The Impact of Length of the School Year on Student Performance and Earnings: Evidence from the German Short School Years*

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Abstract

This paper investigates how changing the length of the school year, leaving the basic curriculum unchanged, affects learning and subsequent earnings. I use variation introduced by the West-German short school years in 1966-67, which exposed some students to a total of about two thirds of a year less of schooling while enrolled. I find that the short school years increased grade repetition in primary school, and led to fewer students attending higher secondary school tracks. On the other hand, the short school years had no adverse effect on earnings and employment later in life.

JEL Classification I21, J24, J31

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

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1 Introduction

Primary and secondary school students in the United States attend school on average for 180 days, and in the UK for 190 days, compared to an OECD average of 195 days and 208 days in East Asian countries.[1] Because of its concerns about the performance of American students, extending the length of the school year was a major policy recommendation of a 1983 presidential commission in its report "A Nation at Risk." The role of time as an educational input became an even bigger focus of a second commission a decade later, in a report entitled "Prisoners of Time." Despite the important role of time in school in the policy debate there is little evidence to what degree the length of the school year 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 curriculum, the highest grade completed, or the 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 and employment.

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 as in other parts of Europe, 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 lose

[1] See NCES (2000) and Lee and Barro (2001).

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. In order to assess academic achievement, I analyze grade repetition among primary school students and show that the short school years did indeed have the effect that more students were held back. The short school years also had a negative effect on the proportion of students entering higher secondary school tracks. On the other hand, I fail to find negative effects on earnings and employment later in life. I also provide some suggestive evidence on political participation, voting, and involvement with music and the arts.

These results may seem surprising in light of the evidence showing that returns to schooling are quite substantial.[2] 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 United States these estimates are clustered around 10 percent but estimates for European countries are also typically high; see Card (1999) for a survey of this literature. Existing IV studies typically rely on variation in schooling for a particular subgroup of individuals, e.g. those subject to compulsory schooling laws. One advantage of this paper is that it analyzes a change in the amount of schooling received by the entire population of students at a

[2] 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. However, Pischke and von Wachter (2005) report that the returns to an additional year of compulsory schooling among lower ability students in Germany are also nil.

certain point in time. Hence, it identifies the average treatment effect in the population.

The estimates of returns to schooling in the previous literature may not be the correct comparison 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 was 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.[3]

However, the results are also 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. I present some evidence consistent with the idea that most students made up any deficiencies in basic skills resulting from the short school years while still in school. Universities and post-secondary vocational schools might also have compensated for material that had been missed in school. Individuals exposed to the short school years graduated earlier, spent more time in the labor market, and hence accumulated more labor market experience. The increased incidence of grade repetition might indicate that particularly slower students were not as able to cope with the in-

[3]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.

creased pace during the short school years. Grade repetition might have been a mechanism that insured that some marginal students eventually learned the same amount.

There are a number of previous results on the effects of term length on student achievement and earnings. Various studies on school quality in the United States include term length at the school level as one of the regressors (for example, Grogger, 1997, Eide and Showalter, 1998). These studies typically find insignificant effects of term length on achievement and earnings. One problem with the school level studies is that term length may proxy for other school attributes, which are unobserved in these equations. But the most important shortcoming is probably that there simply is not enough variation in the length of the school year across schools.

Rizzuto and Wachtel (1980), Card and Krueger (1992), and Betts and Johnson (1998) examined the effect of state level policies, often for earlier periods where there was more variation in term length. The effect of unobserved heterogeneity may also be less of an issue with state level data. 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. Some of the findings by Card and Krueger have been challenged by Heckman, Layne-Farrar, and Todd (1996). But these latter authors also find a zero effect of term length in their re-estimations.

Lee and Barro (2001) correlate student performance across countries with a variety of measures for school resources, among them the amount of time spent in school during the year. They find no effects of the length of the school year on internationally comparable test scores.[4] A more recent study by Wößmann (2003), which also analyzes cross country test score data, corroborates this finding. He finds a significant effect of instructional time, but the size of the effect is negligible. A 10 percent reduction in the time of

[4]The results differ somewhat by subject of the test: longer time in school increased mathematics and science scores, but lowered reading scores.

instruction (a larger change than that implied by the German short school years) leads to drop in test scores of 0.015 standard deviations. Lee and Barro (2001) also look at grade repetition as an outcome, and they find a significant effect of more instructional time. These results therefore basically 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. I am aware of three previous German studies of the impact of the short school years on student achievement by Meister (1972), Schlevoigt, Hebbel and Richtberg (1968) and Thiel (1973), which I will discuss in some detail below.

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, and assesses the external validity of the exercise. Section 3 describes the data sources used to obtain the empirical results in Section 4 on student achievement, earnings, employment, and civic outcomes. I draw conclusions in Section 5.

2 Institutions and Empirical Framework

2.1 Background on the German School System and Identification

Education has been in the political domain of the federal states in post-war West-Germany. After the Second World War, all states except Bavaria started the school year 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 the end of the summer, so that the new school year would commence after the summer vacation.[5] 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 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.[6] 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, every-

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

[6] 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.

body 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). Table 1 summarizes the transitions to the new start of the school year in the various states.

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 during 1966-67. This difference across cohorts is the first dimension.

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 or basic track (Volksschule, later called Hauptschule) ended with the end of compulsory schooling after 8 or 9 grades.[7] The intermediate track (Realschule), ends after grade 10, and the most academic 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 would have graduated by spring 1966 if she had gone to the basic or intermediate track but would have been in school during both short school years if she had gone to the academic track (see Table 2). This interaction of cohort and track helps identify the effects of the short school year.

The third dimension is the contrasts across states. This makes use of the

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

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 the basic and intermediate track in the subsequent years. Every basic track 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 track students attended school for 9 years, even those who were in school during the short school years.

Things were slightly more complicated for intermediate track students. The 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 the academic track were fully affected by the short school years. The length of their schooling was not extended for any cohorts. Hence, Niedersachsen is neither simply a treatment nor a control state, since the variation introduced by the rules in this state imply an interaction of track and cohort effects. In the main analysis, I will use the full interactions of cohort, track, and state effects to identify the effect of the short school years, while controlling for main effects of each of these. I will also check these results for states outside Niedersachsen using only cohort and state differences in the participation in the short school years.

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 state of Schleswig-Holstein decided 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. However, some requirements were also reduced for the students exposed to the short school years.[8] In Baden-Württemberg, on the other hand, the curricula for the short school years were shortened, but there was no change in the requirements for the subsequent school years. However, Thiel (1973), after reading of the directives of the school bureaucracy, claims to find "no specific reductions" in the material to be taught in the core subjects like German, English and math. Additional hours of instruction were added to a minor degree.

Despite these adjustments, 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 repetition and attendance of the higher tracks 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. Unfortunately, there are no uniform standardized tests available in Germany. However, I will present the results of three studies undertaken at the time, which tested

[8] For example, the state of Schleswig-Holstein usually required the reading of three authors for the Great Latin Exam (Grosses Latinum, usually taken after grade 13), but reduced the number to two during the 1966 short school year.

students in school during the short school years. I also 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 largely 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 and grades play less of a role than in later years. 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 academic track, with grade 5, while the intermediate track started in many states only with grade 7.[9] At the end of grade 4, the primary school makes a recommendation based on grades, possibly specific exams, and teacher assessment, whether a student should attend one of the higher tracks. Independent of this recommendation, parents can typically choose to have their child apply to a school in one of these tracks. In case of a negative primary school recommendation, the student may have to take an admissions exam, which determines whether the school will admit the student. Whether a student enrolls in one of the higher tracks therefore depends both on parental choice and on the academic performance of the student. Since low achieving students are unlikely to enter one of the higher tracks, 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

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

short school year, 13,579 students switched into the academic track from the basic or intermediate one, compared to 174,828 students entering the first grade of the academic track 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 a variable 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

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

where y_i is an outcome, like the log of earnings or wages, γ_s is a set of state effects, δ_j 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. Other regressors, like the total number of years of education and training, are not included in this regression. Variables like this would be potentially affected by the short school years, and therefore should not be included in the regression.[10]

The regressor of interest, D_i, is an interaction of state, year of birth, and secondary school track effects. 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

[10] See Angrist and Krueger (1999).

different secondary school track in response to a state's decision to introduce the short school years. Parents moving 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). I find below that the short school years had some impact on the choice of secondary track. Therefore, I also estimate specifications which do not rely on track for the identification, and which do not include track as a regressor.

In addition to accounting for the track attended in the wage regressions, it is necessary to deal with the fact that the basic track was extended from 8 to 9 years in many states during the 1960s as well. In many of the states in the south and west the introduction of the 9th grade coincided with the short school years.[11] Instead of using three dummies for the three tracks, I use four dummies, dividing basic track students into separate groups depending on

[11] 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. See Pischke and von Wachter (2005) for more details on the introduction of the 9th grade in basic track.

whether they graduated after eight or nine years.

The other controls in equation (1), for age, year, and gender, are only included to help increase the precision of the estimates. Notice that the regressions only control for age, and not labor market experience. The students affected by the short school years will have more potential labor market experience. The estimates I present below are a combination of the education and experience effects induced by the short school years. I have made no attempt to separate the two effects. In order to do so, it would be necessary to have an independent estimate of the effect of experience. Because of the collinearity of time, age, and cohort, I do not believe that it is possible to identify the linear portion of the experience effect convincingly. However, the individuals in the samples I use are on average between 32 and 41 years old. Hence, most of the individuals will be in the relatively flat part of their experience profile already, so that the effect due to experience is likely going to be small.

The validity of the identification 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 present 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. Nevertheless, identification could be undermined if there were other changes, which affected some cohorts in some states. While education policy certainly was rather fluid during the 1960s, the design here is likely to be more robust than typical difference-in-difference investigations of policy changes. The reason is that the short school years came into effect, and then ended, so that there are control cohorts both before and after the intervention. Other policy changes during the period tended to be permanent, and hence largely orthogonal to the short school year regressor. One non-linear trend, which differed across states, is demographics. Nevertheless, I do not find any evidence that this affects the results.[12]

In order to probe the issue whether the short school year affected track choice, I estimate a version of equation (1) where y_i is either a dummy variable for graduating from the academic or the intermediate track, while D_i is defined as participating in the the short school years while in primary school. Track is not used in the construction of D_i in this case, so track dummies (and age dummies) are omitted from this regression.

I use aggregate data at the level of state, year and grade for grade repetition in grades 1 - 4. I estimate regressions of the form

$$y_{stg} = \alpha + \beta D_{stg} + \gamma_s + \phi_t + \rho_g + \varepsilon_{stg} \tag{2}$$

where y_{stg} is the fraction of students repeating a grade in state s, year t, and grade g, γ_s is a set of state effects, ϕ_t is a set of time effects, and ρ_g is a set of grade effects. I also run specifications with interactions of state and grade effects $\gamma_s * \rho_g$.

