

# Schooling and Citizenship in a Young Democracy: Evidence from Postwar Germany

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## Abstract

This paper examines whether schooling has a causal impact on individuals' political behavior. Between 1949 and 1969, the number of compulsory years of schooling in the Federal Republic of Germany was gradually increased across all federal states. These legislative changes provide an opportunity to investigate the causal impact of schooling on political behavior. Years of schooling are found to be positively correlated with several political outcomes. However, there is little evidence of a causal effect. This study conjectures that there is ample historical evidence to support the hypothesis that the fundamentals of democracy were already learned earlier in school, potentially outweighing the political returns of schooling in Germany.

*Keywords:* Voting; civic engagement; education; externalities; instrumental variables estimation

*JEL classification:* I2; H4; H23

## I. Introduction

“The higher one’s education, the more likely one is to believe in democratic values and support democratic practices.” [Lipset, 1976, p. 56]

Philosophers, economists, and political scientists have long argued that education plays a major role in the transition from authoritarian rule to democracy and in the sustainability of democratic systems. Lipset (1976) emphasized that better-educated individuals facilitate the functioning of democracy because they are more likely to believe in fundamental democratic principles and to actively support democratic practices. Similarly, Franklin (1996), Nie, Junn, and Stehlik-Barry (1996), and Przeworski, Alvarez, Cheibub, and Limongi (2000) stress that individuals with

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higher education are “better” citizens. They are more likely to vote for democratic parties, to believe in democratic values, to participate and be interested in politics, and to critically observe the activities of the government and politicians.

The question of whether more schooling affects individuals’ democratic attitudes and political behavior has recently been approached for the first time by economists. Dee (2004) investigates the impact of schooling on voter turnout and civic participation in the US, while Milligan, Moretti, and Oreopoulos (2004) exploit the impact of years of schooling on voter participation and political interest in the US and the UK. Both report a positive and statistically significant causal effect of schooling years on voter participation in the US, whereas the latter finds no such effects for the UK. Both studies also report a positive effect on several forms of civic behavior. This paper contributes to this small body of literature in several ways. First, it investigates whether there are any causal effects of education on a broad range of political outcomes, such as political interest, participation in voting, political involvement, and democratic values. This is important because political activities vary in time intensity, and the opportunity cost of time considerations might play a key role when examining the effect of schooling on citizenship. Second, differences in democratic institutions, government forms, electoral rules, teaching environments, and historical experiences across countries may shape democratic citizenship and influence the effect of schooling on political behavior. This study aims to shed light on the effect of schooling on civic behavior in Germany, a country that recently experienced the transition from a brutal dictatorship to democracy and the former educational system is argued to have failed to prevent Nazism and to help to sustain the Third Reich (Merritt, Flerlage, and Merritt, 1971).

There are good reasons to suspect that more schooling might promote democratic citizenship. First, the more education individuals have, the more insight and awareness they gain about basic democratic values, the political system, and the meaning of civil liberties (Nie *et al.*, 1996). Second, having more schooling is likely to increase people’s cognitive and analytical abilities, social skills, and cultural sophistication. This, in turn, might result in higher levels of tolerance toward other people’s political and religious beliefs. For example, Nunn, Crockett, and Williams (1978) found that with more years of schooling, students also showed increased tolerance of freedom of speech. Better cognitive skills might increase individual powers of discernment in choosing more capable politicians, and thus result in an electorate better able to judge the government critically (Milligan *et al.*, 2004). Moreover, Verba, Schlozman, and Brady (1995, p. 304) argue that those with better communication and organizational abilities should find “political activity less daunting and costly”. Third, having more education increases the chance that people will learn from history, develop a better

understanding of the importance of democracy, and also gain more consumption value from political participation.<sup>1</sup> According to the Council of Europe, improving students' political knowledge and civic participation is a central goal of German federal government policies: "The objective of education for democratic citizenship in school is to transmit democratic values and skills to pupils so that they grow up to be responsible citizens. This is emphasized in the curricula of all the Länder [federal states]".<sup>2</sup> Finally, more schooling might influence citizenship indirectly. People with more schooling typically have higher paying jobs; they also have a greater chance of learning politically relevant skills at work and to be surrounded by colleagues who might be politically interested and active (Verba *et al.*, 1995).

In contrast, there are also plausible reasons for why more schooling could actually reduce democratic citizenship. Standard economic theory suggests that individuals with more education are likely to have higher opportunity costs of time and could therefore reduce time devoted to civic activities. This might be particularly true for time-intensive political activities, such as participating in demonstrations or being active in a citizen group. For example, Verba *et al.* (1995, pp. 128–130) report that the most important reason mentioned by respondents for their political inactivity was lack of time. Moreover, with respect to election participation, Dee (2004, p. 1700) mentions that "education could also reduce voter participation by promoting an awareness of voting as an essentially expressive act with an infinitesimally small probability of influencing actual policy".

Investigating whether society as a whole might benefit from increasing the number of years of compulsory schooling is important from a policy perspective. One of the justifications for public financial support of the educational system is that schooling provides important social benefits by promoting tolerance, democratic values, and political involvement. If true, this suggests that there are important social returns over and above the private returns from education usually studied by economists.<sup>3</sup>

This paper investigates whether schooling has a causal impact on individuals' democratic values, political awareness, participation in voting, political involvement, and political group membership in West Germany. Between 1949 and 1969, compulsory schooling was increased from eight to nine years at secondary schools in the Federal Republic of Germany; this change was introduced gradually over time in the different federal states (Pischke and von Wachter, 2008). This reform provides variation in years of schooling, which is exogenous to unobserved characteristics that may affect both

<sup>1</sup> For example, individuals with more schooling have been found to be better informed about Communist and Nazi beliefs (Nunn *et al.*, 1978).

<sup>2</sup> See <http://www.coe.int> for further information.

<sup>3</sup> See, for example, Angrist and Krueger (1991), Oreopoulos (2006), Pischke and von Wachter (2008) in the recent returns to education literature.

education and political outcomes—for example, “social and political values”, “intelligence”, or family background characteristics.

Using pooled cross-sectional data from ALLBUS (the German General Social Survey) and ForsaBus (a very large survey on political attitudes), I find that years of schooling are positively correlated with political awareness, participation in voting, democratic values, and political group membership. However, instrumental variable (IV) estimates provide no evidence that there is a causal effect of education on democratic citizenship in Germany. The IV estimates are considerably smaller than standard probit estimates; most marginal effects are negative or close to zero, and the overwhelming majority are not statistically different from zero.

