAB) and the Bundesinstitut für Berufsbildung (BIBB). This is a repeated cross section of employed workers of German nationality in the age group 15 to 65. I use the three waves for 1979, 1985-86, and 1991-92, each of which samples about 25,000 workers. The large sample sizes are one of the main advantages of this dataset.

[10] In addition, the 1970 edition of *Fachserie A. Bevölkerung und Kultur*, Reihe 10, I, Allgemeines Bildungswesen did not report data which allowed me to construct a measure consistent with the other years. Therefore, data for the school years starting in 1969 or 1970 are missing for some states.

[11] Some other data sources can in principle be used for this analysis. The German Socioeconomic Panel (GSOEP), which has tracked respondents since 1984, would allow annual observations on the same individuals for about 12 years, but does not offer any particular advantages compared to the datasets used here. Administrative data from the German social security system (IAB-Beschäftigtenstichprobe), with much bigger sample sizes, would allow the analysis of various subgroups. I am currently in the process of obtaining a suitable extract of the social security data.

The earnings variable in the surveys is the gross monthly wage. Respondents in the 1979 survey were asked to report their earnings in 13 brackets, in the 1985/86 survey in 22 brackets, and in 1991/92 in 15 brackets. I assign each individual earnings equal to the bracket midpoint.[12] I then convert the variable to an hourly wage by dividing by the number of weekly hours. I also present results using monthly earnings directly.

The year of school entry is not available in the QaC, but it provides year of birth, the year when the individual graduated from secondary school, and the highest secondary school degree attained. This allows various ways to construct variables for the students affected by the short school years. I construct variables for the number of short school years an individual was exposed to using the interaction of cohort and track. This is done in two ways. The first is to use year of birth and the highest secondary school degree obtained. German children enter school in the year after they have reached their 6th birthday. Using this information, it is possible to determine how many short school years an individual should have been exposed to in a state with the short school years. Table 1 displays how this assignment is done for the birth cohorts from 1946 to 1960. There are a few caveats. First, some students enter school early or late, and I do not have any information on this. Secondly, somebody born in 1960 might have entered school either in November 1966 and experienced one short school year, or in summer 1967 missing the short school years altogether. Since approximately an equal number of individuals will have had zero and one short school years, I assign everybody born in 1960 half a short school year. Because I adjust the value of the covariate appropriately, this assignment will lead to a consistent estimate of the effect of the short school years despite the measurement error introduced by not knowing the true value.

[12]Because of the large number of brackets this is unlikely to introduce much more measurement error than is done by respondents' rounding continuous amounts. The top bracket in 1979 was DM 5,000 or more which I assigned a value of DM 7,500, in 1985/86 it was DM 15,000 or more which I assigned a value of DM 17,500, and in 1991/92 it was DM 8,000 or more which I assigned a value of 12,500. Only 1.1 percent of sample observations are in the top income bracket.

Alternatively, I construct a similar measure using the year of birth and year of graduation. There is a similar missing information problem here. Everybody born in 1960 is again assigned half a short school year. Individuals graduating in 1966 might have also experienced either zero or one short school years, and are assigned half a short school year as well. The two measures of exposure to the short-school year will naturally differ. The variable based on year of graduation will count individuals as treated by the short school years if the individual was still in school in 1966/67 because of earlier grade repetition. These individuals will not be assigned short school years using the assignment based on the highest degree. If individuals repeating grades have lower earnings for reasons other than the short school year, then the measure based on highest grade will overestimate the relative earnings of those exposed, while the measure based on school leaving will underestimate these earnings. Of course, there are reasons to believe that both variables have substantial measurement error from other sources as well. There will be misreporting both of the highest degree attained and the year of graduation. To the degree that the measurement error stems from year of birth, there is nothing I can do about this. Measurement error in the other variables can be filtered out by using one of the exposure measures as an instrument for the second, as long as these measurement errors are independent.

Unfortunately, the QaC does not identify the state in which an individual grew up or attended school. Only the state of residence is available. The short school year measures constructed above are set to zero for residents of Bavaria, Hamburg, and Berlin. For residents of Niedersachsen, they are also set to zero for respondents with basic school degrees and the middle school cohorts which were unaffected. The state of residence is only a good proxy for the state an individual went to school in if individuals do not move frequently between states. I present some evidence on this below.