2.3 External Validity

The various possible dimensions of contrasts across states, cohorts, and tracks, as well as the possibility to construct control groups from before and after the treatment leads to a quasi-experimental design which should result in rather good internal validity of the estimates. I have argued that the possible challenges, like mobility of parents and track choice, are unlikely to be a big problem. I will argue below that these and other shortcomings of the data, which result in some measurement error, are also unlikely to invalidate the estimates. A bigger question is whether the estimates are very informative beyond the particular experience of Germany in 1966-67, and hence the external validity of the estimates.

As with many interesting policy experiments, there is the danger that the policy engendered a response specific to the episode. Schools and teachers

[12] For example, I have tried specifications adding the log of population in the relevant age groups in a state as a regressor. This yielded very similar results to those reported below.

may have mobilized additional resources in order to cope with the added pressure of the short school years on the students. Teachers may have increased their effort. Parents may have filled gaps left by the schools. Such responses could be due to the temporary nature of the policy, and may not be forthcoming in response to a more permanent change of instructional time. If this is the case, the German short school years may not be very informative on the broader question of the impact of the length of the school year.

At this point, it is rather difficult to assemble hard evidence on exactly what happened in schools more than 35 years ago. However, I will present a few pieces of evidence on these issues. The two German studies by Meister (1972) and Thiel (1973) both carried out surveys of a small number of teachers during the short school years, asking them about the adjustments that took place and some of the consequences.

Some state education authorities added some class room hours for affected students in certain subjects, and teachers and principals may have shifted additional hours between subjects themselves. Thiel (1973) asked teachers in 2nd, 4th, and 8th grade directly whether they gave additional hours of instruction in writing and math. Out of 21 teachers, only 19 percent report a regular additional hour for math and 33 percent for writing. 14 percent actually report a regular hour less in writing. Slightly more than half report an additional hour in each subject occasionally.[13]

Since primary school classes are typically taught by a single teacher, there is also the possibility that reading, writing and math were stressed more to the detriment of other subjects, without additional hours. According to the survey by Meister (1972), 11 out of 13 primary school teachers report shifting emphasis to reading, writing and math, particularly reading and writing. In addition, 3 of the teachers mentioned cuts in music instruction. Thiel (1973) reports that 72 percent of teachers gave additional homework in math, and 62 percent in writing. 60 percent mention that they perceived parents as working more intensively with their children. On the other hand, only one

[13] The numbers reported in Table 3 on p. 23 of Thiel (1973) do not match exactly his reporting of the results in the text. I report the results given in the table.

out of 13 respondents in Meister's (1972) survey mentioned more parental involvement (although this answer comes from a free form question).

In addition to added instruction, teachers may have increased their effort. The most direct piece of evidence on this is data on teacher absences assembled by Thiel (1972). He surveyed 120 schools in Baden-Württemberg, and received responses from between 77 and 86 of them for the years 1964/65 to 1969/70. The results are displayed in Figure 1, and are measured as the average number of school days missed by teachers during a school year. The numbers for the short school years have been scaled up by the relative reduction in school days during those years to make the numbers comparable across time. The short school years are marked by squares on the figure. Teachers are on average absent for about 8 days a year. During the first short school year, this dropped to just below 6 days (and the change is significant). During the second short school year the number of absences increased to about 8.8 days, i.e. slightly above the level before the beginning of the short school years. Absences increased still a bit further in the first year after the short school years before falling back to their normal level.

This indicates that teachers may have put in additional effort particularly during the first short school year, and may have come to school even with minor illnesses that would have normally kept them at home. This additional effort was not sustainable during the second short school year. The slightly higher level of absences even after the short school years may indicate that teachers may have succumbed to additional illnesses because of the stress caused by the episode. This would suggest that even though the short school years were temporary, they lasted long enough (16 months) so that it was not possible to sustain special effort throughout this period. However, there are other potential explanations at least for the short school year pattern of absences. The first short school year ran from April to November, and hence did not include much of the typical flu season, while the second short school year from December to July included the bulk of the flu season. In any case, the data do not suggest that teachers consistently exerted higher effort.

While the evidence is less than clear cut, it certainly suggests *some* adjustments to the short school years. The role of additional instructional time during the short school years seems to be minor, although additional hours were used in some cases. There also seems to have been a concentration of resources on the core academic subjects, to the detriment of other fields, with music being frequently mentioned. The effort of students (through additional homework) seems to have been somewhat higher during the short school years. There is little evidence that teachers consistently put in extra effort during this period, and it is unclear to what degree parents did. But it also has to be kept in mind that the school system already was under strain during this period because of the large baby boom cohorts being educated, and because of the general expansion of the education system. It also seems that the adjustments that did happen were relatively minor compared to the reduction of instructional time. Hence, it is unlikely that these adjustments were able to undo all or most of the effects of the short school years on students, and this seems to be borne out by some of the evidence presented below.

3 The Data

In order to study the impact of the short school years on student performance, I analyze aggregate data on grade retention. 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 the serial *Fachserie A. Bevölkerung und Kultur*, Reihe 10, I, Allgemeines Bildungswesen. Thus, I have the population data on grade retention available. I use data for the school years 1961-62 to 1972-73. No grade repetition data exist for the school years 1962-63 to 1664-65. I also omit the first short school year in 1966, so that all treated grades in the sample have been exposed to two short school years. This restriction is necessary to balance the data between the treatment and control states.

Earnings data are taken from four micro data sets, each with its own strengths and weaknesses. The main features of the four data sets are summarized in Table 3. 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 in the age group 15 to 65. I use the four waves for 1979, 1985-86, 1991-92, and 1998-99 each of which samples about 25,000 workers. The samples are restricted to respondents of German nationality, and, in the 1991-92 and 1998-99 waves, to those reporting that they grew up in West Germany. The data set has detailed information on schooling and training, which is one of the advantages for this project.

The earnings variable in the surveys is gross monthly earnings. Respondents in the 1979 survey were asked to report their earnings in 13 brackets, in the 1985-86 survey in 22 brackets, in 1991-92 in 15 brackets, and 1998-99 in 18 brackets. I assign each individual earnings equal to the bracket midpoint.[14] 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 of constructing 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. The second is to use the year of birth and year of graduation.

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

[14]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 16,500, and in 1991-92 it was DM 8,000 or more which I assigned a value of 10,500, and in 1998-99 it was DM 15,000 or more which I assigned a value of DM 17,500. These values were chosen based on means for these categories in the ALLBUS, the only data set were earnings are not top coded. Only 1.0 percent of sample observations are in the top income bracket.

school years an individual should have been exposed to in a state with the short school years. Table 2 displays how this assignment is done in the first measure based on tracks for the birth cohorts from 1946 to 1960. There are a few caveats. First, it is necessary to know the month of birth to determine when exactly a student is supposed to enter school, and some students enter school early or late. I do not have any information on either of 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. This averaging will not affect the consistency of the estimates, only their precision.

The second short school year measure is calculated from the year of birth, similarly imputing the year of school entry, and the 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 year, and are assigned half a short school year as well. Both measures of the short school year are scaled so that they measure the amount of instructional time missed in years, and regression coefficients in the earnings regressions are directly comparable to estimates of the returns to schooling.

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 of the year of birth, 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 track degrees and the intermediate track 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. There is no direct information on the amount of time individuals actually spent in school in the data.[15]

The second data set is the German annual labor force survey, called the Micro Census. It is a repeated cross-section, and I use German respondents in the years 1989, 1991, 1993, and all years from 1995 to 2001. Each wave has about 300,000 to 400,000 observations for the west German states. In addition to the large sample sizes, the Micro Census samples both employed individuals and those not working. This allows me to look at employment in addition to earnings.

There is no direct question on earnings in this data set. However, the survey asks respondents for the net monthly income. For the analysis of earnings, I restrict the sample to those who are employed and who report that earnings are their main source of income. The income variable should

[15]In a previous version of this paper, I constructed such a measure from year of birth and graduation year, in order to investigate whether the short school years effectively reduced schooling. However, this measure is highly imperfect, and the measurement error is systematically related to participation in the short school years. This is because the short school years reduced time in school by a fraction of a year, but I can only measure full years of schooling. In addition, some states also changed the rules on school entry at the time of the short school years but this cannot be accounted for without information on month of birth. Hence, these results are not very informative.

approximate earnings very closely for this subgroup. Earnings are also reported in brackets. There were 18 brackets from 1989 to 1999, and 24 brackets in 2000 and 2001, and I assigned midpoints to the brackets again.[16] The monthly income variable is then converted to an hourly wage by dividing by usual weekly hours. The Micro Census only records year of birth, state of residence, and the highest secondary school degree obtained. This only allows me to create the first definition of the short school year indicator, as described above and in Table 2.

The third data set I use is the German General Social Survey (ALLBUS) from 1980 to 2000. This is also a repeated cross section survey. It samples about 3,000 respondents of German nationality who are 18 years or older in each wave. 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 waves after 1990.

The income variable in the survey is again net monthly 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.[17] 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 but is missing for many observations. Because the sample is relatively small to begin with, I use the monthly income directly in the regressions.

The ALLBUS provides year of birth and the highest secondary school de-

[16] The top bracket in 1989 was DM 5,000 or more which I assigned a value of DM 7,500; in 1991-1999 it was DM 7,500 or more which I assigned a value of DM 10,500; in 2000 and 2001 it was DM 35,000 or more which I assigned DM 40,000. Except for 2001, these values were chosen based on means for these categories in the ALLBUS. There are no individuals with earnings above DM 35,000 in the ALLBUS, so I have to make an assumption for the value in this category.

[17] 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.

gree attained,[18] 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 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 and Micro Census data. In addition, the 1991, 1992, 1994, and 2000 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 4 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 (row 1) or the variable asking about the time in the current state (row 2). There is relatively little mobility between birth and age 18, as can be seen in rows 2 to 5. 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 is the results in the last row. 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 Bavaria and 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 and out of these states. If migration is unrelated to the effects of the short school years this measurement error will lead to pure attenuation.

[18] 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.

The impact of this measurement error in a regression framework can also be easily quantified with the ALLBUS data. Assume that state of birth corresponds to the state of schooling at the time of the short school years. This ignores that some students moved between birth and the time they went to school, overstating mobility somewhat. Call the measure of exposure to the short school year constructed based on state of birth D_i^*, and that based on state of residence D_i. If the measure based on year of birth was correct, then the coefficient from a regression of D_i^* on D_i would measure the attenuation from using D_i as a regressor instead of the true measure. Including the other covariates in equation (1), this attenuation factor is 0.84 with a standard error of 0.02, so that the estimates should be inflated by $1.19 = 1/0.84$. This is going to be relatively negligible.

The fourth data set comes from social security records. It is based on the IAB Employee Sample (IAB Beschäftigtenstichprobe), a 1 percent sample of social security records. The sample includes only records on employed individuals, and excludes civil servants, self-employed, and those in marginal employment because these groups are not covered by the general social security system. This includes about 80% of all workers. The data set is a panel. Once sampled, an individual is followed as long as a social security record appears for that individual. The data set is described in more detail in Bender and Hilzdegen (1995) and Bender, Haas, and Klose (2000).

