Zero Returns to Compulsory Schooling in Germany: Evidence and Interpretation

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Abstract

We estimate the impact of compulsory schooling on earnings using the changes in compulsory schooling laws for secondary schools in West German states during the period from 1948 to 1970. While our research design is very similar to studies for various other countries, we find very different estimates of the returns. Most estimates in the literature indicate returns in the range of 10 to 15 percent. We find no return to compulsory schooling in Germany in terms of higher wages. We investigate whether this is due to labor market institutions or the existence of the apprenticeship training system in Germany, but find no evidence for these explanations. We conjecture that the result might be due to the fact that the basic skills most relevant for the labor market are learned earlier in Germany than in other countries.

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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, irrespective of national labor market institutions or schooling systems. However, little is known about how these institutional factors influence the returns to schooling.

In this paper, we investigate the returns to a change in compulsory schooling laws 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 German law changes give rise to a research design that is very similar to that employed 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 estimate of a one year extension in mandatory schooling for some students on the actual schooling obtained by all students is only 0.17 years, although about 60 percent of students should be fully affected. We argue that this small estimate reflects partly the fact that the 9th grade introduction was not completely sudden in all states, as well as attenuation due to measurement error in our schooling variables. However, the ratio of the two estimates results in the standard instrumental variables estimate, which is not subject to measurement error. Our final estimates can safely rule out any returns of the size of the OLS estimates.

Why should the returns to schooling be zero or small in Germany, when returns in the order of 10 to 15 percent are found for other countries? We discuss a variety of potential explanations for our results. One possibility is that the low returns are due to rigid wages in Germany. We look at effects on employment and on the self-employed in order to examine this possibility, but we find no support for this idea. Another potential explanation is based on the role of the German apprenticeship training system, which clearly plays an important role in providing labor market relevant skills. This may reduce the role of secondary schooling for apprenticeship trained workers. However, we find no wage effects for the group of workers who never completed an apprenticeship either.

The high returns found for the US have often been attributed to compulsory schooling mostly affecting high return individuals, who also have high costs of schooling. We argue that this reasoning cannot be used to explain the stark differences in the patterns of returns to schooling we find for Germany without taking into account more fundamental differences in the schooling systems. In particular, while it is possible to argue that many low returns individuals are affected by the change in compulsory schooling laws in Germany, due to the nature of the school system, the same should be also be true for the UK, which had a very similar system at the times when the school leaving age was raised. We feel that the most likely explanation of our results has to do with the fact that German students were much better prepared in basic academic skills by the time they reach grade 9, while the same is not true in many other countries including the UK. We argue that it is those skills which matter ultimately in the labor market for the target population.

Angrist and Krueger (1991) were the first to use compulsory schooling laws to estimate the returns to schooling. By exploiting the features of US compulsory schooling laws, they 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), who adds a comparison between England and Wales, on the one hand, and Northern Ireland, which changed school leaving ages at different times. Oreopoulos also compares these results to 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 (2004) 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 coun-

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

tries have more regulated labor markets than the Anglo-Saxon countries. For Norway, Aakvik et al. (2003) report a return of 10 percent for year of education, while the Meghir and Palme (2004) results imply a return of 0.18.[2] While some features of the results, particularly in the Swedish case, suggest that these results do not identify purely the returns to compulsory schooling, the authors effectively do not offer an alternative explanation.

Two other recent studies, however, find low returns using difference-in-difference analyses of changes in compulsory schooling rules in European countries. Grenet (2004) evaluates the impact of a reform in the French schooling system in 1967, which raised the school leaving age from 14 to 16. Grenet compares students from advantaged and disadvantaged family background because the French reform was at the national level, and finds returns of 3 to 5 percent, below the OLS returns of 7.5 percent in his sample. Oosterbeek and Webbing (2004) analyze the extension of some vocational training programs in the Netherlands from three to four years of length. Because there were pre-existing programs with four years in length, this study compares graduates from the programs which were extended to those whose length stayed the same. Oosterbeek and Webbing generally find small, and sometimes negative effects for the participants of the extended programs.

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 educa-

[2] Meghir and Palme (2004) only report reduced form results. The estimate of 0.18 can be obtained as the indirect least squares (IV) estimate from their results.

tion. All three studies find larger returns to education than by using OLS, in the order of 10 percent.

The existing literature on the causal effects of education suggests that the returns to education are large (see, for example, Card, 1999). The impact of compulsory schooling laws in Germany is of substantial interest because the earnings effects differ so much from the preceeding literature on this topic. In addition, the existing estimates lend credence to the idea that the effects of education are relatively well described by a human capital model, where time in school translates in a simple manner into higher earnings. Our study is also important because it demonstrates that this conclusion is not universally valid. Instead, our results show that the return to education depends on institutions and the organization of the school system.

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 some estimation problems. Section 5 discusses various possible explanations for the results and section 6 concludes.

2 The German schooling and training 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 or a lower level qualification after grade 12, called *Fachhochschulreife*, which allows school leavers to attend a polytechnic.

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 type for the cohorts born from 1925 to 1970.[3]

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, 2004). This implies that basic school students tend to be the lowest ability students, and they also face the least challenging curriculum.[4]

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 and Berlin were the first states to introduce a 9th grade in 1949. During the immediate post-war period, lack of labor market oppor-

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

[4] 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.

tunities and apprenticeships for school leavers were important arguments in the debate about the introduction of the 9th grade. Petzold (1981) claims that the labor market argument was widely used by the government, political parties, and employer associations at that time. Some states introduced a mandatory 9th grade on a temporary basis during the early 1950s. For example, Niedersachsen had laws from 1950 to 1955 allowing the education minister to mandate 9th grade attendance by county or town. Bavaria had a similar law in 1952. Other states introduced the 9th grade on an experimental basis. Two northern states, Schleswig-Holstein and Bremen, permanently introduced the 9th grade in the late 1950s.

By that time, labor market arguments were not the main driving force in the political discussion anymore. According to Petzold (1981), the reasoning had shifted to educational arguments (14 year olds are not mature enough for the labor market, and school still has an important role to play at that age) and the growing skill needs of the economy. LeSchinsky (1981) mirrors the assessment that the extension of compulsory schooling was mainly used to raise educational standards in the basic school track. He also points out that employers at the time stressed the necessity of thorough training in the basic skills, reading, writing, and arithmetic. Two other states, Niedersachsen and Saarland, introduced the 9th grade in this general enviroment during the first half of the 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. Four states introduced the 9th grade in 1967, although this was not done in Bavaria until 1970. Table 1 shows the dates for each state we use in the analysis, as well as our assignment of the first birth cohort, which should have been affected by the change.[5]

The content of the curricula for the additional 9th grade differ somewhat between states. Nordrhein-Westfalen (1962) reports that Berlin used

[5] 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.

the 9th grade primarily for political education, while Bremen stressed "general knowledge." In Niedersachsen, the curriculum was centred around three goals: the consolidation of basic skills, to give students access to the adult world and teach political responsibility, and to acquaint school leavers with the world of work.[6]

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 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.