I also construct a measure of years in school defined as year of graduation minus year of birth minus 6. This measure is fairly noisy, because I do not have detailed enough information on birthdays to know the exact date when the person first entered school, and because there is some parental discretion. Nevertheless, this variable is useful as it lets me assess whether students exposed to the short school years received less schooling. I limit the QaC sample to respondents for whom this length of schooling variable is in the range of 6 to 15 years, in order to minimize the effect of misreporting on the estimates.

The second dataset I use is the German General Social Survey (ALLBUS) from 1980 to 1996. This is also a repeated cross section survey. It samples about 3,000 respondents of German nationality who are 18 years or older. The surveys were conducted every two years with an additional smaller survey for 1991, right after German unification. I only use the west German portion of the 1991 to 1996 waves.

The only income variable in the survey is net monthly income. The questionnaire is not very explicit what types of incomes to include (e.g. whether respondents are supposed to report asset income). Income was elicited as a continuous variable. Respondents refusing to report income were asked a second question, which allows them to report their income in 22 brackets. This increases the response rate substantially. I incorporate the bracketed income information by assigning midpoints again.[13] Despite the different concepts, the distribution of income looks very comparable to the distribution of earnings in the QaC data. A weekly hours variable is available from 1984 onwards. I use the monthly income directly in the regressions in order to include the 1980 and 82 survey years.

The ALLBUS provides year of birth and the highest secondary school degree attained,[14] which allows me to construct the first measure for the number of short school years an individual was exposed to as described above. From 1982 onwards, the survey also collected month of birth. This information is useful to decide whether someone born in 1960 attended one short school year or none. I use the information where available, and assign everybody

[13]The top bracket is DM 15,000 or more to which I assign a value of DM 17,000, the mean among respondents reporting a continuous income amount above DM 15,000.

[14]Starting in 1990, there is also a variable on the total number of years of schooling. I do not use this variable because it is only available for a few waves.

born in 1960 half a short school year in the 1980 wave or if the month of birth information is missing.

The ALLBUS identifies the state of residence in every wave. I use this in the same way as for the QaC data. In addition, the 1991 to 1994 waves also ask about the state of birth and since when an individual has lived in the current state of residence. This information lets me assess to what degree individuals have moved across state lines from the time they grew up. Table 2 displays some summary statistics about the interstate mobility of individuals. It reveals that about 80 percent of all respondents live in their state of birth. The rates differ slightly, depending on whether the calculation is based on the state of birth variable or the variable asking about the time in the current state. There is relatively little mobility between birth and age 18. Therefore, state of birth will be a better indicator than state of current residence for the state in which an individual attended school. Most relevant for the purpose of this paper, more than 80 percent of individuals at risk of participating in the short school years (the birth cohorts 1947 to 1960) have lived in their current state already in 1965. The percentage of people in their state since 1965 or earlier is even higher for current residents of Niedersachsen, but it is very low for residents of Hamburg and Berlin. While the latter are relatively small states, there will be some measurement error introduced by the fact that many individuals move in out of these states. This measurement error will lead to pure attenuation of the effect of the short school years.

4 Estimation Results

4.1 The Impact of the Short School Years on Years in School

The first question raised by the introduction of the short school years is whether affected students did actually spend less time in school. While the nominal time reduction due to the short school years was about two thirds of a school year, students' behavior might have adjusted to undo part of this reduction. In the QaC data I can construct a measure of the length of time a respondent spent in primary and secondary school. Table 3 presents results regressing this variable on the short school year measures. This can be interpreted as the first stage of the problem. These regressions condition on secondary school track and gender as well as a full set of year, year of birth, age, and state of residence dummies. Recall that the short school year measure counts the number of short school years experienced by an individual and can range up to two. Each short school year corresponds to a loss of a third of a year of schooling. The first column in Table 3 uses the short school year measure based on tracks and reveals that each short school year reduced total schooling by about 0.15 years. The effect is far less than one third, and it is quite precisely estimated.

