Schooling and Citizenship in a Young Democracy: Evidence from Post-War Germany

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Abstract

This paper examines whether schooling has a causal impact on individuals' political interest, voting turnout, democratic values, political involvement and political membership, using two national representative samples. Between 1949 and 1969 the number of compulsory years of schooling was increased from eight to nine years in the Federal Republic of Germany, gradually over time and across federal states. These legislative changes allow one to investigate the causal impact of years of schooling on citizenship in a young democracy. Years of schooling are found to be positively correlated with a broad range of citizenship outcomes. However, when exogenous increase in schooling through legislative changes is used, there is little evidence of a causal effect. Several potential explanations are examined: (1) poor or apolitical teaching practices in the post-war period; (2) shortages of qualified teachers and adequate teaching material; (3) extra-curricular civic education; and (4) whether civic knowledge might have been acquired prior to the increase in schooling. The estimates reject the first three explanations, whereas ample historical evidence is in favour of the hypothesis that the fundamentals of democracy were learned already earlier in school.

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"The higher one's education, the more likely one is to believe in democratic values and support democratic practices." (Lipset, 1976, p. 56)

1   Introduction

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, Nie et al. (1996), Franklin (1996), and Przeworski et al. (2000) stress that individuals with 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. It is also commonly acknowledged that individual political behavior is a crucial factor in democratic societies, shaping political life, defining the function of government, and keeping democracy alive (Corbett, 1982). Hence, it has been argued that democratic states with higher levels of education[1] within the population enjoy greater political stability and are thus more likely to survive in the long term (Lipset, 1976; Nie et al., 1996; Glaeser et al., 2004).

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 United States, while Milligan et al. (2004) explore the impact of years of schooling on voter participation and political interest in the United States and the United Kingdom. Both report a positive and statistically significant causal effect of schooling 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 behaviour. This paper contributes to this literature in several ways. First, it

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[1]The terms education and schooling are used interchangeably here.

investigates whether there exist 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 sheds first light on the effect of schooling on civic behaviour in Germany, a country that recently experienced the transition from a brutal dictatorship to democracy and whose former educational system is argued to have failed to prevent Nazism and helped to sustain the Third Reich (Merritt et al., 1971). Third, political returns to education might differ in a country that recently underwent this kind of dramatic transition from dictatorship to democracy as compared to more mature democracies. For example, parents who grew up in the Third Reich might be rather limited in their ability to teach their children democratic values they were not exposed to themselves. As a result, political returns to schooling might be even larger in Germany than in the US or UK. However, particular historical events such as Allied democratization programs after World War II, educational reforms prior to increase in compulsory years of schooling, demolition of schools, teacher shortages, and poor teaching practices might influence the political returns to schooling in post-war Germany as well.

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 to the educational system is that schooling provides important social benefits by promoting tolerance, democratic values, and political involvement. If true, this suggests that there exist important social returns over and above the private returns to education usually studied by economists.[2]

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[2]See, for example, Angrist and Krueger (1991), Harmon and Walker (1995), Acemoglu and Angrist (2000), Pischke and von Wachter (2005), Del Bono and Galindo-Rueda (2006) and Oreopoulos (2006) in the recent

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, a change introduced gradually over time in the different federal states (Pischke and von Wachter, 2005). This reform provides variation in years of schooling which is exogenous to unobserved characteristics that may affect both 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 are examined: (1) poor or apolitical teaching practices in the post-war period; (2) shortages of qualified teachers and adequate teaching material; (3) extra-curricular civic education; and (4) whether civic knowledge might have been learned already earlier in school. The estimates do not support the first three explanations. However, a variety of historical and empirical evidence suggests that civic educational reforms prior to the increase in compulsory years of schooling had a positive impact on citizenship in post-war Germany, diminishing marginal political returns of an additional year of schooling towards zero.

The next section surveys the relevant literature. Data, sample selection, and variables used in the analysis are discussed in Section 3. Section 4 presents the main results concerning the relationship between schooling and democratic citizenship. Section 5 discusses possible expla-

returns to education literature.

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nations why the increase in compulsory years of schooling had no positive effect on citizenship in Germany. Robustness checks are presented in Section 6, and the final section concludes.

2 Relevant Literature

There are two interrelated strands of literature examining the relationship between education and democracy. The first looks at this relationship at a macro-economic level (Barro, 1999; Przeworski et al., 2000; Glaeser et al., 2004; Acemoglu et al., 2005). The second examines the association between years of schooling and political behavior using individual-level data (Nunn et al., 1978; Nie et al., 1996, Dee, 2004; Milligan et al., 2004; Touya, 2006).

It has long been argued that there exists a positive relationship between higher levels of education in a society on the one hand and the democratization of society − and sustainability of this democracy − on the other (Dewey, 1916; Lipset, 1976). Following these early contributions, a number of empirical studies have established a positive relationship between levels of education in a population and democracy (Barro, 1999; Przeworski et al., 2000). However, whether this represents a causal effect is still the subject of controversy. Glaeser et al. (2004) argued that differences in schooling across countries might have a causal impact on the transition to a functioning democracy. In contrast, Acemoglu et al. (2005) found no evidence in favor of a causal effect of education on democracy.

