

indirect trade that we document here is particularly prevalent in high-tariff, weak-governance economies. In addition, it may ultimately be possible to evaluate, for example, the extent to which different source countries are prone to tariff evasion by comparing the relationship between tariffs and indirect trade across exporting countries. We leave these topics for future research.

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## APPENDIX

TABLE A1.—LIST OF COUNTRIES

Country	Annual Observations
Argentina	356
Australia	1,250
Austria	1,789
Canada	1,089
Czech Republic	645
Denmark	797
Finland	961
France	2,209
Germany	2,890
Great Britain	2,246
Greece	204
Hungary	290
Indonesia	1,292
Ireland	448
Italy	2,418
Japan	3,649
Korea	3,363
Mexia	257
Netherlands	1,453
New Zealand	426
Norway	564
Poland	107
Portugal	335
Slovenia	135
Spain	1,279
Sweden	1,390
Switzerland	1,791
Turkey	467
United States	3,569

## ZERO RETURNS TO COMPULSORY SCHOOLING IN GERMANY: EVIDENCE AND INTERPRETATION

Jörn-Steffen Pischke and Till von Wachter\*

*Abstract*—We estimate the impact of compulsory schooling on earnings using changes in compulsory schooling laws in West Germany after World War II. Most estimates in the literature indicate returns in the range of 10% to 15%. While our research design is very similar to studies for various other countries, we find a zero return. We find no evidence that this is due to labor market institutions or the apprenticeship training system in Germany. 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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## I. 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 dropout age. 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.

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 ninth grade for students in this type of school. There is ample within-state variation to identify the effects of the introduction of the ninth 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 United States and by Oreopoulos (2007) for the United States, Canada, and the United Kingdom.

We find that the returns to compulsory schooling are basically zero in Germany. This result is robust to a range of specification checks, across several large data sets, and our main results are precisely estimated. Why should the returns to schooling be zero or small in Germany, when returns in the order of 10% to 15% 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 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 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.

We suggest as an explanation that German students are 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 United States and the United Kingdom. We argue that it is those skills that matter ultimately in the labor market for the target population. Hence, few labor market-relevant skills may have been learned during grade 9.

Typically, empirical studies find a substantial return to a year of additional compulsory schooling. This return is often larger than corresponding OLS estimates.<sup>1</sup> The impact of compulsory schooling laws in Germany is of substantial interest because the earnings effects differ so much from the preceding literature on this topic. Our results suggest that the return to education depends on institutions and the organization of the school system. Two other recent studies also 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, and finds returns of 3% to 5%, below the OLS returns of 7.5% in his sample. Oosterbeek and Webbing (2007) analyze the extension of some vocational-training programs in the Netherlands from three to four years of length. They 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, or father's absence due to the war. The authors do not document at which levels education is affected by these instruments. Given that the ninth grade was not available to basic track students during the war, it is likely that most of the effects are either at lower or at higher grades. Becker and Siebern-Thomas use the urbanization of the region where an individual grew up as instrument. They show that their instruments will pick out primarily differences in grades 10 and above. All three studies find larger returns to education than by using OLS, in the order of 10%. However, these returns mostly refer to very different grades than in our study.

## II. The German Schooling and Training System

After four years of primary school, 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 certificate. The middle track (*Realschule*) ends after grade 10 and is more academically rigorous than the basic track. Middle- and lower-track students 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.<sup>2</sup> Initially after World War II, about 75% of secondary graduates would have attended the basic track in secondary school. The higher-level tracks have expanded rapidly since the 1950s so that the fraction of students in the basic track had dropped to around 50% by the end of our sample period.