Several potential explanations will be discussed: (1) poor or apolitical teaching practices in the postwar period; (2) extracurricular civic education; and (3) whether civic knowledge might have already been learned earlier in school. This paper conjectures that a variety of historical and empirical evidence suggests that civic educational reforms prior to the increase in compulsory years of schooling is likely to have had a positive impact on citizenship in postwar Germany, diminishing the marginal political returns of an additional year of schooling towards zero.

The next section presents the data, sample selection, and variables used in the analysis. Section III discusses the econometric framework and identification strategy. Section IV presents the main results concerning the relationship between schooling and democratic citizenship. Section V discusses possible explanations for why the increase in compulsory years of schooling had no positive effect on citizenship in Germany. Robustness checks are presented in Section VI, and the final section concludes.

## II. The Data

The data are taken from two different datasets, both of which are repeated cross-sectional surveys. The first is the ALLBUS, the German General Social Survey. I use 14 pooled cross-sectional waves from ALLBUS, for survey years 1980, 1982, and every second year thereafter up to and including 2004, plus 1991. The ALLBUS provides comprehensive information on a broad variety of political outcomes, age, sex, year of birth, years of schooling, highest school degree, parents' highest school degree, father's occupational prestige score, federal state of birth, and federal state of residence.

In each survey year, respondents were asked about their highest school certificate, but only respondents to the 1990, 1991, and 1992 surveys were also asked about their years of schooling.<sup>4</sup> The question in these three

<sup>4</sup> Note that most German datasets do not contain any information about years of schooling (Pischke and von Wachter, 2005).

years reads, "How many years of schooling did you complete without any vocational training? If you have a university or technical college degree, please include these years." Since years of primary and secondary schooling are available only for those who did not attend university or technical college, as a first sample selection criterion, I restrict the analysis to individuals who graduated from some form of secondary school but do not have a university or technical college degree.<sup>5</sup> This results in a sample of individuals for whom compulsory schooling reform is likely to be relevant, because individuals with a university degree would probably have acquired the maximum years of schooling irrespective of any compulsory schooling reform. In other words, I exclude individuals who were unlikely to be constrained by compulsory school attendance laws.<sup>6</sup> The sample is composed of native Germans born between 1930 and 1960, currently living in one of the western federal states, and possessing a school certificate from one of the three main schooling tracks but not possessing a university or technical college degree.<sup>7</sup>

There are five types of measures of democratic citizenship in the ALLBUS:

- **Political Awareness:** whether interested in politics;
- **Participation in Voting:** whether voted in last general election;
- **Democratic Values:** whether thinks that every democratic party should have the opportunity to hold power in a parliamentary system, whether thinks that political demonstration is a civil right;
- **Political Involvement:** whether ever active in a citizens' group, whether signed a petition, whether participated in an approved demonstration;
- **Political Group Membership:** whether member of a citizens' group, whether member of a political party.

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<sup>5</sup> Because years of schooling are only available in three surveys, I generate a new education variable that imputes the number of years of schooling for all survey years using a comprehensive set of covariates (year of birth, sex, highest school degree, parents' highest degree, father's occupational prestige score, federal state of residence, and years of schooling provided in the three surveys) by using the switching regression technique following van Buuren, Boshuizen, and Knook (1999). Section VI presents two robustness checks with respect to alternative definitions of years of schooling. Sensitivity analysis shows that the results are robust to these alternative definitions.

<sup>6</sup> Pischke and von Wachter (2005) provide evidence that the introduction of compulsory schooling laws in Germany did not significantly affect school track choices. This suggests that restricting the sample to those with lower levels of education still results in an estimation of causal effects.

<sup>7</sup> Individuals currently living in West Berlin are excluded from the analysis to avoid potential problems from East–West migration.

All dependent variables are dichotomous, with the value one if respondents state expressions of agreement or answer with yes, and zero otherwise. I use a broad range of political outcomes to identify as precisely as possible the impact of schooling on various dimensions of political behavior and to investigate whether the opportunity costs of time considerations play a role in civic participation.

The second dataset is ForsaBus, a large survey on political attitudes. The ForsaBus data used come from the 1991–2006 waves. As with the ALLBUS sample selection, I restrict the sample to Germans born between 1930 and 1960 who currently live in a western federal state and do not hold a university or technical college degree.<sup>8</sup> The ForsaBus allows me to draw inferences from a very large sample, comprising nearly 500,000 individuals. However, the data provide no information on parental background and fewer citizenship outcomes are collected.<sup>9</sup>

### III. Estimation Methods and Identification Strategy

I start by investigating the relationship between years of schooling and democratic citizenship, estimating standard probit regressions. The association between years of schooling and political outcomes for each individual takes the form

$$\Pr(P = 1) = \Phi(\beta_1 S + X\kappa), \quad (1)$$

where  $P$  represents a political outcome,  $S$  indicates years of schooling, and  $X$  is a vector containing the covariate's age, age-squared, year of birth, a full set of year dummies, federal state dummies, and state-specific cohort trends.<sup>10</sup> In a second step, I account for endogeneity of years of schooling by estimating instrumental variable probit regressions.<sup>11</sup> The first-stage regression for each individual has the form

$$S = \alpha_0 + \alpha_1 R + X\lambda + v, \quad (2)$$

<sup>8</sup> Similarly, respondents are assigned a date of introduction of the compulsory schooling reform based on current state of residence, since state of birth information is not available.

<sup>9</sup> See the working paper version (Siedler, 2010) for a brief description of the German educational system, the compulsory schooling reform, detailed definition of the outcome variables in both surveys, and summary statistics.

<sup>10</sup> See, for example, Oreopoulos (2007) and Pischke and von Wachter (2008) for a similar set of explanatory variables.

