The Effect of Compulsory Schooling in Germany

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Abstract

Compulsory schooling laws for secondary schools changed in West German states at different times during the period 1948 to 1970. Using the addition of a further required grade in the most basic secondary schools, we estimate the impact of compulsory schooling on earnings. While our differences-in-differences design resulting from the German law changes is very similar to studies for various other countries, we find very different estimates of the returns. The estimates in the literature indicate returns in the range of 10 to 15 percent. We find no return to compulsory schooling in Germany either in terms of higher wages or employment. We conjecture that this might be due to the very different structure of the German training and schooling system.

JEL Classification J24, J31

Keywords Human capital, returns to schooling, school leaving age, ability bias

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1 Introduction

Compulsory schooling laws have been used extensively in the recent literature to estimate the returns to schooling. Starting with Angrist and Krueger (1991), this research has shown that the returns to schooling are substantial for those individuals leaving school at or near the drop-out age. The returns also tend to be higher than those found by standard OLS regressions. This finding has been replicated for many countries, raising the question whether returns to compulsory schooling are universally high.

In this paper, we investigate the returns to a change in compulsory schooling in Germany. The lowest level of German secondary school used to end after grade 8 after World War II. Soon after the war, some states started to add a compulsory 9th grade for students in this type of school. The 9th grade was introduced at various times in the different states, and it took until 1970 before it was universally in place. This creates ample within state variation to identify the effects of the introduction of the 9th grade on education and earning using a differences-in-differences strategy. The design of the German law changes is very similar to those examined by Acemoglu and Angrist (2000) for the US and by Oreopoulos (2003) for the US, Canada, and the UK.

We find that the returns to compulsory schooling are basically zero in Germany. The effects of the 9th grade introduction on earnings are precisely estimated. On the other hand, it is more difficult to determine the effect of the law change on the amount of schooling actually received by individuals in the German context. Our estimates of a one year extension in mandatory schooling for some students on the actual schooling obtained by all students range from 0.17 to 0.60 years. We argue that the true effect is likely somewhere in the middle of this range. In any case, the effects on schooling are significant, which suggests that our inability of finding an effect on earnings is not due to the fact that the change in compulsory schooling laws was ineffective. We also examine the returns for employment. These are also found to be negligible.

Why should the returns to schooling be zero in Germany, when returns in the order of 10 to 15 percent are found for other countries? The German schooling and training system differs substantially from that in other countries. Obtaining an unskilled job after the end of secondary school is not an option in Germany. Youths in Germany are still compelled to attend a vocational school, even after compulsory secondary schooling ends. Most school leavers attend vocational school part-time, in conjunction with doing a firm based apprenticeship. Hence, it may be skills learned in the apprenticeship that matter for later earnings rather than skills learned in secondary school. We discuss these and other potential explanations for the low returns after presenting the empirical results.

Angrist and Krueger (1991) were the first to use compulsory schooling laws to estimate the returns to schooling. They observed that compulsory schooling laws in the US are in terms of age, not grade. Hence, individuals who start school at a later age will be able to drop out after attending school for a shorter time. Since age at entry is determined to a large extent by the season of birth of an individual, season of birth will be an instrument for the amount of schooling obtained. This allows Angrist and Krueger (1991) to estimate the effects of compulsory schooling within a state and cohort, without making use of changes in compulsory schooling laws. They find returns in the order of 7.5 percent, not very different from the OLS estimates.

Subsequent studies typically used actual changes in compulsory schooling laws. Harmon and Walker (1995) investigate the effect of the extensions of the school leaving age in Britain in 1947 and 1973. They report much higher returns of 15 percent, which is far above the OLS estimate. These results may be questioned on the grounds that they do not control fully for cohort effects.[1]

Their results are corroborated by Orepoulos (2003), however, who exploits the comparison between England and Wales, on the one hand, and

[1] They control for survey year and a quadratic effect in age, and, hence, for a linear effect in birth cohort.

Northern Ireland, which changed school leaving ages at different times. The same paper by Oreopoulos also reports a similar analysis for state level changes in compulsory schooling laws in the US, and for province level changes in Canada. For all countries, the estimated returns are in the order of 10 to 15 percent, and typically above the OLS estimates. Acemoglu and Angrist (2000) also report estimates using changes in US compulsory schooling laws. Their estimates are only in the vicinity of 10 percent, closer to the OLS estimates, and lower than those reported by Oreopoulos (2003).

Changes in compulsory schooling in nordic countries were studied by Meghir and Palme (2003) for Sweden, and by Aakvik, Salvanes, and Vaage (2003) for Norway. The changes in both countries raised compulsory schooling from 7 grades to 9, and were implemented at different times in different municipalities. However, the extensions of compulsory schooling in both countries were part of broader reforms of the school systems, moving from selective secondary schooling to a comprehensive system, for example. This makes the interpretation of the effects as returns to schooling more difficult.

Interpreting the earnings impact of these reforms as returns to education yields sizeable estimates in both countries, despite the fact that these countries have more regulated labor markets than the Anglo-Saxon countries. Aakvik et al. (2003) report a return of 10 percent when calculating returns to a year of education in Norway. Meghir and Palme (2003) only present separate estimates for earnings and schooling. Since these are effectively reduced forms, an indirect least squares (IV) estimate of the returns to schooling can be calculated. For the full sample this yields an estimate of 0.18. However, the authors point out that the effects of the reform on schooling are concentrated on low ability individual with low education fathers, while the effects on earnings are concentrated on high ability individuals with low education fathers. In the group with low education fathers, this would imply returns of 0.09 for low ability individuals but 0.34 for high ability individuals. While these estimates are rather imprecise, they suggest that the reforms may not identify purely the returns to compulsory schooling in Sweden. Nevertheless the authors intrepret the findings for the high ability individuals with low education fathers along the lines of the high cost, high returns story suggested by Card (1999).

The previous IV studies of the returns to education for Germany by Ichino and Winter-Ebmer (1999, 2004) and Becker and Siebern-Thomas (2001) use very different instruments from compulsory schooling laws. The instrument in Ichino and Winter-Ebmer is the exposure of German cohorts to disruptions in schooling during World War II. Becker and Siebern-Thomas use the schooling infrastructure in the region where an individual grew up as instrument. Both of these instruments will pick out differences in secondary schooling. In the German context, these differences eventually tend to imply that an individual either obtains lower secondary schooling plus an apprenticeship or a higher secondary degree plus academic education. All three studies find larger returns to education than by using OLS, in the order of 10 percent.

The impact of compulsory schooling laws in Germany is therefore of substantial interest because the earnings effects differ so much from the preceding literature on this topic. They are also interesting because the IV estimates using compulsory schooling in Germany are much below the corresponding OLS estimates, in contrast to the literature surveyed by Ashenfelter, Harmon, and Oosterbeek (1999).

The remainder of this paper is organized as follows. The next section discusses the relevant aspects of the schooling and training system in Germany, while section 3 describes the datasets used. Section 4 presents the empirical results and discusses various estimation problems. Section 5 interprets the results and section 6 concludes.

2 The German schooling and traning system

Children usually start school in Germany in the year after they turn six and attend a four year primary school. After grade four, the German school system tracks students into three types of secondary schools, which distinguish themselves by the academic content of the curriculum. The lowest level or basic track of secondary school (*Hauptschule*), leads to a school leaving certificate after grade 8 or 9, although some students leave without the qualifications for the certificate. The curriculum in this school has some vocational content, and is supposed to prepare students for an apprenticeship. The middle track (*Realschule*) ends after grade 10 and is more academically rigorous than the basic track. Middle track students also typically enter an apprenticeship or a school based vocational training after finishing school. The most academic track, *Gymnasium*, leads to a university entrance exam (*Abitur*) after grade 13. Students can also leave Gymnasium after grade 12 with a degree called *Fachhochschulreife*. This allows school leavers to attend a polytechnic (*Fachhochschule*), which offer shorter and more practically oriented programs than regular universities, although often in similar fields.

Initially after World War II, about 75 percent of secondary graduates would have attended the basic track in secondary school. The higher level tracks expanded rapidly from the 1950s through the 1970s, as figure 1 demonstrates. This figure plots the shares in each school types for the cohorts born from 1925 to 1970.[2]

Students in the different secondary school tracks will attend a different school depending which track they are in. The placement in one of these types of schools tends to be permanent, and few students move between school types (particularly in the upward direction). Selection into the different types of secondary school depends on a combination of formal exams, primary school grades, recommendations by the primary school teacher, and parental choice. The exact mechanisms differ from state (*Bundesland*) to state and over time. The first two years in secondary school are often regarded as an orientation phase, allowing transitions between the different school types. Some states do not even distinguish explicitly between basic and middle school until grade 7. There is significant selection into school types by academic ability and parental background (see e.g. Dustmann,

[2] This figure was constructed using the QaC sample described below. The shares in the graph are a five year moving average.