Other studies have used individual rather than national-level data to investigate relationships between education and political outcomes in democracies (Nunn et al., 1978; Nie et al., 1996). Dee (2004) and Milligan et al. (2004) were the first to use arguably exogenous variation in years of schooling to investigate a causal impact of schooling on voter turnout and civic participation. Dee (2004) exploited changes in child labor laws and geographic availability of junior and community colleges as a source of exogenous variation for schooling in the US. He found that years of education had a causal positive effect on voter participation, support for

free speech, and the quality of civic knowledge as measured by the frequency of newspaper readership. Milligan et al. (2004) used changes in compulsory schooling legislation and child labor laws across states and over time in the US and the increase of the minimum school-leaving age in the UK to identify the effect of schooling on political citizenship. The authors found a strong and positive effect of schooling on voting in the US, but not in the UK.[3] Furthermore, they report causal positive effects on civic behavior in both countries, for example, on respondents' awareness of public events and their interest in following politics or discussing political issues with friends.

3   Data

The data are taken from two different data sets, 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 2nd 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.[4] The question in these three 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,

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[3]However, when controlling for being registered to vote in the United States, the effect decreases considerably.

[4]Note that most German data sets do not contain any information about years of schooling (Pischke and von Wachter, 2005).

I restrict the analysis to individuals who graduated from some form of secondary school but do not have a university or technical college degree.[5] This results in a sample of individuals for whom compulsory schooling reform is likely to be of relevance 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. The sample is comprised 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 from any university or technical college degree.[6]

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 citizens' group, whether signed a petition, whether participated in approved demonstration;

- **Political Group Membership:** whether member of citizens' group, whether member of political party.

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

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[5]Because years of schooling are only available in three surveys, I generate a new education variable which 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 school degree, father's occupational prestige score, federal state of residence, and years of schooling provided in the three surveys) by using switching regression technique following van Buuren et al. (1999). Section 6 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.

[6]Individuals currently living in West Berlin are excluded from the analysis to avoid potential problems from East-West migration.

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 data set 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 an university or technical college degree.[7] The ForsaBus allows me to draw inferences from a very large sample, comprising nearly 500,000 individuals. However, the data provides no information on parental background and fewer citizenship outcomes are collected.

Appendix A briefly describes the German educational system, the compulsory schooling reform (Table 1) and provides a detailed definition of the outcome variables in both surveys (Table 2). Summary statistics, broken down by whether respondents were affected by compulsory schooling reform, are presented in Table 3, Appendix A.

4   Schooling and Citizenship

4.1   Main Samples

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:

$$Pr(P = 1) = \Phi(\beta_1 S + X\kappa), \tag{1}$$

where P represents a political outcome, S indicates years of schooling and X is a vector containing the covariates age, age-squared, year of birth, a full set of year dummies, federal

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[7]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.

state dummies, and state-specific cohort trends. In a second step, I account for endogeneity of years of schooling by estimating instrumental variable probit regressions.[8] The first-stage regression for each individual has the form:

$$S = \alpha_0 + \alpha_1 R + X\lambda + \upsilon, \tag{2}$$

with S and X defined as in equation (1). The instrumental variable R equals one if the person was affected by the compulsory years of schooling reform, and is 0 otherwise. Identification of the estimates β_1 results from exogenous increases in compulsory years of schooling over time in any given federal state. 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.[9]

Table 1 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. Estimations also include a dummy for female, a quartic in age, individual's year of birth, a maximal set of year dummies, state of residence dummies and state-specific cohort trends. The upper panel presents the estimates based on ALLBUS data, the lower panel shows results based on ForsaBus. Column 1 of Table 1 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 by 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 an additional year of schooling. This is a large effect because 13 percent of respondents in the ALLBUS said

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[8]IV probit estimates are maximum likelihood estimations of Amemiya's generalized least square 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.

[9]As a third identifying strategy, reduced forms of compulsory schooling reform are also estimated.

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 one percent level.[10]

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; Touya, 2006). 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 might also attend 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', i.e., 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 et al. (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 percent. 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 percent according to both ALLBUS and ForsaBus, clearly suggesting that voting participation is overreported in both surveys. Higher self-reported

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[10]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.

turnout rates compared to official figures are also found by Milligan et al. (2004) in both the US and UK. If years of schooling are indeed positively linked to report 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 1 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 based on 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. 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 post-war period. Moreover, results for average treatment effects in column 3 do not indicate that individuals who were forced to attend school an additional year are more likely to report civic activities.

The fourth column of Table 1 shows F statistics from first-stage regressions that the coefficient of the instrument (α_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 number of years of schooling can be easily rejected across all samples. Figure 1 and Tables 1 and 2 in Appendix B further document relationships between schooling and compulsory schooling legislation and provide evidence that these laws were effective in compelling students to stay in school one more year. Moreover, Table 3 in Appendix B reports evidence that the increase in compulsory schooling had no influence on higher educational attainment.

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 heterogenous civic returns to schooling with higher returns for those individuals who are most likely to be affected by an increase in compulsory years of schooling (Imbens and Angrist, 1994).

4.2 Restricted Samples

Table 2 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.[11]

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

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[11]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.

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.[12] 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 et al., 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 shown in Table 2 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 huge differences in the magnitude or significance compared to the results in Table 1, 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 'disadvantaged' family background in Germany.[13]

5 Why are there no Positive Political Returns to Schooling in Germany?

This section discusses possible reasons why the increase in compulsory schooling in Germany had no positive effect on citizenship. A first explanation might be poor teaching practices due to the fact that many teachers in post-war Germany had been part of the Nazi regime or exposed to National-Socialist values and propaganda. In addition, physical destruction of

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[12]This argument is related to the idea that there exists heterogeneity in the wage returns to schooling (Card, 2001).