<sup>11</sup> IV probit estimates are maximum likelihood estimations of Amemiya's generalized least squares estimator (Amemiya, 1978; Newey, 1987) and are estimated using Stata's `ivprobit` command. This method has the advantage that it produces predicted outcomes between 0 and 1. In unreported regressions, I also estimated standard 2SLS on dichotomous outcomes. Linear IV estimations produced comparable results to the IV probit estimates.

with  $S$  and  $X$  defined as in equation (1). The instrumental variable  $R$  equals one if a person was affected by the compulsory years of schooling reform, and is zero otherwise. Identification of the estimate  $\alpha_1$  results from exogenous increases in compulsory years of schooling over time in any given federal state, as previously documented by Pischke and von Wachter (2005, 2008). These laws are ideal instruments because they compel students to stay one year longer in school. The identifying assumption is that, conditional on the covariates included in  $X$ , the point in time when compulsory school was increased in each federal state is orthogonal to factors influencing an individual's political behavior.<sup>12</sup> This implies that the schooling effect  $\alpha_1$  is not identified if unobserved state-specific effects that influence political outcome variables also changed at the same time compulsory schooling laws changed. It is therefore important to control for state-specific trends that might be correlated with the introduction of compulsory years of schooling (Oreopoulos, 2007; Pischke and von Wachter, 2008). Another potential concern with respect to the validity of the law changes as exogenous variation is that states with a higher proportion of educated people might have been more likely to introduce law changes earlier. However, according to Petzold (1981), the main arguments for the expansion of schooling was the lack of labor market opportunities and the belief that students with eight years of schooling were not mature enough for the labor market. Moreover, the robustness section below examines whether the inclusion of potential important political, social, or economic characteristics that might be associated with both the timing of the schooling law implementation and citizenship outcomes is important for the validity of the present results.

Next, I provide evidence that compulsory schooling laws in Germany were effective in increasing the years of schooling and that the law changes provide a strong instrument. As first descriptive evidence, Figure 1 shows the effect of the reforms on the average years of schooling up to seven years before and after the increase in compulsory schooling. Note that because the introduction of compulsory schooling occurred in different years in different federal states, the date at which law reform took place refers to different years. It is apparent that the highest increase in the average years of schooling occurred in the two years after the law changes were implemented.

Table 1 reports the distribution of years of schooling in the two years before and after the law changes were implemented. The table shows that the proportion of individuals with fewer than nine years of schooling decreased from 52% two years before the reform to 36% two years after the implementation of the compulsory schooling reform. Table 2 provides

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<sup>12</sup> I also report the direct effects of compulsory schooling reforms on the various political outcomes (the reduced form).

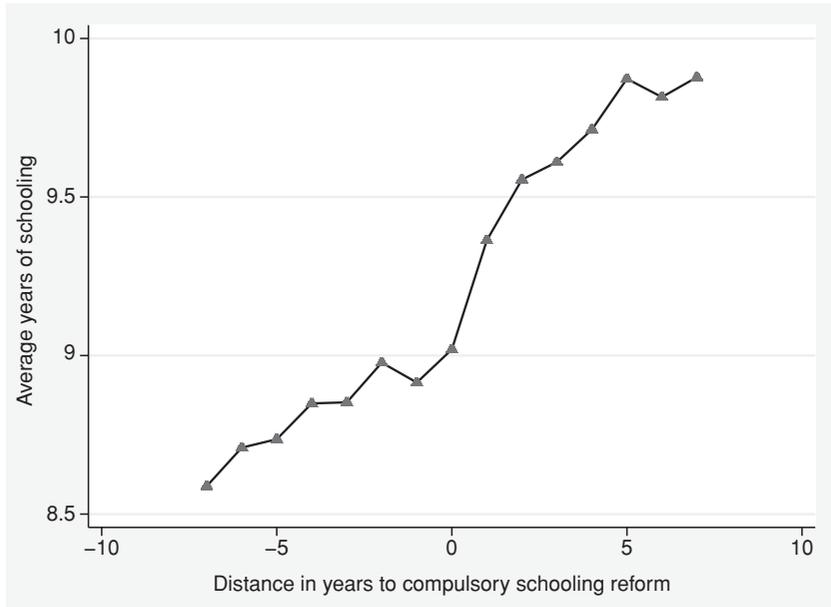


Fig. 1. Effect of compulsory schooling reform on years of schooling

results from first-stage regressions of the effect that compulsory schooling law had on years of schooling for various samples. The estimates show that there is a positive significant relationship between compulsory schooling law and years of education across all samples. The estimates suggest that, on average, increasing compulsory schooling by one year increases years of education by about 0.4–0.5 year. This is in line with Pischke and von Wachter (2005), who find that one more year of compulsory schooling increases school attainment by 0.17–0.6 year.<sup>13</sup> To summarize, the above evidence suggests that the compulsory schooling reforms had a large and significant impact on education in Germany and that the changes in compulsory schooling law provide a strong instrument for individuals' years of schooling.

To clarify the interpretation of the present estimates, it is important to keep in mind that when using IV as an empirical strategy to estimate causal

<sup>13</sup> The working paper version (Siedler, 2010) provides evidence that the law changes were effective in increasing the years of schooling at lower levels of education, but not at the upper end of the educational distribution. These estimates suggest that it is unlikely that unobserved policy or economic conditions that changed at the same time schooling laws were introduced might bias the present estimates. This is consistent with empirical evidence reported in Angrist and Krueger (1991) for the US, and Oreopoulos (2007) for the US, Canada, and the UK.

Table 1. *Distribution of years of schooling before and after compulsory schooling reform*

Years of schooling	Two years before school reform	Two years after school reform
7	31.24	22.96
8	20.49	13.39
9	15.42	19.65
10	15.21	17.04
11	3.65	5.74
12	5.27	7.30
13	4.87	6.61
14	1.62	2.78
15	2.23	4.52

Table 2. *First-stage results (dependent variable: years of schooling)*

Sample for outcome variable	ALLBUS				ForsaBus	
	Interest in politics	Voted in last general election	Signed a petition	Party membership	Interest in politics	Voted in last general election
Age	0.087** (0.021)	0.075** (0.015)	0.066 (0.034)	0.125** (0.019)	0.072** (0.029)	0.099** (0.005)
Age-squared	-0.001** (0.000)	-0.001* (0.000)	-0.001 (0.001)	-0.001** (0.000)	-0.001** (0.000)	0.000 (0.000)
Year of birth	0.032 (0.019)	0.094** (0.008)	0.085** (0.046)	0.062** (0.017)	0.103** (0.024)	0.135** (0.002)
Female	-0.146** (0.032)	-0.149** (0.042)	-0.132* (0.055)	-0.139** (0.017)	-0.403** (0.024)	-0.375** (0.012)
Dummy for compulsory schooling reform	0.538** (0.074)	0.522** (0.085)	0.392** (0.117)	0.520** (0.067)	0.480** (0.041)	0.434** (0.016)
$R^2$	0.006	0.004	0.003	0.005	0.010	0.121
Sample size	14,007	10,100	4,929	16,005	28,720	490,359

Notes: Regressions are estimated by OLS and also include a maximal set of year dummies, state of residence dummies, and state-specific cohort trends. Standard errors corrected for state-year of birth clustering are shown in parentheses. \*\* and \* indicate significance at 1% and 5% level, respectively.

effects, one only identifies the local average treatment effect (LATE) for those who change their schooling attainment due to the reform, the so-called “compliers” (Imbens and Angrist, 1994). In the present context, these individuals are found in the lower end of the educational distribution and, therefore, the estimated effect may differ from the expected political returns to schooling for all individuals in the population (e.g., the average treatment effect).