According to our data, workers obtained many of the skills they actually use on their job during their apprenticeship training (with other important sources being further on-the-job training and work experience). Apprenticeship training (and sometimes further vocational qualifications, like the *Meister* certificate) is often necessary for entry into certain skilled jobs, self-employment, and in order to reach certain pay grades. Pay tends to be highly regulated in Germany, particularly in firms subject to collective bargaining.

Instead of pursuing an apprenticeship, school leavers may also receive

[6] Some states also started offering an optional 10th grade for basic school students after introducing a 9th grade, 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 immediately after 9th grade.

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 is the Micro Census, an annual survey of 1 percent of the households in Germany. We use the surveys from 1989, 1991, 1993, and all years from 1995 to 2001. Each wave has about 300,000 to 400,000 observations for the west German states. The third 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 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.[7]

[7]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

We then convert the variable to an hourly wage by dividing by the number of weekly hours.

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.

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 also 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 Micro Census is an annual representative household survey. The Micro Census samples both employed individuals and those not working, which allows us to look at employment in addition to earnings. The large sample sizes also facilitate the analysis of some smaller subgroups. We restrict the sample to those of German nationality.

There is no earnings measure on this dataset but it contains a variable for net monthly income. Income is also reported in brackets. There were 18 brackets from 1989 to 1999, and 24 brackets in 2000 and 2001. We assign midpoints to the brackets again.[8] Despite the different concepts, this

1991-92 it was DM 8,000 or more which we assigned a value of 12,500. Only 1.8 percent of sample observations are in the top income bracket.

[8] The top bracket in 1989 was DM 5,000 or more which we assigned a value of DM 7,500; in 1991-1999 it was DM 7,500 or more which we assigned a value of DM 10,500; in 2000 and 2001 it was DM 35,000 or more which we assigned DM 40,000.

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.

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 of residence, 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 12,566 cells, based on 2 to 1,447 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. Summary statistics for our samples can be found in Appendix table 1.

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 (called "years of education") 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 55.1 percent in the QaC and Micro Census, respectively. The coefficients in column (1) are similar to these fractions, although they do not match exactly. 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 fact that the results in columns (1) and (2) are very similar indicates that the introduction of the 9th grade in basic school did not affect participation in post-secondary schooling and training, an issue we will return to below.

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, or it could reflect attenuation due to various measurement problems. It is also important to keep in mind 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.[9]

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 columns (4) to (6). Column (4) contains the basic specification. Column (5) adds state specific cohort trends, and column (6) also adds quadratic trends. Students subject to the 9th grade in basic track are slightly less likely to attend the basic track. However, the effects are relatively small, and they are only significant in the specification in column (6) for the Micro Census. If track choice is largely unaffected by the 9th grade introduction, it is possible to limit the sample to basic track students.

Table 3 shows more details for the estimates using the length of schooling variable in the QaC by showing specifications with state specific cohort trends, and specifications limited to students attending the basic track. The estimates change little when state specific cohort trends are included in

[9] An exception is Oreopoulos (2003) who uses school leaving age for the UK, a measure more similar to the length measure used here. It is a valid measure of the increase in education as long as measurement error does not vary across cohorts.

columns (2) and (3). The estimates for the basic track subsample is 0.28 as expected (0.17 divided by 0.6). While these estimates are relatively small, it is important to note that they are highly significant.

In order to investigate the magnitude of the first stage further, we plotted the relationship between the length of schooling and the introduction of the 9th grade in figure 2. The figure shows raw means in the length of schooling variable by year. 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 shortly before the introduction of the 9th grade, and continues after the introduction. This may indicate some mismeasurement in the exact timing of introduction date, for example, due to early school entry, grade repetition, measurement error in year of birth, and mobility between states. The continuing trend towards more schooling after the introduction is most likely 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.

Figure 3 provides additional information on the strength of the first stage using 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.[10] 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. Instead, the change was spread over the adjacent years for some students.

What the results in Figures 2 and 3 indicate is that one should not expect a first stage coefficient of one in the subsample of basic school students.

[10] No data are available for 1968 and 1969 because of a change in the computer system of the statistical authorities. We were also unable to obtain similar data for other states, which introduced the 9th grade earlier.

Instead, the introduction of the 9th grade was not fully effective in a single year, but a more spread out process. In addition, the various measurement problems mentioned above will tend to attenuate both the first stage and reduced form coefficients. Since the relative biases in both the first stage and reduced form coefficients will be the same, instrumental variables estimates will be consistent despite the measurement error for the standard reasons. Since the length of schooling variable in the QaC data is the only variable which picks up the actual increase in duration of schooling, we will rely heavily on presenting reduced form effects from all the data sets. It should be kept in mind, that these have to be divided by the first stage effects. However, it will turn out that our reduced form results are close enough to zero that this does not matter much in practice.

The following tables display regressions for log wages. We start in Table 4 by showing some OLS specifications using the QaC and Micro Census data. Column (1) presents a standard model with a quartic in potential labor market experience as controls. The return to education is between 7 and 8 percent in this OLS specification. Because the 9th grade treatment is constructed on the basis of state of residence and year of birth, it is important to control for state and year of birth main effects in the later 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 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.[11]

Column (4) instruments the endogenous regressor, years of education,

[11] To benchmark the IV estimates of the return from an additional year of schooling, we have also run an OLS regression of log earnings on dummies for completed years of schooling using the length of schooling variable. The results, available from the authors, suggests returns for the 9th grade of schooling of the same order of magnitude as the OLS returns shown in Table 4.

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. Columns (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, particularly when the squared trend is included. The returns estimates are 3 percent in the QaC data and 1 percent in the Micro Census, which are still small returns, although both estimates are significant.

Table 5 presents 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 of residence 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.[12]

The estimates for all three datasets are basically zero. The effect is identified via a differences-in-differences design, because state of residence and year of birth effects are controlled for. It is always a worry in models like these that other state specific trends might be correlated with the treatment. Since the treatment is related to the schooling of individuals, we include trends in individuals' birth cohort, not the survey year, interacted with state. In all datasets, the coefficients increase slightly when state specific trends are controlled for, but for the social security data and the Micro

[12] 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.