[13]In unreported regressions, I estimated average effects of the reform on citizenship for the three restricted samples. In line with the estimates in Table 1 (column 3) there was no evidence of a positive impact of compulsory schooling reform on citizenship.

schools in World War II and shortages of teachers and adequate textbooks during the post-war years might have negatively affected the quality of civic teaching. Alternatively, socialization to democracy was a crucial part of the Allied forces' occupation policy (Merritt, 1995). Thus, the impact of an increase in compulsory years of schooling on civic education might have been mitigated by extra-curricular socialization programs. Another explanation might lie in the specific context of the German education system. The increase in schooling from eight to nine years might have had no effect on citizenship because the fundamentals of democracy were being imparted at an earlier stage in school. The following sub-sections discuss these issues in more detail.

5.1 Denazification, Teaching Practices, Textbooks, Physical Destruction of Schools, and Political Sozialisation

Denazification and Teaching Practices In the early post-war years, the Western Allies abandoned their views of 'collective German guilt' and denazification programs were limited by administrative and technical difficulties. Fulbrook (1999: 51) writes: "Where there was no immediate suspicion of being classified as a war criminal, denazification in the western zones rapidly became a matter of issuing 'Persil certificates' (to use the popular term *Persilscheine*), providing assurance of political cleansing so that people could return to their former offices and take up normal life again. Despite a few hiccups along the way, the vast majority of teachers [...] regained their former positions [...]." Since many teachers in post-war Germany had been part of the Nazi regime and had joined the National Socialist Teachers' League or were exposed to National-Socialist propaganda (Gieseke, 1976), the effectiveness of civic teaching and education in the post-war period may very well have been limited.

In addition, the absence of positive political returns in Germany might be due to apolitical or authoritarian teaching practices. Händle (2002: 1-2) notes: "Until the late 50s, the look at

Germany's National-Socialist past, for example, had been avoided" and Matthewes (1961: 39) writes: "It must not be forgotten that, during the first years after 1945, there was a certain reluctance among many teachers to include recent history in the curriculum."[14] In line with these studies, Merritt et al. (1971) point out that apolitical and authoritarian teaching practices eased over time and were mainly a problem during the immediate years after World War II.[15] Moreover, shortages of qualified teachers might have negatively affected the quality of teaching (Merritt, 1995; Webb, 1998). Thus, 'lax' denazification, apolitical or authoritarian teaching practices and a shortage of qualified teachers might explain the absence of positive political returns to education in post-war Germany.

Textbooks The Allied forces knew that schoolbooks were permeated by Nazi propaganda and identified the need for new teaching material (Tent, 1982). Due to the huge demand for new materials as well as to paper shortages and overburdened printing establishments, adequate textbooks were in short supply after the war. However, this problem existed mainly in the early 1950s and was overcome thereafter through the considerable efforts of the Allies, the newly founded UNESCO and the Council of Europe to provide new teaching material (Zink, 1957; Soysal, 2000). Bilateral textbook commissions were established in cooperation with educators from other countries such as the US and Israel for the screening of textbook content to ensure adequate representation of German history. Several scholars have argued that the treatment of antisemitism as part of German history was indeed adequate and "addressed the issue of responsibility and co-responsibility of German citizens during the Third Reich seriously and in detail" (Wehrmann, 1998). Similarly, Langenbeck (1961) points out that a great deal of space in the new textbooks used in secondary schools was devoted to institutions of the state, governments, the functioning of the parliament, the electoral system and the rule of law.

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[14]Similarly, parents might have been reluctant to tell their children too much about the cruelties of the Nazi era.

[15]See also Matthewes (1961) for a similar argument. In contrast to Händle (2002), the authors argue that reluctance among teachers to teach recent historical events "was generally isolated".

Overall, this suggests that a shortage of adequate classroom material was mainly a problem during the early 1950s in Germany.

Destruction of Schools and Food Shortages Ichino and Winter-Ebmer (2004) present evidence that Germans of elementary school age during or immediately after World War II received less schooling than individuals in previous and later cohorts. The authors argue that the disruption of secular trends toward more schooling during the 1930s in Germany was a direct consequence of World War II. During the war, access to schools was limited and in some cases disrupted due to destruction and closure of schools and demolition of infrastructure. Similarly, Zink (1957: 1999) writes: "With a large proportion of the German school buildings destroyed or badly damaged by bombing and fighting, places to house the schools was a major problem [...]." and shortage of fuel in the immediate years after 1945 made it difficult to provide adequate heating in schools. Thus, poor housing, inadequate heating and food shortages during the early post-war years might have negatively affected the quality and efficacy of teaching.

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). The occupational forces used several means to achieve this. For example, the American Military Government initiated educational and career programs, cultural events, movies, youth centers, study tours to the US and provided access to information centers ("Amerika-Häuser") whose visitors could use library facilities and visit art exhibitions (Zink, 1957; Schwartz, 1993). Several commentators have discussed differences between the political socialization programs of the British and French and those of the Americans. Merritt (1995: 270) writes: "The United States, with a wealth of resources at its disposal, undertook by far the most extensive program among the Allies to socialize Germans to Western-style democracy".[16] In addition, there existed regional differences in educational programs and cultural events, with a greater amount of programs offered in cities than in the countryside.

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[16]See also Lawson (1963).