I obtained cell level means, medians, and standard deviations of earnings, as well as characteristics of the individuals spanning the period 1975 to 1995. The sample is restricted to Germans living in the west German states. The cells are based on year, age, state, and level of schooling. The regional indicator is the state of the workplace. Every individual was assigned the state where they worked in 1975 or when they first entered the data set. The education indicator only distinguishes academic track students from students completing one of the lower tracks, but it does not distinguish basic and middle track students. Hence, the short school year indicator is assigned based on average participation in these tracks.

The earnings measure provided is gross pay subject to social security contributions, and it is truncated at the social security maximum. For each cell, I know how many observations are at the maximum, and I only use cells where the fraction at the maximum is 50 percent or less. I also discard 106 cells based on a single observation. The sample used in the analysis has 8,605 cells, based on 2 to 1,447 observations. The mean number of observations in the cells is 206, the median is 82, and there are in total more than 1.7 million micro records underlying the cell statistics.

The advantage of the social security data is its large sample size. However, this is mitigated by the fact that it is a panel with repeated observations on the same individuals. Another drawback is the coarse information on education. As a result, the QaC and the Micro Census are the preferred data set for this analysis, and I will present the most detailed results from these data sources.

4 Results

4.1 The Impact on School Performance

The most direct method of assessing school performance is to compare the results of standardized tests. There is no standardized testing system in Germany which allows such a comparison. However, I will discuss the results of three studies undertaken at the time of the short school years. The authors of two of them tested the affected students themselves, while the third study relied on tests routinely given as part of the secondary track selection procedure. I will also present some indirect results based on grade repetition and transition into the higher secondary school tracks.

The first two studies are dissertations, and both authors administered tests themselves. One tested students in the Saarland (Meister, 1972), the second students in Baden-Württemberg (Thiel, 1973). Both exploit the quasi-experimental design of the short school years. Both in terms of the experimental design and the statistical analysis, these studies are very competently executed. Nevertheless, both have a number of limitations as well.

One problem is that both studies relied on existing tests, which may not be exactly appropriate for the stage at which the students were tested. The samples were not overly large: 435 and 449 children in 13 classrooms for the Saarland, and between 146 and 365 students in 5 to 10 classrooms in Baden-Württemberg.

The third study was conducted in the city of Frankfurt in the state of Hessen (Schlevoigt, Hebbel and Richtberg, 1968) and relied on tests routinely given to 4th graders there. The tests were specific to the grade level, and similar in format to regular tests during the school year. The samples were much larger, covering between 1,148 and 3,124 students (with the exception of one subtest, where only 291 students were tested after the short school years). The treatment groups were tested in 1968, i.e. one year after the end of the short school years, so that they were in 2nd and 3rd grade during the short school years. The control groups were tested in 1963 or 1965. The quality of this investigation is more difficult to assess, since the only publication is a terse, two page article in a journal for teachers. The results of the studies are summarized in Table 5. Results were the students in the treatment group performed better are shaded in grey.

The study by Meister (1972) for the Saarland focused on teaching methods in the early primary grades, and hence he only tested 2nd graders. The tests for the treatment group were performed after the end of the second short school year, i.e. after the tested students had been exposed to two short school years. The control group consisted of students in the same schools who started school in summer 1967, i.e. after the short school years were over. They were tested after a period that was equivalent to the short school years, i.e. during the middle of their second school year. The author chose this timing because he was interested in a control group of the same age as the treatment group. The design therefore is trying to establish whether learning was faster during the short school years but not whether the same amount of material could be learned by the end of the school year. The results in the first panel of Table 5 show that the treated students consistently performed much better than the control group.

One problem with the design is that the tests used may have been most appropriate for the end of the second school year. The students exposed to the short school year should therefore have covered all the required material, while some of the tested material might not have been as easy for the control group.[19] Another problem is the possibility of knock-on effects of the short school years: if teachers expanded more effort during the short school years, they could have been doing a worse job in the subsequent years, so that teaching quality for the control group was worse.

The second study by Thiel (1973) for Baden-Württemberg addresses more directly the question I am interested in here, namely whether the affected students learned the same amount as students in regular school years. He tested 2nd, 4th and 8th graders. The treatment group was tested twice, first at the end of the second short-school year, and the second time at the end of a period equivalent to two regular school years. The treated students will generally have been in the following grade during this retest, and they were given parallel forms of the same test at the two testing dates. The control groups used in this study are generally samples of tested students used to norm the tests, i.e. these tests will have been performed prior to the short school years in all the states of West Germany. The problem with this comparison is that the curriculum may be different in other states, and the test may therefore be more appropriate for the treatment or the control group. Some of the control tests also date back various years, and standards in schools may have changed over time. In some cases, the control group results were collected by the author or his colleagues in Bavaria (with no short school years) or in Baden-Württemberg after the short school years (as in the case of the Saarland study).

The second panel in Table 5 shows that the results for the second graders generally confirm the findings of the Saarland study: treated students performed better after spending two full calendar years in school (the regular-

[19] Thiel (1973), who uses the same test describes it as designed to test knowledge at the end of the 2nd grade. On the other hand, he goes on to say that the test is too easy for the 2nd graders in Baden-Württemberg at that stage.

regular comparison). Nevertheless, they did not reach the control group standards at the end of the short school years (the regular-short comparison), although the writing difference is not significant. Thiel (1973) discounts the results for the writing test somewhat because he believes that it is too easy for the 2nd graders in Baden-Württemberg.

The results for 4th graders (third panel in Table 5) generally shows the treated students at par with the control group at the end of the short school year (the regular-short comparison) and at par or outperforming the control group after a similar time in school (the regular-regular comparison). The exceptions are the reading, vocabulary, and mental arithmetic subtests. Thiel (1973) attributes the reading and vocabulary results to the fact that learning in these skill categories may be more influenced by maturation and hence age (because a lot of reading takes place outside school) rather than training. Since the treated students are about eight months younger at the end of the short school year than the control group students, this difference may explain the results. The test for mental arithmetic contained a number of questions on fractions, a subject not covered in Baden-Württemberg until grade 5. This would explain the lower performance of the treatment group compared to the national control after the short school years, and the catching up when the same students were retested during the 5th grade.

Thiel (1973) also tested one group of 4th graders in 1969, i.e. two years after the short school years. This group would have been affected by the short school years during the first two grades. Results for this group are displayed in the last column. Except in reading and vocabulary the results for this group are at par with the control group. Even though he found deficiencies for the 2nd graders in writing and math right after the short school years, the children seemed to have made up these deficiencies within the next two years. The students are again weaker than the control group in reading and vocabulary, which may also be attributable to their lower age. On the other hand, the results for this group do not bear out Thiel's interpretation for the mental arithmetic test. The 4th graders who were affected by the short school years earlier were able to perform as well as the norm despite the fact that they would not have been instructed in calculations involving fractions.

Schlevoigt et al. (1968) do not report means of the scores on the tests or t-statistics, but only tabulate the distribution. I have therefore calculated the fraction of students scoring at some level around the median on each test. I have also constructed t-statistics based on the counts given in the paper. These will be somewhat inexact because of rounding in the publication. They will also overstate the true significance levels, because they do not take into account the sampling at the class room and school level.

The results in the Schlevoigt et al. (1968) study fall somewhere between the regular-short comparison and the two years later comparisons in Thiel (1973), since the treated students in Frankfurt experienced the short school years in grades 2 and 3. The results differ markedly from those obtained by Thiel. In all tests, the treated students performed worse, although the difference is only large and clearly significant in writing. Curiously, writing is the subject where the students tested by Thiel actually did slightly better after the short school years.

Finally, students affected by the short school year in 8th grade generally performed as well as the control group in the Thiel (1973) study. However, the testing instrument may have been weak for that group. The writing test only tested spelling but not punctuation, and hence focused on skills generally acquired earlier. Similarly, the math test contained numerous questions on material of the earlier grades.

In summary, these studies show that the affected students may have had some deficiencies at the end of the short school years in the core subjects of reading, writing, and math, although these subjects presumably received the most attention at the time.[20] On the other hand, the students were always on par and typically ahead of their peers when tested at the same age. The

[20] Thiel (1973) also presents results from a small survey of teachers, informally assessing the knowledge of the students at the end of the short school years. He distinguishes the teachers who taught the students during the short school years, and those who taught them in the subsequent year. In both cases, 62 percent of the respondents thought that the affected students learned the required material fully, while 38 percent saw deficiencies. The teachers saw the most problems outside the core subjects.

results also reveal that any immediate effects on learning seem to have been eradicated when students were tested two years later. This indicates that the eventual effects of the short school years on learning of the affected cohorts should be small at best.

In order to probe these findings, I present some results on grade repetition and on the fraction of students going on to one of the higher secondary school tracks. The raw data on grade retention of affected and unaffected grades in primary school are presented in Table 6. 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. Berlin and Hamburg are control states, because schools adapted the curriculum to the long school year, since students would eventually graduate after the normal length of total schooling. Recall that no grade repetition data exist for the school years 1962-63 to 1664-65. Retention rates are presented for the school year 1961-62, 1965-66, the last year before the transition, the 2nd short school year (1966-67) and the following six regular school years. During the first years after 1967, 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 repetition rates did not fluctuate much over the period in either the states with the short school year or Bavaria, while there is a decline between 1962 and 1966 in the long school year states. Things look different for 2nd to 4th grades, and the contrasts between the short school year states and Bavaria are also displayed in Figures 2 to 4. Affected grades are marked by boxes in these figures and the school years with missing data are indicated by short dashes. 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 during this period. Similar effects are visible for grades 3 and 4.

However, the figures also reveal some additional interesting features. Grade repetition increased somewhat gradually for the affected cohorts, reaching a peak of about 1.5 percentage point three years after a cohort was exposed to the short school years. This indicates that poorly performing students may not have repeated a grade immediately at the end of the short school years. Instead, some students seem to have been promoted initially, only to fail in a subsequent grade. This may be indicative of the possibility that the pace of instruction was also higher in subsequent years. Alternatively, students might have hung on initially but were still behind in the following grades, and failed eventually.

A second feature is that higher rates of grade repetition are also visible for the first cohort after the short school years. This indicates that there may have been knock-on effects of the short school years. This could have been the case because teachers were under more stress during the short school years, and teaching in the subsequent year suffered.

Table 7 presents regression results for the effects of the short school years on grade repetition. Controlling for grade, year, and state effects, I find sizeable effects of the short school year on grade repetition. Repetition rates have increased by about 0.9 to 1.1 percentage 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. Repetition rates are higher in some states in certain grades but 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.

These results are not changed very much in other robustness checks, which are not reported. Introducing a dummy variable for the first cohort after the short school years in affected states (to allow for knock-on effects) raised the estimates slightly. The estimate of the knock-on effect is relatively small, and only significant in the regressions for grades 2-4. Introducing a full set of second level interactions for grade, state, and school year also raised the coefficients slightly.