Census these changes are small. The QaC is the only dataset where a slightly positive and significant effect emerges with squared state specific trends, which mirrors the IV results in table 4. There is often a worry that changes in compulsory schooling laws may be correlated with other changes in 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 5 suggests the exact opposite: the introduction of the 9th grade in Germany was, if anything, negatively correlated with other state specific trends in wages across cohorts.

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. The results are shown in columns (4) to (6) of table 5. The results are again clustered around zero. In fact, the estimates are throughout smaller than those for the full sample. This further supports the notion that the true return is zero since any positive return should be amplified in this subsample of students who were actually affected by the policy.

We have argued above that these estimates might be affected by a variety of measurement problems. Hence, scaling the reduced form estimates by the first stage estimate using the length of schooling results from the QaC is the most appropriate since these estimates should be subject to the same problems. For the QaC data, this is simply the instrumental variables estimate. For the other datasets we divide the reduced form results in table 5 by the appropriate first stage estimates from the QaC in table 3. This is effectively a two sample indirect least squares estimate.[13]

[13] These standard errors are derived using the delta method. Denote the first stage coefficient by $\widehat{\pi}$, the reduced form coefficient by $\widehat{\delta}$, and the ILS estimate by $\widehat{\beta}$. Then

$$var(\widehat{\beta}) = \frac{1}{\widehat{\pi}^2} var(\widehat{\delta}) + \frac{\widehat{\delta}^2}{\widehat{\pi}^4} var(\widehat{\pi})$$

because $\widehat{\pi}$ and $\widehat{\delta}$ are estimated from independent samples, and hence there is no covariance term. In practice, only the first term of this expression is really going to matter because the reduced form effects $\widehat{\delta}$ are so small. This means, the main adjustment that has

These estimates are displayed in table 6. Unsurprisingly, most of the estimates in the table are small and insignificant. The largest estimate for the Mirco Census is found in the specification with quadratic trends, which is 0.029. The only large estimate is found for the QaC data with quadratic trends. This estimate is 0.11. This would be a large return. However, we discount this result for two reasons. First, the QaC is the smallest dataset and the QaC estimate is the least precisely estimated. Furthermore, we do not find a similar result when we limit the sample to basic track students in columns (4) to (6) of the table. The estimates are smaller now in both the QaC and the Micro Census data. This suggests that the large estimate of 0.11 is actually not due to higher earnings of the basic school students, who are the ones who were affected by the change in compulsory schooling. This is further evidence that the true return is likely to be zero, and the relatively large estimate for the QaC data in column (3) may be simply due to sampling variation.

Moreover, except for the QaC data, the coefficients are also estimated fairly precisely. The estimate from the Micro Census in column (6), for example, implies a confidence interval of -0.028 to 0.034. In summary, the evidence presented here suggests that the return to compulsory schooling in Germany is small and most likely zero. This is true although our reduced form estimates are likely affected by various sources of measurement error. However, the same is true of the first stage estimate using the length of schooling variable, and hence IV or ILS estimates are consistent. Measurement error can therefore not explain the small effects. Even the upper end of the confidence interval of our estimates is well below the 15 percent return reported by Harmon and Walker (1995) and Oreopoulos (2003) for the Anglo-Saxon countries. This suggests that the returns to a year of

to be made to the reduced form standard errors is also to scale them up by the first stage coefficient. The precision of our ILS estimates is slightly lower than that reported by for the UK by Harmon and Walker (1995) and for the US, Canada, and the UK by Oreopoulos (2003). The reduced form 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.

compulsory schooling in Germany is very different from the returns in these earlier studies.

5 Interpretation

Why are the returns to compulsory education zero in Germany while they are large in most other countries where these effects have been studied? In this section we turn to a discussion of various explanations for our finding. We feel that we can rule out some leading possibilities, and we provide some suggestive evidence regarding a skill based explanation.

The first set of explanation is based on the idea that the productivity of 9th grade school leavers is actually higher but for some reason this is not refelected in higher wages. One 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. In this case, 9th grade graduates are indeed more productive but the effects do not show up in wages. However, employers should be more interested in employing the more productive 9th grade graduates than the equally expensive but less productive school leavers from 8th grade. Hence we would expect to see employment effects for the 9th grade.

Table 7 explores this possibility by replacing the dependent variable with a dummy variable for whether the respondent is employed. This can only be done in the Micro Census. The employment effects are only sizeable in the specification with no state specific trends, but vanish once these trends are included. The effects are even smaller or negative when the sample is restricted to men (results for men are not shown in the table): the coefficient is 0.003 with a standard error of 0.004 without state specific trends. Selective labor force participation is less of an issue for men. So these results suggests that there are no employment effects of the increase in compulsory schooling age.[14]

[14] Grenet (2004) also presents results for employment and but similarly fails to find any effects for France.

An additional implication of the wage rigidity explanation for the absence of wage returns is that 9th grade graduates should be more likely to choose self-employment if their higher productivity is not rewarded in the more regulated wage and salary sector. In addition, depending on the resulting selection, self-employment wages should also be higher for 9th grade graduates. We do not find any support for this possibility either. Results are shown in the lower two panels of table 7. We first analyze self-employment rates. The effects of the 9th grade are zero or negative, and none of the coefficients is significant. The wages of self-employed workers are also not higher for 9th grade school leavers. In fact, the coefficient estimates are consistently negative, and some estimates are significant at the 5 percent level. Hence, there is no evidence for higher self-employment rates, or higher earnings among the self-employed either. Together with the results on employment this provides fairly strong evidence against the wage rigidity explanation.[15]

A second explanation might be that the lack of returns to the additional grade in secondary school had no effect because of the role played by the apprenticeship system in Germany. Most basic track students will complete an apprenticeship. The length of apprenticeships was not adapted when the 9th grades were introduced. One simple view would be that the introduction of the 9th grade did not really change the "highest grade completed" in Germany, if this is the 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

[15] A slightly more intricate version of the wage rigidity story would be that there are indeed positive wage effects, but our research design is unable to pick up the effect. Wages are frequently set through collective bargaining agreements, and there is pattern bargaining in Germany. This means 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. However, if this is true, we would still expect to see effects on employment and for the self-employed.

to complete an additional grade, and a zero return in Germany may therefore not be surprising.

In order to probe the possibility that we do not find any returns to the 9th grade because of the role of apprenticeship training, we will look at those individuals who do not complete an apprenticeship. For these individuals, the 8th or 9th grade they complete in secondary school is their final level of schooling.[16] This is only a small fraction of all school leavers even from basic school, about 17 percent in our sample. Hence, this group is much more comparable to, for example, high school dropouts in the US who are affected by compulsory schooling laws since this group is also very small (see Angrist and Krueger, 1991).