Table 3 provides an overview of the various possible explanations for the absence of positive political returns to schooling in Germany. Column (1) lists the potential reasons, column (2) indicates the time periods and column (3) the regions for which the influences of the different political and historical factors were likely to be strongest. The table shows that the problems of apolitical teaching, shortages in teachers and textbooks and physical demolition of schools were likely to be most severe during the early 1950s (Panels A-C in Table 3). To account for these potential offsetting effects, I restrict the sample to Germans born between 1950 and 1960 only. The rationale here is that by focusing on younger cohorts the aforementioned factors were less likely to confound the estimated civic returns to education. Table 4, Panel A, reports the results. Estimates are based on ForsaBus to ensure large enough samples sizes. The marginal effects of the standard probit models are again positive and significant and are of comparable magnitude to the ones reported in Table 1. For example, one more year of schooling is associated with a one percentage point increase in reporting being a member of a political party in both Table 1 (lower panel) and Table 4 (Panel A). Similarly, marginal effects from IV regressions in Table 4, Panel A are negative or close to zero, suggesting that the absence of positive political returns to schooling in Germany are unlikely to be driven by the educational difficulties during the 1950s.

Next, I examine whether civic education programs outside the classroom might have mitigated the impact of an additional year of schooling. As mentioned before, several historians have pointed out that extra-curricular civic education programs were more widely available in the American zone than in the French or British zones. I exploit this variation together with differences in supply of educational programs between rural and urban areas. Table 4, Panel B presents the results after excluding individuals currently living in the area of the former American zone who were likely to be exposed to more civic education programs outside the classroom than those living in the French or British zones.[17] Again, the IV estimates based on

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[17]The American zone comprised the present-day areas of Bavaria, Hesse and the northern part of Baden-Wuerttemberg. To identify individuals living in the northern part of Baden-Wuerttemberg, I make use of

ForsaBus in Panel B, Table 4 are not very different from the corresponding ones in Table 1.

Panel C in Table 4 presents results for individuals living in rural areas of the former French and British zones. Since there were fewer extra-curricular civic programs in the countryside, the returns to schooling estimates in Panel C may have an ever lower likelihood of being contaminated with general educational socialization effects than those in Panel B, Table 4. The results in Panel C confirm previous estimates of zero civic returns to schooling. Thus, even among individuals living in regions with lower levels of educational programs in the area of the former French and British zones there is no evidence of positive civic returns to schooling.

In summary, the results so far suggest that the absence of positive returns to schooling in Germany is unlikely to be driven by adverse post-war educational factors such as shortage of teachers and classroom material, destruction of schools, apolitical or poor teaching or civic educational programs outside the classroom.

5.2 Civic Education, History and Curricula

Were the basic concepts of democracy already fully communicated to pupils by the ninth grade? For instance, Pischke and Wachter (2005) conjecture that the increase in compulsory years of schooling in Germany had no effect on earnings because students acquired the basic skills needed in the job either by 8th 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 increase in compulsory years of schooling.

In fact, civic and historical education in schools was a key goal of the Occupation Forces after the war. In June 1947, the Allies published ten principles for the democratization of education in Germany. One of the statements reads: "All schools should lay emphasis upon

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the administrative district information in ForsaBus and exclude people living in the districts Karlsruhe and Stuttgart. Note that this broadly mirrors the northern part of Baden-Wuerttemberg in the American zone of occupation.

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: 46). In 1959 ordinance directions for the teaching of history had been issued with the profound goal to teach pupils about the Nazi past and to confront them with the committed crimes against humanity. Leichtfuss (1960: 142) notes with respect to these ordinances: "By this, insurance is given that every student who leaves school will have been thoroughly instructed on the history of the most recent past. [...] teachers of history and civics have also been requested to discuss the cruelties which had been committed during the dictatorship by its leaders who ignored even the most primitive human rights." As a result, the Holocaust was taught in several school subjects such as 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. Note that these improvements in civic and historical teaching were also aimed at tackling the problem of apolitical teaching practices discussed previously. Merritt et al. (1971) point out that these reforms were the ones that gained the greatest recognition among teachers. Wehrmann (1998: 3) writes: "Instead, the primary political and educational objective for confronting young Germans with their country's darkest past and their ancestors' guilt is, above all, to make them understand the consequences of Hitler's dictatorship, the uniqueness of the Holocaust, and to make them appreciate the values and institutions that protect freedom and democracy." Most importantly, emphasis has been placed upon teaching 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

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preparation for democratic citizenship is significant today"(Lawson, 1963: 185, 194). Similarly, Banaschewski (1955) writes that civic education and political understanding were important topics in the fifth to seventh school years of lower secondary schooling in post-war Germany. Krippendorff (1961) surveys civic education in school curricula at German schools in 1958, at a time when only pupils in Hamburg and Bremen were supposed to attend nine years of schooling. His overview suggests that civic teaching was an important part of the curricula prior to increase in compulsory years of schooling in the majority of federal states.

Lawson (1963) reports from a survey of teachers in the states 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 and politics and history were mentioned most often and this was equally true across teachers from the three main school types (Lawson, 1963: 152). Moreover, a clear majority of the teachers interviewed expressed positive attitudes towards educational influences of the former occupation powers in Germany.

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. Post-war reformers were convinced that civic education would be more effective if democratic structures in schools and teaching procedures would accompany it. By the end of the 1950s, nearly every school was supposed to have these democratic institutions in place. A survey among German teachers in the late 1960s revealed that they named the introduction of these democratic institutions as secondary in importance only to the development of civic teaching in post-war years (Merritt et al., 1971).

To summarize, by the time compulsory years of schooling were increased in the majority of federal states in the late 1960s, Germany had already experienced several structural reforms such as curricula 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.

A natural question at this point concerns the limits of the present study. First, despite the extensive evidence by educators and historians referred to in Table 3, it is difficult to identify exactly when these various historical factors ceased completely to interfere with the quality of education. For example, a critique might argue that apolitical teaching or the influence of Nazi-propaganda might have not entirely vanished even during the 1960s. Second, there might have been considerable variation in the teaching quality of civic education and history across schools which cannot be taken into account here because of data limitations.