2001). This implies that basic school students tend to be the lowest ability students, and they also face the least challenging curriculum.

In addition to the three selective types of schools, some states also offer comprehensive schools, where students can obtain all the possible school leaving degrees without making a decision at age 10 or 11. Numerically, these schools are not very important. In addition, completing the requisite grade in a higher level school typically allows students to leave school with a lower level school leaving certificate. For example, students in middle school can leave with a basic school certificate by passing grade 9.

Education policies are set by the individual states within framework agreements, which ensure that the school systems in all states remain comparable enough. Basic school used to last up to grade 8 before World War II. Hamburg was the first state to introduce a 9th grade in 1949. Various other states followed in the 1950s and 1960s. In the 1964 Hamburg Accord, an agreement struck by the prime ministers of the states, all states agreed that the basic school track should last up to 9th grade starting in 1967. A number of states introduced the 9th grade in 1967, although this was not done in Bavaria until 1970. Table 1 shows the dates for each state, as well as our assignment of the first birth cohort, which should have been affected by the change.[3]

In addition some states also started offering an optional 10th grade for basic school students, although this did not become important until the 1970s. The 10th grade in the basic track of secondary school allows students to obtain the 10th grade certificate typically received after attending the middle track. In addition, it is often chosen as an option by students who do not find an apprenticeship place after 9th grade.

After completing the basic or middle track, students typically enter a firm- or school-based vocational training course, most commonly an apprenticeship. In addition to the firm based training, an apprentice will attend

[3] In four states, which introduced the 9th grade in 1966-67, the introduction coincided with the short school years due to a transition in the start of the school year (see Pischke, 2003, for details). This is reflected in the assignments of the birth cohort.

a part-time vocational school. Compulsory schooling does not necessarily end in Germany with the completion of secondary school but extends to a part-time vocational school. Hence, school leavers cannot generally take up a job that is not part of an apprenticeship program.

The apprenticeship training is highly regulated. There are nationally agreed curricula for the apprenticeship occupations. Training firms are overseen by the chambers of commerce and crafts, who also carry out the graduation exams for the apprentices. Apprentices obtain an allowance, which differs by occupation, and is about a third of the unskilled wage. The allowance is negotiated in union wage bargains.

Apprenticeships are not at all homogeneous, and the content and quality of training will vary greatly between occupations and training firms. Apprenticeships typically lasts two to four years, depending on the occupation and prior qualification of the apprentice. The same occupation (say vehicle mechanic) may be learnt either in a small crafts firm, or in a large industrial enterprise. In the small firm, the training will be largely on-the-job, and apprentices are well integrated into the business activity of the firm. In the large firm, the first year of training may be spent entirely in a training workshop. Afterwards, apprentices may rotate to different types of jobs within the firm.

The vocational training system in Germany encompasses a wide variety of occupations. An apprenticeship may be in a traditional crafts profession (baker, cook, mechanic, hair dresser), in business and clerical occupations, sales, or in health professions (medical secretary, dental assistant). Vocational training may lead to a qualification, which would be considered a fairly low skilled occupation in most countries (child care worker or sales clerk). But it may also lead to an occupation, which would require at least some college education in other countries, particularly when it combines an apprenticeship with more advanced school based training (technician or middle manager).

Instead of pursuing an apprenticeship, school leavers may also receive vocational training in full-time vocational schools, or attend such schools in order to obtain a higher level academic credential. Those school leavers who fail to find an apprenticeship are typically channeled into various types of preparatory vocational programs, in order to give them another attempt at finding an apprenticeship the following year. School leavers very rarely take unskilled jobs right after leaving school.

3 The data

The data are taken from three data sets. The first is the Qualification and Career Survey (QaC) collected by the Institut für Arbeitsmarkt- und Berufsforschung (IAB) and the Bundesinstitut für Berufsbildung (BIBB). The QaC is a repeated cross section of employed workers of German nationality in the age group 15 to 65. We use the four waves for 1979, 1985-86, 1991-92, and 1998-99 each of which samples about 25,000 workers. The second data set comes from social security data, and is based on the IAB Employee Sample (IAB Beschäftigtenstichprobe), a 1 percent sample of social security records for 1975 to 1995. The third data set is the Micro Census, an annual survey of 1 percent of the households in Germany. We use the 1993 and 1996 surveys. The samples used below contain the cohorts living in the 10 west German states (excluding Berlin), who are born from 1930 to 1960.

The Qualification and Career Survey is appealing despite the smaller sample size because it contains the most information on schooling. 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. We assign each individual earnings equal to the bracket midpoint.[4] We then convert the variable to an hourly wage by dividing by the number of weekly hours.

[4] 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 we assigned a value of DM 7,500, in 1985-86 and 1998-99 it was DM 15,000 or more which we assigned a value of DM 17,500, and in 1991-92 it was DM 8,000 or more which we assigned a value of 12,500. Only 1.0 percent of sample observations are in the top income bracket.

German data do not typically contain a variable with the number of years of schooling or the highest grade attended. Instead, the QaC provides the year when the individual graduated from secondary school, the highest secondary school degree attained (this basically identifies the track attended), as well as comprehenisve information on post-secondary educational attainment and training. The typical approach to constructing years of schooling for Germany is to assign the usual number of years taken for an educational route. Krueger and Pischke (1995) demonstrate that OLS returns in Germany are reasonably linear in the various credentials.

The QaC does not contain information on the state where an individual went to school, only the current state of residence. Using this information, the year of birth, and the information in Table 1, we imputed whether an individual will have graduated after 8 or 9 years in the basic track. However, since the dataset contains a variable for the secondary school graduation year, we can calculate a measure of the length of primary and secondary schooling. Using the typical primary school enrollment rule (students start school in the year after they turn 6), we calculate the length of schooling as secondary school graduation year minus year of birth minus six.

The IAB Employee Sample (IAB Beschäftigtenstichprobe) 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 dataset is a panel. Once sampled, an individual is followed as long as a social security record appears for that individual. The dataset is described in more detail in Bender and Hilzdegen (1995) and Bender, Haas, and Klose (2000).

We 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 dataset.

The earnings measure provided is gross pay subject to social security contributions, and it is truncated at the social security maximum. For each cell, we know how many observations are at the maximum, and we only use cells where the fraction at the maximum is 50 percent or less. We also discard 489 cells based on a single observation. The sample used in the analysis has 12566 cells, based on 2 to 1447 observations. The mean number of observations in the cells is 192, the median is 56, and the cells are based on about 2.4 million micro records.

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. The data set only distinguishes students who graduated from the academic track of secondary school, but does not allow us to distinguish basic and middle track students.

The Micro Census is an annual representative household survey. There is no earnings measure on this dataset but it contains a variable for net monthly income, which is reported in 18 brackets. We again assinged bracket midpoints to this variable. Despite the different concepts, this variable looks very much like earnings in the QaC data for employed persons. For example, OLS returns to education are very similar. We convert the variable to an hourly wage by dividing by the number of weekly hours.

The data also provides the highest secondary school degree attained, and some basic information on post-secondary educational attainment and training. We use this again, together with the number of years usually taken to obtain a degree, to calculate a measure for years of education. Geographical information is again limited to the state of residence.

4 Empirical results

The standard approach to estimating the returns to compulsory schooling is to run a regression with the years of schooling instrumented with an indicator for the compulsory schooling regime. We start in table 2 by presenting first stage regressions for this two stage least squares problem. Recall that we do not have a direct measure of the "years of schooling" actually attended by an individual in any of our datasets. Rather our constructed measure of years of education assigns 8 or 9 grades to basic track students on the basis of their year of birth, as well as assigning the years necessary for a certain degree to the degree information. All regressions also include a dummy for female, a quartic in age, and the maximal sets of year dummies, state of residence dummies, and year of birth dummies.