## IV. Results

### *Main Samples*

Table 3 presents the findings for the relationship between years of schooling and political outcomes for the main samples. I report only marginal effects, and each estimate presents the results of a separate regression. The

Table 3. *The impact of schooling on citizenship*

Outcomes	Probit (1)	IV (2)	Average effect of reform (3)	First-stage <i>F</i> -statistic (4)	<i>N</i> (5)
<b>ALLBUS</b>					
<i>Political awareness</i>					
Interest in politics	0.041** (0.002)	-0.096** (0.021)	-0.060** (0.015)	82.84	14,007
<i>Participation in voting</i>					
Voted in last general election	0.008** (0.001)	0.005 (0.018)	0.000 (0.009)	51.44	10,100
<i>Democratic values</i>					
Opportunity to come to power for democratic parties	0.028** (0.008)	-0.037 (0.066)	-0.022 (0.031)	29.84	2,673
Demonstrating is civil right	0.021* (0.008)	-0.001 (0.073)	-0.003 (0.034)	34.25	2,683
<i>Political involvement</i>					
Active in citizen group	0.028** (0.003)	-0.050 (0.051)	-0.036 (0.024)	12.93	4,840
Signed a petition	0.045** (0.004)	-0.074 (0.051)	-0.036 (0.024)	14.54	4,929
Participated in demonstration	0.027** (0.002)	-0.016 (0.045)	-0.007 (0.016)	13.60	4,789
<i>Political group membership</i>					
Member of a citizen group	0.002** (0.000)	-0.003 (0.008)	-0.001 (0.003)	91.06	10,973
Member of a political party	0.004** (0.001)	-0.021 (0.016)	-0.009 (0.005)	95.25	16,005
<b>ForsaBus</b>					
Interest in politics	0.082** (0.002)	-0.008 (0.013)	-0.019 (0.025)	138.83	28,720
Voted in last general election	0.005** (0.0002)	0.0002 (0.002)	0.0006 (0.009)	775.76	490,359
Member of a political party	0.009** (0.0002)	-0.010* (0.002)	-0.003 (0.007)	560.92	458,984

*Notes:* Explanatory variable is years of schooling. Each estimate represents the marginal effect from a different regression. Column (1) presents the marginal effects of years of schooling from probit regressions. Column (2) presents the marginal effects of years of schooling from IV probit regressions. Column (3) presents the marginal effects of the compulsory schooling law dummy from probit regressions. Regressions also include a dummy for female, age, age-squared, individual's year of birth, and a maximal set of year dummies, state of residence dummies, and state-specific cohort trends. Standard errors corrected for state-year of birth clustering are shown in parentheses. \*\* and \* indicate significance at 1% and 5% level, respectively.

upper panel presents the estimates based on ALLBUS data, and the lower panel shows results based on ForsaBus. Column (1) of Table 3 shows a positive association between years of schooling and all political outcomes. For example, increasing schooling by an additional year is associated with an increase in voting participation of nearly one percentage point in both ALLBUS and ForsaBus. Similarly, the likelihood of having participated in an approved political demonstration increases by nearly three percentage points with one additional year of schooling. This is a large effect, as 13% of respondents in the ALLBUS said that they had at some point participated in a political demonstration. The standard errors for these outcomes are 0.002 or lower, which implies that estimates are statistically different from zero at the 1% level.<sup>14</sup>

The positive association between years of schooling and a broad range of political outcome measures is in line with several studies (Almond and Verba, 1963; Dee, 2004; Milligan *et al.*, 2004). If the associations represent causal effects, they would be consistent with Lipset's modernization theory: individuals with more education are more likely to believe in democratic values and to participate in politics. However, these results could be due to unobserved characteristics or omitted variables that have an effect on political outcomes and years of schooling. For instance, "more capable" individuals or those with more highly educated parents might inherit their parents' political interests and democratic attitudes, and they might also have more years of schooling. Hence, standard estimates might be biased upwards. Another reason for upwardly biased estimates in the probit estimates might be a positive correlation between years of schooling and "social desirability"; that is, the tendency of respondents to answer questions in a way they think is most socially acceptable and desirable (Corbett, 1982). For example, Jackman (1978) and Bernstein, Chadha, and Montjoy (2001) found that individuals with higher education were more likely to recognize the socially accepted answer and respond accordingly, even though they might actually think otherwise.

Official turnout rates of people aged 40 and above who voted in the 2002 and 2005 general elections in Germany were around 80%. The proportions among all eligible voters in both years were 78.3% and 79.6%, respectively (Statistisches Bundesamt, 2006). This compares with a voter turnout of more than 90% according to both ALLBUS and ForsaBus, clearly

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<sup>14</sup> In unreported regressions, I also controlled for non-linearity in state-specific time trends by including state-specific cohort trends squared. Moreover, to ensure that the outcomes for voting in the last general election, participating in a demonstration, and membership in a political party really capture democratic behavior and do not include extremist voting or participation in demonstration of extremist groups, I also ran regressions excluding respondents who reported having voted for an extremist party in the last general election. Overall, this did not change the results.

suggesting that voting participation is overreported in both surveys. Higher self-reported turnout rates than official figures are also found by Milligan *et al.* (2004) in both the US and the UK. If years of schooling are indeed positively linked to reporting socially acceptable political behavior, probit estimates might be upwardly biased. In contrast, measurement errors in years of schooling might result in downwardly biased estimates (Card, 2001). Because the omitted ability bias and “social desirability bias” might cause overestimation, while measurement error would cause underestimation, simple probit estimates of political returns to schooling can be either overestimates or underestimates of the true returns to schooling, depending on the relative magnitudes of these biases.