We start in table 8 by investigating whether the incidence of apprenticeship or other vocational post-secondary training was affected by the 9th grade introduction. This only makes sense for basic track students because upper track students have additional post-secondary options. Hence, the samples in table 8 are restriced to basic school students. As can be seen in the top panel, the 9th grade introduction did not seem to have affected post-school training. The coefficients are small, with some estimates positive, and one of them negative.

If there is no selection into who completes an apprenticeship, we can analyze wages for those school leavers who do not complete any post-secondary training. This is the group which is much more comparable to US high school drop-outs or UK school leavers with no post-compulsory schooling. If the structure of German vocational training is responsible for the fact that we find very different results from the Anglo-Saxon countries, then we should find wage returns for this (small) subgroup of basic track leavers. The lower panel in table 8 shows that this is not the case. If anything, the point estimates are actually negative, although they are not significant. Hence, an explanation of our results simply based on the training system in

[16] Most of the individuals without completed apprenticeship training have never started an apprenticeship. Unfortunately, we do not have this information available in the Micro Census data. The QaC data suggest that only about 20 percent of those school leavers without a completed apprenticeship actually start an apprenticeship.

Germany also seems unlikely.[17]

A possible explanation for the fact that our estimated returns are so low may be the presence of general equilibrium effects. All school leavers eventually attended the 9th grade, so the total supply of human capital went up. However, we find it extremely unlikely that this could lead the return to drop to zero. Suppose there is a market for units of human capital. If the elasticity of demand for units of human capital was -1, total earnings of basic school leavers after introduction of the 9th grade are the same as before. Nevertheless, this does not mean that there is no return. The presence of general equilibrium effects supposes that 8th grade and 9th grade skills are highly substitutable. But since those who leave after the 8th grade have fewer units of human capital than those leaving after the 9th grade, they will have lower earnings than the 9th grade leavers. The return to a unit of human capital has declined but as long as it is still positive there will be a return to a 9th grade education. Hence, the presence of general equilibrium effects may imply a smaller return than if these returns were absent but the return should not be near zero if the original return was clearly above zero.

For example, suppose that skills learned in all grades are perfect substitutes, the elasticity of demand for units of human capital is -0.5, and supply is inelastic. If the original return to a year of education is 8 percent, then moving everybody from an 8th grade to a 9th grade education would imply that the return drops to 6 percent. This is an upper bound estimate for these parameter values since in our setup there are both 8th and 9th grade school

[17]A more complicated explanation based on the German apprenticeship system might still be consistent with our results. Say the additional knowledge acquired during the 9th grade 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 only at the national level again. This could explain a zero return estimate because the effect is being absorbed by our cohort dummies. Those without apprenticeship training have no returns since 9th grade skills are not useful without the additional training. We cannot rule out this possibility based on our evidence. However, it would imply a very different, and highly country specific channel of returns in Germany that is very different from the reasons for the high returns in other countries. Hence, this is not a particularly attractive explanation to reconcile our results with the literature.

leavers in the market at the same time, so the total increase in human capital is actually less. In our sample only about a third of school leavers have passed through the 9th grade. To what degree this is the relevant number will depend on how substitutable human capital is across age groups. But these calculations illustrate that general equilibrium effects cannot explain the small returns unless the demand for human capital is extremely inelastic. With a third of basic track students having a 9th grade education, the return would fall to zero only if the demand elasticity is as low as -0.04. Since our data come from the labor market experience of individuals many years after the change in compulsory schooling this is a long-run elasticity. Such a low elasticity, even at low values of the return, seems very unlikely. Hence we conclude that general equilibrium effects cannot explain the results.

A second set of explanations of our results implies that the productivity of 9th grade school leavers in the labor market is actually not any higher than the productivity of their peers who only completed 8 grades. One explanation of that type is a pure signalling model where schooling does not enhance productivity at all. Given the structure of the German school system, the signal will have to depend on the secondary track chosen rather than the grade completed. This means that the signalling value of a basic track education would be unchanged after the introduction of the 9th grade. Such a model would be consistent with the results from other countries if the signal in the US or UK, for example, depended on the highest grade completed and employers did not discriminate between individuals who completed an additional grade voluntarily and those, who were forced to because of compulsory schooling laws.

While it is not possible to rule out a signalling explanation with our data, such a model seems extreme, however. Hence, it is of interest to consider other explanations which allow for a role of human capital but are consistent with a low or zero return to the 9th grade in basic track. One of the most commonly advanced explanation for high returns to compulsory schooling, particularly for the US, 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, 2001). 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.

One possible interpretation to reconcile our results with those for the US may be that the drop-outs who are driving the results in US studies have indeed high returns, while the school leavers compelled to stay on longer in Germany are predominantly low return individuals. The US and German school systems present youths with a rather different set of alternatives. In the US, a student has to make a decision at the school leaving age, say at 14, 15, or 16 years of age, whether to leave school or to stay on. In Germany, students and parents make an important decision about school leaving effectively at age 10 or 11 when students have to choose a secondary track. Once a student chose to stay in the basic track, that student had no alternative to leaving school after grade 8 as long as the state had not introduced a 9th grade. Moving up to a higher track is virtually impossible at that stage. Of course, students in the higher tracks in Germany still have the possibility to drop out of school once they reach the highest manadory grade.

These differences in schooling systems imply that a small number of students is affected by compulsory schooling laws in the US. In contrast, a large number of students was affected by the introduction of the 9th grade in Germany. These two groups of students are potentially quite different. For example, Angrist and Krueger (1991) estimate that about 4 percent of school leavers born in the 1940s and subject to an age 16 schooling leaving age drop out as soon as they can, and are therefore directly affected by the school leaving age. This small group of students could have both high costs of schooling and high returns and hence be responsible for the large returns to compulsory schooling. In our German samples, about 60 percent of students have a basic school degree during the introduction of 9th grades, and were therefore affected by the change. Most of these individuals may be low return students. In fact, this may be the reason they chose to remain in the basic track at age 11.[18]

While early tracking offers a promising rationalization of our results, we believe that the differences in schooling systems are more fundamental than an explanation based on tracking alone would suggest. In particular, the school system in the UK resembled the German school system much more than the one in the US at the times of the raising of the school leaving age. The UK school system also tracked students into two types of secondary schools at age 11. Only students in the lower level schools (secondary modern schools) were directly affected by the change in the school leaving age, since students in the more academic track (Grammar schools) were expected to continue their schooling up to age 18 in any case. According to Oreopoulos (2003), about 50 percent of students in Great Britain were affected by the increase in the school leaving age from 14 to 15 in 1947. This is close to the fraction affected by the German policy change. Since returns for the British students are high, this would have to imply that all these British students have to have high returns to schooling. But this begs the question why there should be such a different number of high return students in the various countries.