Basic school used to last up to grade 8 before World War II. Hamburg and Berlin were the first states to introduce a ninth grade in 1949. During the immediate postwar period, lack of labor market opportunities and apprenticeships for school leavers were important arguments in the debate about the introduction of the ninth grade (Petzold, 1981). Some states introduced a mandatory ninth grade on a temporary basis during the early 1950s. For example, Niedersachsen had laws from 1950 to 1955 allowing the education minister to mandate ninth grade attendance by county or town. Bavaria had a similar law in 1952. Two northern states, Schleswig-Holstein and Bremen, permanently introduced the ninth 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 (see also LeSchinsky, 1981). Two other states, Niedersachsen and Saarland, introduced the ninth grade in this general environment 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 ninth grade starting in 1967. Four states introduced the ninth grade in 1967, although this was not done in Bavaria until 1970.<sup>3</sup>

The content of the curricula for the additional ninth grade differ somewhat between states. Nordrhein-Westfalen (1962) reports that Berlin used the ninth grade to focus more on political education, while Bremen stressed "general knowledge." In Niedersachsen, the curriculum was centered around three goals: to consolidate basic skills, to give students access to the adult world and teach political responsibility, and to acquaint school leavers with the world of work. However, these are differences in emphasis. In all states students would typically have instruction in the same subjects during the ninth grade

<sup>2</sup> 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. There is significant selection into school types by academic ability and parental background (see for example Dustmann, 2004). This implies that basic school students tend to be the lowest-ability students, and they also face the least challenging curriculum.

<sup>3</sup> Education policies are set by the individual states within framework agreements, which ensure that the school systems in all states remain comparable enough. In four states, which introduced the ninth 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, 2007, for details). See the working-paper version of our paper (Pischke & von Wachter, 2005) for the complete list of ninth grade introduction dates.

<sup>1</sup> For excellent surveys of this literature see Card (1999) and Oreopoulos (2007).

as in previous years: mathematics, German language, sciences, social sciences, as well as some pre-vocational training.<sup>4</sup>

After completing the basic or middle track, students typically enter a firm- or school-based vocational training course, most commonly an apprenticeship. Compulsory schooling does not necessarily end in Germany with the completion of secondary school but extends to a part-time vocational school.<sup>5</sup> In addition to the firm-based training, an apprentice will attend a part-time vocational school. Apprenticeship training is often necessary for entry into certain skilled jobs or 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.

### III. The Data

The data are taken from two 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% of the households in Germany. We use the surveys from 1989, 1991, 1993, and all years from 1995 to 2004. Each wave has about 300,000 to 400,000 observations for the west German states. The samples used below are restricted to Germans living in the ten west German states (excluding Berlin), who were born from 1930 to 1960.

Neither of the two data sets has direct information on whether an individual finished after eight or nine grades. German data do not typically contain a variable with the number of years of schooling or the highest grade attended. The Qualification and Career Survey is appealing despite the smaller sample size because it contains more detailed information on schooling. 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 comprehensive information on postsecondary educational attainment and training. The typical approach to constructing years of education 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.<sup>6</sup> Using this information, the year of birth, and information on the timing of the introduction of the ninth grade across states, we imputed whether an individual will have graduated after eight or nine years in the basic track. However, since the data set 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

<sup>4</sup> Some states also started offering an optional tenth grade for basic school students after introducing a ninth grade, although this did not become important until the 1970s. The tenth grade in the basic track of secondary school allows students to obtain the tenth 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 ninth grade.

<sup>5</sup> 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.

<sup>6</sup> Pischke (2007) reports that in the 1990s about 85% of individuals in these cohorts still live in the same state as in 1967.

6), we impute the number of years in school as secondary school graduation year minus year of birth minus six.

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.

There is no earnings measure on this data set but it contains a variable for net monthly income.<sup>7</sup> 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 times 4.3.

The data also provide the highest secondary school degree attained, and some basic information on postsecondary 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 also limited to the state of residence.