Table 3 also provides IV estimates to control for endogeneity that may plague simple probit estimates. The IV estimates in column (2) show no positive significant impact of years of schooling on any of the political outcomes under investigation. Controlling for potential endogeneity of years of schooling results in a dramatic drop in marginal effects. In fact, the majority of estimates are reduced to close to zero or become negative. One political outcome (interest in politics) based on ALLBUS becomes negative and even statistically significant. However, this result cannot be confirmed by ForsaBus.

It is well known that IV estimates have larger standard errors than standard estimation methods such as OLS or probit. Thus, overcoming possible biases comes at a cost of efficiency, which might be particularly worrisome when samples are relatively small. For this reason, estimates using ForsaBus are also presented. Indeed, IV estimates based on ForsaBus are much more precisely estimated. This is particularly true for the outcomes “voted in last general election” and “being member of a political party”. The point estimates are based on sample sizes that include more than 450,000 individuals. In line with the results based on ALLBUS, the marginal effects are also close to zero or negative, suggesting that there is no evidence of positive political returns to schooling in Germany in the postwar period. Moreover, results for average treatment effects in column (3) do not indicate that individuals who were forced to attend school for an additional year are more likely to report civic activities.

The fourth column of Table 3 shows the results from an  $F$ -test from first-stage regressions testing the hypothesis that the coefficient of the instrument ( $\alpha_1$ ) in equation (2) is equal to zero (Staiger and Stock, 1997). Apparently the hypothesis that increasing compulsory schooling by one year has no impact on the number of years of schooling can be easily rejected across all samples. Taken together, the results do not point to a positive causal relationship between individuals’ years of schooling and a broad range of political outcomes in Germany. Next, I examine whether there are heterogeneous civic returns to schooling with higher returns for those

Table 4. *The impact of schooling on citizenship by school certificate, father's school certificate, and father's occupational prestige score (restricted samples)*

	Individuals with basic schooling or no school degree <sup>a</sup>		Father has basic school certificate or less <sup>b</sup>		Father with occupational prestige score below median <sup>b</sup>	
	Probit	IV	Probit	IV	Probit	IV
<i>Political awareness</i>						
Interest in politics	0.015 (0.011)	-0.005 (0.018)	0.037** (0.002)	-0.117** (0.024)	0.046** (0.004)	-0.096** (0.042)
	N = 14,604		N = 10,551		N = 5,603	
<i>Participation in voting</i>						
Voted in last general election	0.008** (0.002)	-0.002 (0.003)	0.007** (0.002)	-0.004 (0.020)	0.012** (0.003)	-0.028 (0.047)
	N = 143,914		N = 7,768		N = 4,089	
<i>Democratic values</i>						
Demonstrating is civil right			0.013 (0.009)	0.008 (0.064)	0.030 (0.016)	-0.028 (0.300)
			N = 2,177		N = 1,174	
<i>Political involvement</i>						
Signed a petition			0.041** (0.005)	-0.097 (0.050)	0.039** (0.007)	0.073 (0.089)
			N = 3,784		N = 2,065	
<i>Political membership</i>						
Member of a political party	-0.001 (0.001)	-0.011** (0.003)	0.003** (0.001)	-0.001 (0.013)	0.002** (0.001)	-0.001 (0.009)
	N = 212,637		N = 12,187		N = 6,492	

Notes: <sup>a</sup> Estimates based on ForsaBus. Individuals with no or lowest school degree. <sup>b</sup> Estimates based on ALLBUS. Explanatory variable is years of schooling. Each estimate represents marginal effect from a different regression. Regressions also include a dummy for female, age, age-squared, individual's year of birth, and a maximal set of year dummies, state of residence dummies, and state-specific cohort trends. Standard errors corrected for state-year of birth clustering are shown in parentheses. \*\* and \* indicate significance at 1% and 5% level, respectively.

individuals who are most likely to be affected by an increase in compulsory years of schooling (Imbens and Angrist, 1994).

### Restricted Samples

Table 4 provides further evidence of the relationship between years of schooling and political behavior for three restricted samples: (1) individuals at the lower end of the educational distribution who attended either a Hauptschule or have no school degree at all; (2) individuals with fathers who have a “low” level of education or none at all; and (3) individuals whose fathers have an occupational prestige score below the sample's median value.<sup>15</sup>

<sup>15</sup> Sample selections (2) and (3) are only possible with ALLBUS, since ForsaBus does not contain information on parental background. I also estimated the models on a sample of individuals with both parents having basic schooling or less. This did not change the results.

Examining the impact of compulsory schooling on individuals with “disadvantaged” family backgrounds is important because there might be heterogeneity in the political returns to schooling across the population. For instance, children with “academic” parents might inherit their parents’ democratic values and interest in politics regardless of how many years of school they themselves actually attended. Hence, their political returns to schooling could be quite low. In contrast, for individuals whose parents have low education, no political awareness, and low levels of democratic values, more years of schooling could have a profound influence on their political and democratic attitudes. Put differently, the political returns for individuals with disadvantaged family backgrounds could be considerably higher.<sup>16</sup> IV estimates on the three restricted samples allow me to estimate marginal political returns of education for individuals most likely to be affected by compulsory schooling reforms (Angrist, Imbens, and Rubin, 1996).

For the sake of brevity and because sample sizes decrease due to sample selections imposed, I report only estimates for political outcomes with the largest sample sizes. The majority of estimates from simple probit regressions shown in Table 4 point to a positive significant relationship between years of schooling and citizenship. With the exception of the sample of individuals with basic schooling or no school degree, there are no large differences in the magnitude or significance compared to the results in Table 3, suggesting no systematic variation in political returns to schooling among individuals with different family backgrounds. Turning to the IV results, the majority of estimates are negative or close to zero, indicating that there is no empirical evidence of a positive causal relationship between years of schooling and democratic citizenship for individuals with a “disadvantaged” family background in Germany.<sup>17</sup>

## **V. Why are there no Positive Political Returns to Schooling in Germany?**

This section briefly discusses possible reasons why the increase in compulsory schooling in Germany had no positive effect on citizenship.<sup>18</sup>

<sup>16</sup> This argument is related to the idea that there is heterogeneity in the wage returns to schooling (Card, 2001).