We believe that an an explanation based on the skills actually learned around the time of the compulsory school leaving age, and the relevance of these skills for the labor market may be a likely explanation of the results. The different schooling and training systems in different countries may play

[18]Suppose 4 percent of students are high cost/high return types in both countries and these students have a return to an additional year of schooling of 0.15. In Germany, these students are drop-outs from one of the higher tracks. Furthermore, suppose that the return for the basic track students in Germany is zero, and they account for 60 percent of all students. The expected IV estimate for Germany in this case would be 0.15*0.04/(0.04 +0.6) = 0.01. This is not far from of the estimates we find for the full sample in table 6. Also consistent with this explanation would be the fact that we find more positive estimates for the full sample than for basic track students only.

some role in this. Suppose that workers use two types of skills, academic skills and vocational skills. Vocational skills are learned in apprenticeship training or on the job. In terms of academic skills, school leavers from basic track only use the most basic skills: reading, writing, and basic arithmetic. Our claim is then that students do not learn any of these labor market relevant skills in the 9th grade in basic track in Germany. On the other hand, students in other countries like the US, Canada, or the UK may still be learning these skills even at an age like 14 or 15. This could be because the German school system does a better job in teaching these skills earlier than school systems in the Anglo-Saxon countries.

The QaC data set asks questions in all waves about the location where individuals learned the skills relevant for their job. Only 6 percent of basic school leavers name general schooling as one of the venues were these skills were learned. The most important venues are vocational training and experience on the job. Even for those basic school leavers with no further training the fraction learning skills in school is less than 8 percent. Of course, this may not necessarily mean that none of the skills learned in school are relevant to the labor market.

A bit more direct evidence is available in some of the waves in the QaC data. Respondents were asked whether they used certain specific skills on their job. Most of these skills were vocational skills or skills typically learned in tertiary education. However, three skills were clearly skills learned in primary or secondary schooling. These are math, German language skills, and foreign language skills. More details about the intensity of these skills are available on the 1979 wave of the data. Table 9 displays some results on the usage of these academic skills. In general, it is obvious that usage of academic skills is much higher for graduates of the higher tracks. However, almost half of basic track respondents say they use math skills on their job. However, the skills they actually use are basic arithmetic, and to some extend very simple algebra (like forming percentages, calculating interest, or solving a simple equation with one unknown). These are skills typically learned in Germany by grade 7. Very few basic track students use any more advanced math skills.

The disparity between basic track and higher track students is even more apparent with respect to language skills. Only about a third of basic track students need to communicate orally on their job or write simple letters, while twice as many students from the higher tracks use these skills. This is basically due to the fact that basic track leavers typically end up occupying blue collar positions, while low level white collar positions (like secretary) would typically be occupied by middle track students. Few basic track students need to communicate about complex matters on their job, and hardly any use a foreign language. Despite the fact that the basic track group is large, this evidence corroborates our claim that they use only relatively basic academic skills.

The second part of our claim is that students at the bottom end of the ability distribution in Germany are more proficient in these basic academic skills than their peers in other countries, where higher returns to compulsory schooling were found. Table 10 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 including 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 10 shows the quantitative scores at the 5th and 25th percentiles of the distributions for the IALS countries for which studies on compulsory schooling exist. France is not included in the OECD report. 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 10 reveals the well known result that Germans at the lower end of the ability distribution score relatively highly, particularly when compared to Anglo-Saxon countries. This is particularly apparent at the 5th percentile, and for the older respondents. In fact, for this group Germans also outperform the Swedes and Norwegians.[19] 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 11, we report some results for 13 year olds in six relevant countries: Germany, the US, England, France, the Netherlands 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 any of the other countries. In basic arithmethic the Germans outperform particularly those from the US, England and 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

[19] However, the Netherlands look much more like Norway and Sweden than Germany in this table, although Oosterbeek and Webbing (2004) found zero returns to compulsory schooling for the Netherlands as well. However, the Dutch result may well be explained by the fact that the raising of the school leaving age affected a group of students in particular vocational programs. Hence, basic skills may not be very relevant in explaining the different results for the Netherlands.

table, only 10 percent of students in Germany answered only 10 or fewer questions, while 37 percent of Swedish, 32 percent of American, and 29 percent of English students did equally poorly. France and the Netherlands fall inbetween these extremes.

These results broadly corroborate those from the IALS. 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 school system. German students had learned the labor market relevant skills by the time they graduated from secondary school, while in the Anglo-Saxon countries, the most marginal students are still learning these skills at age 14 or 15. This leaves open the question whether keeping students in school during 9th grade has other beneficial effects. Economists have recently become interested in the effects of schooling on health, family formation and fertility, criminal behavior, etc. While estimating these effects would be extremely interesting, we are not able to explore these issues with the data we have at hand.

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. Using instrumental variables rules out the possibility that the small estimates are simply due to attenuation from measurement error.

We have also argued that some headway can be made in understanding why the German results differ so much from those found for other countries.

We argue that the most likely explanation is due to a different structure of the German school system, which tracks students early and does well in training the lower end of the ability distribution in basic academic skills (or at least did so until the beginning of the 1970s). In contrast, there may be potentially many students at the lower end of the ability spectrum in other countries who still benefit from general schooling at age 14 or 15. We do not find support for an important role of rigid wages, apprenticeship training, or occupational qualifications.

Our results are imporant because they are in stark contrast to the exisiting literature on the causal effects of education. They caution against extrapolating from even a large body of existing results to circumstances with other institutional features. This suggest that the attention of researchers should shift from simply studying the impact of the amount of education on earnings to focusing more attention on the organization and content of the education.