### IV. 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. Table 1 shows the resulting first-stage regressions. We can only construct a sensible first stage in the QaC data using the imputed number of years of school based on the secondary school graduation year. Column 1 shows that the coefficient on this measure is only 0.19 with a *t*-statistic of about 5. Even among basic school students (about 60% of the sample), the introduction of the ninth grade only led to an increase of their schooling by about 0.28 years (column 2). This indicates that the introduction of a compulsory ninth grade was more gradual in practice (something we actually observe in the states where we can check it using published attendance data), and it reflects attenuation due to various measurement problems and migration.<sup>8</sup> However, note that this is measurement error in the instrumental variable we will use below. This measurement error does not lead to bias unless it is systematically correlated with both the ninth grade introduction and earnings, which is rather unlikely.

An important question is whether the introduction of the ninth grade affected track choice directly. We find in column 3 of table 1 that students subject to the ninth grade in basic track are slightly less likely to attend the basic track. However, the effects are small and not significant. The estimates are essentially zero for the much larger samples in the Micro Census. If track choice is truly unaffected by the ninth grade introduction, conditioning the sample on basic track students, as we do in column 2 and below, will still result in causal estimates. In particular, since this is the group of students most affected by the change in compulsory schooling, we should see the strongest and most precisely estimated effects for this group. Nevertheless, we start by presenting results for the full sample first.

Table 2 displays regressions for log wages. All regressions include a dummy for females, a quartic in age, and the maximal set of state,

<sup>7</sup> See the appendix for a discussion of earnings and income variables in our two data sets.

<sup>8</sup> We document that all of these problems are important in the working-paper version (Pischke & von Wachter, 2005). The attenuation is exacerbated by the large number of other controls in the regression (particularly cohort dummies) which are correlated with the ninth grade indicator.

TABLE 1.—FIRST-STAGE REGRESSIONS

Independent Variable	Dependent Variable		
	Imputed Number of Years in School (primary + secondary)		Attends Basic Track
	Full Sample (1)	Basic Track Only (2)	Full Sample (3)
<i>Qualification and Career Survey</i>			
Dummy for cohort with ninth grade in basic track	0.190 (0.039)	0.285 (0.033)	-0.012 (0.011)
Number of observations	54,126	32,970	54,126
<i>Micro Census</i>			
Dummy for cohort with ninth grade in basic track	—	—	-0.003 (0.004)
Number of observations	—	—	939,736

Note: All regressions are estimated by OLS. All regressions also include a dummy for female, a quartic in age, and the maximal sets of year dummies, state of residence dummies, year of birth dummies, and interactions of state of residence dummies with a linear trend in year of birth. The standard errors (in parentheses) are adjusted for clusters at the state × year of birth level.

survey year, and year of birth effects. We start in columns 1 and 2 by showing standard OLS regressions. Column 1 uses the a constructed measure of years of education based on the secondary and postsecondary degrees obtained. The returns are between 6% and 7% in this OLS specification. Column 2 shows that the results are very similar when we use the imputed number of years in school in the QaC data.

In column 3 we turn to the effect of compulsory schooling by showing the reduced-form effect for the introduction of the ninth grade. The effect is identified via a differences-in-differences design, because state of residence and year of birth effects are controlled for. In these regressions we also control for linear state-specific cohort trends (something that makes virtually no difference in the OLS regressions). It is always a concern 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.

The estimate for the Micro Census is close to zero, while the estimate in the QaC is somewhat larger. Obviously, the reduced-form estimates are affected by measurement problems, just like the first-stage estimates.

The measurement error is in the assignment of the ninth grade, our instrument. Since even nonclassical measurement error in the instrument will generally affect the first-stage and reduced-form estimates proportionately, it cancels in the instrumental variable estimate. We thus turn to instrumental variables estimates shown in column 4. We instrument the endogenous regressor, imputed number of years of schooling, with a dummy variable for whether the state had introduced the ninth grade in basic track for the respective birth cohort. For the QaC data, this is the standard instrumental variables estimate. For the Micro Census, we divide the reduced-form result in column 3 by the appropriate first-stage estimates from the QaC in table 1. This is effectively a two-sample indirect