Table 2 shows the resulting first stage regressions. In column (1) the total number of years of schooling and training is regressed on an indicator for whether the 9th grade in basic track has been introduced in the student's state of residence. Since the number of years of secondary schooling assigns 9 years to basic track students whenever the 9th year was in place, the first stage of this regression is rather mechanical, and the first stage coefficient should reflect the fraction of students in the basic school track. These fractions are 60.9 and 55.8 percent in the QaC and Micro Census, respectively. The coefficients in column (1) are slightly lower than this. They are closer in column (2), where the dependent variable is limited to years of primary and secondary schooling, excluding years of post-secondary education and training. The similarity of the results in columns (1) and (2) indicates that the introduction of the 9th grade in basic school did not affect participation in post-secondary schooling and training.

Column (3) shows the same regression using a measure of the length of school based on the secondary school graduation year, constructed in the QaC data. The coefficient on this measure is only 0.17. This suggests that even basic school students (60 percent of the sample) only increased their schooling by about 0.28 of a year. This indicates that the introduction of a compulsory 9th grade might not have been completely effective. However, it should be pointed out that the measure of the length of schooling used here is not what is typically used in the literature. The length measure includes grade repetition, and contains noise because some students start school only at age 7. The typical measure of years of schooling is instead based on the highest grade attended or completed.[5] There are also reasons to believe that the estimate on this measure are biased downwards, to which we will turn momentarily.

A final check in table 2 is whether the introduction of the 9th grade affected track choice. This is not the case as can be seen in column (4). In this case, it is possible to limit the sample to basic track students, since this is not an outcome which is affected by the compulsory schooling change. Column (5) shows that the first stage coefficient in this subsample for the length measure is indeed 0.28 (the coefficient in column (3) divided by the fraction attending the basic track).

In order to investigate this further, we plotted the relationship between the length of schooling and the introduction of the 9th grade in figure 2. All states are pooled, and birth cohorts are aligned with respect to the year in which the 9th grade was introduced. While a discrete jump is apparent at the time of the treatment, there is also some trend in the length of schooling variable. This trend emerges basically after the introduction of the 9th grade. It may be due to the fact that some states also introduced an optional 10th grade in the basic track, which necessitates the introduction of a 9th grade. Alternatively, the introduction of the 9th grade may not have had its complete effect immediately, since some students continued to leave the basic track after grade 8.

Figure 3 provides some information on this issue from administrative data from the four states which introduced the 9th grade in 1966. The figure plots the average grade for school leavers from 1963 to 1967.[6] It is apparent from the figure that some students attended a 9th grade already before 1966. In 1967, slightly more than 80 percent of the school leavers have 9 years of schooling, but a significant fraction is still leaving school after grade 8. This demonstrates that the change in school attendance did not happen sharply when a 9th grade was introduced for most students.

[5] An exception is Oreopoulos (2003) who uses school leaving age for the UK, a measure more similar to the length measure used here.

[6] No data are available for 1968 and 1969 because of a change in the computer system of the statistical authorities.

Instead, the change was spread over the adjacent years for some students.

Returning to figure 2, this suggests that some of the increase immdiately before and after the treatment date should be attributed to the introduction of the 9th grade. Variations in the age at school entry, grade repetition, and measurement error in the data will introduce further fuzziness in the jump around the introduction date. While the increase in the number of years of schooling at the introduction date is only about 0.2 years, it is almost half a year from 4 years before to 4 years after the introduction date. This might be a more reasonable measure of the actual effect of the 9th grade introduction.

Much of the trend, which is visible in the length of schooling in figure 2, will be removed by the introduction of year of birth effects. Figure 4 shows the same graph for the residuals from a regression on all the covariates used in the regression models. If some of this trend is due to the 9th grade introduction, the actual effect will be attenuated in the full regression. It is apparent from figure 4 that all of the upward trend in the length of schooling has indeed been eliminated, although a jump of the original magnitude is still visible at the introduction date in figure 4.

There is another reason why the effect of the 9th grade introduction on the length of schooling might be understated. The four states in figure 3 introduced the 9th grade at a time when they also shortened two school years, in order to move the beginning of the school year from spring to fall (see Pischke, 2003, for a detailed discussion of this event). Since these four states make up almost 60 percent of the sample, the 9th grade introduction will be correlated with participation in the short school years. Students affected by the short school years will report attending school for up to a year less, even though the number of grades they attended was the same. This will also attenuate the effect of the 9th grade introduction on the length of schooling.

Column (4) of table 2 addresses this issue by controlling for participation in the short school years (constructed like measure 1 in Pischke, 2003). In addition, the regression in that column eliminates the birth cohort right at the introduction of the 9th grade, as well as one cohort before and after. This should ameliorate some of the measurement problems due to the school entry age, grade repitition, and measurement error. The resulting coefficient is slightly above 0.4 now. However, this result still does not account for the possibility that some of the effect of the introduction of the 9th grade is hidden in a slowly moving trend around the date of introduction, as discussed in the previous paragraphs. This suggests that the correct effect is probably somewhere between the estimate of 0.4 and one. In any case, the results establish that there is a strong and significant first stage in the data.

Table 3 displays wage regressions for the QaC data. Column (1) presents a standard model with a quartic in potential labor market experience as controls. The return to education is about 7 percent in this OLS specification. Because the 9th grade treatment is constructed on the basis of state and year of birth, it is important to control for state and year of birth main effects in the regression, to use only the within state variation in the identification. Column (2) introduces year of birth dummies. However, controlling for experience, this is a slightly odd specification. Age is a linear combination of survey year and year of birth, and hence a linear term in age is simultaneously controlled for in this regression. Column (3) replaces the experience control with age, which makes somewhat more sense. The coefficients in these specifications are slightly lower than in column (1). This is typically the case when experience is replaced with age.

Column (4) instruments the endogenous regressor, years of education, with a dummy variable for whether the state had introduced the 9th grade in basic track for the respective birth cohort. The coefficient now drops basically to zero, and is insignificant. Column (5) and (6) probe these results by adding a state specific cohort trend and its square to the regression. The returns to schooling estimates increase slightly, to 2 and 3 percent, with the latter being significant. These returns are still small, although it is possible that they are understated if years of education increased by less than one for affected basic track students.

These estimates are fairly precise. Standard errors are calculated using the cluster-command in Stata, allowing for correlations within state and year of birth cells, the level of the treatment. Standard errors are in the order of 0.01, which means that the confidence intervals exclude even modestly positive returns. The precision of the estimates is comparable to the returns to schooling estimates for the UK by Harmon and Walker (1995) and for the US, Canada, and the UK by Oreopoulos (2003).

Table 4 shows reduced form estimates for the introduction of the 9th grade for all three datasets. Here, the wage is regressed directly on the dummy for the 9th grade introduction. For the QaC and Micro Census data, these are standard OLS regressions. The social security earnings are truncated at the taxable maximum. We deal with the truncation by using an estimator similar to Powell's (1984) censored least absolute deviations estimator. Chamberlain (1994) suggested a version of this estimator for cell level data. It amounts to estimating a linear regression on the cell medians which are not subject to truncation, using the cell sizes as weights. The standard errors are calculated using Stata's aweight option, and clustering the data again at the level of the state and year of birth. This accounts nonparametrically for the serial correlation in earnings across cells due to the fact that the same individuals are sampled repeatedly over time.[7]

The estimates for all three datasets are basically zero. The effect is identified via a differences-in-differences design, because state and year of birth effects are controlled for. It is always a worry in models like this that other state specific trends might be correlated with the treatment. For the social security data and the Micro Census, the estimates change little when state specific trends and squared trends are included in the regression. The QaC is the only dataset where a slightly positive and significant effect emerges with squared state specific trends. Nevertheless, there is a suggestion that the estimates of the 9th grade effect are slightly higher in all the datasets once state specific trends are included. There is often a worry that changes

[7] We found in Monte Carlo experiments that this estimator for the standard errors works very well in practice for our data design. See the appendix of Pischke (2003) for a comparison with a more parametric estimator of the covariance matrix.

in compulsory schooling laws may be correlated with other changes education policies or the behavior of individiuals, which may bias the effects of compulsory schooling upwards (see, for example, Lleras-Muney, 2002). The pattern in table 4 suggests the exact opposite: the introduction of the 9th grade in Germany was, if anything, negatively correlated with other state specific trends in wages.

An alternative way of estimating the effects of the introduction of the 9th grade is to focus on basic school students. Limiting the sample by education is valid since the 9th grade treatment does not affect the track attended directly. Table 5 shows analogous reduced form regressions to table 4. The results are again clustered around zero. Interestingly, there is less evidence among this directly affected subgroup of any positive wage effects even when square state specific trends are included. Figure 5 shows these results graphically for the QaC data in an analogous way to the first stage relationship in figure 4. There is no evidence of a sharp change at the date of the introduction of the 9th grade, and wage residuals fluctuate in a relatively narrow band around zero.