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

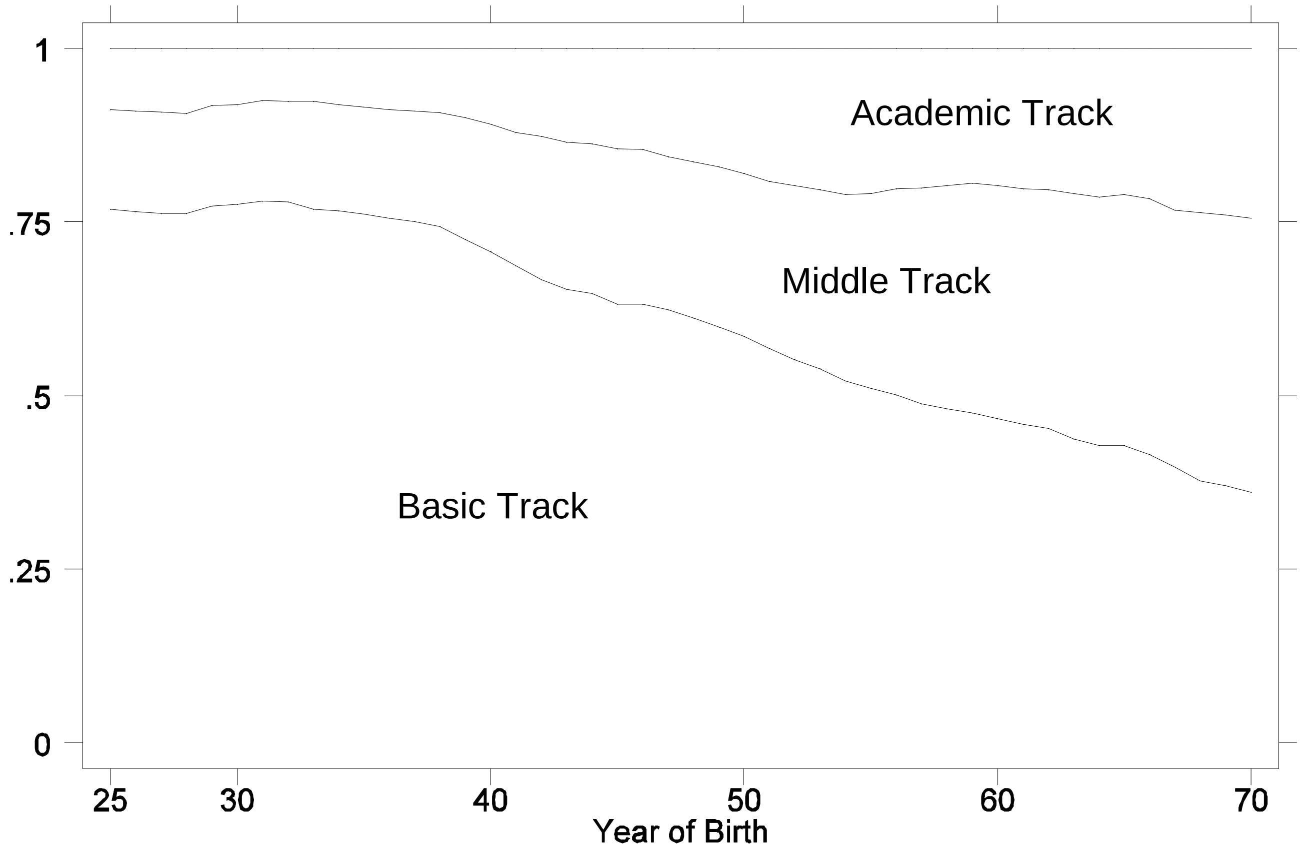


Figure 2
Number of Years Spent in School by Basic Track Students

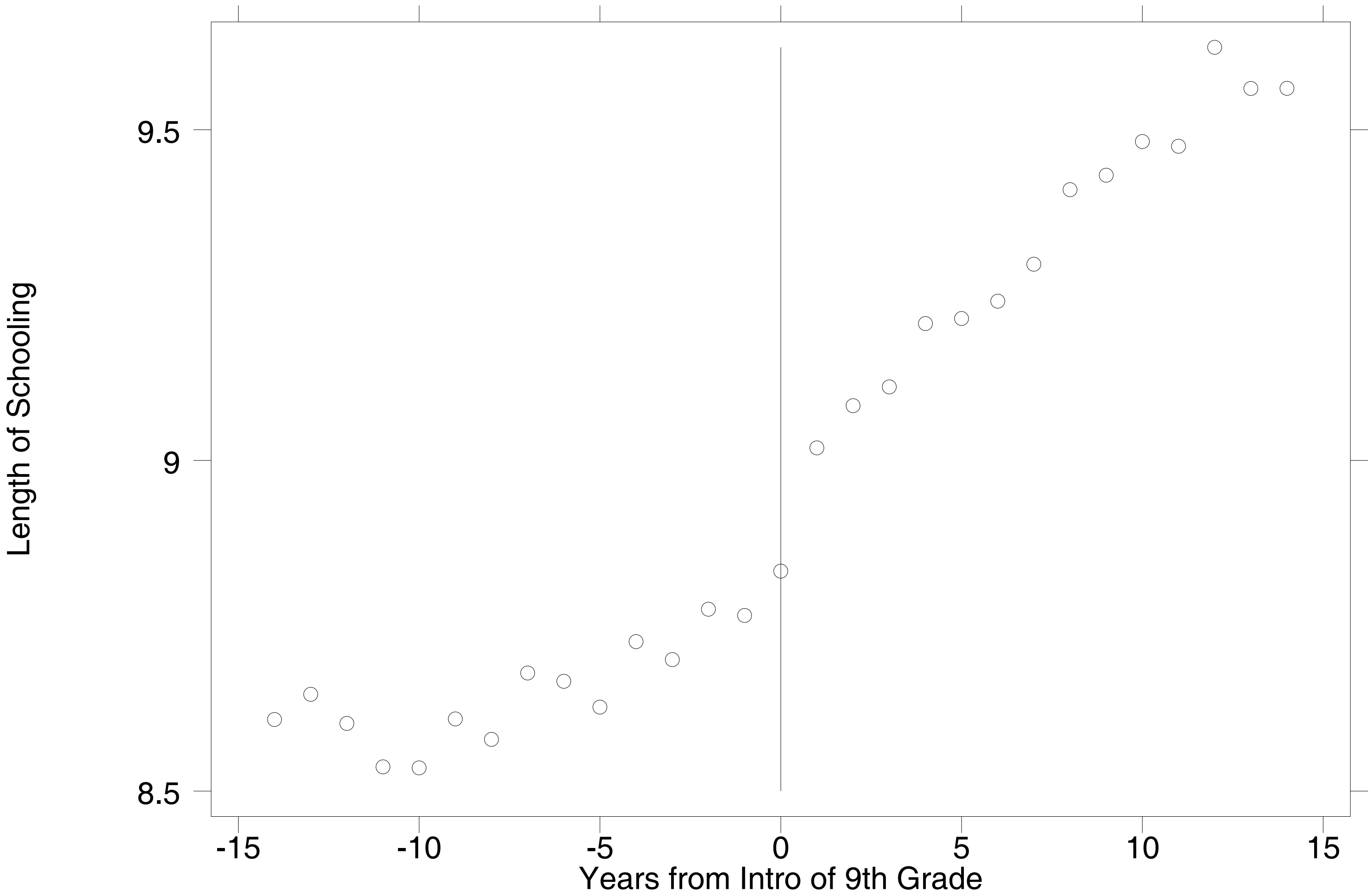
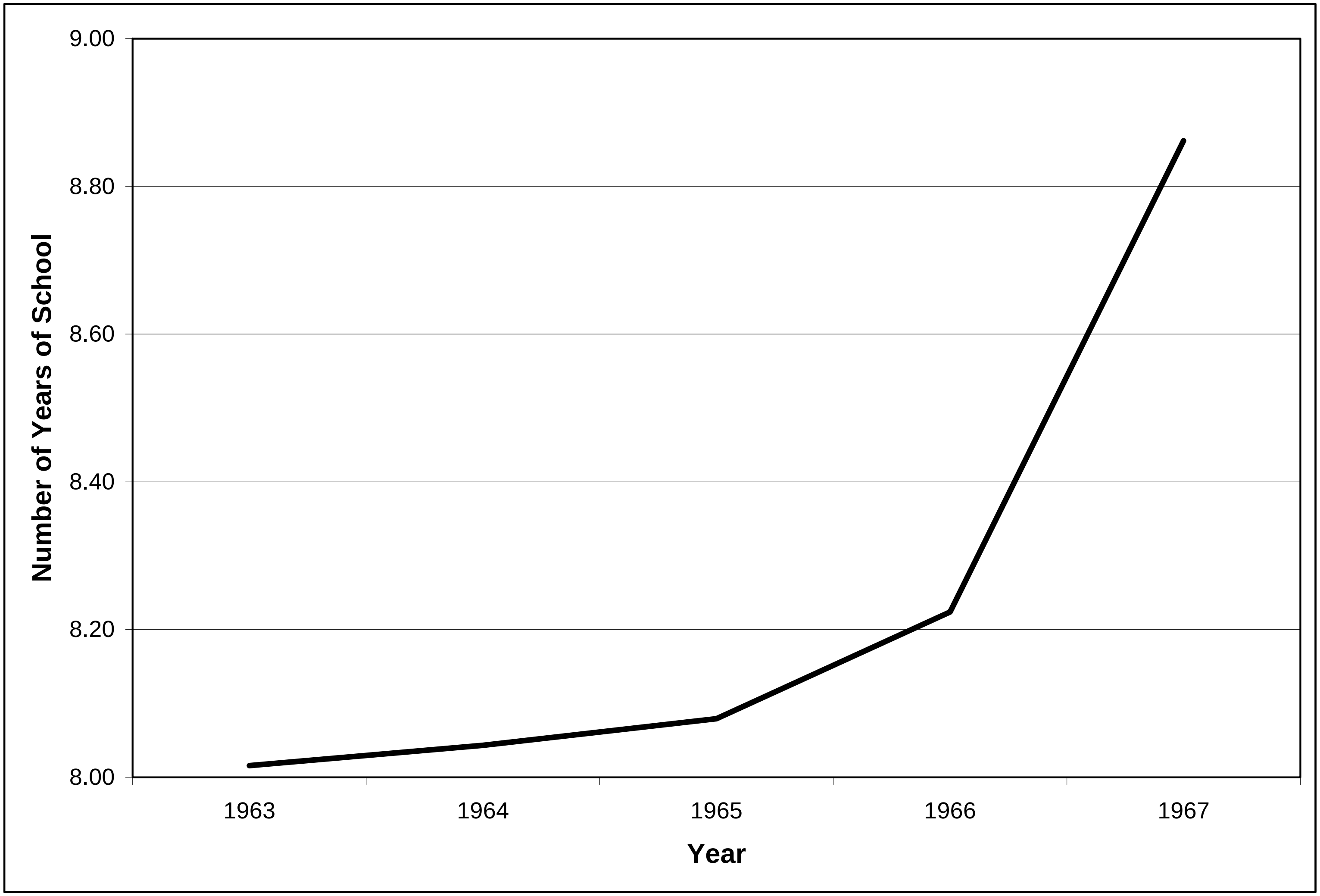


Figure 3
Average Number of Grades Attended by School Leavers in NRW, Hessen, RPF, and Baden-Württemberg



Note: 9th Grade Was Formally Introduced in 1966

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)

	Dependent Variable					
	Years of Education	Years of School (primary + secondary)	Length of Schooling (primary + secondary)	Attends Basic Track		
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Qualification and Career Survey						
Dummy for Cohort with 9^{th} Grade in Basic Track	0.569 (0.051)	0.588 (0.034)	0.171 (0.040)	-0.016 (0.011)	-0.012 (0.011)	-0.021 (0.013)
Micro Census						
Dummy for Cohort with 9^{th} Grade in Basic Track	0.568 (0.021)	0.548 (0.014)	---	-0.003 (0.005)	-0.003 (0.004)	-0.012 (0.004)
State Specific Cohort Trends					✓	✓
State Specific Cohort Trends Squared						✓

Note: Number of observations is 54,126 in the Qualification and Career Survey and 750,416 in the Micro Census. 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.

Table 3
First Stage Regressions
Qualification and Career Survey
Dependent Variable: Length of Schooling
(Standard Errors in Parentheses)

	Full Sample			Basic Track Only		
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Dummy for Cohort with 9th Grade in Basic Track	0.171 (0.040)	0.190 (0.039)	0.180 (0.048)	0.280 (0.040)	0.285 (0.033)	0.233 (0.037)
