The Economic Journal, **117** (*October*), 1216–1242. Published by Blackwell Publishing, 9600 Garsington Road, Oxford OX4 2DQ, UK and 350 Main Street, Malden, MA 02148, USA.

THE IMPACT OF LENGTH OF THE SCHOOL YEAR ON STUDENT PERFORMANCE AND EARNINGS: EVIDENCE FROM THE GERMAN SHORT SCHOOL YEARS*

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This article 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 1966–7, 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.

Primary and secondary school students in the US 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 (NCES, 2000; Lee and Barro, 2001). 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 article, I study the impact of a reform in the West German school system in 1966–7 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 an autumn 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

* I thank Fabian Waldinger for excellent research assistance. I thank Josh Angrist, David Autor, Jens Ludwig, Jack Porter, Justin Wolfers, referees for this Journal and for the *Quarterly Journal of Economics*, and participants at various seminars for helpful comments. I thank ZUMA Mannheim for their hospitality in allowing me access to the German Micro Census data. Some of the data used in this article have been obtained from the German Zentralarchiv für Empirische Sozialforschung at the University of Köln (ZA). Neither the producers of the data nor the ZA bear any responsibility for the analysis and interpretation of the data in this paper. This paper was submitted before Jörn Steffen Pischke was invited to become an editor of the JOURNAL.

that many students in this state did not lose 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 analyse 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.

These results may seem surprising in light of the evidence showing that returns to schooling are quite substantial.[1] The estimates of returns to schooling in the previous literature may not be the relevant 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 highest grade completed or 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.

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 will discuss 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 labour market and hence accumulated more labour market experience. The increased incidence of grade repetition might indicate that slower students in particular were not as able to cope with the increased pace during the short school years. Grade repetition might have been a mechanism that insured that some marginal students 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 US include term length at the school level as one of the regressors (Grogger, 1996; Eide and Showalter,

[1] Acemoglu and Pischke (1999) report OLS returns to schooling of 7 to 8% 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.

1998) but typically found insignificant effects. 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 *et al.* (1996). But these latter authors also find a zero effect of term length in their re-estimation.

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.[2] A more recent study by Wößmann (2003), which also analyses 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% reduction in the time of 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 *et al.* (1968) and Thiel (1973), which I will discuss in Section 3 below.

The remainder of the article is organised as follows. Section 1 starts by laying out some background about the German school system and the short school years, and discusses what type of variation is used to identify the short school year effects. It also discusses the measurement framework and assesses the external validity of the exercise. Section 2 describes the data sources used to obtain the empirical results in Section 3 on student achievement, earnings and employment. I draw conclusions in Section 4. Additional results can be found in the working paper version of this article (Pischke, 2006).

1. Institutions and Empirical Framework

1.1. *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

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

in spring. To reduce the resulting frictions, 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 moving 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.[3] The accord was to be implemented by the beginning of the 1967 school year.

A heated debate ensued on how to accomplish the transition from a start of the school year after Easter to the new date in summer. An early consensus emerged among the states, which was based on a prolonged school year, lasting from April 1966 to summer of 1967. This solution was supposed to avoid children in school during this time graduating 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. Several of the southern states, for example Rheinland-Pfalz, decided to use the 1966–7 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 unravelled as more and more states decided to opt for the short school years. Eight states carried out the transition by having a short school year starting April 1, 1966 and ending November 30, 1966, and a second short school year starting December 1, 1966 and ending July 31, 1967.[4] The two city states of West Berlin and Hamburg stuck to the solution with a single long school year. Starting in 1967, the school year would begin in August and end in July in these states. Graduating classes which participated in the long school year, however, would graduate at the end of March after a shortened final year. Hence, everybody in Hamburg and Berlin attended school for the regular amount of time despite the transition. Bavaria, which already started in summer, had a regular length school year during the transition period. Finally, Niedersachsen adopted the short school years during 1966–7 but added additional school periods in subsequent years for some types of schools (see below for details). Table 1 summarises the transitions to the new start of the school year in the various states.

Participation in the short school years depended on three student characteristics, which can be used for identification. Student cohort is the first characteristic since the short school year affected only cohorts who were attending school during 1966–7. The second characteristic 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. The intermediate track (Realschule) ends after grade 10 and the most academic track (Gymnasium) leads to graduation after 13 grades. This means that

[3] 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.