TABLE 2.—RETURNS TO EDUCATION

Independent Variable	Dependent Variable: Log Wage					
	Full Sample				Basic Track Only	
	OLS (1)	OLS (2)	OLS (RF) (3)	IV (4)	OLS (RF) (5)	IV (6)
<i>Qualification and Career Survey</i>						
Years of education	0.061 (0.001)	—	—	—	—	—
Imputed number of years in school	—	0.066 (0.002)	—	0.058 (0.038)	—	-0.005 (0.034)
Dummy for cohort with ninth grade in basic track	—	—	-0.010 (0.008)	—	-0.001 (0.010)	—
Number of observations	54,126	54,126	54,126	54,126	32,970	32,970
<i>Micro Census</i>						
Years of education	0.074 (0.001)	—	—	—	—	—
Imputed number of years in school	—	—	—	0.016 (0.015)	—	0.013 (0.011)
Dummy for cohort with ninth grade in basic track	—	—	0.003 (0.003)	—	0.004 (0.003)	—
Number of observations	939,736	939,736	939,736	939,736	507,623	507,623

Note: 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. The reduced-form and IV results (cols. 3 to 6) also include interactions of state of residence dummies with a linear trend in year of birth. Columns 4 and 6 report standard IV results instrumenting length of schooling with a dummy for cohort with a ninth grade in basic track for the QaC, and two-sample indirect least squares estimates for the Micro Census, with the first stage taken from the QaC. Imputed number of years in school is secondary graduation year minus year of birth minus 6. The standard errors (in parentheses) are adjusted for clusters at the state × year of birth level.

TABLE 3.—VARIOUS REDUCED-FORM REGRESSIONS, MICRO CENSUS

Independent Variable	Dependent Variable					
	Employed	Self-Employed	Log Wage	Any Postsec. Training	Log Wage	Body Mass Index
	Sample					
	All (1)	Employed (2)	Self-Employed (3)	Employed (4)	No Postsec. Training (5)	All (6)
<i>Full Sample</i>						
Dummy for cohort with ninth grade in basic track	0.003 (0.002)	-0.001 (0.002)	-0.006 (0.014)	—	—	-0.160 (0.056)
Number of observations	1,637,291	939,736	97,623			94,877
<i>Basic Track Only</i>						
Dummy for cohort with ninth grade in basic track	0.006 (0.003)	0.001 (0.002)	0.006 (0.021)	0.007 (0.003)	-0.003 (0.009)	-0.222 (0.081)
Number of observations	1,006,407	507,623	40,389	507,623	85,177	60,182

Note: All regressions are estimated by OLS. They also include a dummy for female, a quartic in age, and the maximal sets of year dummies, state of residence dummies, year of birth dummies, and interactions of state of residence dummies with a linear trend in year of birth. The standard errors (in parentheses) are adjusted for clusters at the state × year of birth level.

least squares estimate.<sup>9</sup> The resulting coefficient for the QaC is slightly below the OLS estimate and it drops to less than 2 percentage points in the Micro Census. We have also replicated all these estimates with a large administrative data set from the German Social Security Administration, and run specifications without state-specific trends, and with quadratic trends. The estimates are always close to zero except in the QaC when trends are included (see Pischke & von Wachter, 2005).<sup>10</sup>

An alternative way of estimating the effects of the introduction of the ninth grade is to focus on basic school students. Limiting the sample in this way is valid since the ninth grade treatment did not affect the track attended very much. The results are shown in columns 5 and 6. The results are again essentially 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 in the reduced form should be amplified in this subsample of students who were actually affected by the policy.

Overall, we interpret our results to mean that the returns from the introduction of the ninth year of schooling in Germany is zero or small. The only estimate substantially larger than zero is from the QaC. However, we discount this result for two reasons. First, we do not find a similar result when we limit the sample to basic track students in columns 5 and 6 of the table. This suggests that the larger estimate in columns 3 and 4 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. Instead the results in the QaC data in columns 3 and 4 may be simply due to sampling variation.