The results on grade repetition, together with the earlier studies on achievement, suggest a clear impact of the short school years, particularly on weaker students.[21] Grade repetition in primary school increased by about 25 percent, and 2nd and 4th graders generally scored lower on tests right after the two short school years. This suggests that the short school years did indeed involve a faster pace of instruction. Any compensatory mechanisms, like additional hours, shifting instruction time to core subjects, and higher effort on the part of teachers, parents, and students, as far as they existed, did not make up for the time lost due to the short school years. In particular, one might have thought that increases in teacher effort could have been concentrated on weaker students, -hence avoiding additional grade repetition. This suggests that the short school years did affect learning, despite the temporary nature of the experience. However, these effects were likely short lived. The large impact on grade repetition also suggests that there was no shading of standards.

It is also interesting to look at the impact of the short school years on total completed education. However, the German education system involves many different educational tracks, and various post-secondary training programs. Nevertheless, the main distinction in completed education for most Germans turns out to be between attendance of one of the lower secondary tracks plus an apprenticeship versus attendance of the academic secondary track plus university. As a result, secondary track choice turns out to be the key predictor of eventual educational success. In order to investigate this issue, I analyze secondary track choices in Table 8. In addition, I also present some results on total completed education.

The first two columns in Table 8 present results for secondary track choice using data from both the Qualification and Career Survey and from the Micro Census. The sample includes the cohorts born in 1952 to 1964. These are

[21] This actually contrasts with the findings of Meister (1972), who looks at percentile comparisons across the distribution of test results. He does not find any evidence that weaker students performed worse during the short school years. The same is true in the results presented in Schlevoigt et al. (1968).

the cohorts who experienced the short school years during grades 1 to 4, plus four adjacent cohorts before and after. Berlin and Bremen are excluded from the sample because entry into the higher tracks was only after grade 6. The regressions are linear probability models with a dummy variable for graduating from the academic or intermediate track as the dependent variable. The key regressor is whether the individual experienced the short school years during grades 1 - 4.

The results indicate insignificant effects of the short school years on academic track choice. The point estimates are in the order of one to two percentage points, and are of opposite signs in the two data sets. This seems to indicate that the short school years had no impact on academic track attendance. The point estimates are more consistent for the intermediate track. Children exposed to the short school years in primary school are about three percentage points less likely to attend the intermediate track. This estimate is significant in the Micro Census. Roughly 30 percent of students in the cohorts in question attended the intermediate track. Hence, this is a reduction of about 10 percent, which is sizeable.

One caveat with analyzing track choice as an academic outcome might be that the fractions of a cohort attending a higher track may be rationed (for example, by the capacity of the available schools). However, attendance of the higher tracks was growing sharply during this period anyway. One would presumably expect the short school years to make attendance of a higher track less likely, as is mostly borne out by the results. But basic track schools would have had the most open capacity during this period, to the degree that there was any. Hence, there was likely scope for a significant number of students to alter their track choice in response to the short school years.

A further dimension according to which education could have affected education is by resulting in different choices of post-school training or university attendance. In order to probe this, columns (3) and (4) in Table 8 present estimates for the total number of years of education. This variable is constructed by adding up the number of years typically necessary for the completion of an educational program. The construction does not take into account the actual length of a school year, i.e. the short school years are counted as one full year just as regular school years. Hence, there is no direct effect of the short school years on this variable. Any effect only manifests itself through the choice of different educational programs.

Column (3) presents a regression analogous to those in columns (1) and (2), i.e. this regression reflects the effect of track choice. The estimate for the QaC data is zero. Because the short school years had opposite effects on attendance of the academic and intermediate track in the QaC data, this is consistent with there being no effect beyond that on track choice. In order to probe this further, column (4) partials out secondary track choice. The effect of -0.06 of a year is small and insignificant. The results are slightly different for the Micro Census. Here the estimate in column (3) is negative and relatively large, about a quarter of a year. However, the effect on track choice was negative for both the academic and intermediate track in this data set, so that we would expect an effect on total schooling merely because of the different track choices. Indeed, controlling for track in column (4), the effect is again zero. This indicates that any effect of the short school years seems to have been through their effect on track choice. There is no evidence on any effect on post-school training or education.

The grade repetition and secondary track results therefore give a picture that complements the earlier discussion of the testing results. The grade repetition results indicate that predominantly 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 in the upper end of the ability distribution seem to have been less affected by the short school years, as evidenced by academic track choice results. There might have been some effects on students in the middle of the distribution, since fewer students attended the intermediate track when they were affected by the short school years. This also highlights that it will be important to probe the robustness of the later earnings results to conditioning on track.

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 9.7 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. This is not very large compared to the initial reduction of two thirds of a school year.

4.2 The Impact on Earnings

Table 9 presents regressions of log wages and earnings on the short school year indicators using the QaC and Micro Census data. The regressions control for the largest possible 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.[22] 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). Different sources of identification are explored below. The top panel in the table reports results for the QaC, the bottom panel for the Micro Census. Results are shown with both log hourly wages as the dependent variable, as well as using log monthly earnings.[23]

[22] Regressions, which include all the second-level interactions and therefore rely only on the full interaction of state, cohort, and track effects for identification, yield typically more positive, and sometimes large estimates with standard errors which are two to three times as large as those in table 9.

[23] The reported standard errors are adjusted for for clustering at the level of track * year of birth * state. This solves the Moulton (1986) problem. It does not adress potential serial correlation in the errors, say within states, as stressed by Bertand, Duflo, and Mullainathan (2004). The solutions they suggest do not neatly fit the design in this study, because the treatment is defined at the level of a state, cohort, and track. Serial correlation is most likely at the state and survey year level, however. The most conservative method

Recall that the coefficients on the short school year measures can be interpreted analogously to a return to a year of school. The results for the measure based on tracks in column (1) are basically zero for the QaC and slightly positive for the Micro Census. They are also relatively precisely estimated. The 95 percent confidence interval for the effect of reducing time in school by a year ranges from -0.03 to 0.02 in the QaC and from -0.005 to 0.040 in the Micro Census. Taking a return to schooling of 8 percent as the benchmark, the estimates in column (1) imply that negative effects of the short school years greater than 40 percent of the conventional return to schooling are outside the QaC confidence region. 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 are unlikely.[24]

The second measure of the short school years based on graduation year is only available in the QaC. Using this measure in column (2) yields similar results. The coefficients are also not very different when the second measure is used as an instrument for the first, as is shown in column (3). In particular, the coefficient is not more negative than the one in column (1). This indicates that measurement error (to the degree that the second measure is uncorrelated with these errors) is not a major issue in column (1). 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 close to zero in both data sets.[25]

would be to allow for arbitrary correlelation of the errors within states. Unfortunately, there are only eleven states. The block bootstrap and the unrestricted covariance matrix estimator implemented in Stata's cluster command did not perform well in the Bertrand et al. (2004) simulations for such a small number of states. When I cluster standard errors in Stata at the level of the state, the resulting standard errors are generally smaller or of similar size as those reported in table 9.

[24]In principle, one should assess the effect of the short school years on the present value of life-time earnings, rather than earnings in just one year. The short school years increased time in the labor market as well as grade repetition (which involves the loss of a year in the labor market). These effect will not be captured by the results. However, they are small because working lives are relatively long and the effects on grade repetition were modest in the aggregate.

[25]The results from the QaC are robust to excluding either Bavaria or Hamburg and Berlin from the control group. Hamburg and Berlin had somewhat different demographic

Table 10 probes the specification further by changing the exact set of treatment and control cohorts included in the sample. In particular, the different columns in this table include different subsets of students in the treatment according to when they were affected by the short school year. One possibility is that the short school years had the most effect on students towards the end of their schooling, when there was little time left to make up any shortfalls in learning. Column (1) only uses cohorts, which were affected by the short school years while they were in primary school, column (2) uses those affected in grades 1 to 9, and column (4) uses those affected in secondary school. 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 consistent pattern to the results, suggesting that any differences are likely due to sampling variation. In particular, the idea that students affected in later grades had less time to make up for lost instruction time would imply more negative coefficients in column (4) than in column (1). This is not systematically the case.

The identification in the specifications in columns (1) and (2) 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. Recall that I found above that exposure to the short school years in primary school had some effect on track choice. Hence, it is preferable not to condition on track choice. The results are slightly more positive, indicating that controlling for track does not bias the results upwards.[26] Notice, however, that the results in column (3) are not estimated very precisely since

trends for the age group 6 to 14 during this period. Controlling for the log of the number of 6 to 14 year olds in the state and cohort group in the regression also does not affect the results.

[26]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.

secondary school track is a potent covariate in explaining earnings.

Rather than just concentrating on the impact of the short school years on primary versus secondary cohorts, in principle it is also possible to assess how the impact of the short school years differs depending on the grade when a student was affected. The most detrimental effect of the short school years should only arise for students in the highest grades, when these students had little time to catch up with missed material before graduation. This can be investigated by repeating the regressions for the control cohorts 1943-46 and 1961-64 plus a single one of the affected cohorts. Figure 5 plots the coefficients of this exercise for the QaC together with a 95 percent confidence band. The grade by grade estimates are less precise, and the width of the confidence interval is about 10 percent and wider for low and high grades. Nevertheless, the plot again reveals no particular pattern of the coefficients by the grade level when students were affected.[27]

One result of the analysis of the impact of the short school years on student performance in school was that weaker students seemed to have been harmed. Hence, it is interesting to analyze the impacts of the short school years on individuals in the lower part of the earnings distribution. The differences-in-differences framework can be applied to quantiles of the outcome distribution just as well as to the mean (see, for example, Meyer, Viscusi, and Durbin, 1995). Table 11 presents quantile regression estimates for the median, as well as for the 25th and 10th percentiles.[28] The median estimates are fairly similar to the OLS estimates. In the QaC data there is no particular pattern to the estimates across the lower quantiles, while in the Micro Census the estimates are actually higher at the bottom end of the

[27] It is also possible that the effect of the short school years differed by secondary track. Interacting the short school year treatment with the track in secondary school also did not show any particular pattern of results.

[28] The standard errors for the quantile regressions are not adjusted for any clustering, and hence are likely too small. It is common practice in applied work to report bootstrap standard errors for quantile regressions. However, this is not feasible in our case for the Micro Census data. These regressions were run on the computers of ZUMA, Mannheim, who graciously let us use the data at their facilities. Bootstrapping is not feasible in this environment because one quantile regression takes about 2 hours to run.

earnings distribution. Hence, there is no evidence of the short school years actually having a negative impact even for the least able individuals. This is what one would expect if weaker students, who were affected by the short school years had to repeat a grade, and this allowed them to catch back up.

The results from the ALLBUS, shown in Table 12, indicate a slightly negative impact of the short school years. The point estimate in column (1) is -0.018, implying almost a 2 percent loss in earnings for each year less in school. 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 four waves from 1991, 1992, 1994, and 2000. The basic story changes little in this subsample. Since these 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.