One concern with limiting the sample to basic track students is that this is a seleced sample. Because the share of students in the basic track falls over time (figure 1), and this decline happens at different rates in different states, the ability composition of the sample changes over time. We have tried to address this selection, in the spirit of Gronau (1974), by including a controls for the fraction of a birth cohort in the state who attended a higher level track but this had no effect on the estimates. We have also experimented with limiting the sample to men, but the results were again fairly similar.

A possible explanation for the lack of wage returns might be that the wage setting institutions in Germany prevent the necessary adjustments to reflect any returns. If this is the case, but 9th grade graduates are indeed more productive, the effects might show up in employment effects instead. Table 6 explores this possibility by replacing the dependent variable with a dummy variable for whether the respondent is employed, and a dummy variable for whether the respondent is in the labor force. The results are largely negative, as the employment effects are also zero. There is some evidence of employment effects when linear state specific trends are included, in which case the estimate is marginally significant. There are similar differences in participation. Nevertheless, these estimates are not very robust to slight changes in the specification.

Our estimates should be attenuated because we do not observe the exact state were an individual attends school. In order to address this issue, we looked at data from a further data set, the ALLBUS. It is a biannual cross sectional of Germans age 18 and older, with a supplemental survey after German unification in 1991. The 1991, 1992, 1994, and 2000 waves contain both the state of birth and the state of residence of a respondent. In addition, respondents were asked how long they have lived in their current state of residence.

Table 7 uses these data to present some evidence on mobility. Around 80 percent of respondents are living in their state of birth. The results differ slightly depending on whether this is calculated based on the question on the state of birth and the length of residence in the current state. The discrepancy is likely due to individuals who moved out of their state of birth but returned later. The table also demonstrates that there is relatively little difference between being in your current state since birth and since age 12 or 18. This indicates that most mobility takes place at a later age, and hence that state of birth is a better indicator for the state of school attendance than the state of residence (although it is not perfect). The last row shows that 85 percent of respondents have lived in their state since before the introduction of the 9th grade in that state. The fractions are even higher among basic track students, who tend to be less mobile.

Suppose the true indicator for the introduction of the 9th grade for an individual is D_i^*, and the indicator constructed on the basis of the state of residence D_i. If mobility between states is random, the effect of using D_i instead of D_i^* is pure attenuation of the estimates. The coefficient from a regression of D_i^* on D_i measures the attenuation factor. Table 8 presents regressions of this type using the assingment based on state of birth and state of residence. The regressions also include the other covariates used in the regression models above. The attenuation factor is about 0.80 in the full sample and 0.87 for the basic track students. This indicates that the coefficient estimates for the full sample should be scaled up by a factor of about 1.25 to make them equivalent to estimates using state of birth for the assignment, as is done, for example, in Oreopoulos (2003).

The largest estimate we have found in the reduced forms was 0.019 in column (3) of table 4. Scaling this up by 1.25 to account for the attenuation due to not knowing state of birth raises this to 0.024. Furthermore, the discussion of table 2 and other evidence suggested that the affected students did not all receive a full additional year of schooling. Instead, we argued that the effect is likely to be somewhere between 0.4 and 1 for basic school students. Using the midpoint 0.7, and multiplying this by 0.6, the fraction of basic school students, would give a two stage least squares estimate of the return to schooling of about 0.057. This is still a fairly small return and well below the 15 percent return reported by Harmon and Walker (1995) and Oreopoulos (2003) for the Anglo-Saxon countries. However, recall that none of the other specifications and datasets yielded estimates nearly as large. The preponderance of evidence presented here suggests that the return to compulsory schooling in Germany is small and possibly zero, various sources of attenuation notwithstanding.

5 Interpretation

Why are the returns to compulsory education zero in Germany while they are large in the other countries where these effects have been studied? In this section we turn to a discussion of various explanations for our finding. Unfortunately, it is rather difficult to distinguish some of these various explanations empirically, and we make no real attempt in doing so here.

The first possibility is related to the fact that the specifications we estimate identify the social, rather than the private returns to education (see the discussion in Acemoglu and Angrist, 2000). If the external returns are negative in Germany, this might explain the low social return even if the private return is positive. The comparison of tables 4 and 5 speaks to this issue, since the external returns are likely to show up for students outside the basic track. However, the estimates in the two tables are very similar. Hence, students in other tracks were not affected by the introduction of the 9th grade in basic track (and running the same regressions only for students in higher tracks corroborates this). This casts doubt on any explanation based on the external returns to schooling.

The most commonly advanced explanation for high returns to compulsory schooling has focused on the idea that there is heterogeneity in the underlying returns to schooling. In this case, IV estimates will result in larger returns than OLS estimates if the instrument picks out variation among high return sub-groups of the population (see Imbens and Angrist, 1994, and Card, 1999). One interpretation of the empirical results is that high costs of schooling prevent some low education but high return individuals to continue in school, and there is some empirical evidence for this interpretation (Card 1995, 2000).

Possible reasons for high costs of schooling among drop-outs may be credit constraints, psychological costs of schooling, myopia, or special circumstances, like health or pregnancy. The credit constraints story implies that students and parents cannot borrow against their future human capital, and the constrained households give up the returns to schooling for a smoother consumption stream. Psychological costs might be high for 14 or 15 year olds, who dislike school a lot. Hence, these individuals discontinue schooling rationally, even though they realize they are giving up large returns. Mypoia implies that the school leavers do not value the future returns to schooling enough compared to the current gains from dropping out.

All these alternatives may makes sense if individuals have to take a decision at age 14 or 15 on whether to stay in school or to drop out. The German schooling and training system presents individuals with a rather different set of alternatives, however. The decision to leave school early is not taken at age 14 or 15, but effectively at age 10 or 11 when students have to choose a secondary track. The track chosen then largely determines whether the student leaves school after 8 or 9, 10, or 13 years in school. Once a student chose to stay in the basic track, that student has no alternative to leaving school after grade 8 as long as the state has not introduced a 9th grade. Moving up to a higher track is virtually impossible at that stage.

Return to the various reasons for high costs of schooling, and apply these to the German case. Except for the case of explicit credit constraints, students at age 10 or 11 or their parents might think of the school leaving decision at 14 or 15 very differently, than they would four years later. Whether the family can finance the schooling of its children easily can presumably be forseen a few years in advance. On the other hand, explicit credit constraints are probably not the most likely explanation for why school leavers face high costs of schooling. The psychological costs of schooling for a 14 year old, on the other hand, are difficult to assess for a pre-teen. Schooling decisions at 10 or 11 are most likely made by the parents anyway, who might discount these costs. Similarly, a myopic student would think differently about dropping out at 14 at age 10, then when the drop out age is actually being reached. Special circumstances, like health problems or teen pregnancies are also not necessarily easy to forecast.

These stories therefore suggest that Germans may not make their track decisions based on the same reasoning as drop-outs in other countries. Basic track students are likely to be lower ability students, and may have lower returns than many of the students in the other tracks. By grade 8 their returns may be close to zero. So far, the story would be relatively consistent with the peculiarities of the German school system.

The problem with this reasoning is that many of the high return students, who drop out in other countries, find themselves in the middle or academic secondary track in secondary school in Germany. But dropping out after grade 8 or 9 of these tracks is possible if the student wants to. Whatever reason causes their peers in other countries to drop out early should make similar German students also leave school early. However, we do not see much dropping out of the higher tracks once this becomes possible. To the degree that some such drop-outs exist in Germany, and to the degree that the introduction of the 9th grade in basic school made them stay a year longer in a higher track, we should see any earnings effects on these students in the reduced form results for all students in table 4. The fact that there is no evidence of any returns for any students suggests that this story does not seem to be consistent with the German data.

A further possibiltiy is that the effects of the introduction of the 9th grade were indeed positive, but our research design is unable to pick up the effect. One reason might be the wage setting process in Germany. Wages, particularly for apprenticeship trained workers, are frequently set through collective bargaining agreements. There is pattern bargaining in Germany, so that the collective agreements for one sector and region often set a pattern for other sectors and regions. In fact, the unions try fairly explicitly to avoid the emergence of regional wage dispersion. This means that it may not have been possible for positive state level wage differentials to emerge for the students in states which introduced the 9th grade earlier. Instead, effects might have emerged slowly at the national level, and these effects would be absorbed by the cohort or time effects in our specifications.