6   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. For the sake of brevity and because sample sizes drop considerably for some robustness checks, 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 data set are presented here.

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. This could also result in mismatch in the identification of individuals affected by compulsory schooling reform. 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 based on ALLBUS. I distinguish between respondents who are still

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living in their state of birth or moved between states that introduced compulsory schooling law in the same year and those who moved from one state to another.[18] Nearly 84 percent of individuals still live in their state of birth and slightly less than five percent moved between the four states that introduced the reform law in the same year. Estimates of these restricted samples are reported in Table 4, Panel A. The first row of Panel A reports results for respondents for whom federal state of birth information is available. The second row presents estimates on a sample of individuals who still live in their federal state of birth or moved between states with legislative changes in the same year (non-movers). Overall, the results yield similar conclusions to those in Table 1, 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, as indicated in Appendix A, Table 1. 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 4 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. I address this issue by controlling for the following state characteristics at federal state levels during the time period 1946-1966: number of firms per 10,000 inhabitants, number of lower, intermediate, and upper-level secondary general

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[18]The federal states North Rhine-Westphalia, Hesse, Rhineland-Palatinate and Baden-Wuerttemberg all introduced nine years of compulsory schooling in 1967 (see Appendix A, Table 1).

schools per 10,000 inhabitants[19], 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). Summary statistics of these state characteristics are given in Appendix A, Table 4. Estimates in Table 4, Panel C show that including proxy variables for political, social, and economic environment around the time of introduction of schooling legislation does not change the general conclusions.[20]

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 4. 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 introduction of the reform were assigned eight years of schooling, compared to nine for those with the same graduation certificate after introduction of the reform. Note that this assignment mechanism assumes perfect compliance with compulsory schooling laws and has the advantage that repetition of a grade is not treated as more years of schooling. Column (1) of Table 5 reports marginal effects for years of schooling as reported by respondents

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[19]These school types were previously called Volksschule, Mittelschule and höhere Schulen, respectively.

[20]In the majority of regressions, the state characteristics have no significant impact on citizenship.

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 provide little evidence of causality.[21]

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.[22]

7 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 over the period 1949 to 1969 across federal states.

Several potential explanations for the absence of positive civic returns to education in post-war Germany were examined. First, teacher shortages, destruction of schools, food and fuel shortages, apolitical teaching practices and poor civic teaching due to teachers' former experi-

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[21]Note that the power of the instrument decreases considerably in the first specification. IV estimates for two outcome variables in Table 5, Column 1 exceed probit, suggesting higher political returns to education, but are not precisely estimated.

[22]I also checked sensitivity of results to the definition of outcome variables, using original categorical instead variables of derived dichotomous outcomes. This did not change the results.

ence of the Nazi period could have negatively affected the efficacy of civic education. Second, Allied socialization programs after World War II could have had a positive influence on German's democratic values and political behaviour, potentially outweighing the benefits of more schooling. Various empirical estimates suggest that these historical events were unlikely to have affected the civic returns to schooling in post-war Germany.

Finally, this study examined whether civic school reforms prior to 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 school. 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 basics 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 UK, whereas the present study finds no positive influence. These cross-country differences mirror recent results in the economic literature on the wage returns to schooling. Pischke and Wachter (2005) find zero returns to schooling on earnings in Germany, whereas previous studies for the US and UK report positive significant effects in the order of 10 to 15 percent. Together, this body of research suggests that institutional and historical factors can play an important role in shaping economic and political returns to schooling in a society.

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Table 1: The Impact of Schooling on Citizenship

Outcomes	Probit	IV	Average effect of reform	First-stage F statistic	N
	(1)	(2)	(3)	(4)	(5)
ALLBUS					
Political awareness					
Interest in politics	0.041**	-0.096**	-0.060**	*82.84*	*14,007*
	(0.002)	(0.021)	(0.015)		
Participation in voting					
Voted in last general election	0.008**	0.005	0.000	*51.44*	*10,100*
	(0.001)	(0.018)	(0.009)		
Democratic values					
Opportunity to come to power for democratic parties	0.028**	-0.037	-0.022	*29.84*	*2,673*
	(0.008)	(0.066)	(0.031)		
Demonstrating is civil right	0.021*	-0.001	-0.003	*34.25*	*2,683*
	(0.008)	(0.073)	(0.034)		
Political involvement					
Active in citizen group	0.028**	-0.050	-0.036	*12.93*	*4,840*
	(0.003)	(0.051)	(0.024)		
Signed a petition	0.045**	-0.074	-0.036	*14.54*	*4,929*
	(0.004)	(0.051)	(0.024)		
Participated in demonstration	0.027**	-0.016	-0.007	*13.6*	*4,789*
	(0.002)	(0.045)	(0.016)		
Political group membership					
Member of a citizen group	0.002**	-0.003	-0.001	*91.06*	*10,973*
	(0.000)	(0.008)	(0.003)		
Member of a political party	0.004**	-0.021	-0.009	*95.25*	*16,005*
	(0.001)	(0.016)	(0.005)		
ForsaBus					
Interest in politics	0.082**	-0.008	-0.019	*138.83*	*28,720*
	(0.002)	(0.013)	(0.025)		
Voted in last general election	0.005**	0.0002	0.0006	*775,76*	*490,359*
	(0.0002)	(0.002)	(0.009)		
Member of a political party	0.009**	-0.010*	-0.003	*560.92*	*458,984*
	(0.0002)	(0.002)	(0.007)		

Notes: 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. **, * indicate significance at 1- and 5-percent level, respectively.