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

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

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 graduation from			Number of short school years		
Year of birth	Quarter of birth	Year of school entry	Basic track	Middle track	Academic track	Basic track	Middle track	Academic track
1946	all	1953	1962	1963	1966	0	0	0
1947	all	1954	1963	1964	1966/Dec	0	0	1
1948	all	1955	1964	1965	1967	0	0	2
1949	all	1956	1965	1966	1968	0	0	2
1950	all	1957	1966	1966/Dec	1969	0	1	2
1951	all	1958	1966/Dec	1967	1970	1	2	2
1952	all	1959	1967	1968	1971	2	2	2
1953	all	1960	1968	1969	1972	2	2	2
1954	all	1961	1969	1970	1973	2	2	2
1955	all	1962	1970	1971	1974	2	2	2
1956	all	1963	1971	1972	1975	2	2	2
1957	all	1964	1972	1973	1976	2	2	2
1958	all	1965	1973	1974	1977	2	2	2
1959	all	1966	1974	1975	1978	2	2	2
1960	1	1966/Dec	1975	1976	1979	1	1	1
1960	2	1966/Dec	1975	1976	1979	1	1	1
1960	3	1967	1976	1977	1980	0	0	0
1960	4	1967	1976	1977	1980	0	0	0

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.

some students, who were born in the late 1940s and were close to graduation by the mid-1960s, will have been affected by the short school years and others will not, 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 to identify the effects of the short school year.

The third characteristic is the state where a student went to school. This makes use of the fact that Bavaria, Hamburg and Berlin did not have short school years. The state of Niedersachsen provides an additional source of variation. Niedersachsen decided not to have students enter 1st grade for the school year starting December 1966, but only in August 1967. This decision freed up resources (class rooms and teachers) which were used to lengthen the final school year for students attending 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 from 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 possibly also 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.[5] 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

[5] 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.

material to be taught in the core subjects like German, English and mathematics. 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 analyse grade repetition and attendance of the higher tracks as outcomes directly below. Such behaviour, 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 standardised test would be the preferred choice for assessing the effects on student achievement and learning. Unfortunately, there are no uniform standardised tests available in Germany. However, I will briefly present the results of three studies undertaken at the time, which tested students in school during the short school years. Grade repetition and secondary track choice are the only academic outcomes available for the relevant time period. 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 US, 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, where 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.[6] 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 enrols 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 short school year, only about

[6] 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.

7% of total accessions into the academic track were from the basic or intermediate one after grade 5. Most of this lateral movement takes place by grade 7.

1.2. *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 wage, 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 (Angrist and Krueger, 1999).

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 or decided to send their child to a different secondary school track in response to a state's decision to introduce the short school years. 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 neighbouring 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.[7] Instead of using three dummies for the three tracks, I use four dummies, dividing basic track students into separate groups depending on whether they graduated after eight or nine years.

The other controls in (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 labour market experience. The students affected by the short school years will have more potential labour 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 probably 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.

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

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

[7] 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.

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 $\gamma_s{}^* \rho_g$.

1.3. *External Validity*

The various possible dimensions of contrasts across states, cohorts and tracks, as well as the possibility of constructing 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 1966–7 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 may have mobilised 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 mathematics. Out of 21 teachers, only 19% report a regular additional hour for mathematics and 33% for writing. 14% actually report a regular hour less in writing. Slightly more than half report an additional hour in each subject occasionally.[8] Taken together, these estimates suggest that instructional time due to additional classes was about 3–4% higher. This is small compared to the loss of instructional time of about 33% each year due to the short school years.

Since primary school classes are typically taught by a single teacher, there is also the possibility that reading, writing and mathematics 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 mathematics, particularly reading and writing. In addition, 3 of the teachers mentioned

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

cuts in music instruction. Thiel (1973) reports that 72% of teachers gave additional homework in mathematics and 62% in writing. 60% mention that they perceived parents as working more intensively with their children. On the other hand, only one 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 1964/5 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, by coming 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 is another potential explanation 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. Even with this alternative explanation, the data do not suggest that teachers consistently exerted higher effort.