Finally, I estimated the same model on the social security data. Recall that the social security earnings are truncated at the taxable maximum. In order to deal with the truncation and the grouped nature of the data, I only used median regression in this case. I follow Chamberlain's (1994) suggestion to estimate a regression on the cell medians which are not subject to truncation, using the cell sizes as weights. This estimator is similar to Powell's (1984) censored least absolute deviations estimator for the underlying micro data. It can be interpreted as a minimum distance estimator. The calculation of the standard errors, which account for the serial correlation introduced by the panel character of the data, is described in Appendix 1.

The estimate of the short school year effect is 0.019 with a standard error of 0.020. Like in the Micro Census, this estimate is slightly positive but it is also not significantly different from zero. The most useful way to look at all the estimates together, is to combine them into a single meta-estimate. The mean of the estimates, weighted by the inverse of their sampling variances, is 0.010.[29] Assuming that the samples are drawn from the same population, and that the estimates reflect the same parameter, the sampling variance of the meta-estimate, v_m, is given by

$$\frac{1}{v_m} = \left(\frac{1}{v_1} + \frac{1}{v_2} + \frac{1}{v_3} + \frac{1}{v_4} \right).$$

This yields a standard error of 0.008. Overall, the results from the three data sets do not indicate any negative effects of the short school years on earnings. The combined estimate is precise enough to rule out any sizeable negative effects: the 95 percent confidence interval ranges from -0.005 to 0.026.

Various checks on the specification indicated that this is not because the estimates are biased upwards. However, a variety of measurement errors in the data may yield some attenuation in the results. The resulting bias from multiple sources of measurement error is difficult to assess analytically. Therefore, I conducted a small Monte Carlo experiment, incorporating measurement error in year of birth and the secondary school track, random mobility between states, and grade repetition. I assumed amounts of mobility and grade repetition similar to those estimated in the data. Even with sizeable amounts of measurement error in year of birth and secondary track, the mean attenuation was not larger than 50 percent. Using sample sizes and error variances similar to the QaC data, and a true effect of the short school years of -8 percentage points, similar to the OLS return to schooling, the p-value for the QaC estimate for wages in column (1) of Table 9 (-0.006) is below the 0.1 percent level. If the true effect is half this size, the p-value is 4 percent, and it rises to 26 percent if the true effect is only -2 percentage points.[30] Notice that these results are only for one of the data sets used, and

[29] This uses the estimate for monthly earnings in table 9, column (1) from the QaC and the Micro Census. These estimates should be most comparable to the other data sets.

[30] See Appendix 2 for details on the design of the Monte Carlo experiment.

the one with the most negative results. Hence, it is safe to conclude that attenuation due to measurement error very unlikely explains the finding of a zero effect, if the true effect is negative and sizeable. The estimates provide fairly strong evidence that a moderate reduction of term length in Germany did not have adverse effects on earnings.

4.3 The Impact on Employment

One possible reason for the lack of any earnings effects of the short school years may be that wages in Germany are relatively rigid. Students who were affected by the short school years may indeed be less productive but the lower productivity may not show up in wages or earnings. In this case, firms should be less inclined to hire these less productive workers, and we should see negative effects of the short school years on employment instead. This hypothesis can be tested using the Micro Census data, which is a household sample. The QaC and social security data only sample the employed, and the ALLBUS is a relatively small sample for this exercise, and has no particular advantages over the Micro Census data. The Micro Census data are also well suited because they cover the 1990s, a period of relatively high unemployment in Germany. Hence, I only present results from the Micro Census in Table 13.

The results show a significant *positive* effect of the short school years on employment. The average employment rate in the sample is 79 percent, and students affected by the short school years are about 1.6 percentage points more likely to be employed. The estimate is again in terms of years missed due to the short school years, and it shows a sizeable effect. Part of the effect stems from the behavior of women. The effect for men in column (2) is also positive and sizeable at 1.3 percentage points but only significant at the 8 percent level. Comparing the results in columns (3), (4), and (6) shows that the effects tend to be larger for those who are affected during secondary school rather than during primary school, similar to the results for wages obtained with the Micro Census data. Column (5) shows that omitting the state of Niedersachsen and track dummies does not lead to lower effects.

One possible explanation for positive employment effects is that participants in the short school years entered the labor market at an earlier age. Hence, they may be less likely still to be in school or university. Although only about 13 percent of sample members in the Micro Census are age 30 or below, running the regressions on the subsample older than 30 yields much smaller estimates. These are shown in the bottom panel of the table. None of the estimates on this subsample is significant at the 5 percent level, and the effect for men is basically zero. It seems therefore unlikely that there are any employment effects of the short school years.

4.4 The Impact on Civic Outcomes

My findings so far indicate that the short school years had little detrimental impact on the learning of key labor market relevant skills, on later earnings, and employment. Nevertheless, these result may have come about because educators shifted resources away from subjects like music, arts, and physical education to the core academic subjects. In addition, schools may have had less time to spend on activities like civic education. Economists have recently become rather interested in these aspects of education. There have been a number of recent studies investigating the impact of schooling on health, crime, and voting behavior.[31]

Following some of this work, I use the ALLBUS data to look at these issues. The data set contains various questions on political participation and voting behavior. I use these to create a variety of measures of political disinterest, and I run regressions similar to the ones in Table 12. Table 14 displays the results. Every survey asked respondents which party they would vote for if there was a national election next Sunday. Because participation in general elections is typically high, and respondents may not want to admit to not voting, only 6.1 percent of the sample indicate that they would not

[31] See Currie and Moretti (2003) and Lleras-Muney (2005) on health, Lochner and Moretti (2004) on crime, and Dee (2004) and Milligan, Moretti, and Oreopoulos (2004) on voting.

vote. Being affected by the short school year does not alter that fraction.

Slightly more respondents, 9.8 percent, say that they did not actually vote in the last national election. However, this question was not asked in 1980 and 1982, 1994, and 2000, resulting in a slightly smaller sample. Those affected by the short school years are slightly more likely to respond that they did not vote but the difference is not significant. Another question asks respondents to assess their political interest on a five point scale (missing in 1988 and 2000). 23 percent of respondents show little or no political interest (the two lowest categories). Political interest is higher among those affected by the short school year by 2 percentage points, and the result is again not significant. Using these three different measures, my results are clustered around zero, and show no systematic impact of the short school years.

Apart from political participation, I would like to evaluate whether individuals affected by the short school yeas are more likely to sympathize with more radical parties. The voting questions are not very helpful in this regard, because the fraction of the vote going to extreme parties is tiny. However, in 1980, 1984, and 1994, the survey also asked respondents to assess how much they liked various parties. I classify the NPD and Republikaner as extreme parties on the right, and the DKP and PDS as extreme parties on the left. The answers are given on an eleven point scale, so that five points are associated with a positive attitude, five with a negative attitude and one with a neutral attitude. I consider any of the five positive answers for one of the extreme parties as favoring this party to some degree. The mean of this variable is 5.8 percent, and the impact of the short school years is very large with 4.4 percentage points in comparison. The coefficient is marginally significant with a p-value of 12 percent.

Many of the reports by teachers have singled out music as one of the subjects that was frequently subject to reduced hours or attention during the short school years. In order to test whether this might be the case, and whether it could have an effect on later behavior, I exploit the fact that Germany has an important culture of participation in clubs. All surveys except the 2000 one asked about club membership. I aggregate all the answers for participation in a choir, orchestra, or other music related club or group. 3.6 percent of respondents are members of such a club, and exposure to the short school years reduces membership by 1.8 percentage points, with a p-value of 15 percent.

These last two results indicate that the short school years may have had an effect on civic attitudes and participation in music or the arts. While the point estimates are large, the results in the small ALLBUS samples are only marginally significant at best, so that it is not possible to draw strong conclusions. Nevertheless, these results indicate that the short school years may have had some cost in terms of civic education and appreciation for the arts.

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, and, importantly, of the curriculum studied. I find some direct impacts on learning, as evidenced by increased grade repetition and lower track choice. This suggests strongly that students were affected by the shorter instructional time, a result which is also borne out by the exisiting literature in education, which tested students at the time of the reform. These results are inconsistent with the idea that compensatory mechanisms during the short school years completely offset the effect of shorter schooling. I do not find negative effects of shorter schooling on earnings and employment. This is also consistent with the literature on learning outcomes, which also did not show any consistent and permanent negative effects of the reduced instruction time. Taken together, the results suggest that the effects of the short school years were mostly short lived, students quickly caught up, and there were no long term effects on human capital accumulation. I have argued that these results are real, and cannot be easily explained by measurement problems.

What general lessons can be drawn from the German experience? In order to answer this question, it is important to understand why the short school years did not result in any long run educational and labor market effects. One obvious explanation would be that returns to education are simply zero in Germany. Although Pischke and von Wachter (2005) also find a zero return to compulsory schooling in Germany, this is extremely unlikely as a general conclusion given the evidence for high returns in many countries (Card, 1999). In addition, the literature suggests that there is a payoff to academic skills in the labor market (Murnane, Willett, and Levy, 1995, Freeman and Schettkat, 2001), and these skills are presumably developed in school. This evidence on skills also seems inconsistent with a second explanation, that the findings are purely the result of sheepskin effects.

Hence, the most likely explanation for the results is that the short school years did not lead to a reduction in human capital accumulation. This conclusion is supported by the evidence that the students exposed to the short school years made up any shortfalls in learning within a fairly short time frame, and most marginal students caught up by repeating a grade. The result is consistent with the existing literature which studies term length as supposed to the impact of additional grades (Card and Krueger, 1992; Lee and Barro, 2001; and Wößmann, 2003). The identification in this literature uses variation in term length across jurisdictions, which is very different from the present paper. This suggests that the result in this paper is not simply specific to the German context and the particular episode studied.

The contrast between the findings on term length and on the returns to additional years of schooling suggests that returns to time in school are not governed by a simple linear human capital model, where each hour or day of education has the same effect. Since an extra year of school involves new material that the students are supposed to learn, the difference is most likely due to the content of schooling, i.e. the curriculum. If this content is not altered, as in the case of a marginal variation in term length, eventual learning and human capital accumulation is not affected much. If new material is studied, this will have an effect on learning and earnings. To further investigate this claim, it would be useful for the literature on human capital to focus not just on time in school but explicitly examine the effects of the content of curricula.[32]

These conclusions are not encouraging for policy makers who wish to use a lengthening of the school year as a measure to boost the performance of their students. The enthusiasm of the authors of a "Nation at Risk" for longer school years may therefore have been misplaced. Interestingly, the 1994 study "Prisoners of Time," while putting time in school at the center of their agenda, moves away from simply adding instructional time to the use of more of the existing time for core academic activities, which may indeed be the correct conclusion.

There has been a discussion in west Germany after unification about 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 is the fact that the East German school system only required 12 years for the same degree. Apart from possible cost savings, this has also been 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. After some experimentation, the west German states have now started to implement such a reduction. The short school year experience and the existing literature suggest that it might be possible to eliminate the last year of Gymnasium without much adverse effects on the labor market performance of the students.