<sup>17</sup> In unreported regressions, I estimated the average effects of the reform on citizenship for the three restricted samples. In line with the estimates in Table 3 (column (3)), there was no evidence of a positive impact of compulsory schooling reform on citizenship.

<sup>18</sup> See the working paper version (Siedler, 2010) for a more comprehensive discussion of potential explanations for the absence of positive political returns to education.

### *Denazification and Teaching Practices*

In the early postwar years, the Western Allies abandoned their views of “collective German guilt”, and denazification programs were limited by administrative and technical difficulties (Fulbrook, 1999). Many teachers who had been part of the Nazi regime and had joined the National Socialist Teachers’ League or who were exposed to National-Socialist propaganda could return to their former jobs. This may very well have limited the effectiveness of civic teaching and education in postwar Germany. For instance, students may not have developed a better understanding of the importance of democracy through a better knowledge of history because their teachers were not able to teach objectively about the recent National-Socialist history. In addition, the absence of positive political returns of schooling might be due to apolitical or authoritarian teaching practices (Matthewes, 1961; Händle, 2002), resulting from a shortage of qualified teachers and adequate textbooks (Merritt, 1995; Webb, 1998).<sup>19</sup>

### *Political Socialization outside the Classroom*

An important goal of the Allies after World War II was the democratic socialization of the German population (Schwartz, 1993). For example, the American military government initiated educational and career programs, cultural events, movies, youth centers, and study tours to the US. They also provided access to information centers (*Amerika-Häuser*) whose visitors could use library facilities and visit art exhibitions (Zink, 1957; Schwartz, 1993). Thus, the impact of an increase in compulsory years of schooling on civic education might have been mitigated by extracurricular socialization programs.

In the working paper version (Siedler, 2010), I examine whether these political and historical circumstances might explain the absence of political returns to education in Germany. In summary, the empirical results suggest that the absence of positive returns to schooling in Germany is unlikely to be driven by adverse postwar educational factors such as a shortage of teachers and classroom material, destruction of schools, apolitical or poor teaching, or civic educational programs outside the classroom. However, I should carefully point out that despite the extensive evidence by educators and historians, it is difficult to empirically identify exactly when these

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<sup>19</sup> Alternatively, the destruction of schools, inadequate heating in schools, and food shortages during the early postwar years might have negatively affected the quality and efficacy of teaching (Zink, 1957; Ichino and Winter-Ebmer, 2004). For instance, poor diets and nutrition deficits might have had a direct negative effect on students’ cognitive development and an indirect negative impact on their formation of civic skills. The destruction of schools and school heating problems might have negatively influenced students’ acquired fundamental skills and civic knowledge because of fewer history and civic school hours taught.

various historical factors completely ceased to interfere with the quality of education. Moreover, there might have been considerable disparities in the teaching quality of civic education and history across schools, which could not be taken into account in the empirical estimates because of data limitations.

### *Civic Education, History, and Curriculum*

Were the basic concepts of democracy already fully communicated to pupils by the ninth grade? Pischke and von Wachter (2008), for example, conjecture that the increase in compulsory years of schooling in Germany had no effect on earnings because students acquired the basic skills needed for a job either by eighth grade or in vocational schools. A related explanation for the absence of positive political returns to schooling might be that civic values and the fundamental principles of democracy were already learned at an earlier age, prior to the increase in compulsory years of schooling. In fact, civic and historical education in schools was a key goal of the Occupying Forces after the war. In June 1947, the Allies published 10 principles for the democratization of education in Germany. One of the statements reads: “All schools should lay emphasis upon education for civic responsibility and a democratic way of life, both by means of the curriculum and by the organization of the school itself” (Lawson, 1963, p. 46). In 1959, ordinance directions for teaching history had been issued with the profound goal of teaching pupils about the Nazi past and of confronting them with the crimes committed against humanity. As a result, the Holocaust was addressed in several school subjects, including history classes, civic studies, current affairs classes, and religion or ethics classes (Wehrmann, 1998). Teaching about the Nazi period was also an integral part of German literature classes, and the objective of educators was to teach about the Holocaust not only using historical facts but by other means as well. Most importantly, emphasis has been placed on covering World War II, the Third Reich, and democratic values in all secondary school tracks.

Lawson (1963) even points out that the number of hours per week devoted to civic education was considerably higher in lower secondary schooling (*Volksschule*, 4–7 hours) compared to middle and upper secondary schooling (*Mittelschule*, 2–5 hours; *Gymnasium*, 1–2 hours). The author also argues that, “there is ample evidence that German schools are giving considerable attention to national mistakes of the recent past and to international relations” and “that the emphasis in the schools on preparation for democratic citizenship is significant today” (Lawson, 1963, pp. 185, 194). Similarly, Banaschewski (1955) and Krippendorff (1961) argue that civic education and political understanding were important topics in the fifth to seventh school years of lower secondary schooling in postwar

Germany. Lawson (1963) reports from a survey of teachers in the states of Hesse, Hamburg, and Bavaria that respondents mentioned the “democratization of school and administration” as the most important innovation in the German school system since 1945. When teachers were asked, “In which subjects have reforms since 1945 had the greatest effect?”, social studies, politics and history were mentioned most often, and this was equally true across teachers from the three main school types (Lawson, 1963, p. 152). Thus, the direct provision of knowledge about history and democracy might have increased citizenship considerably because individuals have a better knowledge about the functioning of political institutions and a higher appreciation of the importance of democracy.<sup>20</sup>

By the time the majority of federal states had increased the number of compulsory years of schooling in the late 1960s, Germany had already experienced several structural reforms. These included curricular changes, the introduction of civic and historical teaching in several subjects, an increased supply of adequate teaching material, and the introduction of democratic institutions in schools. Together, these educational reforms might have already had a cumulative positive impact on pupils’ civic values and knowledge prior to the increase in compulsory years of schooling, potentially outweighing the political benefits of an additional year of education.

## **VI. Robustness Checks**

This section provides further robustness checks. Again, I report marginal effects for years of schooling, and each estimate presents the result from a separate regression.<sup>21</sup>

### *Geographic Mobility*

A first concern is that the present estimates might be biased due to selective geographic mobility. For example, families might have anticipated the introduction of compulsory schooling and moved to a new federal state.<sup>22</sup> To account for this, I re-estimated the models on a sample of individuals for whom valid information on their federal state of birth is available. Nearly 84% of individuals still live in their state of birth, and slightly less than 5%

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<sup>20</sup> Civic education was also accompanied by the introduction of student councils and classroom spokesmen aimed at increasing pupil participation in school affairs and at countering authoritarian school structures and classroom practices.