Second, the coefficients are also estimated more precisely in the Micro Census than the QaC, a much smaller data set. The estimate from the Micro Census in column 6, for example, implies a confidence interval of -0.008 to 0.034. In summary, the evidence presented here suggests that the return to compulsory schooling in Germany is small and most likely zero. Measurement error in our instrument cannot explain the small effects, since it does not affect the instrumental variable estimates. Even the upper end of the confidence interval of our estimates is well below the 15% return reported by Harmon and Walker (1995) and Oreopoulos (2007) for the Anglo-Saxon countries. This suggests that the returns to a year of compulsory schooling in Germany is very different from the returns in these earlier studies.

### V. 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.<sup>11</sup>

The first set of explanations is based on the idea that the productivity of ninth grade school leavers is actually higher but for some reason this is not reflected in higher wages. One possible explanation for the lack of wage returns might be that the wage-setting institutions in Germany prevent the adjustments necessary to reflect any returns. However, employers should be more interested in employing the more productive ninth grade graduates than the equally expensive but less productive school leavers from eighth grade. Hence we would expect to see employment effects for the ninth grade.

Table 3 explores this possibility in the Micro Census. In column 1 we focus on all individuals and replace the dependent variable with a dummy variable for whether the respondent is employed. The employment effects are small in the full sample but larger among basic track students. But these effects vanish once we include squared

<sup>9</sup> These standard errors are derived using the delta method. Denote the first-stage coefficient by  $\hat{\pi}$ , the reduced-form coefficient by  $\hat{\delta}$ , and the ILS estimate by  $\hat{\beta}$ . Then

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

because  $\hat{\pi}$  and  $\hat{\delta}$  are estimated from independent samples, and hence there is no covariance term. 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.

<sup>10</sup> One worry is selective labor force participation. When we restrict the sample to men, we find in the Micro Census an implied IV estimate of 0.074 with a standard error of 0.038 in the full sample but only a return of 0.016 with a standard error of 0.013 among basic track students. Given the sampling variation in the full sample and the lack of a positive return among basic track students we feel that the larger coefficient in the full sample is likely due to sampling variability. We do not find any other qualitative differences between the results for men and the results presented in the tables.

<sup>11</sup> We discuss some other explanations in the working-paper version (Pischke & von Wachter, 2005).

state-specific trends. The effects are smaller or negative when the sample is restricted to men (not shown in the table). Selective labor force participation is less of an issue for men. Hence we find no robust employment effects of the increase in compulsory schooling age.

An additional implication of the wage rigidity explanation for the absence of wage returns is that ninth 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 ninth grade graduates. We do not find any support for this possibility either in columns 2 and 3 of table 3.<sup>12</sup>

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 ninth grade was introduced. One simple view would be that the introduction of the ninth 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 eight or nine grades of school. Hence, the German experiment is very different from forcing a U.S. high school dropout 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 ninth grade because of the role of apprenticeship training, we will look at those individuals who do not complete an apprenticeship. For these individuals, the eighth or ninth grade they complete in secondary school is their final level of schooling.<sup>13</sup> This is only a small fraction of all school leavers even from basic school, about 17% in our sample. Hence, this group is much more comparable to, for example, high school dropouts in the United States who are affected by compulsory schooling laws (see Angrist & Krueger, 1991).

We start in column 4 of table 3 by investigating whether the incidence of apprenticeship or other vocational postsecondary training was affected by the ninth grade introduction. This only makes sense for basic track students because upper-track students have additional options without further training. The ninth grade introduction raised post-school training slightly. The effect is economically small, and it vanishes in a specification where we introduce squared state-specific trends. If there is no selection into who completes an apprenticeship, we can analyze wages for those school leavers who do not complete any postsecondary training. This is the group that is much more comparable to U.S. high school dropouts or U.K. school leavers with no postcompulsory 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. Column 5 in table 3 shows that this is not the case. The point estimate is basically zero and not significant. Hence, we do not find support for an explanation of our results simply based on the training system in Germany either.