Table 2: 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[a]		Father has basic school certificate or less[b]		Father with occupational prestige score below median[b]	
	Probit	IV	Probit	IV	Probit	IV
Political awareness						
Interest in politics	0.015	-0.005	0.037**	-0.117**	0.046**	-0.096**
	(0.011)	(0.018)	(0.002)	(0.024)	(0.004)	(0.042)
	N = 14,604		*N = 10,551*		*N = 5,603*	
Participation in voting						
Voted in last general election	0.008**	-0.002	0.007**	-0.004	0.012**	-0.028
	(0.002)	(0.003)	(0.002)	(0.020)	(0.003)	(0.047)
	N = 143,914		*N = 7,768*		*N = 4,089*	
Democratic values						
Demonstrating is civil right			0.013	0.008	0.030	-0.028
			(0.009)	(0.064)	(0.016)	(0.300)
			N = 2,177		*N = 1,174*	
Political involvement						
Signed a petition			0.041**	-0.097	0.039**	0.073
			(0.005)	(0.050)	(0.007)	(0.089)
			N = 3,784		*N = 2,065*	
Political membership						
Member of a political party	-0.001	-0.011**	0.003**	-0.001	0.002**	-0.001
	(0.001)	(0.003)	(0.001)	(0.013)	(0.001)	(0.009)
	N = 212,637		*N = 12,187*		*N = 6,492*	

Notes: [a] Estimates based on ForsaBus. Individuals with no or lowest school degree. [b] 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. **, * indicate significance at 1- and 5-percent level, respectively.

Table 3: Potential Explanations for the Absence of Positive Political Returns to Schooling in Germany

Explanation	Time Period	Region	Sources
A. Denazification, teaching practice and teacher shortage	Mainly during the 1950s.	All Western zones.	Fulbrook (1999) Gieseke (1976) Merritt (1995) Tent (1982) Händle (2002) Matthewes (1961).
B. Shortage of adequate teaching material	Mainly during the early 1950s.	All Western zones.	Clay (1950) Langenbeck (1961) Soysal (2000).
C. Physical destruction of schools	Mainly during the early 1950s.	All Western zones. Destruction was more severe in cities.	Ichino and Winter-Ebmer (2004) Zink (1957).
D. Extra-curricular civic education		All Western zones, most extensive programmes in the US zone. More programmes in urban than in rural areas.	Merritt (1995) Zink (1957) Schwartz (1993) Lawson (1963).

Table 4: The Impact of Schooling on Citizenship (Younger Cohorts)

Outcomes	Voted in last general election		Member of a political party	
	Probit	IV	Probit	IV
Panel A				
All individuals born	0.007**	0.003	0.008**	-0.007
between 1950-1960	(0.0003)	(0.003)	(0.0004)	(0.004)
	N = 208,263		*N = 193,793*	
Panel B				
Individuals living in the former	0.007**	0.001	0.009**	-0.001
French or British zones	(0.0004)	(0.004)	(0.0003)	(0.005)
	N = 128,929		*N = 119,527*	
Panel C				
Individuals living in rural areas in the	0.006**	-0.025	0.008**	-0.008
former French or British zones	(0.0008)	(0.014)	(0.001)	(0.015)
	N = 15,845		*N = 16,829*	

Notes: Estimates based on ForsaBus. Sample of individuals born between 1950 and 1960. 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. **, * indicate significance at 1- and 5-percent level, respectively.

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
Panel A (Geographic mobility)						
Individuals with valid	0.046**	-0.113**	0.016**	0.022	0.007**	0.003
state of birth information	(0.003)	(0.030)	(0.002)	(0.035)	(0.001)	(0.024)
	N = 4,745		*N = 2,814*		*N = 4,737*	
Non-movers[a]	0.045**	-0.112**	0.014**	0.041	0.008**	-0.008
	0.004	(0.031)	(0.003)	(0.039)	(0.002)	(0.024)
	N = 4,206		*N = 2,479*		*N = 4,136*	
Panel B (Timing of reform)						
Individuals affected by law						
2 years before and after	0.041**	-0.098**	0.008**	0.003	0.004**	-0.023
introduction are excluded	(0.002)	(0.020)	(0.001)	(0.019)	(0.001)	(0.017)
	N = 13,725		*N = 9,885*		*N = 15,185*	
Panel C (Environment controls)						
Controls for state						
characteristics	0.048**	-0.107*	0.017**	0.014	0.007**	0.004
during 1950s	-0.002	(0.045)	(0.003)	(0.108)	(0.002)	-0.065
	N = 3,872		*N = 2,298*		*N = 3,868*	
Panel D (German reunification)						
Outcomes measured during	0.031**	-0.062	0.005**	-0.033	0.001	-0.027
survey years 1980 1988	(0.004)	(0.055)	(0.002)	(0.063)	-0.001	-0.031
	N = 4,879		*N = 3,989*		*N =6,882*	

Notes: [a] Non-movers are defined as respondents who still live in their federal state of birth or moved between states which introduced compulsory schooling law in the same year. 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. **, * indicate significance at 1- and 5-percent level, respectively.

Table 6: Alternative Measures of Years of Schooling (ALL-BUS)

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

Notes: Explanatory variable is years of schooling. N denotes number of individuals. F denotes First-Stage F-statistic. 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. **, * indicate significance at 1- and 5-percent level, respectively.

[a] The explanatory variable 'years of schooling' equals eight if respondents received secondary general school (Hauptschule) before and is nine if respondents received secondary general school after compulsory schooling reform. Individuals with intermediate school certificate and grammar school certificate are assigned 10 and 13 years of schooling, respectively. Respondents with a technical school certificate are assigned 12 years of schooling.