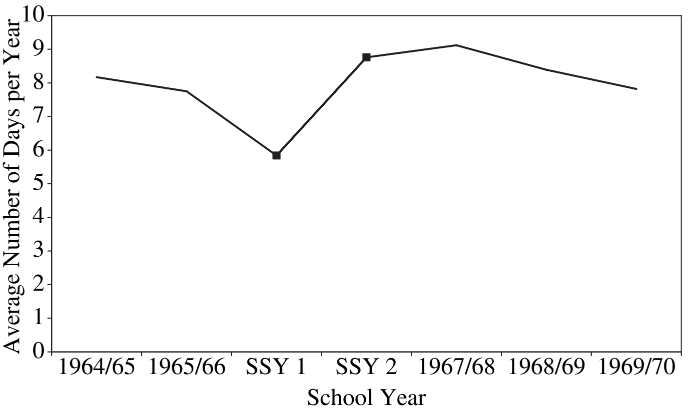

Fig. 1. *Teacher Absences*

While the evidence is less than clear cut, it suggests *some* adjustments to the short school years, but these were minor. The role of additional instructional time during the short school years was basically negligible. 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. It also has to be kept in mind that the school system was already under strain during this period because of the large baby boom cohorts being educated and because of the general expansion of the education system. 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. This is borne out by the evidence on outcomes presented below.

2. The Data

In order to study the impact of the short school years on student performance, I analyse 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 1961–2 to 1972–3. No grade repetition data exist for the school years 1962–3 to 1964–5. 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 two micro data sets, the Qualification and Career Survey and the Micro Census, each with its own strengths and weaknesses. The Qualification and Career Survey (QaC) collected by the Institut für Arbeitsmarkt- und Berufsforschung (IAB) and the Bundesinstitut für Berufsbildung (BIBB) is a repeated cross-section of employed workers in the age group 15 to 65. I use the four waves for 1979, 1985–6, 1991–2 and 1998–9, each of which samples about 25,000 workers. The samples are restricted to respondents of German nationality, and, in the 1991–2 and 1998–9 waves, to those who grew up in West Germany. An advantage of this data set is the detailed information on schooling and training.

The earnings variable in the surveys is gross monthly earnings, which is reported in 13 brackets in the 1979 survey, in the 1985–6 survey in 22 brackets, in 1991–2 in 15 brackets and 1998–9 in 18 brackets. I assign each individual earnings equal to the bracket midpoint.[9] I then convert the variable to an hourly wage by dividing by the number of weekly hours.

[9] 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 to which I assigned a value of DM 7,500, in 1985–6 it was DM 15,000 or more to which I assigned a value of DM 16,500, and in 1991–2 it was DM 8,000 or more to which I assigned a value of 10,500, and in 1998–9 it was DM 15,000 or more to which I assigned a value of DM 17,500. These values were chosen based on means for these categories in the ALLBUS, a smaller data set covering the same period. Only 1.0% of sample observations are in the top income bracket.

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. 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 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 these. 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 therefore 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 1966/7 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, which is the case in Germany.[10] There is no direct information on the amount of time individuals actually spent in school in the data.

The second data set is the German annual labour 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.[11] 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 for respondents' 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 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.[12] 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.

3. Results

3.1. *The Impact on School Performance*

The most direct method of assessing school performance is to compare the results on standardised tests. There is no standardised testing system in Germany which allows such a comparison. However, three studies were undertaken at the time of the short school years, which tested students (Meister, 1972; Thiel, 1973; Schlevoigt *et al.*, 1968). I discuss the results from these studies in detail in the working paper (Pischke, 2006). While these studies differ in many of the details of their findings, three main results emerge. First, the students affected by the short schools years had some deficiencies at the end of the short school years in the core subjects of reading, writing and mathematics, although these subjects presumably received the most attention at the time. This indicates that the short school years had some immediate effect on learning. The

[10] According to a smaller data set, the ALLBUS data, more than 80% 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; see Pischke (2006) for details. If migration is unrelated to the effects of the short school years this measurement error will lead to pure attenuation. The impact of this measurement error in a regression framework can be easily quantified. Assume that state of birth corresponds to the state of schooling at the time of the short school years. 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 (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.

[11] The data are from the anonymised 70% sample of the Micro Census (ZUMA file) and were used at ZUMA Mannheim.

[12] The top bracket in 1989 was DM 5,000 or more to which I assigned a value of DM 7,500; in 1991-9 it was DM 7,500 or more to which I assigned a value of DM 10,500; in 2000 and 2001 it was DM 35,000 or more to 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.

second result is that the affected students were always on par and typically ahead of their peers when tested at the same age. This indicates that learning was faster during the short school years. Finally, there were no differences between affected and unaffected students when students were tested two years after the short school years. This indicates that the immediate effects of the short school years seemed to fade out after a relatively short time.