One caveat that has to be kept in mind is that there are some students

[32] The small existing literature on this by economists is generally favorable to this view. Machin and McNally (2004) find that the method of teaching reading matters for reading achievement in England. Wößmann (2003) finds positive effects across countries of central examinations and a centralized curriculum on test scores in TIMSS. A series of papers for the US examine the returns to specific high school courses, particularly maths. While Altonji (1995) finds only small returns to math and science courses, the results of similar studies by Levine and Zimmerman (1995) and Rose and Betts (2004) are more optimistic. However, none of these papers have a particularly credible identification strategy.

who were hurt by the short school years: those who ended up repeating a grade as a result of the reform, and this result is also mirrored by Lee and Barro (2001) in their cross country evidence. The most poorly performing students may not be able to keep up with an increased pace implied by a shorter school year. This indicates that the length of instructional time matters differently for different students. Of course, grade repetition seems a rather inefficient mechanism to overcome the problems of poorly performing students. Targeted remedial education involving additional instruction for poorly performing students seems to be a more adequate response.[33] Another cost of shorter instructional time may be a shift away from civic education, but more study of this issue is certainly necessary before any firm conclusions can be drawn.

[33] See Jacob and Lefgren (2004) and Lavy and Schlosser (2004) for more direct evidence on this issue.

6 Appendix

6.1 Estimation of the Standard Errors for the Social Security Data

The structure of the social security data is similar to the problem posed in Chamberlain (1994). I fit a weighted linear regression through the cell medians using the cell size as the weight, i.e. I estimate

$$\hat{\beta} = \arg\min(m - X\beta)'W(m - X\beta) \tag{3}$$

where m is the vector of cell medians, X is the matrix of regressors, and W is a diagonal matrix with elements $w_j = n_j/n$ on the diagonal, where n_j is the size of cell j. This can be thought of as a minimum distance estimator. Hence the covariance matrix would have the form

$$\Lambda_1 = \frac{1}{n}\left(X'WX\right)^{-1} X'W\Omega WX \left(X'WX\right)^{-1} \tag{4}$$

where Ω is a diagonal matrix with the sampling variance of the cell median on the diagonal.

The sampling variance of the median involves the density of the data. I assume that earnings are distributed log normally in each cell. If the earnings data are truncated, I calculate the standard deviation of the uncensored distribution σ_j using the estimate of the median, the censoring point c_j, the fraction at the maximum p_j, and the normality assumption using

$$\sigma_j = \frac{c_j - m_j}{\Phi^{-1}(1 - p_j)}$$

where $\Phi(\cdot)$ is the cumulative standard normal distribution function. Given the uncensored standard deviation, the j-th element of Ω is calculated as

$$\omega_j = \frac{\sigma_j^2}{4w_j\phi(0)^2}$$

where $\phi(\cdot)$ is the standard normal density function.

Chamberlain also suggests an adjustment to the covariance matrix to allow for the fact that the model estimated in eq. (3) does not fit the data exactly (e.g. the medians do not line up linearly). In this case, the estimates can be thought of as a linear approximation. Define $r = m - X\beta$ as the vector of approximation errors. In this case the covariance matrix will be equal to $\Lambda_1 + \Lambda_2$ where

$$\Lambda_2 = \frac{1}{J} \left(X'WX\right)^{-1} X'W \text{diag}(w_1^{-1} r_1^2, w_2^{-1} r_2^2, ..., w_J^{-1} r_J^2) WX \left(X'WX\right)^{-1}$$

and J is the number cells.

A further complication arises from the fact that the cell level medians are calculated from a panel, so the same individuals will recur in different cells. The median estimates within a cohort will therefore be correlated. In order to allow for this serial correlation, I need an estimate of the covariance matrix of earnings. I have up to 21 years of data for some cohorts, and I am not aware of any such estimate for Germany (or any other country) for such a long time span. Hence, I use the results reported in Card (1994) for the United States. Card estimates a parametric model for the earnings process on eight years of data from the PSID. Using this model, I calculate the first 20 implied autocorrelations. Biewen (2005) presents estimates for German household level income. The autocorrelations reported by Biewen are about 20 to 30 percent lower than those calculated from Card's model.

Let S denote the resulting autocorrelation matrix, and S_k the submatrix for the k-th cohort (which may have less than 21 observations in the data). The middle part of Λ_1 in eq. (4) can therefore be written

$$\begin{aligned} X'W\Omega'WX &= X_1'W_1\Omega_1^{1/2}S_1\Omega_1^{1/2}W_1X_1 + X_2'W_2\Omega_2^{1/2}S_2\Omega_2^{1/2}W_2X_2 + \\ &\quad \ldots + X_K'W_K\Omega_K^{1/2}S_K\Omega_K^{1/2}W_KX_K \end{aligned}$$

where the subscripts now refer to one of K cohorts rather than cells. An analogous adjustment is made for Λ_2.

This autocorrelation adjustment is likely to overstate the degree of serial correlation. While individuals occur repeatedly in the data, some individuals enter and leave the dataset. The correlation of the cell statistics over time should therefore be lower than the correlation of the individual level data. Hence, the standard errors estimated in this way are likely to be rather conservative. An alternative way to estimate standard errors is by using Stata's aweights (to allow for the cell level data) and cluster by cohort (to allow for a non-parametric estimate of the serial correlation structure). The Stata standard error on the short school year variable is 0.013 compared to 0.020 calculated with the procedure described above.

6.2 Monte Carlo Experiment for the Effects of Measurement Error

The design of the Monte Carlo experiment for measurement error was as follows. I generated data for individuals uniformly born between 1932 and 1970. The observations were distributed across 11 states and three secondary schooling levels representing the distribution in the actual data. I then allowed for 25 percent of observations to repeat at least one grade, and 2.5 percent to repeat two grades. Fertig (2004) reports that about 13 percent of students repeat at least one grade in the 1970s to the 1990s but grade repetition was probably higher in the 1960s. Based on the actual month of birth and school attendance, I calculated participation in the short school years, and in the 8th grade of the basic track. I then introduced measurement error in year of birth, secondary school track, and random mobility across state. I assumed that year of birth is mismeasured for about 32 percent of individuals, track is mismeasured for 10 percent of individuals, and I reassigned the state for 20 percent of individuals. The measurement error in year of birth was calculated as the integer value of a normal pseudo random variable with mean 0 and standard deviation 1. The transition matrix for secondary school track was

		mismeasured track		
		basic	middle	academic
	basic	0.90	0.08	0.02
true track	middle	0.08	0.90	0.02
	academic	0.05	0.05	0.90

20 percent of observations were randomly assigned a new state, using the original state distribution. Since this results in some individuals being reassigned to their original state, so that in practice that about 17 percent are in a different state. This fraction is close to the level of mobility found in Table 3.

I then recalculated the assignment of the short school years, and the 8th grade in basic track using these mismeasured data. Finally, I limit the sample to individuals with measured years of birth from 1943 to 1964. I calculate the number of years in school (S) as highest grade completed minus time lost due to the short school years, and constructed a wage according to

$$w = \beta S + \varepsilon$$

with β set either to 0.08, 0.04, or 0.02, and ε a normal pseudo random variable with mean 0 and standard deviation 0.4. The mean squared error of the regression for wages in column (1) of Table 8 is 0.397, and therefore reflects a similar amount of residual variation. I generate 65,000 observations. Because the sample is limited to observations with birth years from 1943 to 1964, the effective sample size is about 44,000 in each simulation, similar to the regressions with the QaC.

I then run regressions of the wage on the short school year variable and a full set of track, state, and year of birth dummies, performing 2,500 replications. The standard deviation of the estimates of $\widehat{\beta}$ in the simulations is about 0.011, similar to the estimated standard error of 0.012 in column (1) of Table 8. The mean estimate of $\widehat{\beta}$ is -0.053 when the true value of $\beta = 0.08$.

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Table 1
Transition to Fall Start of the School Year by State

State	Transition	1st school year	2nd school year	Group
Schleswig-Holstein	SSY	Apr 1966 – Nov 1966	Dec 1966 – July 1967	Treatment
Hamburg	LSY	Apr 1966 – July 1967	---	Control
Niedersachsen	SSY	Apr 1966 – Nov 1966	Dec 1966 – July 1967	Treatment/ Control
Bremen	SSY	Apr 1966 – Nov 1966	Dec 1966 – July 1967	Treatment
Nordrhein-Westphalen	SSY	Apr 1966 – Nov 1966	Dec 1966 – July 1967	Treatment
Hessen	SSY	Apr 1966 – Nov 1966	Dec 1966 – July 1967	Treatment
Rheinland-Pfalz	SSY	Apr 1966 – Nov 1966	Dec 1966 – July 1967	Treatment
Baden-Württemberg	SSY	Apr 1966 – Nov 1966	Dec 1966 – July 1967	Treatment
Bayern	None	Aug 1966 – July 1967	---	Control
Saarland	SSY	Apr 1966 – Nov 1966	Dec 1966 – July 1967	Treatment
Berlin	LSY	Apr 1966 – July 1967	---	Control

Note: SSY denotes two Short School Years, LSY denotes one Long School Year. Students in LSY states graduated at the end of March of their final year in school. See text for more details.

Table 2
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 Track	Year of Graduation from Middle Track	Year of Graduation from Academic Track	Number of Short School Years Basic Track	Number of Short School Years Middle Track	Number of Short School Years Academic Track
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 track.

Table 3
Data Sets Used in the Analysis

Data Set	Time Period	Outcome Measures	Sample Size	Advantages
Qualification and Career Survey (QaC)	1979-1999	Secondary track, total education, wages, earnings	43,883	Different measures of short school year
Micro Census	1989-2001	Secondary track, total education, wages, earnings, employment	723,470	Sample size, employment as outcome
ALLBUS	1980-2000	Earnings, civic outcomes	6,215	State of birth, civic outcomes
Social Security Data	1975-1995	Earnings	1,769,681	Sample size

Note: Sample sizes for the earnings analysis are shown.

Table 4
Percentage of Respondents Who Have Lived
in Current State Since Specific Age or Time
Cohorts born 1947 to 1960
ALLBUS, 1991, 1992, 1994, and 2000 Waves

Has Lived in Current State Since	State of Current Residence			
	All States	Bavaria	Niedersachsen	Berlin/ Hamburg
Birth (State of Birth)	83	91	88	54
Birth (In State Since)	79	85	82	45
Age 6	82	85	83	52
Age 12	84	86	86	56
Age 18	85	88	86	59
1965 or earlier	84	86	86	56

Note: The first row is based on whether state at birth is the same as state of current residence. The other rows are based on a question asking how long the respondent has lived in the state of current residence. Number of observations is 1,133 for all states, 237 for Bavaria, 125 for Niedersachsen, and 75 for Berlin/Hamburg. There are slightly fewer observations for the first row (respondent still in state of birth) in each case.