9 Appendix A: The Education System in Germany

(NOTE: Appendix A and B are for referees only and not intended for publication for the sake of brevity.)

In Germany, education is the responsibility of the federal states. After primary school, pupils between the age of 10 and 14 are channelled into different secondary school tracks. The three main tracks are: lower secondary school (Hauptschule), intermediate secondary school (Realschule), and upper secondary school (Gymnasium). Hauptschule currently ends after nine years of compulsory schooling with a formal certificate and offers the lowest level of secondary education, in terms of both curriculum and duration. Realschule leads to a formal certificate of graduation after ten years of schooling and is usually followed by an apprenticeship in combination with attending a vocational school or, in some cases, a move to a Gymnasium. Gymnasium, the most prestigious and academically-oriented track, ends after 12-13 years of schooling with the Abitur, the highest-level certificate of graduation which allows recipients to enter universities and other institutions of higher education. Regardless of the school type attended, students currently have to complete at least nine years of schooling. For a more detailed description of the educational system in Germany, see Winkelmann (1996) and Dustmann (2004).

Before World War II, compulsory schooling ended after only eight years of schooling. Hamburg was the first state to increase compulsory schooling to nine years in 1949. Over the next 20 years, a process of expansion of compulsory schooling by an additional year began. Increases in schooling took place gradually over time and across states. Nine years of compulsory schooling was agreed on in the Hamburg Accord of 1964, in which the prime ministers of all federal states agreed that nine years of compulsory schooling should be introduced in all federal states by 1967. Bavaria was the last state to increase compulsory schooling in 1969. Table 1 in this section provides an overview of the introduction of nine years of compulsory schooling across states in Germany (taken from Pischke and von Wachter, 2005).[23]

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[23]Note that introduction of law changes coincided with short school years in four federal states. When

Table 1: Introduction of Nine Years of Compulsory Schooling in Germany by State

Federal state (Bundesland)	First year when all students were supposed to have nine years of schooling	First birth cohort with nine years of schooling
Schleswig-Holstein	1956	1941
Hamburg	1949	1934
Lower Saxony	1962	1947
Bremen	1958	1943
North Rhine-Westphalia	1967	1953
Hesse	1967	1953
Rhineland-Palatinate	1967	1953
Baden-Wuerttemberg	1967	1953
Bavaria	1969	1955
Saarland	1964	1949

Source: Pischke and von Wachter (2005)

assigning the reform status to individuals according to year of birth information, short years are taken into account (see Pischke and von Wachter, 2005 and Pischke, 2007).

Table 2: Definition of Outcome Variables

Political Outcome	Question reads:	Variable in original data set	Years information is available	Definition of political outcome variable
ALLBUS				
Interest in politics	"How interested in politics are you?"	The variable takes five different values: (1) very strongly; (2) strongly; (3) middling; (4) very little; (5) not at all.	Biennial from 1980-1986, 1990, 1991, biennial from 1992-2004.	Variable equals one if a respondent indicates (1) or (2), and zero otherwise.
Importance of politics and public life	"We would like to know how important each of these spheres of life is for you." One of the items listed is: "Politics and public life".	Respondent can answer on a scale from 1 to 7, with 1 representing "unimportant" and 7 "very important".	1980, 1982, 1986, 1990, 1991, 1992, 1998.	Variable equals one if a respondent indicates (6) or (7), and zero otherwise.
Voted in last general election			Biennial from 1984-1990, 1991, 1992, 1996, 1998, 2002, 2004.	Variable equals one if a respondent voted and zero if eligible, but did not vote.
Opportunity to come to power for democratic party	"Every democratic party should in principle have the opportunity to come to power."	Respondent can answer on a scale from -3 ("completely disagree") and +3 ("completely agree")	1982, 1988.	Variable equals one if a respondent indicates +2 or +3, and zero otherwise.
Demonstrating is civil right	"Every citizen has the right to take to the street.	Respondent can answer on a scale from -3 ("completely disagree") and +3 ("completely agree")	1982, 1988.	Variable equals one if a respondent indicates +2 or +3, and zero otherwise.
Active in citizen group Signed a petition Participated in a demonstration	Respondents are asked: "Which of these things have you already done, what have you already taken part in? Please hand me the corresponding cards." Among the listed items were (1) participated in a citizen's action group; (2) took part in an authorized demonstration; (3) signed a petition.	The variables take five different values: (1) participated; (2) in important circumstances; (3) in special circumstances; (4) never; (5) do not know.	1988, 1990, 1991, 1992, 2000.	Variables equal one if a respondent indicates to have participated, and are zero otherwise. Respondents who answered with "do not know" where excluded from the analysis.
Member of a citizen group	"Are you currently a member of an organization or association? Please go through this list and tell me what you are a member of. Among the listed items was: "Citizen's action group".		Biennial from 1980-1990, 1991, 1992, 1998.	Variable equals one if a respondent names a citizen's group, and zero otherwise.
Member of a political party	"Are you currently a member of a political party?"		Biennial from 1980-1990, 1991, biennial	Variable equals one if a respondent said "yes", and zero if answered with "no".
ForsaBus				
Interest in politics	"How strong is your interest in politics?".	The variable takes four different values: (1) very strong; (2) strong; (3) not so strong; (4) not at all.	1997, 1998	Variable equals one if a respondent indicates (1) or (2), and zero otherwise.
Voted in last general election	"Which party did you vote for in the last general election?"		All waves.	Variable equals one if a respondent voted for a democratic party and zero if eligible, but did not vote.
Member of a political party	"Are you currently a member of a political party?"		All waves.	Variable equals one if a respondent said "yes", and zero otherwise.