An alternative set of explanations of our results implies that the productivity of ninth grade school leavers in the labor market is actually not any higher than the productivity of those who completed

only eight grades. For example, in a pure signaling model schooling does not enhance productivity at all. Given the structure of the German school system, the signal would likely be the secondary track chosen rather than the grade completed. This means that the signaling value of a basic track education would be unchanged after the introduction of the ninth grade. Such a model would be consistent with the results from other countries if the signal in the United States or the United Kingdom, 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 signaling explanation with our data, such a model seems extreme, however. For example, education seems to have a causal effect on other outcomes like health or political participation, which points toward a human capital explanation. Column 6 in table 3 demonstrates that individuals exposed to the ninth grade have a lower body mass index, a measure commonly used in the health literature.<sup>14</sup> Hence, it is of interest to consider other explanations that allow for a role of human capital but are consistent with a low or zero return to the ninth grade in basic track. We believe that 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 some role in this. Suppose that workers use two types of skills, academic and vocational. Vocational skills are learned in apprenticeship training or on the job. In terms of academic skills, school leavers from basic track use only 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 ninth grade in basic track in Germany. On the other hand, students in other countries like the United States, Canada, or the United Kingdom 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.

We present a few pieces of evidence consistent with this explanation. First, in the QaC, only 6% of basic school leavers name general schooling as one of the venues where these skills were learned. Even for those basic school leavers with no further training, the fraction learning skills in school is less than 8%. Second, while half of basic track respondents say they use math skills on their job, the skills they actually use are basic arithmetic, and to some extent 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. The disparity between basic track and higher track students is even more apparent with respect to language skills. This evidence corroborates our claim that students in the basic group use only relatively basic academic skills.

Third, results of the International Adult Literacy Survey (IALS) and the First International Mathematics Study (FIMS) suggest that for the cohorts relevant to our study, German students at the bottom end of the skill distribution show higher proficiency in literacy and quantitative skills than students in the United Kingdom or the United

<sup>12</sup> Negotiated wages are typically tied to occupational groups, but employers always have the option to use job assignments to change wages. Hence, we wouldn't really imagine wage rigidity to preclude any returns.

<sup>13</sup> 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% of those school leavers without a completed apprenticeship actually start an apprenticeship.

<sup>14</sup> For the United States, see Currie and Moretti (2003) and Lleras-Muney (2005) on health, Lochner and Moretti (2004) on crime, and Dee (2004) and Milligan, Moretti, and Oreopoulos (2004) on voting. We do not find any effects of the ninth grade on other health outcomes. We also find no effects on voting or political attitudes using data from the Politbarometer, but both of these sets of results are fairly noisy.

States.<sup>15</sup> These results imply that basic academic skills of Germans are better at the bottom end of the distribution. The FIMS results show that these skills are present by age 13.

Hence, we conclude that a possible 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.

## VI. Conclusion

While previous estimates of the returns to compulsory schooling are typically in the range of 10% to 15%, we find that the returns to an additional grade in Germany are basically zero. We establish this result using two large data sets. The results are all fairly consistent, and our main results are 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 suggest a possible explanation 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, or apprenticeship training.

Our results are important because they are in stark contrast to the existing literature on the causal effects of education. They caution against extrapolating from even a large body of existing results to environments with other institutional features. This suggests 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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## DATA APPENDIX

The earnings variable in the QaC 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. 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 DM 12,500. Only 1.8% of sample observations are in the top income bracket. We then convert the variable to an hourly wage by dividing by the number of weekly hours.

In the Micro Census, income is also reported in brackets. There were 18 brackets from 1989 to 1999, and 24 brackets from 2000 to 2004. We assign midpoints to the brackets again. The top bracket in 1989 was DM 5,000 or more, which we assigned a value of DM 7,500; in 1991–99 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 a value of DM 40,000; and from 2002 to 2004 it was €18,000 or more, which we assigned a value of €20,500.

<sup>15</sup> See Pischke and von Wachter (2005) for a detailed discussion.