Table 5
Test Results of Short School Year Students
(t-statistics for Control-Treatment Differences in Parentheses)

Test	Mean of	Control Groups Tested After		Treatment Groups Tested After		
		Short	Regular	Short	Regular	2 Years Later
2nd graders (Meister, 1972)						
Reading	Mistakes	19.6		16.2 (5.4)		
Writing	Mistakes	15.0		8.3 (8.8)		
Math	Mistakes	16.3		6.7 (11.3)		
2nd graders (Thiel, 1973)						
Writing	Mistakes		11.8	13.5 (1.5)	8.5 (3.9)	
Math	Correct Answers		27.0	18.2 (4.5)	29.9 (1.9)	
4th graders (Thiel, 1973)						
Reading	Correct Answers		12.7	10.2 (7.3)	12.7 (0.1)	11.9 (2.1)
Vocabulary	Correct Answers		19.8	17.1 (5.1)	20.5 (1.9)	18.9 (1.7)
Writing	Correct Answers		13.5	14.1 (1.6)	15.4 (6.8)	14.0 (0.9)
Mental Arithmetic	Correct Answers		9.5	7.7 (4.4)	9.9 (1.3)	9.2 (0.6)
Written Arithmetic	Correct Answers		11.3	11.5 (0.5)	11.4 (0.3)	11.3 (0.0)
Math Problems	Correct Answers		11.2	11.0 (0.3)	12.3 (3.3)	12.1 (2.1)
4th graders (Schlevoigt et al., 1968)						
Text Comprehension	Fraction 12+ Points (20 total)		0.58	0.54 (2.7)		
Writing	Fraction <12 Mistakes		0.55	0.38 (9.5)		
Arithmetic	Fraction 20+ Points (30 total)		0.60	0.57 (2.0)		
Math Problems	Fraction 10+ Points (20 total)		0.59	0.57 (0.7)		
8th graders in basic school track (Thiel, 1973)						
Writing	Mistakes		17.9	18.6 (0.9)	18.0 (0.4)	
Math	Correct Answers		30.2	32.9 (0.9)	34.2 (1.6)	

Notes: Fractions and t-statistics for Schlevoigt et al. (1968) are calculated by the author from the tabulated distribution of results.

Table 6
Fraction of Students Repeating Primary Grades
1962 to 1973 by State Group

	Grade 1	Grade 2	Grade 3	Grade 4
1961-62 School Year				
States with Short School Years	0.044	0.045	0.037	0.037
Bavaria	0.037	0.024	0.019	0.014
States with Long School Years	0.051	0.063	0.054	0.054
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
1971-72 School Year				
States with Short School Years	0.055	0.042	0.032	0.027
Bavaria	0.040	0.026	0.017	0.015
States with Long School Years	0.037	0.044	0.034	0.029
1972-73 School Year				
States with Short School Years	0.055	0.040	0.031	0.025
Bavaria	0.041	0.024	0.015	0.013
States with Long School Years	0.035	0.043	0.029	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 grade repetition data are available for the school years 1962 to 1965 and Berlin data are not available for the 1967-68 school year.

Table 7
Regression Estimates of the Effect
of the Short School Years on Grade Repetition
(Standard Errors in Parentheses)

	Sample		
	Grades 1- 4		Grades 2 - 4
Independent Variable/Specification	(1)	(2)	(3)
Mean of Dependent Variable	0.0381	0.0381	0.0356
Affected by Short School Years (Niedersachsen is Treatment)	0.0094 (0.0017)	0.0090 (0.0015)	0.0082 (0.0017)
Affected by Short School Years (Niedersachsen is Control)	0.0110 (0.0016)	0.0120 (0.0014)	0.0125 (0.0015)
Affected by Short School Years (Sample without Niedersachsen)	0.0112 (0.0012)	0.0110 (0.0011)	0.0107 (0.0011)
Year Dummies	✓	✓	✓
State Dummies	✓	✓	✓
Grade Dummies	✓	✓	✓
State*Grade Interactions		✓	✓
Number of Observations (incl. Niedersachsen)	387	387	290

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 1961 and 1966 to 1973. Berlin data are missing for the 1967-68 school year, and Saarland did not have a regular fourth grade in the 1961-1962 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.

Table 8
Regression Estimates of the Effect of the Short School Years on Education Cohorts Born 1952 - 1964
(Standard Errors in Parentheses)

	Dependent Variable			
	Academic Track	Intermediate Track	Total Education	
Independent Variable	(1)	(2)	(3)	(4)
Qualification and Career Survey				
Short School Year during Primary School	0.020 (0.016)	-0.028 (0.028)	-0.016 (0.102)	-0.061 (0.053)
Number of Observations	25,605	25,605	23,058	23,058
Micro Census				
Short School Year during Primary School	-0.011 (0.006)	-0.028 (0.010)	-0.279 (0.088)	0.016 (0.015)
Number of Observations	627,051	627,051	532,094	532,094
Secondary School Track Dummies				✓
Year Dummies	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓
Year of Birth Dummies	✓	✓	✓	✓
Female Dummy	✓	✓	✓	✓

Note: Standard errors are adjusted for clusters at the year of birth * state level. Berlin and Bremen are excluded from the sample.

Table 9
Earnings Regressions
Cohorts Born 1943-64
(Standard Errors in Parentheses)

Independent Variable	OLS (1)	OLS (2)	IV (3)	Only Men OLS (4)
		Qualification and Career Survey		
		Dependent Variable: Log Hourly Wage		
Short School Year Definition Based on Tracks	-0.006 (0.012)	---	0.007 (0.014)	0.005 (0.015)
Short School Year Definition Based on Graduation Date	---	0.006 (0.012)	---	---
		Dependent Variable: Log Monthly Earnings		
Short School Year Definition Based on Tracks	-0.005 (0.015)	---	0.010 (0.016)	0.009 (0.017)
Short School Year Definition Based on Graduation Date	---	0.008 (0.014)	---	---
Number of Observations	43,883	43,883	43,883	26,050
		Micro Census		
		Dependent Variable: Log Hourly Wage		
Short School Year Definition Based on Tracks	0.017 (0.011)	---	---	0.001 (0.011)
		Dependent Variable: Log Monthly Earnings		
Short School Year Definition Based on Tracks	0.017 (0.011)	---	---	0.005 (0.014)
Number of Observations	723,470	---	---	430,859
Secondary School Track Dummies	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓
Year of Birth Dummies	✓	✓	✓	✓
Age Dummies	✓	✓	✓	✓
Female Dummy	✓	✓	✓	

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 10
Earnings Regressions
(Standard Errors in Parentheses)

Cohorts Affected in	Primary School	Grades 1-9		Secondary School
Cohorts	1943-46 1957-64	1943-46 1952-64		1943-55 1961-64
Independent Variable	(1)	(2)	(3)	(4)
Qualification and Career Survey				
Dependent Variable: Log Hourly Wage				
Short School Year Definition Based on Tracks	0.009 (0.018)	0.002 (0.014)	0.028 (0.048)	-0.013 (0.015)
Dependent Variable: Log Monthly Earnings				
Short School Year Definition Based on Tracks	-0.006 (0.021)	-0.011 (0.017)	0.010 (0.050)	-0.005 (0.018)
Number of Observations	22,699	33,784	30,826	32,477
Micro Census				
Dependent Variable: Log Hourly Wage				
Short School Year Definition Based on Tracks	-0.012 (0.013)	-0.004 (0.012)	0.000 (0.065)	0.031 (0.013)
Dependent Variable: Log Monthly Earnings				
Short School Year Definition Based on Tracks	-0.004 (0.014)	-0.006 (0.013)	-0.004 (0.074)	0.026 (0.014)
Number of Observations	400,673	567,704	514,974	545,362
Secondary School Track Dummies	✓	✓		✓
Year Dummies	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓
Year of Birth Dummies	✓	✓	✓	✓
Age Dummies	✓	✓	✓	✓
Female Dummy	✓	✓	✓	✓

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 11
Quantile Regressions for Earnings
Cohorts Born 1943-64
Dependent Variable: Log Hourly Wage
(Standard Errors in Parentheses)

	OLS	Quantile Regression Quantile		
		0.50	0.25	0.10
Independent Variable	(1)	(2)	(3)	(4)
Qualification and Career Survey				
Short School Year Definition Based on Tracks	-0.006 (0.012)	-0.011 (0.008)	-0.004 (0.011)	-0.010 (0.018)
Number of Observations	43,883	43,883	43,883	43,883
Micro Census				
Short School Year Definition Based on Tracks	0.017 (0.011)	0.011 (0.0003)	0.013 (0.003)	0.025 (0.005)
Number of Observations	723,470	723,470	723,470	723,470
Secondary School Track Dummies	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓
Year of Birth Dummies	✓	✓	✓	✓
Age Dummies	✓	✓	✓	✓
Female Dummy	✓	✓	✓	✓

Note: OLS standard errors are adjusted for clusters at the track * year of birth * state level. Conventional standard errors are reported for the quantile regression models. 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 12
Earnings Regressions
ALLBUS 1980-2000
Dependent Variable: Log Monthly Earnings
(Standard Errors in Parentheses)

Waves Independent Variable	All (1)	1991, 1992, 1994, and 2000 (2)	 (3)	 (4)
Short School Year Definition Based on State of Residence	-0.018 (0.033)	-0.005 (0.070)	-0.005 (0.071)	---
Short School Year Definition Based on State of Birth	---	---	---	0.041 (0.071)
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	6,215	1,649	1,649	1,649

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

Table 13
Employment Regressions
Micro Census
Dependent Variable: Dummy for Being Employed in the Survey Week
(Standard Errors in Parentheses)

Cohorts Affected in	Primary and Secondary School		Primary School	Grades 1-9		Secondary School
			1943-46	1943-46		1943-55
Cohorts	1943-64		1957-64	1952-64		1961-64
Sample	All	Men	All	All	All	All
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Full Sample						
Short School Year	0.016	0.013	0.005	0.006	0.014	0.024
Definition Based on Tracks	(0.006)	(0.007)	(0.010)	(0.008)	(0.013)	(0.008)
Number of Observations	1,032,744	509,770	579,086	810,873	738,130	782,630
Age 31 and Over						
Short School Year	0.008	-0.003	-0.001	0.001	0.010	0.012
Definition Based on Tracks	(0.005)	(0.005)	(0.009)	(0.006)	(0.013)	(0.006)
Number of Observations	971,064	478,996	517,406	749,193	683,021	730,089
Secondary School Track Dummies	✓	✓	✓	✓		✓
Year Dummies	✓	✓	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓	✓	✓
Year of Birth Dummies	✓	✓	✓	✓	✓	✓
Age Dummies	✓	✓	✓	✓	✓	✓
Female Dummy	✓	✓	✓	✓	✓	✓

Note: Estimates are from linear probability models. Standard errors are adjusted for clusters at the track * year of birth * state level. Observations from Niedersachsen are omitted from the specification in column (5).