One reason why the length of schooling variable may not be picking up the effect of the short school years correctly is that the length variable can only be computed in full years. Individuals, who attended the first short school year, should report graduating an entire year ahead of the schedule had they just attended school years of regular length (because both the beginning and the end of the first short school year were during the same calendar year). Since most affected individuals participated in both short school years, this might actually lead to an overstatement of the effect, since the measured length of schooling would be reduced by a whole year rather than two thirds of a year. Since the measured length of schooling is based on the graduation year, it is also unlikely that participants in the short school years reported less schooling because they disregarded the short school years. However, the short school years might have had effects on grade retention, hence they may have actually lengthened total schooling for some individuals, conditional on secondary track.

It is worthwhile keeping in mind that the measurement of the short school year regressor is likely to be imperfect as well. Individuals presumably misreport both their year of birth and their highest level of schooling. For example, Ashenfelter and Rouse (1998) find that about 6 percent of the variance in highest grade completed is due to measurement error in a sample of twins. In addition, some individuals will have moved between states since they went to school. The impact of this latter measurement error can be assessed with the aid of the ALLBUS data, which have both state of residence and state of birth. These data come from the 1991-94 waves of the ALLBUS, therefore respondents are slightly older on average than in the QaC sample, so that the degree of mobility in the ALLBUS data is likely somewhat overstated. In addition, assuming that state of birth corresponds to the state of schooling ignores that some students moved between birth and the time they went to school, again overstating mobility. Nevertheless, using a measures of exposure to the short school year based both on year of birth, call it D_i^*, and year of residence, D_i, allows me to quantify the bias from measurement error. If the measure based on year of birth was correct, then the coefficient from a regression of D_i^* on D_i measures the attenuation from using D_i as a regressor instead of the true measure. Including the other covariates in Table 4, this attenuation factor is 0.84 in the ALLBUS with a standard error of 0.03. This implies that each short school year reduced schooling by $0.142/0.84 = 0.170$ years.

Column (2) uses the second measure of the short school year, based on the graduation date instead of the track of secondary school. This measure shows a positive coefficient. This result is likely to be due to the fact that both the regressor and the dependent variable involve the graduation date, so that mis-measurement in the graduation date will lead to a spurious positive correlation. Not surprisingly, using the short school year measure based on tracks as an instrument turns the sign around and we get a coefficient of -0.15 again. Comparing this estimate to column (1) indicates that the amount of measurement error filtered out by using both measures in conjunction is negligible.

4.2 The Impact on School Performance

One channel through which the reduction in schooling induced by the short school years might have been undone is by students repeating grades more often. In addition, looking at grade repetition gives us some idea about the impact of shorter schooling on student performance. I also present results on the fraction of students going on to Gymnasium, the highest secondary school track, after grade 4. Data on grade retention of affected and unaffected grades in primary school are presented in Table 4. States are grouped into one of three groups: seven states with the short-school year, Bavaria with the regular school year, and Berlin and Hamburg with the long school year. I exclude Niedersachsen from this table because of its special provisions for graduation, which makes it unclear whether students attending primary grades should have actually been affected by the short school years. Berlin and Hamburg are control states, because schools should have adapted the curriculum to the long school year, since students would eventually graduate after the normal length of total schooling. Retention rates are presented for the school year 1965-66, the last year before the transition, the 2nd short school year (1966-67) and the following four regular school years. During those years, older grades will have been affected by the transition, but not new grades entering since 1967. This allows a variety of contrasts.

Looking at first grade, it is apparent that retention rates did not fluctuate much over the period in either the states with the short school year or the control states. Things look different for 2nd grade. In both years when 2nd grades are affected, grade repetition jumps by about 1 percentage point in the short school year states, and remains rather steady in Bavaria and the long school year states. Similar effects are visible for grades 3 and 4. Grade repetition drifts up by about 1 percentage point in the two years after the short school years and then drops back by about the same amount for the unaffected grades entering school after the short school years. The effects of the short school years on grade repetition often seem to long lived, and are visible even a few years after the short school years. This may be due to the fact that material had to be taught more quickly during the school years immediately after 1966 as well.