Alternatively, it may not be schooling per se which translates into additional human capital relevant in the labor market for basic track students in Germany. Instead, the additional knowledge acquired in school is being mediated by the apprenticeships, which most basic school leavers complete. It is the human capital created by the apprenticeship, which eventually matters for labor market outcomes. However, apprenticeships are regulated at the national level, and any updating of the regulations, which would accommodate the additional skills of school leavers, would show up at the national level again. Either of these channels could explain a zero return estimate, even if the true return is positive. Of course, it would be extremely difficult to disentangle this effect empirical from any other time series change happening concurrently.

The apprenticeship system in Germany might well be key to the explanation, even if the true returns to the 9th grade are zero. One simple view would be that the introduction of the 9th grade does not really change the "highest grade completed" in Germany, if this is an apprenticeship. A successful apprentice will in essence have the same credential with 8 or 9 grades of school. Hence, the German experiment is very different from forcing a US high school dropout to complete an additional grade, and a zero return in Germany is therefore not surprising.

Of course, it cannot be possible that it does not matter at all how many grades of primary and secondary school children attend before entering an apprenticeship. Presumably, someone leaving school after 2nd grade may be quite ill prepared to start an apprenticeship (apart from the lack of maturity). Hence, the implicit claim in this reasoning is that students do not learn labor market relevant skills in the 9th grade in basic track in Germany, while they do learn such skills in other countries like the US, Canada, or the UK. This may be the case because the skills relevant for school leavers at that age are basically reading, writing, and arithmetic. The German school system may do a better job in teaching these skills earlier than school systems in other countries.

Table 9 sheds some light on this issue. It presents some results from the 1994-98 International Adult Literacy Survey (IALS), taken from OECD (2000). The IALS is designed to assess the literacy skills of the adult population in a variety of countries. The survey tasks are used to construct three literacy scales for prose, document, and quantitative literacy. The survey uses a 0 - 500 scale, and a score of 225 on any of the scales denotes a very rudimentary mastery of the category. For example, a quanitative IALS task associated with a score of 225 asks the respondent to fill in the total costs with handling in an order form. The previous two lines in the form give the cost of the item of 50, and the handling charge of 2. There is no other distracting information.

Table 9 shows the quantitative scores at the 5th and 25th percentiles of the distributions for the countries, for which studies on compulsory schooling exist. The scores at these low percentiles are probably most relevant to assess the knowledge of students who are likely at the margin of dropping out of school early. Most relevant for our purpose is the age group 56-65, since the compulsory schooling law changes date back far enough in most countries, that younger individuals would not be affected. However, the sample sizes in the IALS are not particularly large (a few thousand per country), and we report the results for the whole sample as well. Table 9 reveals the well known result that Germans at the lower end of the ability distribution score relatively highly compared to other countries. This is particularly apparent at the 5th percentile. Similar results would be obtained using the IALS prose and document scores.

Of course, in order to infer the quality of schooling across countries in the 1950s and 1960s, it might be more informative to look at data on students during this time directly, rather than the reports on the IALS, which was carried out with adults many years later. The earliest international comparison test, which contains relevant information for our case is the First International Mathematics Study (FIMS) in 1964. This test was conducted for 13 year olds and in the final grade in secondary school in ten countries. In table 10, we report some results for 13 year olds in four relevant countries: Germany, the US, England, and Sweden. The means are from scaled scores, i.e. scores that have been converted to an international distribution with a mean of zero and standard deviation of one. Again, it can be seen that German students score better than those from other countries, particularly those from the US but also Sweden. More interesting than the mean is again the distribution at the lower end. Unfortunately, the report on the FIMS (Husén, 1967) does not report percentile scores. Instead, we constructed the distribution for particpants who answered up to 20 out of 70 items correctly. As can be seen from the table, the 25th percentile of the German distribution is somewhere between 15 and 20 correct answers, while it is between 5 and 10 correct answers for the other three countries.

These results corroborate those from the IALS. Interestingly, Swedish students performed relatively poorly on the FIMS, even though Swedish respondents do as well as Germans on the IALS. However, even in the IALS, the 5th percentile scores in Sweden and Norway were slightly below the scores for Germans among the 56 - 65 year old group. This might indicate that German students were comparatively better prepared in basic skills even than those in these nordic countries during the relevant period.

Hence, we conclude that the likely explanation for the absence of any returns to compulsory schooling in Germany are to be found in the institutional features of the German schooling and training system or labor market. Unfortunately, we are unable to determine whether the true returns are positive, but the structure of the German apprenticeship system or wage setting process will tend to make any wage returns only show up at the national level, or the true returns are indeed zero because 9th graders in Germany do not learn relevant skills in school anymore.

6 Conclusion

While previous estimates of the returns to compulsory schooling are typically in the range of 10 to 15 percent, we find that the returns to an additional grade in Germany are basically zero. We establish this result using three large data sets, and the results are all fairly consistent and precisely estimated. It is more difficult in the German case to pin down exactly how many grades the relevant population attended. It is clear from the data, however, that the compulsory schooling change was effective for a large number of students, and there is no doubt that we would uncover earnings effects of the usual magnitude in our data. We also rule out that attenuation bias might explain our results because we only know the state of residence and not the state in which an individual went to school.

It is much more difficult to gain an understanding of the reasons why the German results differ so much from those found for other countries. We have argued that the most likely explanation lies in the fact that the German schooling and training system differs substantially from that in other countries. In particular, the importance of the German apprenticeship system may imply that skills learned in school are not as relevant for school leavers as they are in other countries. Moreover, German students may have a better command of the basic skills which matter for them then their peers abroad. However, we are unable to distinguish this explanation from one where the returns to schooling are actually positive but we are unable to estimate them. This might be the case if any returns only show up at the national level, either through the wage setting process or through changes in the apprenticeship system.

In either case, our results are of interest because they are in such stark contrast to the existing literature. There is essentially no evidence to date on the causal effect of schooling in the German speaking countries with an important apprenticeship tradition. The existing IV studies by Ichino and Winter-Ebmer (1999, 2004) and by Becker and Siebern-Thomas (2001) focus on the differences between the various secondary tracks. Therefore, these studies have little to say about the effects of schooling for the students within the lower track.

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Figure 1
Shares of Students in Basic, Middle, and Academic Track of Secondary School