Table 14
Political Participation and Attitudes, Interest in Music
ALLBUS 1980-2000, various years
(Standard Errors in Parentheses)

Dependent Variable	Would not Vote Next Sunday (1)	Did not Vote Last Election (2)	Little Political Interest (3)	Likes Extreme Party (4)	Member of Choir, Orchestra (5)
Mean of Dependent Variable	0.061	0.098	0.226	0.058	0.036
Short School Year	-0.002	0.016	-0.020	0.044	-0.018
Definition Based on State of Residence	(0.017)	(0.030)	(0.030)	(0.028)	(0.012)
Secondary School Track Dummies	✓	✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓	✓
Year of Birth Dummies	✓	✓	✓	✓	✓
Age Dummies	✓	✓	✓	✓	✓
Female Dummy	✓	✓	✓	✓	✓
Number of Observations	6,057	4,477	5,952	2,029	6,925

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

Figure 1: Teacher Absences

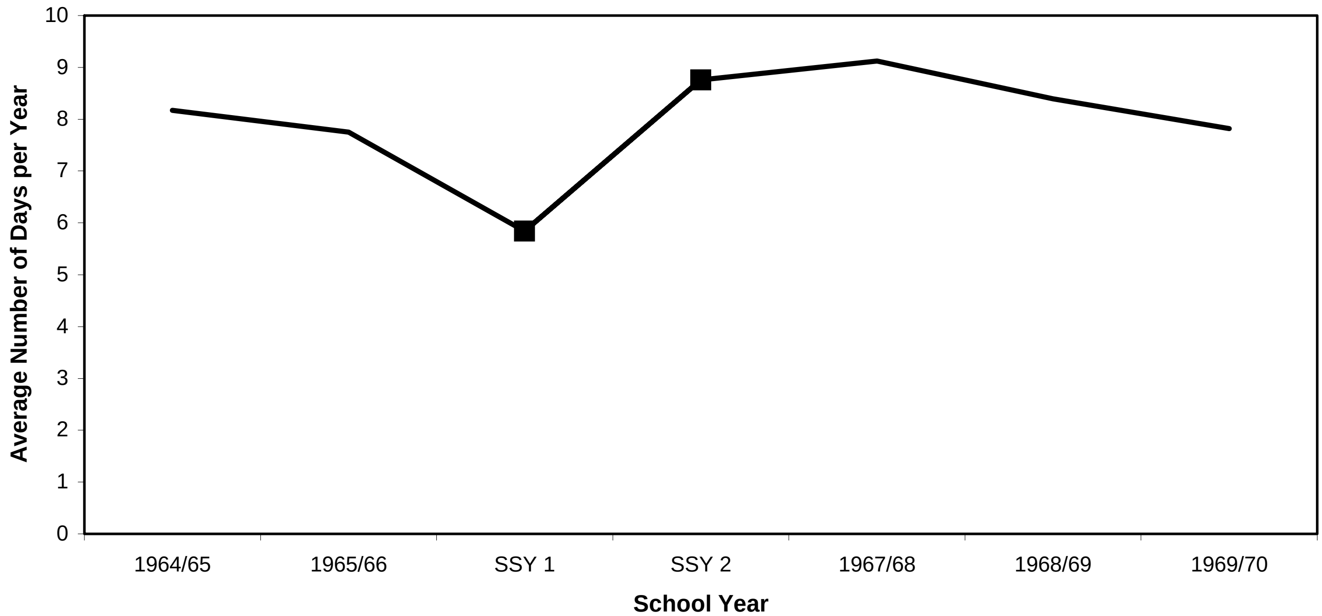

Figure 2: Grade Repetition Rates Grade 2

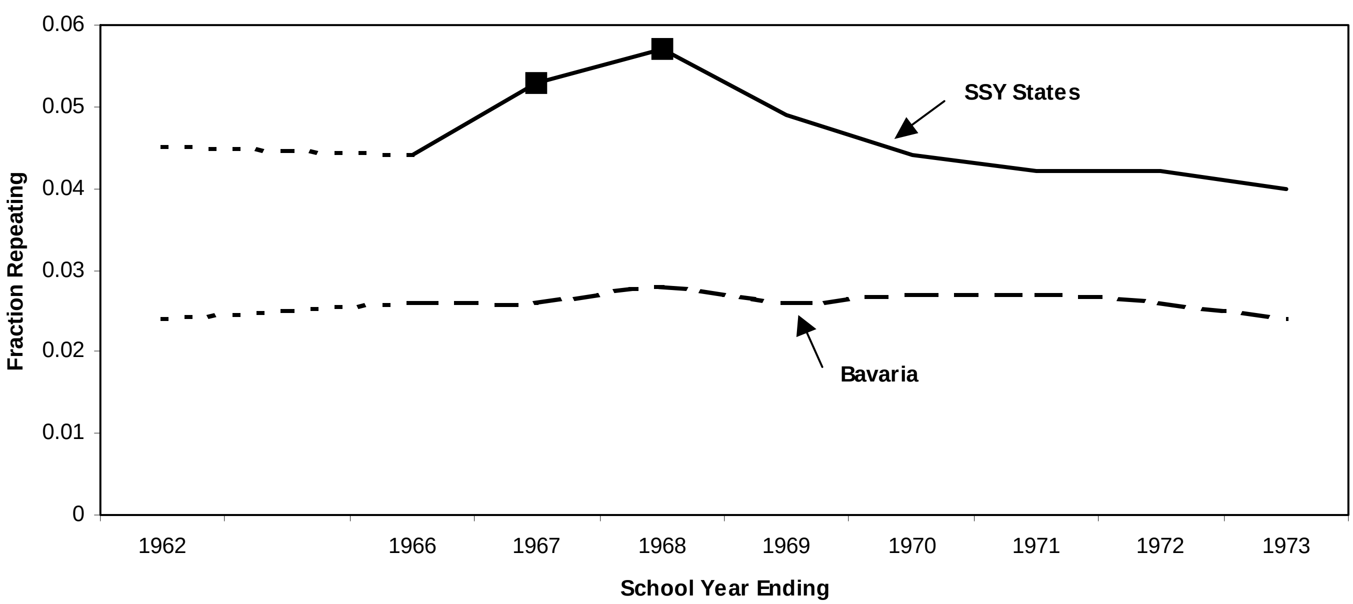

Figure 3: Grade Repetion Rates Grade 3

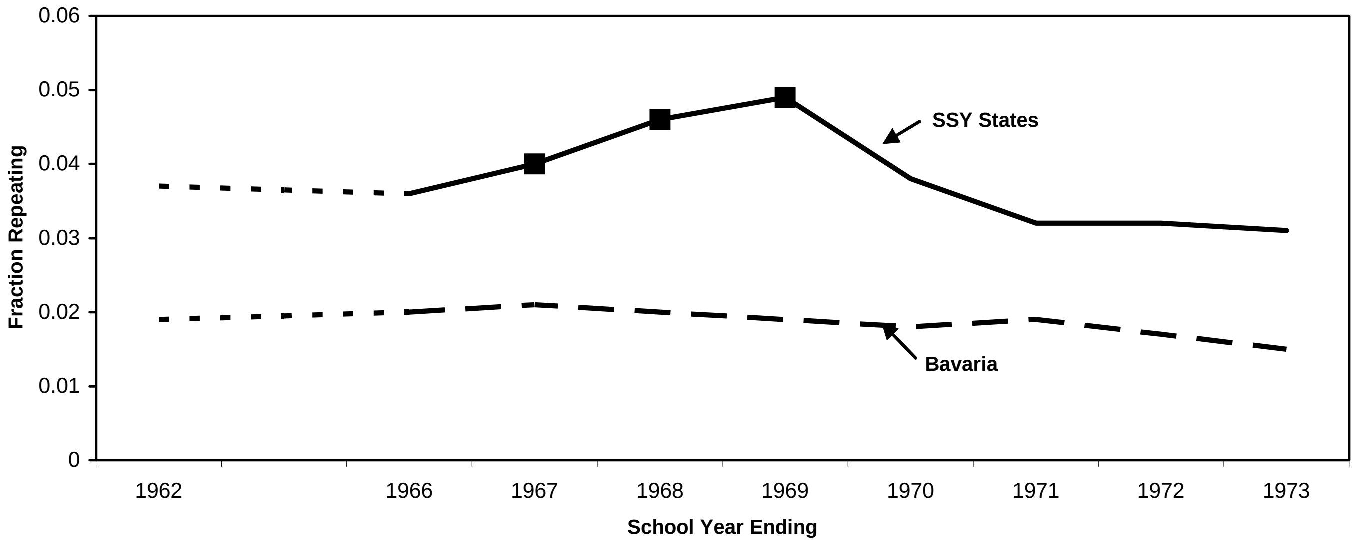

Figure 4: Grade Repetion Rates Grade 4

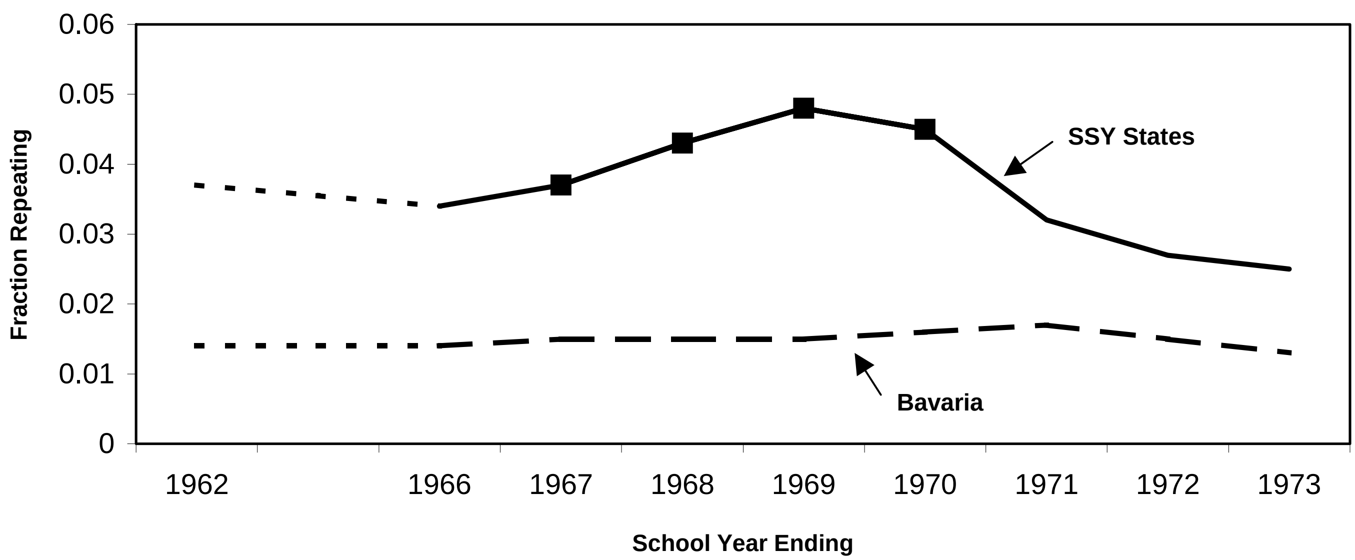

Figure 5: Earnings Effects of the Short School Years by Grade

Qualification and Career Survey