Data on the fraction of students entering Gymnasium are presented in Table 5. These data are presented for the cohorts entering 5th grade from 1964 to 1971. The three years from 1964 to the beginning of the 1st short school year in 1966 are pre-treatment years, since students completing fourth grade at that time were unaffected by the short school years. The next transition is presented for 1967, the first regular school year after the short school years. Students starting grade 5 during the years 1967 to 1969 will have been exposed to both short school years in the treatment states. 1971 represents a post treatment year.[15] The treatment states are being compared to Bavaria and Hamburg, since the data on Berlin are not comparable. Results for Niedersachsen are also presented in the table but it is again unclear whether Niedersachsen should be a treatment or control state.

A notable feature of Table 5 is that the fraction of students attending Gymnasium increased over this period. Furthermore, the upward trends seem to differ across states. They are much more moderate in the treatment states and Bavaria then in Hamburg. The strong rise of Gymnasium enrollment in Hamburg may stem from the fact that these results are calculated from the number of students entering Gymnasium in a state. For example, some of the students starting Gymnasium in Hamburg might have come from primary schools from outside the state (which is basically a city) as more and more suburban parents sent their children to attend city schools during this period when Gymnasium enrollments expanded. Gymnasium enrollment in Niedersachsen is slightly below trend during the treatment years. Therefore, it matters exactly how this state is treated in assessing the impact of the short school years.

Table 6 presents regression results for the effects of the short school years on grade repetition and entering Gymnasium. Controlling for grade, year, and state effects, I find sizeable effects of the short school year on grade retention. Retention rates have increased by about 0.8 to 0.9 percentage

[15] Data for 1970 are not available for all states.

points due to the short school years and the estimates are highly statistically significant. The effects are also large in magnitude, since only 2 to 5 percent of students repeat grades every year. The results do not depend very much on whether Niedersachsen is treated as a treatment or control state or dropped from the sample altogether. Column (2) shows that the results are changed little when state*grade interaction effects are controlled for. Column (3) presents results that are limited to grades 2 to 4, where grade repetition is most likely to reflect academic achievement. The results are again very similar.

The last column in Table 6 presents the results for entering Gymnasium. As was obvious from Table 5, here the treatment of Niedersachsen matters more. When Niedersachsen is treated as a treatment state, the effects on track choice are zero. On the hand, the data suggest that more students exposed to the short school years attended Gymnasium when Niedersachsen is treated as a control state. However, in neither case do the data suggest that reducing the length of school during primary grades led to fewer students attending Gymnasium.

These results therefore give a picture of the effect of the short school years on student performance, which may at first seem somewhat conflicting. The grade repetition results indicate that weaker students may have been hurt by the reduction in the length of the school year, maybe because these students need more repetition to effectively grasp the material being taught. Students further up in the ability distribution do not seem to have been adversely affected by the short school years, as evidenced by the results on Gymnasium entry. At the time, about 80 percent of students did not enter Gymnasium after grade 4, so that these results speak on impacts fairly high up in the ability distribution.

How much of the reduction in the length of schooling will be undone by the fact that reducing term length will cause some students to repeat grades? Students on average stayed in school for 10.1 years. Someone affected by the short school years will have on average 5 more years of schooling after the short school years. Taking an impact of 0.009 on grade repetition as representative, and assuming that this effect persists for affected students for each year after primary school, implies that grade repetition added about 0.05 of a school year to the average time students spent in school, which is not very large compared to the initial reduction of two thirds of a school year.

4.3 The Impact on Earnings

Table 7 presents regressions of log wages and earnings on the short school year indicators using the QaC data. The regressions control for the maximal set of year, age, and year of birth dummies, secondary school track, state of residence, and gender. This means that identification is achieved by using both the second and third level interactions implied by the short school year measures. The regressions use the cohorts potentially affected by the short school years (1947 to 1960) as well as four adjacent birth cohorts (i.e. the sample consists of the cohorts 1943 to 1964). The absence of second or third level interactions of year of birth, state, and track, apart from effects due to the short school years, should be most plausible in this relatively narrow sample. Different sources of identification are explored below. The top panel in the table reports coefficients using log hourly wages as the dependent variable, while the bottom panel reports similar regressions using log monthly earnings.