<sup>21</sup> For the sake of brevity, I only present estimates for the outcomes with the largest sample size. Similarly, since some of the sensitivity analysis is only feasible with ALLBUS data, only estimates based on this dataset are presented here.

<sup>22</sup> This could also result in a mismatch of the identification of individuals affected by compulsory schooling reform.

Table 5. *Robustness checks (ALLBUS)*

Outcomes	Interest in politics		Voted in last general election		Member of a political party	
	Probit	IV	Probit	IV	Probit	IV
<i>Panel A (Geographic mobility)</i>						
Individuals with valid state of birth information						
Specification 1	0.046** (0.003) <i>N</i> = 4,745	-0.113** (0.030)	0.016** (0.002) <i>N</i> = 2,814	0.022 (0.035)	0.007** (0.001) <i>N</i> = 4,737	0.003 (0.024)
Specification 2	0.046** (0.003) <i>N</i> = 4,745	-0.121** (0.031)	0.016** (0.002) <i>N</i> = 2,814	0.037 (0.044)	0.007** (0.001) <i>N</i> = 4,737	-0.005 (0.033)
Non-movers <sup>a</sup>	0.045** (0.004) <i>N</i> = 4,206	-0.112** (0.031)	0.014** (0.003) <i>N</i> = 2,479	0.041 (0.039)	0.008** (0.002) <i>N</i> = 4,136	-0.008 (0.024)
<i>Panel B (Timing of reform)</i>						
Individuals affected by law two years before and after introduction are excluded						
	0.041** (0.002) <i>N</i> = 13,725	-0.098** (0.020)	0.008** (0.001) <i>N</i> = 9,885	0.003 (0.019)	0.004** (0.001) <i>N</i> = 15,185	-0.023 (0.017)
<i>Panel C (Environment controls)</i>						
Controls for state characteristics during the 1950s						
	0.048** (-0.002) <i>N</i> = 3,872	0.107* (0.045)	-0.017** (0.003) <i>N</i> = 2,298	0.014 (0.108)	0.007** (0.002) <i>N</i> = 3,868	0.004 (-0.065)
<i>Panel D (German reunification)</i>						
Outcomes measured during survey years 1980–1988						
	0.031** (0.004) <i>N</i> = 4,879	-0.062 (0.055)	0.005** (0.002) <i>N</i> = 3,989	-0.033 (0.063)	0.001 (-0.001) <i>N</i> = 6,882	-0.027 (-0.031)

Notes: <sup>a</sup> Non-movers are defined as respondents who still live in their federal state of birth or moved between states that introduced compulsory schooling law in the same year. Each estimate represents the marginal effect from a different regression. Regressions also include a dummy for female, age, age-squared, individual's year of birth, and a maximal set of year dummies, state of residence dummies, and state-specific cohort trends. Standard errors corrected for state-year of birth clustering are shown in parentheses. \*\* and \* indicate significance at 1% and 5% level, respectively.

moved between the four states that introduced the reform law in the same year. Estimates of these restricted samples are reported in Table 5, Panel A. The first row of Panel A reports results for respondents for whom federal state of birth information is available and for whom the introduction of the schooling reform is generated using current state of residence information (Specification 1). The second row uses state of birth and year of birth information to observe whether a person was affected by the compulsory schooling law, and it also controls for federal state of birth dummies and state of birth-specific time trends (Specification 2). The third row presents estimates on a sample of individuals who still live in their federal state of birth or who moved between states with legislative changes in the same year (non-movers). Overall, the results yield similar conclusions to those in

Table 3, suggesting that geographic mobility is unlikely to be a cause for concern.

### *Timing of Reform*

Another concern is that in some cases, the introduction of compulsory schooling reforms took place after the official date (Pischke and von Wachter, 2005). For example, some schools in Bremen may have increased compulsory schooling in 1959 rather than in 1958, when it was officially introduced. To account for this, I draw a sample of individuals excluding those who were supposed to be the first cohort affected by the reform, together with individuals in the two years before and after the legislative changes went into effect. Panel B of Table 5 contains the results. Consistent with earlier findings, the estimates show a positive and significant correlation between years of schooling and all political outcomes. However, IV estimates do not point to a causal link between years of schooling and democratic citizenship.

### *Political, Social, and Economic Environment*

Increases in compulsory schooling might have been conflated with general political, social, or economic changes in federal states at that time, which might result in omitted variable bias. For example, the timing of the reform may have been influenced by the demand for the reform such that highly educated parents lobby for early implementation or that states with, on average, higher levels of education get early implementation because it is less costly if a large proportion of the population already has a high level of schooling. I address this issue by controlling for the following state characteristics at federal state levels from 1946 through 1966: number of firms per 10,000 inhabitants, number of lower, intermediate, and upper-level secondary general schools per 10,000 inhabitants,<sup>23</sup> percentage of displaced persons (*Vertriebene*) in the population, percentage of the population who are Catholic, population density (inhabitants per square kilometer), percentage of households in the population registered to receive public broadcasting services, and the proportion of votes received in state elections by the Social Democratic Party (SPD).<sup>24</sup> Estimates in Table 5, Panel C, show that including proxy variables for political, social, and economic environment around the

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<sup>23</sup> These school types were previously called *Volksschule*, *Mittelschule*, and *höhere Schulen*, respectively.

<sup>24</sup> Summary statistics of these state characteristics are reported in the working paper version (Siedler, 2010).

time of introduction of schooling legislation does not change the general conclusions.<sup>25</sup>

### *German Reunification*

Differences in the political climate before and after German reunification in 1990 might have had an impact on individual political behavior due to the unique process of political transformation after the fall of the Berlin Wall in 1989. To account for this, I re-estimated regressions measuring citizenship in the surveys prior to German reunification only, using ALLBUS survey years 1980–1988. Estimates are reported in Panel D of Table 5. Again, it is apparent that the results do not change the general conclusions.