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. In order to illustrate the grade repetition effects, Figure 2 displays grade repetition rates for students in grade 3. The treatment states include all the short school year states except Niedersachsen. Repetition rates for Bavaria are displayed as a control. Affected grades are marked by boxes and the school years with missing data are indicated by short dashes.

In each year when 3rd graders were affected, grade repetition increased somewhat gradually, reaching a peak of about 1.5 percentage points three years after a cohort was exposed to the short school years. This indicates that some poorly performing students seem to have been promoted initially, only to fail in a subsequent grade. This could be because 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 visible in the Figure is that the first cohort after the short school years also had a slightly higher rate of grade repetition, possibly indicating knock-on effects of the short school years. This could be due to teachers being under more stress during the short school years, and teaching in the subsequent year suffered as a consequence. Similar results (not displayed) emerge for grades 2 and 4 but not for the 1st grade.

Table 3 presents regression results for the effects of the short school years on grade repetition. Controlling for grade, year and state effects, I find an increase in repetition rates 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% of students repeat grades every year. The results do not depend very much on whether Niedersachsen is treated as a treatment or control state or dropped from the sample altogether. Column (2) shows that the results are changed little when state $\times$ 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.

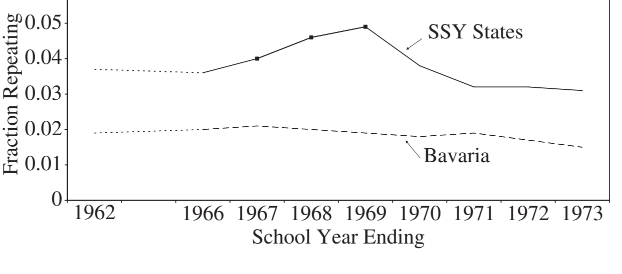

Fig. 2. *Grade Repetition Rates Grade 3*

Table 3

Regression Estimates of the Effect of the Short School Years on Grade Repetition

	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	0.0090	0.0082
	(0.0017)	(0.0015)	(0.0017)
Affected by short school years (Niedersachsen is control)	0.0110	0.0120	0.0125
	(0.0016)	(0.0014)	(0.0015)
Affected by short school years (Sample without Niedersachsen)	0.0112	0.0110	0.0107
	(0.0012)	(0.0011)	(0.0011)
Year dummies	✔	✔	✔
State dummies	✔	✔	✔
Grade dummies	✔	✔	✔
State×Grade interactions		✔	✔
Number of observations (incl. Niedersachsen)	387	387	290

Standard errors in parentheses. 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 or 2 to 4 and the school years ending 1961 and 1966 to 1973. Berlin data are missing for the 1967–8 school year, and Saarland did not have a regular fourth grade in the 1961–2 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.

It is also interesting to look at the impact of the short school years on total completed education. The German education system involves many different educational tracks, and various post-secondary training programmes. Nevertheless, the main distinction in completed education for most Germans turns out to be between attendance at one of the lower secondary tracks plus an apprenticeship versus attendance at 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 analyse secondary track choices in Table 4. In addition, I also present some results on total completed education, including post-secondary education and training.

The first two columns in Table 4 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 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% of students in the cohorts

Table 4

Regression Estimates of the Effect of the Short School Years on Education

	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	✔	✔	✔	✔

Standard errors in parentheses are adjusted for clusters at the year of birth × state level.
Cohorts born 1952–64. Berlin and Bremen are excluded from the sample.

in question attended the intermediate track. Hence, this is a reduction of about 10%, which is sizeable.

A further dimension according to which education could have affected education is by resulting in different choices of post-school training or university attendance. Columns (3) and (4) in Table 4 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 programme. 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 programmes. Column (3) presents regressions analogous to those in columns (1) and (2), i.e. these regressions reflect the effect of track choice, while column (4) partials out secondary track choice.

The results in column (3) and (4) are slightly different for the QaC data and the Micro Census again because the results on track choice were somewhat different in the two data sets. Overall, any effect on total education seems to be due to the effect on track choice. There is no evidence of any effect on post-school training or education within tracks, since the effects in column (4) are small and insignificant.

The results on grade repetition and track choice, together with the earlier studies on achievement, suggest a clear impact of the short school years on learning and this impact might have been particularly large in the lower half of the ability distribution. Grade repetition in primary school increased by about 25%, the fraction of students attending the intermediate track fell by about 10%, 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 might have been concentrated on weaker students, hence avoiding additional grade repetition. Instead, the short school years did affect learning, despite the temporary nature of the experience. However, these effects were probably short-lived. The large impact on grade repetition also suggests that there was no shading of standards. The results on track choice highlight 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 on average have almost 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. Similarly, the impact on track choice suggests a relatively small aggregate reduction in the amount of schooling students received.