The coefficients on the short school year measure based on tracks in column (1) are basically zero and they are relatively precisely estimated. Recall that the short school year measure counts the number of short school years an individual was exposed to. The 95 percent confidence interval for the effect of both short school years on wages ranges from -0.02 to 0.02. Taking a return to schooling of 7.5 percent as the benchmark, we might expect the short school years to lower wages by up to 5 percent, since they eliminated two thirds of a year of schooling. The estimates in column (1) suggest that the negative effect of the short school years was at most 40 percent as large.[16]

[16] If the results in Table 3 above are correct, the short school years together eliminated

These results indicate that the short school years did not seem to have any detrimental effect on the earnings of affected students, and large effects can definitely be ruled out.

Using the second measure of the short school years based on graduation year in column (2) yields very similar results. Coefficients are slightly positive when the second measure is used as an instrument for the first, as is shown in column (3). This indicates that measurement error may bias the results in column (1) towards zero, but the true coefficient is positive, rather than negative. Column (4) shows regressions which are limited to men for whom selective labor force participation should not be much of an issue. The effects are again slightly positive.

Table 8 probes the specification further by changing the exact set of treatment and control cohorts included in the sample. Column (1) only uses cohorts in primary school during the short school years, and column (2) uses those affected in grades 1 to 9. These specifications also include the adjacent unaffected cohorts born from 1943-46 and 1961-64 again. The coefficient estimates change little from the previous table, and there is no particular pattern to the results for earnings and wages, suggesting that any differences are likely due to sampling variation. The identification in these specifications only relies on the interaction of state and year of birth but not secondary school track, since everybody in grades 1 to 9 in a treatment state was affected by the short school years. The only exception to that rule is the state of Niedersachsen. Column (3) therefore uses the same sample as column (2) without Niedersachsen. It is then possible to omit the controls for secondary school track. The results are again somewhat more positive, indicating that controlling for track does not bias the results upwards.[17] This is not surprising, since the short school years did not seem

only about a third of a school year. In this case, the returns to schooling benchmark would be only 2.5 percent, which is closer to the boundary of the 95 percent confidence interval.

[17] The coefficients in column (3) are also more positive when compared to a regression that excludes the Niedersachsen observations and includes track dummies, which is the relevant comparison here.

to affect track choice very much in Table 6.

Column (4) includes only secondary school students, but omits primary school students from the sample. The differences are small when comparing the results to the primary school sample in column (1). This indicates that the absence of effects of the short school year is not particular to reducing term length in either primary or secondary school. Thus, it seems to matter little whether students had time after the short school years to "catch up," or whether that was impossible because their graduation was imminent. Unfortunately, it is not possible to compare results for students affected in a particular grade with samples of this size, since the estimates would become too imprecise.

Finally, column (5) includes all cohorts available in the three QaC waves, which were born in 1943 or later. Estimates are again slightly positive, indicating that it might be potentially problematic to include more cohorts in the control group. In this sample, there is not even a gain in precision from extending the sample, because few additional observations are added.

The results from the ALLBUS, shown in Table 9, indicate a slightly negative impact of the short school years. The point estimate for the sample including the cohorts born 1943 to 1964 in column (1) is -0.01, implying a 2 percent loss in earnings for participating in both short school years. Unfortunately, the ALLBUS samples are much smaller, leading to a relatively imprecise and insignificant estimate. This is true even more in column (2), where the sample is restricted to the three waves from 1991, 1992, and 1994, but the estimate again changes little in this subsample. Since the 1991-94 waves of the ALLBUS data identify state of birth, they allow a coding of the short school year measure which should be more accurate than the measure based on state of residence. In fact, a comparison of results using the two measures in columns (3) and (4), including state of birth effects, reveals that measurement error may play some role, but the coefficient based on the measure using state of birth in column (4) is again more positive. This finding also suggests that it is unlikely that the true coefficient is negative, and the finding of a small effect is simply due to attenuation from mobility across states. But the precision of the results does not allow any strong conclusions.

Overall, the results do not indicate any negative effects of the short school years on earnings. The estimates with the QaC data are precise enough to rule out any sizeable negative effects. Various checks on the specification and potential biases from measurement error all indicate that the coefficients might be biased downwards rather than upwards. One possible explanation for this finding might be that students did not really loose the full two thirds of a school year, as suggested in Table 3. Nevertheless, there is fairly strong evidence that a moderate reduction in term length in Germany did not have adverse effects on earnings.