### *Alternative Measures of Explanatory Variable*

Finally, I use two alternative measures of years of schooling. First, I estimated regressions using years of schooling as reported by ALLBUS respondents in the survey years 1990–1992. Second, following Pischke and von Wachter (2005), I converted highest educational attainment into approximate years of schooling in a mechanical way, according to individuals' exposure to compulsory school reforms. For example, ALLBUS respondents with lower-level secondary schooling (*Hauptschule*) who finished school before the reform was introduced were assigned eight years of schooling, compared to nine for those with the same graduation certificate after the reform was introduced.<sup>26</sup> Column (1) of Table 6 reports marginal effects for years of schooling as reported by respondents in survey years 1990–1992 on citizenship. Column (2) reports estimates for years of schooling generated according to the individual's highest schooling degree and year of birth. Again, the estimates point to a positive association between years of schooling and democratic citizenship, but they provide little evidence of causality.<sup>27</sup>

Overall, various robustness checks confirm the earlier finding that there is a strong and positive correlation between years of schooling and democratic citizenship, although there is little evidence of causality of education on a broad range of political outcome measures.<sup>28</sup>

<sup>25</sup> In the majority of regressions, the state characteristics have no significant impact on citizenship.

<sup>26</sup> This assignment mechanism assumes perfect compliance with compulsory schooling laws and has the advantage that repetition of a grade is not treated as having more years of schooling.

<sup>27</sup> Note that the power of the instrument decreases considerably in the first specification. IV estimates for two outcome variables in Table 6, column (1), exceed the simple probit estimates, suggesting higher political returns to education, but they are not precisely estimated.

<sup>28</sup> I also compared the sensitivity of results to the definition of outcome variables, using categorical variables instead of dichotomous outcomes. This did not change the results.

Table 6. *Alternative measures of years of schooling (ALLBUS)*

	Years of schooling observed in surveys, 1990–1992 (1)		Generated variable years of schooling <sup>a</sup> (2)	
	Probit	IV	Probit	IV
<i>Political awareness</i>				
Interest in politics	0.054** (0.006)	-0.154** (0.060)	0.075** (0.003)	-0.069* (0.017)
	<i>F</i> = 6.33 <i>N</i> = 2,742		<i>F</i> = 277.66 <i>N</i> = 14,007	
<i>Participation in voting</i>				
Voted in last general election	0.010** (0.004)	0.112 (0.070)	0.013** (0.002)	0.001 (0.013)
	<i>F</i> = 6.96 <i>N</i> = 2,127		<i>F</i> = 177.12 <i>N</i> = 10,100	
<i>Political involvement</i>				
Signed a petition	0.043** (0.007)	-0.030 (0.109)	0.058** (0.005)	-0.049 (0.033)
	<i>F</i> = 5.57 <i>N</i> = 2,680		<i>F</i> = 76.90 <i>N</i> = 4,929	
<i>Political membership</i>				
Member of a political party	0.007** (0.002)	-0.009 (0.035)	0.009** (0.001)	-0.011 (0.009)
	<i>F</i> = 5.69 <i>N</i> = 2,652		<i>F</i> = 251.14 <i>N</i> = 16,005	

Notes: Explanatory variable is years of schooling. *N* denotes number of individuals. *F* denotes first-stage *F*-statistic. Each estimate represents the marginal effect from a different regression. Regressions also include a dummy for female, age, age-squared, individual's year of birth, and a maximal set of year dummies, state of residence dummies, and state-specific cohort trends. Standard errors corrected for state-year of birth clustering are shown in parentheses. \*\* and \* indicate significance at 1% and 5% level, respectively.

<sup>a</sup> The explanatory variable "years of schooling" equals eight if respondents received secondary general school (*Hauptschule*) before and equals nine if respondents received secondary general school after compulsory schooling reform. Individuals with an intermediate school certificate and a grammar school certificate are assigned 10 and 13 years of schooling, respectively. Respondents with a technical school certificate are assigned 12 years of schooling.

## VII. Summary and Conclusions

It has been widely argued by influential scholars and politicians that education is the driving force behind political interest, democratic values, and political participation. This paper exploits historical increases in years of schooling induced by compulsory school reforms in Germany to investigate causal relationships between education and political interest, voting participation, democratic values, political involvement, and political group membership. Simple probit estimates suggest a strong and significant positive relationship between years of schooling and a broad range of political outcomes under study. However, the analysis finds no convincing evidence that these correlations represent a causal effect of schooling on citizenship,

using exogenous variation in secondary schooling in Germany from 1949 through 1969 across federal states.

Several potential explanations for the absence of positive civic returns to education in postwar Germany were discussed. First, teacher shortages, destruction of schools, food and fuel shortages, apolitical teaching practices, and poor civic teaching due to teachers' former experience of the Nazi period could have negatively affected the efficacy of civic education. For example, the destruction of schools might have negatively influenced political behavior directly through fewer history and civic school hours taught and indirectly by hampering the development of skills (e.g., speaking and writing) that are likely to be relevant for political participation (Verba *et al.*, 1995). Second, Allied socialization programs after World War II could have had a positive influence on German's democratic values and political behavior, potentially outweighing the benefits of more schooling. Finally, civic school reforms prior to the increase in compulsory years of schooling in Germany might have been effective in teaching pupils citizenship so that the fundamentals of democracy had already been learned at an earlier stage of schooling. Indeed, a broad range of empirical and historical studies suggest that civic educational reforms after the war in Germany were successful in teaching students the basic values of democracy at an early age, prior to the increase in compulsory years of schooling.

This study contributes to a small body of literature that investigates the civic returns to schooling. Recent work by Dee (2004) and Milligan *et al.* (2004) report positive political returns to schooling in the US and the UK, whereas the present study finds no positive influences. These cross-country differences mirror recent results in the economic literature on the wage returns to schooling. Pischke and von Wachter (2005) find zero returns to schooling on earnings in Germany, whereas a large body of studies for the US and UK report significant positive effects in the order of 10% to 15%. Thus, the empirical results for Germany of zero economic and political returns to schooling suggest that institutional and historical factors, school curriculum, and the organization of the school system can play important roles in shaping returns to schooling in a society. The present results are not only important because they are orthogonal to previous studies for other countries and because they point to the importance of institutional differences across countries, but also because the combination of the empirical analysis and cited historical evidence are likely to be relevant for thinking how teaching and the structure of the curriculum might affect democratic behavior in other countries. This is particularly relevant and important for countries that experienced the transition from a dictatorship to a democracy and that might face similar problems and challenges to educators in postwar Germany during the 1950s and 1960s.

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