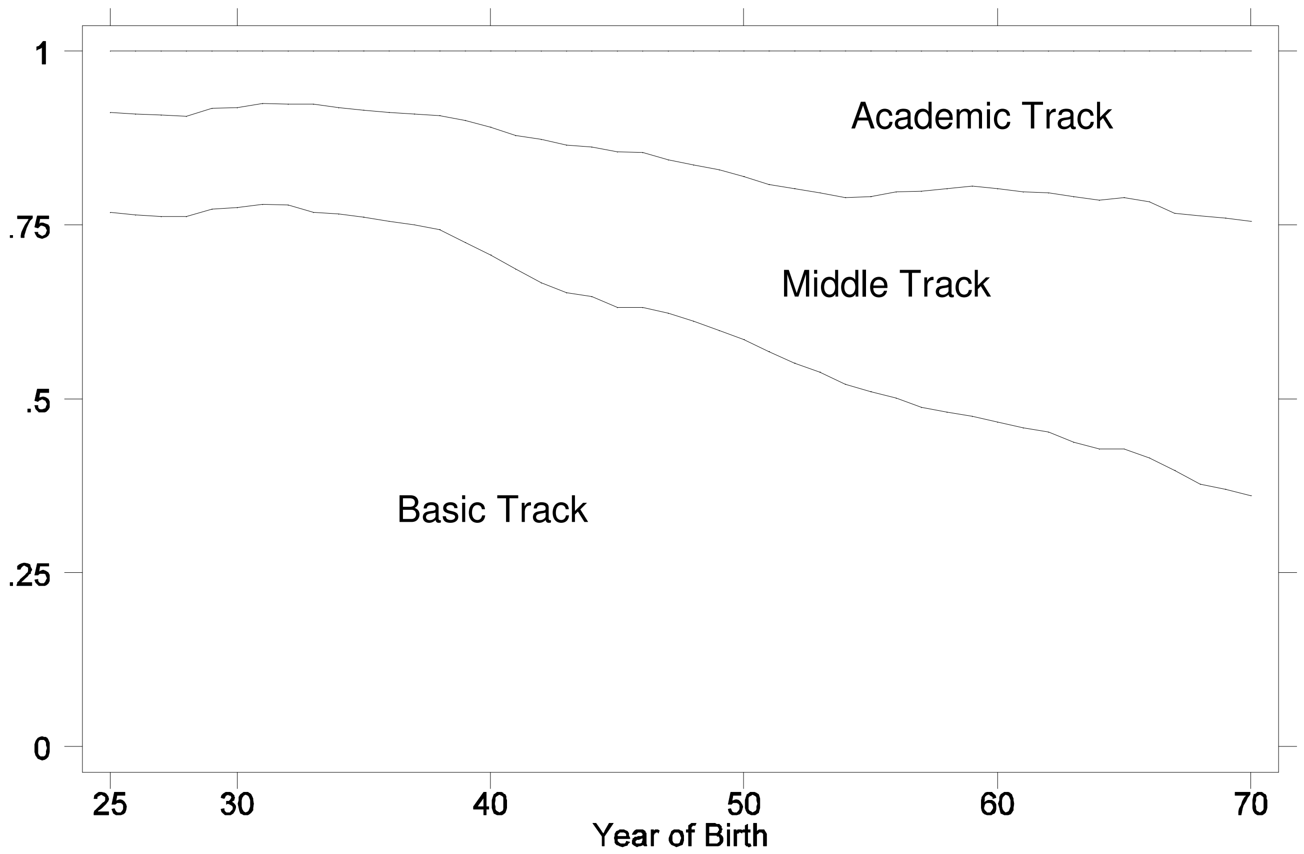


Figure 2
Number of Years Spent in School by Basic Track Students

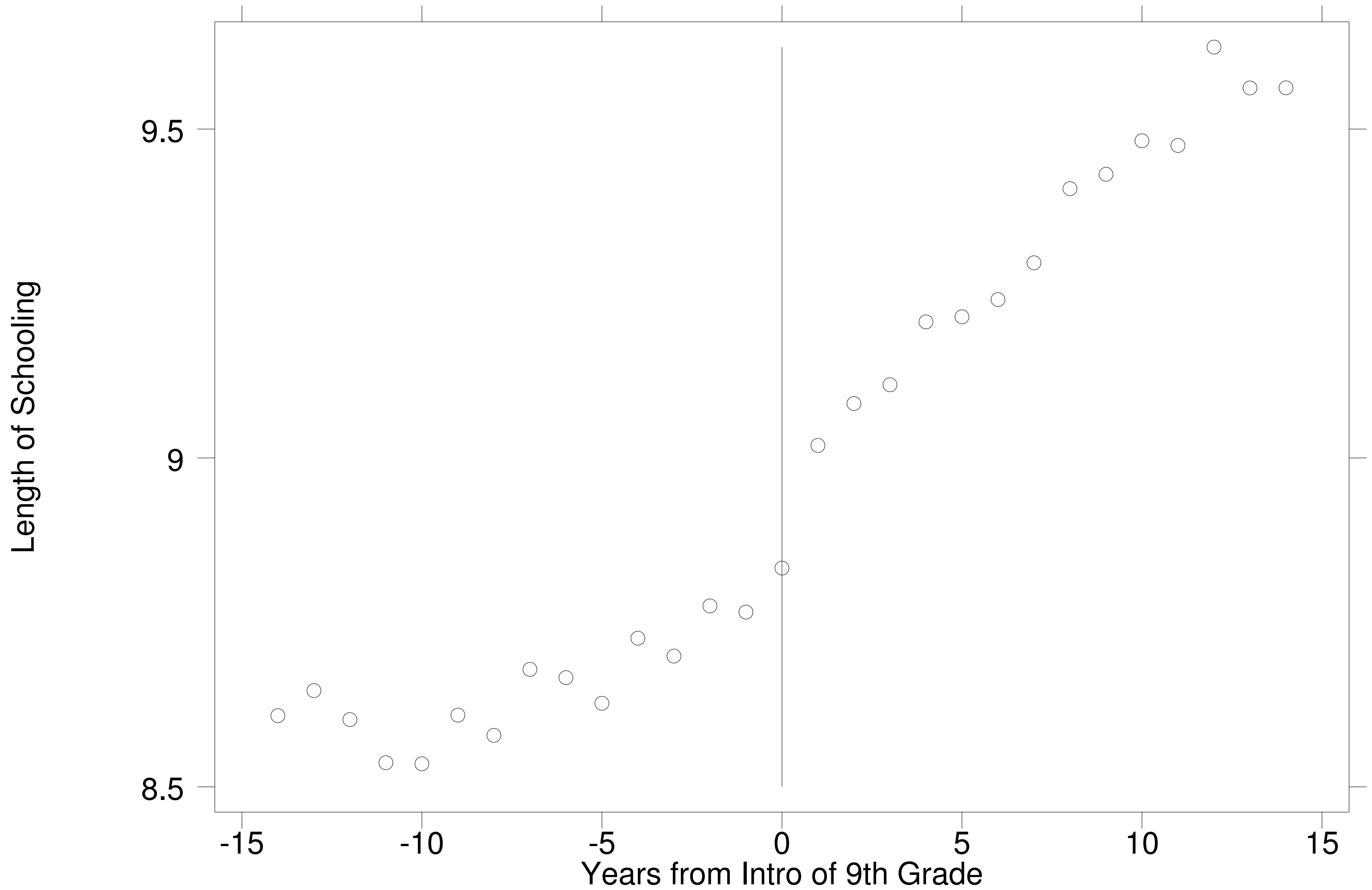
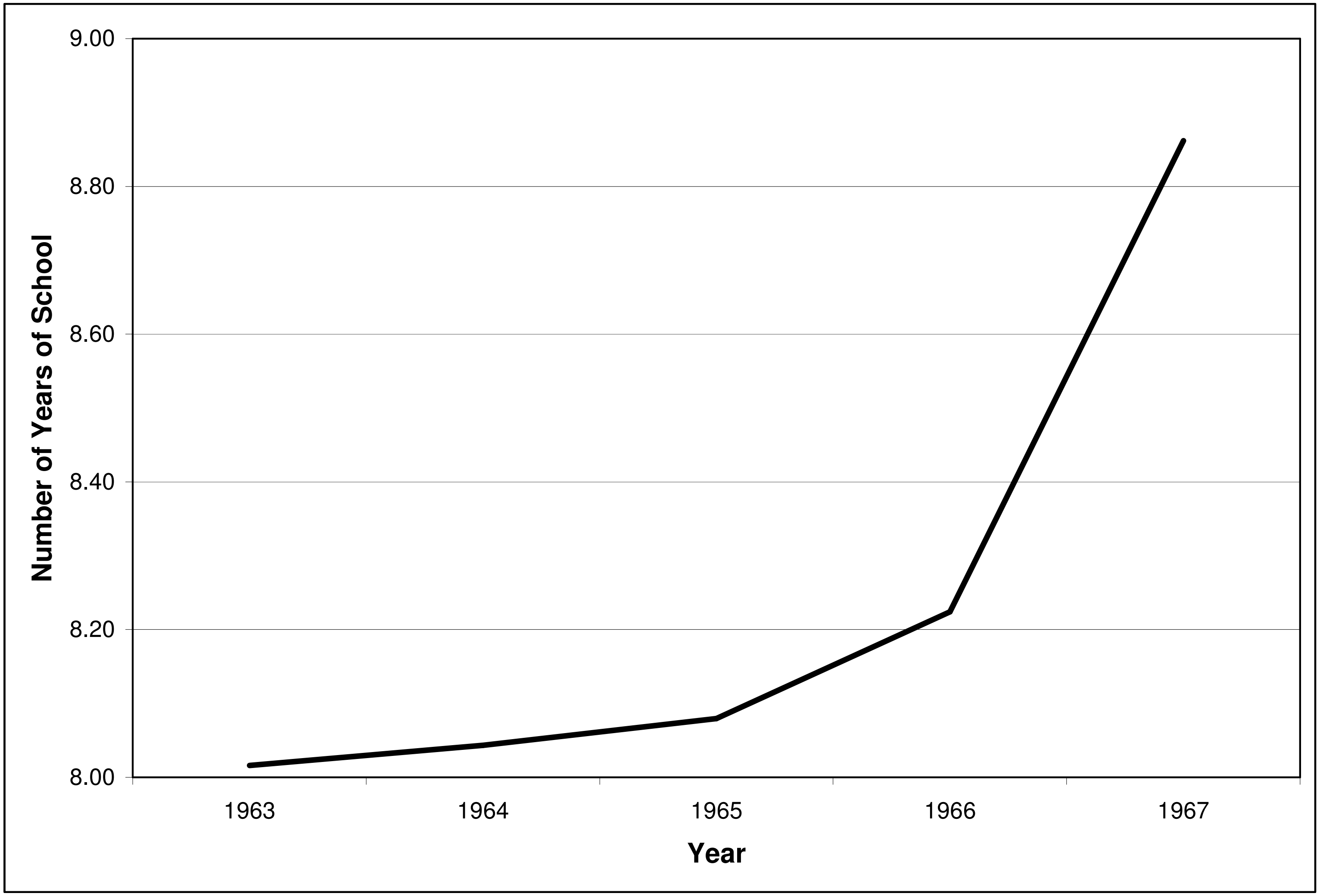