5 Conclusion

This paper presents estimates from a reform in the West-German school system which manipulated the length of schooling for affected students without affecting the highest grade completed or secondary school degree obtained directly. The results of this paper therefore speak directly to the impact of changes in term length or other changes in the length of schooling which are independent of the highest grade completed. The results suggest that some of the reduction in instructional time is being undone by students, for example through grade repetition. Apart from increased grade repetition, I do not find negative effects of shorter schooling. Neither the secondary school track attended nor later earnings seem to have been affected adversely by the short school years.

These findings are encouraging for policy makers or school administrators who wish to use a reduction in term length in order to save resources, which could be spent on other dimensions of school quality, like reducing class size. A two week reduction of the school term in the United States would reduce total instructional time by 24 weeks by the time a student graduates from high school. This is about the same as the reduction in the length of schooling generated by the short school years in Germany. Of course, it is an open question whether the German results would fully carry over to the American school system.

Many states in west Germany currently consider reducing the time to reach the university entrance qualification Abitur (obtained at the end of the Gymnasium track) from 13 to 12 years. One reason for this proposal is the fact that the east German states only require 12 years for the same degree. Apart from the possible cost savings, it is also seen as a useful device to reduce the age at which university graduates enter the job market. Critics object to these proposals on the grounds that educational quality might be compromised. The short school year experience suggests that this is not the case, and that it might be possible to eliminate the last year of Gymnasium without adverse effects on the labor market performance of the students.

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Table 1
Numbers of Short School Years by Birth Cohort and Secondary School Track

Year of Birth	Quarter of Birth	Year of School Entry	Year of Graduation from Basic School	Middle School	Gymnasium	Number of Short School Years Basic School	Middle School	Gymnasium
46	all	53	62	63	66	0	0	0
47	all	54	63	64	66/Dec	0	0	1
48	all	55	64	65	67	0	0	2
49	all	56	65	66	68	0	0	2
50	all	57	66	66/Dec	69	0	1	2
51	all	58	66/Dec	67	70	1	2	2
52	all	59	67	68	71	2	2	2
53	all	60	68	69	72	2	2	2
54	all	61	69	70	73	2	2	2
55	all	62	70	71	74	2	2	2
56	all	63	71	72	75	2	2	2
57	all	64	72	73	76	2	2	2
58	all	65	73	74	77	2	2	2
59	all	66	74	75	78	2	2	2
60	1	66/Dec	75	76	79	1	1	1
60	2	66/Dec	75	76	79	1	1	1
60	3	67	76	77	80	0	0	0
60	4	67	76	77	80	0	0	0

Note: This table shows years of school entry and graduation based on school entry in the year after the 6th birthday, no grade repetition, and 9 years of basic school.

Table 2
Percentage of Respondents Who Have Lived in Current State Since Specific Age or Time
ALLBUS, 1991-94 Waves

Has Lived in Current State Since	State of Current Residence: All States	Bavaria	Niedersachsen	Berlin	Hamburg
All Respondents					
Birth (State at Birth)	83	86	86	57	56
Birth (In State Since)	80	82	81	44	53
Age 6	83	83	84	46	58
Age 12	84	84	85	51	62
Age 18	86	86	86	59	67
1965 or earlier	63	63	62	35	51
Respondents Born 1947-1960					
Birth (State at Birth)	83	89	86	43	50
Birth (In State Since)	78	84	80	33	46
Age 6	82	84	83	37	57
Age 12	84	85	84	40	64
Age 18	85	88	85	47	64
1965 or earlier	83	85	84	40	64

Table 3
Regressions for Number of Years of Schooling
Qualification and Career Survey
Cohorts Born 1943-64
(Standard Errors in Parentheses)

Independent Variable	OLS (1)	OLS (2)	IV (3)
Short School Year	-0.142	---	---
Definition Based on Tracks	(0.018)		
Short School Year	---	0.163	-0.150
Definition Based on Graduation Date		(0.042)	(0.019)
Secondary School Track Dummies	✓	✓	✓
Year Dummies	✓	✓	✓
State of Residence Dummies	✓	✓	✓
Year of Birth Dummies	✓	✓	✓
Age Dummies	✓	✓	✓
Female Dummy	✓	✓	✓

Note: Number of observations is 35476. Standard errors are adjusted for clusters at the track * year of birth * state level. The short school year measure based on tracks is used as an instrument for the short school year measure based on graduation date in column (3).