Number of Observations	54,126	54,126	54,126	32,970	32,970	32,970
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.

Table 4
Log Wage Regressions
(Standard Errors in Parentheses)

Independent Variable	OLS (1)	OLS (2)	OLS (3)	IV (4)	IV (5)	IV (6)
Qualification and Career Survey						
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)
Micro Census						
Years of Education	0.083 (0.0004)	0.079 (0.002)	0.074 (0.001)	0.005 (0.006)	0.005 (0.005)	0.010 (0.005)
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 54,126 in the QaC and 750,416 in the Micro Census. 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 5
Reduced Form Regressions
Dependent Variable: Log Wage
(Standard Errors in Parentheses)

	Full Sample			Basic Track Only		
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
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)	-0.013 (0.010)	-0.001 (0.010)	0.010 (0.011)
Number of Observations	54,126	54,126	54,126	32,970	32,970	32,970
Micro Census						
Dummy for Cohort with 9th Grade in Basic Track	0.003 (0.003)	0.003 (0.003)	0.005 (0.003)	0.001 (0.004)	0.002 (0.003)	0.001 (0.004)
Number of Observations	750,416	750,416	750,416	413,276	413,276	413,276
Social Security Data						
Dummy for Cohort with 9th Grade in Basic Track	-0.003 (0.005)	0.005 (0.004)	0.004 (0.005)	---	---	---
Number of Observations	12,566	12,566	12,566			
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 WLS regressions for the cell level median of the log wage using cell sizes as weights.