Figure 3
Average Number of Grades Attended by School Leavers in NRW, Hessen, RPF, and Baden-Württemberg



Note: 9th Grade Was Formally Introduced in 1966

Figure 4
Number of Years Spent in School by Basic Track Students
(After Removing Gender, Age, Year of Birth, Survey Year, and State Effects)

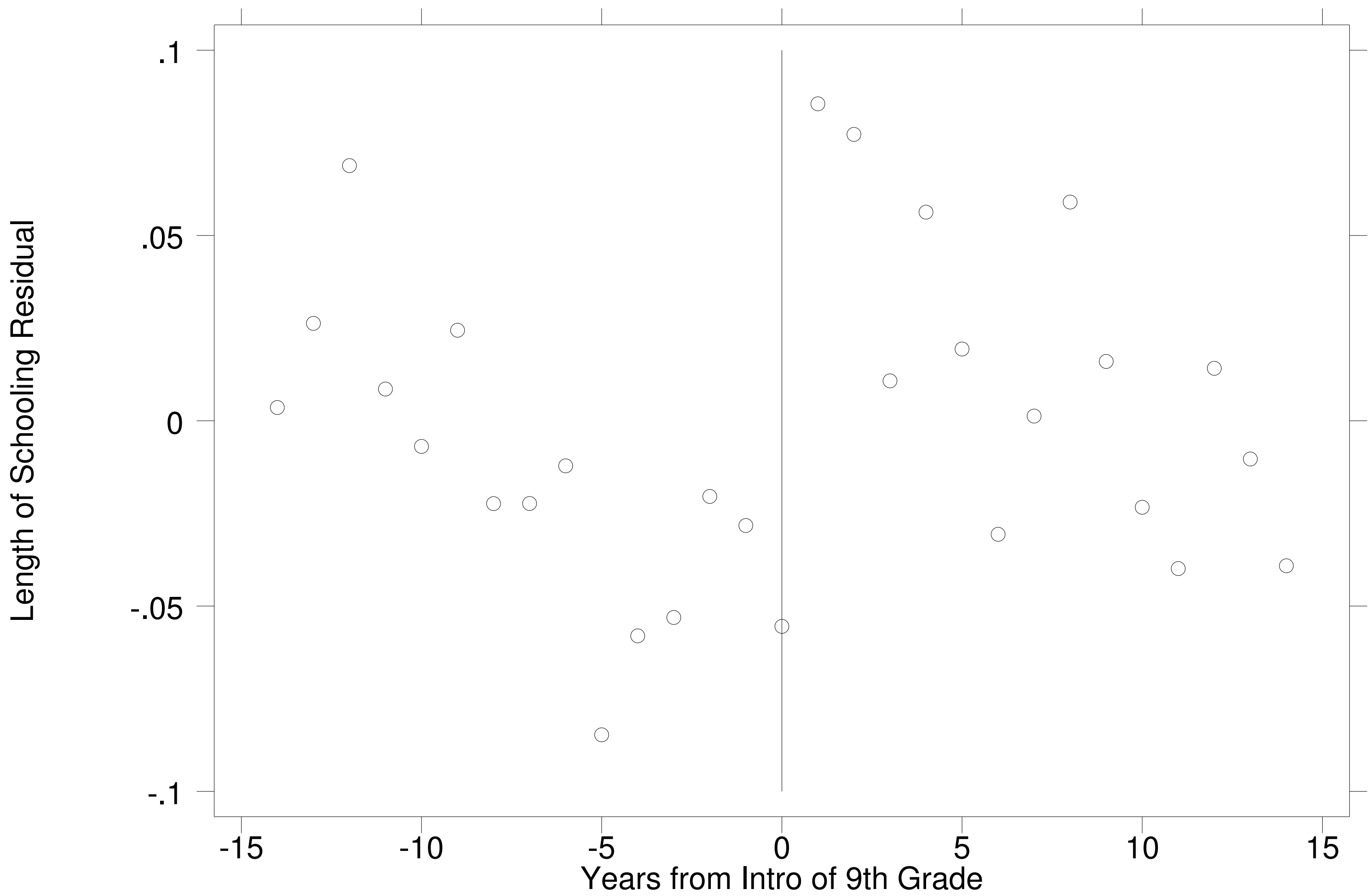


Figure 5
Log Wage of Basic Track Students
(After Removing Gender, Age, Year of Birth, Survey Year, and State Effects)

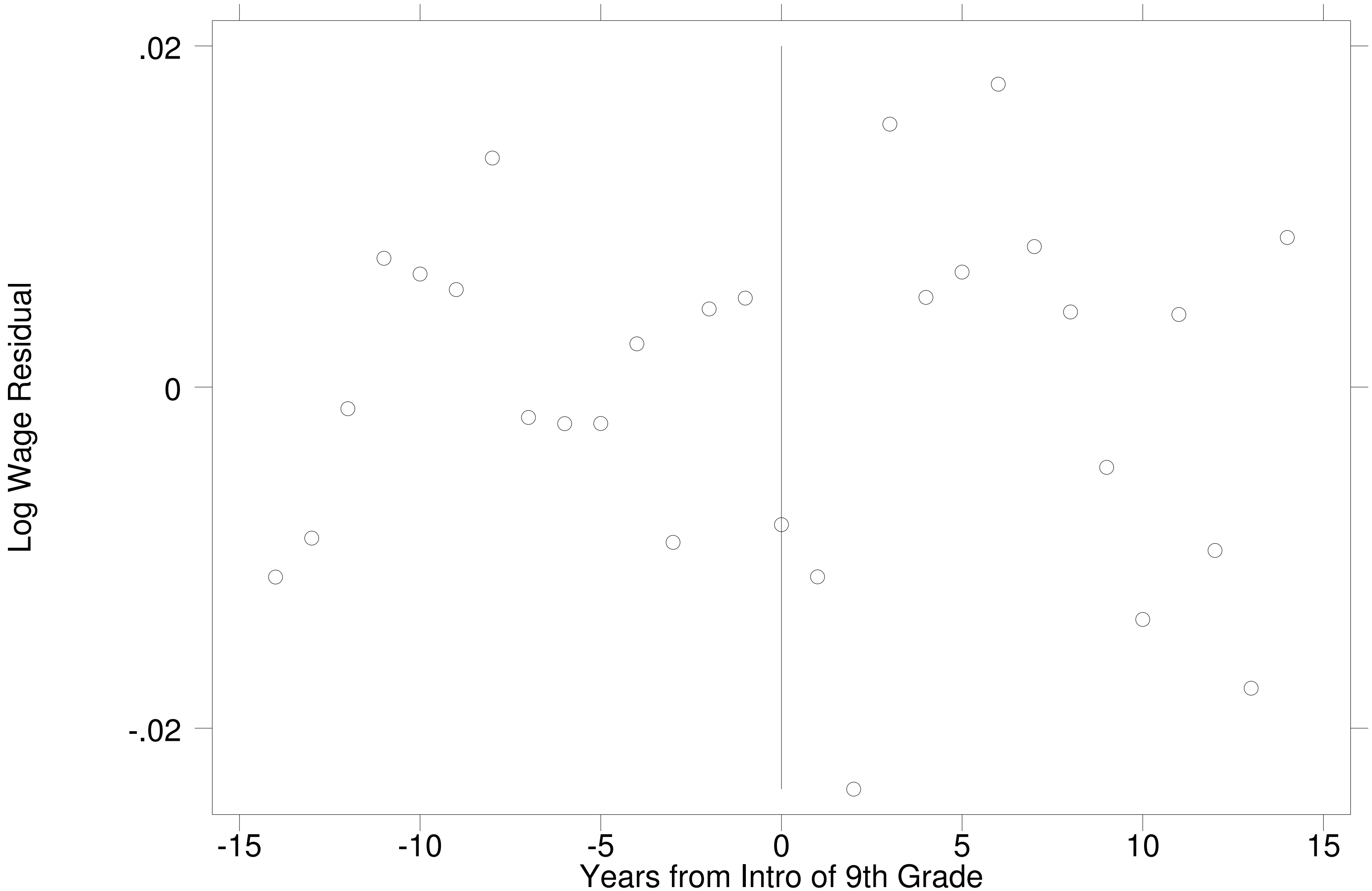


Table 1
Introduction of 9th Grade in Basic Track of Secondary School

State (Bundesland)	First Year when all Students are Supposed to Graduate after 9 Years	First Birth Cohort with 9 Years of School
Schleswig-Holstein	1956	1941
Hamburg	1949	1934
Niedersachsen	1962	1947
Bremen	1958	1943
Nordrhein-Westphalen	1967	1953
Hessen	1967	1953
Rheinland-Pfalz	1967	1953
Baden-Württemberg	1967	1953
Bayern	1969	1955
Saarland	1964	1949