3.2. *The Impact on Earnings*

Table 5 presents regressions of log wages 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.

Table 5

Wage Regressions

Independent variable	OLS (1)	OLS (2)	IV (3)	Only Men OLS (4)
	Qualification and Career Survey			
Short school year	−0.006		0.007	0.005
Definition based on tracks	(0.012)	—	(0.014)	(0.015)
Short school year	—	0.006	—	—
Definition based on graduation date		(0.012)		
Number of observations	43,883	43,883	43,883	26,050
	Micro Census			
Short school year	0.017	—	—	0.001
Definition based on tracks	(0.011)			(0.011)
Number of observations	723,470	—	—	430,859
Secondary school track dummies	✔	✔	✔	✔
Year dummies	✔	✔	✔	✔
State of residence dummies	✔	✔	✔	✔
Year of birth dummies	✔	✔	✔	✔
Age dummies	✔	✔	✔	✔
Female dummies	✔	✔	✔	

Dependent variable is the log hourly wage. Cohorts born 1943–64. Standard errors in parentheses 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).

This means that identification is achieved by using both the second and third level interactions implied by the short school year measures.[13] 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.[14]

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% 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% as the benchmark, the estimates in column (1) imply that negative effects of the short school years greater than 40% 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.

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 labour force participation should not be much of an issue. The effects are again close to zero in both data sets.[15]

Table 6 probes the specification further by distinguishing whether students were affected by the short school years in primary or in secondary school. This specification check is interesting for two reasons. First, it seems that students might have made up material missed during the short school years in subsequent school years. Those students affected in higher grades will have less time to do so. Secondly, it is important to check whether the results are robust to omitting track as a covariate. This can only be done when the treatment group is limited to students in the earlier grades.

[13] 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 5.

[14] 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 *et al.* (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 would be to allow for arbitrary correlation of the errors within states. Unfortunately, there are only eleven states, and the covariance estimators suggested in the Bertrand *et al.* (2004) study did not perform well in simulations for such a small number of states. When I cluster at the level of the state, the resulting standard errors are generally smaller than or of similar size to those reported in Table 5.

[15] 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 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.

Table 6

Wage Regressions: Additional Specifications

	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			
Short school year	0.009	0.002	0.028	−0.013
Definition based on tracks	(0.018)	(0.014)	(0.048)	(0.015)
Number of observations	22,699	33,784	30,826	32,477
	Micro Census			
Short school year	−0.012	−0.004	0.000	0.031
Definition based on tracks	(0.013)	(0.012)	(0.065)	(0.013)
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	✔	✔	✔	✔

Dependent variable is the log hourly wage. Standard errors in parentheses are adjusted for clusters at the track × year of birth × state level. Observations from Niedersachsen are omitted from the specification in column (3).

Column (1) in Table 6 includes only 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. The adjacent unaffected cohorts born from 1943–6 and 1961–4 are also included in all models as a control group. 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.[16] Notice, however, that the results in column (3) are not estimated very precisely since secondary school track is a potent covariate in explaining earnings.

[16] 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.

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 1943–6 and 1961–4 plus a single one of the affected cohorts. Figure 3 plots the coefficients of this exercise for the QaC together with a 95% confidence band. The grade by grade estimates are less precise, and the width of the confidence interval is about 10% 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.[17]

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 analyse the impacts of the short school year 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 *et al.* (1995). Table 7 presents quantile regression estimates for the median, as well as for the 25th and 10th percentiles.[18] 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 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.

In the working paper (Pischke, 2006), I also report results from two other data sets, the ALLBUS and a sample of social security records. The results from

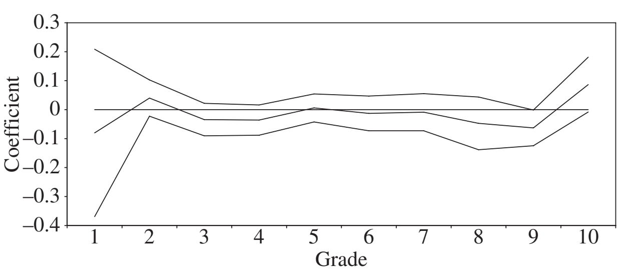

Fig. 3. *Earnings Effects of the Short School Years by Grade*
Note: Outside lines show the 95% confidence band

[17] 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.