Table 6
Indirect Least Squares Estimates
Dependent Variable: Log Wage
(Standard Errors in Parentheses)

	Full Sample			Basic Track Only		
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
			Qualification and Career Survey			
Length of Schooling	0.024 (0.045)	0.058 (0.038)	0.108 (0.046)	-0.047 (0.037)	-0.005 (0.034)	0.042 (0.046)
			Micro Census			
Length of Schooling	0.017 (0.019)	0.017 (0.016)	0.029 (0.021)	0.004 (0.013)	0.006 (0.011)	0.003 (0.016)
			Social Security Data			
Length of Schooling	-0.015 (0.031)	0.025 (0.024)	0.022 (0.030)	---	---	---
State Specific Cohort Trends		✓	✓		✓	✓
State Specific Cohort Trends Squared			✓			✓

Note: Standard IV estimates are shown for the Qualification and Career Survey using a dummy for cohorts with 9th grade in basic track as instrument. Two sample indirect least squares estimates are shown for the other data sets. First stage coefficients with the same instrument are taken from the Qualification and Career Survey. Reduced form standard errors are adjusted for clusters at the state * year of birth level.

Table 7
Reduced Form Regressions for Employment, Self-Employment, and Self-Employed Wages
Micro Census
(Standard Errors in Parentheses)

	Full Sample			Basic Track Only		
Independent Variable	(1)	(2)	(3)	(4)	(5)	(6)
Sample: All -- Dependent Variable: Employed						
Dummy for Cohort with 9th Grade in Basic Track	0.007 (0.003)	0.002 (0.002)	0.000 (0.003)	0.009 (0.004)	0.005 (0.003)	0.002 (0.004)
Number of Observations	1,225,877	1,225,877	1,225,877	752,636	752,636	752,636
Sample: Employed – Dependent Variable: Self-Employed						
Dummy for Cohort with 9th Grade in Basic Track	-0.003 (0.002)	-0.002 (0.002)	-0.004 (0.002)	-0.001 (0.002)	0.000 (0.002)	-0.004 (0.003)
Number of Observations	750,416	750,416	750,416	413,276	413,276	413,276
Sample: Self-Employed – Dependent Variable: Log Wage						
Dummy for Cohort with 9th Grade in Basic Track	-0.021 (0.014)	-0.020 (0.015)	-0.014 (0.017)	-0.040 (0.021)	-0.030 (0.022)	-0.051 (0.025)
Number of Observations	73,426	73,426	73,426	31,142	31,142	31,142
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.

Table 8
Reduced Form Regressions for Post-Secondary Training and Wages of Workers with No Training
Micro Census – Basic Track Only
(Standard Errors in Parentheses)

Independent Variable	(1)	(2)	(3)
Sample: Employed -- Dependent Variable: Has Any Post-Secondary Training			
Dummy for Cohort with 9th Grade in Basic Track	0.006 (0.005)	0.007 (0.003)	-0.003 (0.003)
Number of Observations	413,276	413,276	413,276
Sample: No Post-Secondary Training -- Dependent Variable: Log Wage			
Dummy for Cohort with 9th Grade in Basic Track	-0.003 (0.009)	-0.006 (0.009)	-0.006 (0.011)
Number of Observations	70,209	70,209	70,209
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.

Table 9
Primary and Secondary School Skills Used on the Job
Qualification and Career Survey

	Basic Track	Higher Tracks	No. of obs.
Uses math on the job	45.5	56.7	37,198
Arithmetic	48.4	59.7	17,291
Simple algebra	29.4	52.6	17,291
More advanced algebra	6.9	19.4	17,291
Geometry	8.4	16.7	17,291
Calculus, statistics	1.4	9.8	17,291
Uses knowledge of German language on the job	38.2	64.4	23,942
Communicate orally	34.8	60.3	17,291
Write simple letters	25.1	53.4	17,291
Write complex text	9.6	38.3	17,291
Communicate about complex circumstances	10.0	39.2	17,291
Uses foreign language on the job	3.8	24.7	37,198

Source: Qualification and Career Survey. Results on math usage and foreign language usage is based on 1979, 1992 and 1999 waves, results on knowledge of German language is based on 1979 and 1999 waves, and detailed math and German usage is based on 1979 wave.

Table 10
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
Netherlands	201	261	183	241
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 11
Scores on the First International Mathematics Study
Results for 13 Year Olds

	Country					
	Germany	US	England	France	Netherlands	Sweden
Mean Total	0.16	-0.35	0.05	-0.14	-0.11	-0.52
Mean Basic Arithmetic	0.28	-0.19	-0.19	-0.12	0.12	-0.40
Correct Answers	*Percent of Students*					
0	0	2	4	1	0	1
5 or less	4	18	19	10	7	17
10 or less	10	32	29	22	19	37
15 or less	21	46	40	38	33	55
20 or less	34	60	49	54	50	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

Appendix Table 1
Sample Means
(Standard Deviations in Parentheses)

Variable	QaC	Micro Census Employed	Micro Census All	Social Security Data
Survey year	1986.9	1995.3	1995.4	1984.8
	(7.0)	(3.7)	(3.7)	(5.9)
Year of birth	1947.1	1948.7	1946.8	1945.9
	(8.5)	(7.7)	(8.5)	(8.8)
Age	40.1	46.0	47.9	38.9
	(10.0)	(7.7)	(8.6)	(10.0)
Female	0.36	0.38	0.50	---
Years of Schooling	9.4	9.7	9.5	---
	(1.7)	(1.8)	(1.7)	
Length of Schooling	10.0	---	---	---
	(2.0)			
Years of Post-Secondary Training	2.3	2.4	2.1	---
	(1.5)	(1.4)	(1.4)	
Years of Education	11.7	12.1	11.6	---
	(2.8)	(2.9)	(2.8)	
Basic track: 8th grade	0.40	0.35	0.43	} 0.94
Basic track: 9th grade	0.21	0.20	0.18	
Middle track: 10th grade	0.23	0.22	0.21	
Academic track: 12th grade	0.03	0.05	0.04	0.02
Academic track: 13th grade	0.13	0.18	0.14	0.04
Employed	1.00	1.00	0.70	1.00
Self-employed	---	0.10	---	---
Number of observations	54,126	750,416	1,225,877	2,414,205