Table 2
First Stage Regressions
(Standard Errors in Parentheses)

	Full Sample				Basic Track Only	
Dependent Variable	Years of Education	Years of School (primary + secondary)	Length of Schooling (primary + secondary)	Attends Basic Track	Length of Schooling (primary + secondary)	Length of Schooling (primary + secondary)
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Qualification and Career Survey						
Dummy for Cohort with 9th Grade in Basic Track	0.569 (0.051)	0.588 (0.034)	0.171 (0.040)	-0.015 (0.011)	0.280 (0.040)	0.424 (0.039)
Number of Observations	54126	54126	54126	54126	32970	29473
Micro Census						
Dummy for Cohort with 9th Grade in Basic Track	0.476 (0.034)	0.490 (0.022)	---	0.010 (0.006)	---	---
Number of Observations	183094	183094	---	183094	---	---
Short School Year Dummy						✓

Note: All regressions estimated by OLS and also include a dummy for female, a quartic in age, and the maximal sets of year dummies, state of residence dummies, and year of birth dummies. The sample in column (6) excludes observations one year before, the year of, and one year after the switch to 9th grade in basic track. The standard errors are adjusted for clusters at the state * year of birth level.

Table 3
Log Wage Regressions
Qualification and Career Survey
(Standard Errors in Parentheses)

Independent Variable	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Years of Education	0.072 (0.001)	0.064 (0.005)	0.061 (0.001)	0.007 (0.013)	0.019 (0.012)	0.032 (0.013)
Female Dummy	✓	✓	✓	✓	✓	✓
Quartic in Experience	✓	✓				
Quartic in Age			✓	✓	✓	✓
Year Dummies	✓	✓	✓	✓	✓	✓
State of Residence Dummies	✓	✓	✓	✓	✓	✓
State Specific Cohort Trends					✓	✓
State Specific Cohort Trends Squared						✓
Year of Birth Dummies		✓	✓	✓	✓	✓

Note: Number of observations is 54126. Years of education is instrumented with a dummy for cohorts with a 9^{th} grade in basic track in the state in columns (4) to (6). Standard errors are adjusted for clusters at the state * year of birth level.

Table 4
Reduced Form Regressions
Dependent Variable: Log Wage
(Standard Errors in Parentheses)

Independent Variable	(1)	(2)	(3)
	Qualification and Career Survey		
Dummy for Cohort with 9th Grade in Basic Track	0.004 (0.008)	0.010 (0.008)	0.019 (0.008)
	Social Security Data		
Dummy for Cohort with 9th Grade in Basic Track	-0.003 (0.005)	0.005 (0.004)	0.004 (0.005)
	Micro Census		
Dummy for Cohort with 9th Grade in Basic Track	-0.002 (0.005)	0.003 (0.004)	0.001 (0.005)
State Specific Cohort Trends		✓	✓
State Specific Cohort Trends Squared			✓

Note: All regressions estimated by OLS and also include a dummy for female, a quartic in age, and the maximal sets of year dummies, state of residence dummies, and year of birth dummies. The standard errors are adjusted for clusters at the state * year of birth level. Regressions for the Social Security data are OLS regressions for the cell level median of the log wage. Number of observations is 54126 for the Qualification and Career Survey, 12566 for the Social Security data, and 183094 for the Micro Census.

Table 5
Reduced Form Regressions
Dependent Variable: Log Wage
Basic Track Students Only
(Standard Errors in Parentheses)

Independent Variable	(1)	(2)	(3)
Qualification and Career Survey			
Dummy for Cohort with 9th Grade in Basic Track	-0.013 (0.010)	-0.001 (0.010)	0.010 (0.011)
Micro Census			
Dummy for Cohort with 9th Grade in Basic Track	-0.005 (0.005)	-0.003 (0.005)	-0.007 (0.007)
State Specific Cohort Trends		✓	✓
State Specific Cohort Trends Squared			✓

Note: All regressions estimated by OLS and also include a dummy for female, a quartic in age, and the maximal sets of year dummies, state of residence dummies, and year of birth dummies. The standard errors are adjusted for clusters at the state * year of birth level. Number of observations is 32970 for the Qualification and Career Survey, and 102282 for the Micro Census.

Table 6
Reduced Form Regressions for Employment and Participation
Micro Census
(Standard Errors in Parentheses)

Independent Variable	(1)	(2)	(3)
Dependent Variable: Employed			
Dummy for Cohort with	0.002	0.008	0.002
9^{th} Grade in Basic Track	(0.004)	(0.004)	(0.004)
Dependent Variable: In the Labor Force			
Dummy for Cohort with	0.007	0.014	0.007
9^{th} Grade in Basic Track	(0.004)	(0.004)	(0.004)
State Specific Cohort Trends		✓	✓
State Specific Cohort Trends Squared			✓

Note: All regressions estimated by OLS and also include a dummy for female, a quartic in age, and the maximal sets of year dummies, state of residence dummies, and year of birth dummies. The standard errors are adjusted for clusters at the state * year of birth level. Number of observations is 310975.

Table 7
Percentage of Respondents Who Have Lived in Current State Since Specific Age or Time
ALLBUS

Has Lived in Current State Since	Sample: Full Sample	Only Basic Track Students
	(1)	(3)
Birth (State of Birth)	82	88
Birth (In State Since)	76	81
Age 6	79	85
Age 12	81	86
Age 18	83	89
Before Introduction of 9th Grade	85	91

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 2590 for the full sample (2436 for the first row), 1411 for basic track students (1331).

Table 8
Attenuation in the Education Regressor Due to Mobility
ALLBUS
(Standard Errors in Parentheses)

	Dependent Variable: Dummy for 9th Grade (Based on State of Birth)		
	Sample		
	Full Sample		Basic Track Students Only
Independent Variable	(2)	(3)	(4)
Dummy for Cohort with 9th Grade in Basic Track (Based on State of Residence)	0.802 (0.028)	0.809 (0.023)	0.866 (0.031)
Female Dummy	✓	✓	✓
Quartic in Age	✓	✓	✓
Year Dummies	✓	✓	✓
State of Residence Dummies	✓	✓	✓
State of Birth Dummies		✓	
Year of Birth Dummies	✓	✓	✓
Number of Observations	2436	2436	1331

Note: All regressions estimated by OLS. Standard errors are adjusted for clusters at the state * year of birth level.

Table 9
Quantitative Test Scores from the 1994-98 International Adult Literacy Survey

	Age 16 – 65		Age 56 – 65	
	Percentile			
Country	5th	25th	5th	25th
USA	138	237	143	235
Canada	155	247	122	192
UK	142	231	123	205
Germany	218	265	207	252
Sweden	216	276	188	250
Norway	209	269	182	245

Note: Scores range from 0 to 500. Source: OECD (2000), tables 2.1 and 3.4

Table 10
Scores on the First International Mathematics Study
Results for 13 Year Olds

	Country			
	Germany	US	England	Sweden
Mean Total	0.16	-0.35	0.05	-0.52
Mean Basic Arithmetic	0.28	-0.19	-0.19	-0.40
Correct Answers	*Percent of Students*			
0	0	2	4	1
5 or less	4	18	19	17
10 or less	10	32	29	37
15 or less	21	46	40	55
20 or less	34	60	49	70

Note: The reported means are from a scaled score distribution (international mean of 0, standard deviation of 1). Source: Husén (1967), tables 1.2 and 1.8