Table 4
Fraction of Students Repeating Primary Grades
1966 to 1971 by State Group

	Grade 1	Grade 2	Grade 3	Grade 4
1965-66 School Year				
States with Short School Years	0.045	0.044	0.036	0.034
Bavaria	0.036	0.026	0.020	0.014
States with Long School Years	0.037	0.052	0.043	0.040
1966-67 School Year (2nd Short School Year)				
States with Short School Years	0.045	0.053	0.040	0.037
Bavaria	0.038	0.026	0.021	0.015
States with Long School Years	0.029	0.048	0.039	0.034
1967-68 School Year				
States with Short School Years	0.047	0.057	0.046	0.043
Bavaria	0.040	0.028	0.020	0.015
1968-69 School Year				
States with Short School Years	0.048	0.049	0.049	0.048
Bavaria	0.037	0.026	0.019	0.015
States with Long School Years	0.034	0.043	0.028	0.030
1969-70 School Year				
States with Short School Years	0.053	0.044	0.038	0.045
Bavaria	0.038	0.027	0.018	0.016
States with Long School Years	0.033	0.048	0.034	0.025
1970-71 School Year				
States with Short School Years	0.053	0.042	0.032	0.032
Bavaria	0.039	0.027	0.019	0.017
States with Long School Years	0.034	0.044	0.032	0.027

Source: Statistisches Bundesamt, *Fachserie A. Bevölkerung und Kultur*, Reihe 10, I, Allgemeines Bildungswesen, Stuttgart: Kohlhammer, various issues.
Note: States with short school years are Schleswig-Holstein, Bremen, Nordrhein-Westfalen, Hessen, Rheinland-Pfalz, Saarland, and Baden-Württemberg (Niedersachsen is excluded from this group), states with long school years are Berlin and Hamburg. Shaded areas indicate grades affected by the short school years. No Berlin data on grade repetition are available for the 1967-68 school year.

Table 5
Fraction of Students Entering Gymnasium after Grade 4
1963 to 1971 by State Group

School Year	States with Short School Years	Bavaria	Hamburg	Niedersachsen
1964	0.198	0.188	0.217	0.140
1965	0.227	0.214	0.251	0.175
1966 (start of 1st short school year)	0.237	0.213	0.271	0.182
1967	0.248	0.220	0.290	0.173
1968	0.256	0.220	0.395	0.183
1969	0.273	0.231	0.405	0.162
1971	0.289	0.266	0.380	0.225

Source: Statistisches Bundesamt, *Fachserie A. Bevölkerung und Kultur*, Reihe 10, I, Allgemeines Bildungswesen, Stuttgart: Kohlhammer, various issues.
Note: States with short school years are Schleswig-Holstein, Nordrhein-Westfalen, Rheinland-Pfalz, Saarland, and Baden-Württemberg (Bremen, Hessen and Niedersachsen are excluded from this group). Shaded area indicates grades affected by the short school years.

Table 6
Regression Estimates of the Effect of the Short School Years on Grade Repetition and Secondary School Track Choice
(Standard Errors in Parentheses)

	Dependent Variable			
	Grade Repetition			Entered Gymnasium
Independent Variable/Specification	(1)	(2)	(3)	(4)
Mean of Dependent Variable	0.0389	0.0389	0.0372	0.237
Affected by Short School Years	0.0078	0.0067	0.0070	0.004
(Niedersachsen is Treatment)	(0.0018)	(0.0017)	(0.0021)	(0.011)
Affected by Short School Years	0.0088	0.0094	0.0109	0.017
(Niedersachsen is Control)	(0.0017)	(0.0016)	(0.0019)	(0.009)
Affected by Short School Years	0.0096	0.0088	0.0096	0.010
(Sample without Niedersachsen)	(0.0013)	(0.0011)	(0.0012)	(0.011)
Year Dummies	✓	✓	✓	✓
State Dummies	✓	✓	✓	✓
Grade Dummies	✓	✓	✓	
State*Grade Interactions		✓	✓	
Number of Observations	256	256	192	70