[18] The standard errors for the quantile regressions are not adjusted for any clustering and hence are likely to be 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 me use the data at their facilities. Bootstrapping is not feasible in this environment because one quantile regression takes about 2 hours to run.

Table 7
Quantile Regressions for Wages

	OLS	Quantile regression		
		Quantile		
		0.50	0.25	0.10
Independent variable	(1)	(2)	(3)	(4)
Qualification and Career Survey				
Short school year	−0.006	−0.011	−0.004	−0.010
Definition based on tracks	(0.012)	(0.008)	(0.011)	(0.018)
Micro Census				
Short school year	0.017	0.011	0.013	0.025
Definition based on tracks	(0.011)	(0.0003)	(0.003)	(0.005)
Secondary school track dummies	✔	✔	✔	✔
Year dummies	✔	✔	✔	✔
State of residence dummies	✔	✔	✔	✔
Year of birth dummies	✔	✔	✔	✔
Age dummies	✔	✔	✔	✔
Female dummy	✔	✔	✔	✔

Dependent variable is the log hourly wage. Cohorts born 1943–64. Number of observations is 43,883 in the QaC and 723,470 in the Micro Census. Standard errors are reported in parentheses. 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.

these data sets confirm the findings from the QaC and Micro Census. A meta estimate across the four data sets is 0.010 with a standard error of 0.08, which indicates no evidence of a negative effect of the short school years on earnings.

Various checks on the specification indicated that this finding is not due to an upward bias of the estimates. 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 observed 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%. 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 in column (1) of Table 5 (−0.006) is below the 0.1% level. If the true effect is half this size, the p-value is 4%, and it rises to 26% if the true effect is only −2 percentage points.[19] Notice that these results are only for one of the data sets used, and the one with the most negative results. Hence, it is safe to conclude that attenuation due to measurement error is very unlikely to explain 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.

[19] See Pischke (2006) for details on the design of the Monte Carlo experiment.

3.3. *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 data cover the 1990s, a period of relatively high unemployment in Germany. I present results in Table 8.

The results show a significant *positive* effect of the short school years on employment. The average employment rate in the sample is 79%, 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 behaviour 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% 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 labour market at an earlier age. Hence, they may be less likely still to be in school or university. Although only about 13% 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% level and the effect for men is basically zero. It seems therefore unlikely that there are any significant employment effects as a result of the short school years.

4. Conclusion

This article 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 article 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 existing 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

Table 8

Employment Regressions

	Cohorts affected in					
	Primary and Secondary school		Primary school	Grades 1–9		Secondary school
Cohorts	1943–64		1943–46 1957–64	1943–46 1952–64		1943–55 1961–64
Sample / Independent variable	All (1)	Men (2)	All (3)	All (4)	All (5)	All (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	✔	✔	✔	✔	✔	✔

All Estimates are from linear probability models using the Micro Census. The dependent variable is a dummy for being employed in the survey week. Standard errors in parentheses are adjusted for clusters at the track × year of birth × state level. Observations from Niedersachsen are omitted from the specification in column (5).

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 labour 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 labour market (Murnane *et al.*, 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 rather than the impact of additional grades (Card and Krueger, 1992; Lee and Barro, 2001; Wöß mann, 2003). The identification in this literature uses variation in term length across jurisdictions, which is very different from the present article. This suggests that the result in this article 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 are not much affected. If new material is studied, this will have an effect on learning and earnings. To investigate this claim further, 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.[20]

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

[20] The small existing literature on this by economists is generally favourable 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 centralised curriculum on test scores in TIMSS. A series of papers for the US examine the returns to specific high school courses, particularly mathematics. While Altonji (1995) finds only small returns to mathematics 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.

putting time in school at the centre 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 the West German states 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 labour market performance of the students.

One caveat that has to be kept in mind is that there are some students 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.[21] Another cost of shorter instructional time may be a shift away from activities, which are not directly related to labour market relevant human capital. In the working paper (Pischke, 2006), I present some results on voting and participation in arts related activities. I find some detrimental effects, although these are suggestive at best.

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Submitted: 29 March 2005
Accepted: 11 May 2006

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[21] See Jacob and Lefgren (2004) and Lavy and Schlosser (2005) for more direct evidence on this issue.

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