Note: States with short school years are Schleswig-Holstein, Bremen, Nordrhein-Westfalen, Hessen, Rheinland-Pfalz, Saarland, and Baden-Württemberg. Niedersachsen is treated differently in different specifications. Data on grade repetition cover grades 1 to 4 and the school years ending 1966 to 1971. Berlin data are missing for the 1967-68 school year. The regressions are weighted by the number of students in each grade, year, and state. Column (3) only includes grades 2 to 4. Data on entering gymnasium cover the years 1964 to1971, Bremen and Berlin are excluded, and there are missing observations for Hessen in 1969 and Hamburg, Niedersachsen, Baden-Württemberg and Bavaria in 1970. The regressions are weighted by the number of fourth graders in the year and state.

Table 7
Earnings Regressions
Qualification and Career Survey
Cohorts Born 1943-64
(Standard Errors in Parentheses)

Independent Variable	OLS (1)	OLS (2)	IV (3)	Only Men OLS (4)
Dependent Variable: Log Hourly Wage				
Short School Year	-0.000	---	0.003	0.004
Definition Based on Tracks	(0.005)		(0.006)	(0.006)
Short School Year	---	0.002	---	---
Definition Based on Graduation Date		(0.005)		
Dependent Variable: Log Monthly Earnings				
Short School Year	-0.001	---	0.004	0.007
Definition Based on Tracks	(0.006)		(0.006)	(0.007)
Short School Year	---	0.003	---	---
Definition Based on Graduation Date		(0.005)		
Secondary School Track Dummies	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓
Year of Birth Dummies	✓	✓	✓	✓
Age Dummies	✓	✓	✓	✓
Female Dummy	✓	✓	✓	✓
Number of Observations	35476	35476	35476	21426

Note: Standard errors are adjusted for clusters at the track * year of birth * state level. The short school year measure based on graduation date is used as an instrument for the short school year measure based on tracks in column (3).

Table 8
Earnings Regressions
Qualification and Career Survey
(Standard Errors in Parentheses)

Cohorts Affected in	Primary School	Grades 1-9		Secondary School	All
	1943-46	1943-46		1943-55	
Cohorts	1957-64	1952-64		1961-64	1943-75
Independent Variable	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Log Hourly Wage					
Short School Year	0.003	-0.001	0.010	-0.003	0.004
Definition Based on Tracks	(0.007)	(0.006)	(0.015)	(0.006)	(0.005)
Dependent Variable: Log Monthly Earnings					
Short School Year	-0.003	-0.009	0.000	-0.002	0.006
Definition Based on Tracks	(0.008)	(0.007)	(0.016)	(0.007)	(0.006)
Secondary School Track Dummies	✓	✓		✓	✓
Year Dummies	✓	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓	✓
Year of Birth Dummies	✓	✓	✓	✓	✓
Age Dummies	✓	✓	✓	✓	✓
Female Dummy	✓	✓	✓	✓	✓
Number of Observations	17934	27023	24795	26413	39279

Note: Standard errors are adjusted for clusters at the track * year of birth * state level. Observations from Niedersachsen are omitted from the specification in column (3).

Table 9
Earnings Regressions
ALLBUS 1980-96
Dependent Variable: Log Monthly Earnings
(Standard Errors in Parentheses)

Waves Independent Variable	All (1)	1991-94 (2)	1991-94 (3)	1991-94 (4)
Short School Year	-0.010	-0.009	-0.011	---
Definition Based on State of Residence	(0.012)	(0.025)	(0.025)	
Short School Year	---	---	---	0.016
Definition Based on State of Birth				(0.026)
Secondary School Track Dummies	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓
State of Birth Dummies			✓	✓
Year of Birth Dummies	✓	✓	✓	✓
Age Dummies	✓	✓	✓	✓
Female Dummy	✓	✓	✓	✓
Number of Observations	5317	1215	1215	1215

Note: Samples include employed workers in cohorts born 1943-64. Standard errors are adjusted for clusters at the track * year of birth * state level.