Notes: In some cases, the exact wording of the questions changed slightly over the years outcomes are measured.

Table 3: Summary Statistics by Compulsory Schooling Reform

	Not affected by reform	Affected by reform	Number of individuals
ALLBUS			
Political awareness			
Interest in politics	0.304	0.289	14,007
Participation in voting			
Voted in last general election	0.934	0.914	10,100
Democratic values			
Opportunity to come to power for democratic parties	0.784	0.792	2,673
Demonstrating is civil right	0.669	0.791	2,683
Political involvement			
Active in citizen group	0.173	0.208	4,840
Signed a petition	0.397	0.479	4,929
Participated in demonstration	0.092	0.186	4,789
Political group membership			
Member of a citizen group	0.010	0.011	10,973
Member of a political party	0.050	0.038	16,005
Female	0.522	0.528	16,005
Age	49.18	35.66	16,005
Year of birth	1940.97	1954.81	16,005
Years of schooling[a]	8.54	9.63	16,005
Years of schooling[b]	8.93	10.16	16,005
Father with basic schooling track	0.843	0.780	14,846
Mother with basic schooling track	0.890	0.849	12,658
Fathers occupational prestige score (Treiman)	38.82	39.68	12,921
ForsaBus			
Female	0.524	0.521	490,359
Age	56.16	42.36	490,359
Year of birth	1941.37	1955.17	490,359
Years of schooling	9.47	10.42	490,359
Political awareness			
Interest in politics	0.491	0.449	28,720
Participation in voting			
Voted in last general election	0.944	0.927	490,359
Political group membership			
Member of a political party	0.079	0.058	458,984

Notes: The number of observations for the outcomes varies considerably because corresponding questions were asked more and less frequently throughout survey years. Means for covariates are computed on the largest sample size used in the analysis. [a] Explanatory variable used in the main analysis. [b] Generated variable years of schooling. See notes to Table 6 for definition. This definition is the same as for the ForsaBus data.

Table 4: Summary Statistics of Federal State Variables

Explanatory variables	Mean	Standard deviation
Number of secondary general schools[a]	1169.56	74.53
Number of intermediate schools[a]	61.12	40.09
Number of grammar schools[a]	146.82	19.80
Displaced people ("Vertriebene", June 1953)	0.18	0.06
Catholic (September 1950)	0.46	0.21
Votes for SPD (1946-1966)	0.38	0.07
Households with broadcasting service in the population (April 1953)	0.70	0.03
Population density (inhabitants per square kilometer, September 1950)	430.17	375.01
Number of firms (per 10,000 inhabitants, July 1953)	235.09	7.00
Sample Size	*4,650*	

Notes: [a] Per 10,000 inhabitants, May 1952. Means are computed on the largest sample size used in the analysis for which we have valid state of birth information. Individuals born in Saarland are excluded because information for many variables was not available. Sources: Regional statistical offices of federal states and Statistisches Jahrbuch fr die Bundesrepublik Deutschland 1954.

10 Appendix B

This section provides evidence that compulsory schooling laws in Germany were effective in increasing years of schooling and that the law changes provide a strong and valid instrument.

As first descriptive evidence, Figure 1 shows the effect of the reforms on average years of schooling up to seven years before and after increase in compulsory schooling. Note that because 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 average years of schooling occurred in the two years after implementation of the law changes.

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

Table 2 provides results from First-stage regressions of compulsory schooling law dummy (R) 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 of a 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 years. The bottom of the table reports a second measure for strength of instrument, the partial R^2 (Bound et al. 1995). According to both measures (F-statistics reported in Table 1 and partial R^2), the hypothesis that increasing compulsory schooling by one year has no impact on the number of years of schooling can be rejected at conventional significance levels across all samples.

As discussed previously, compulsory schooling is mainly binding at the bottom of the educational distribution since it increased education from eight to nine years. In the light of Angrist

and Krueger (1991), I provide various tests of whether increase in compulsory schooling influenced education even for individuals who were not constrained by compulsory schooling laws. The rationale here is that one would not expect an impact of compulsory schooling law on individuals with post-secondary schooling since they are likely to have attained higher education irrespective of increases in compulsory schooling at the bottom of the educational distribution. To test this, I draw a new sample including individuals with technical college degrees and university degrees. Table 3 provides evidence that the law changes were effective in increasing years of schooling at lower levels of education, but not at the upper end of the educational distribution. The first row in Table 3 presents estimated OLS coefficients of compulsory schooling dummy on years of schooling for individuals at the lower end of the educational distribution who attended either Hauptschule or Realschule. It is apparent that there exists a strong relationship for this group and the effect is statistically different from zero at the 1-percent level. The second row shows the association between compulsory schooling law and years of schooling for individuals with higher levels of schooling. The association is much lower in magnitude and imprecisely estimated.

The lower panel in Table 3 shows marginal effects of probit models on various dichotomous measures of post-secondary educational attainment. The first row in the lower panel shows the effect of compulsory schooling law on having a technical college entrance qualification. The remaining rows show the impact on having a university entrance qualification, a technical college degree or a university degree, respectively. It is apparent that there exists no effect of compulsory schooling laws on higher educational attainment. For all outcomes, marginal effects are very close to zero. Moreover, the higher the educational attainment, the lower the effects of compulsory schooling reforms on post-secondary attainment. This is consistent with empirical evidence reported in Angrist and Krueger (1991) for the United States.

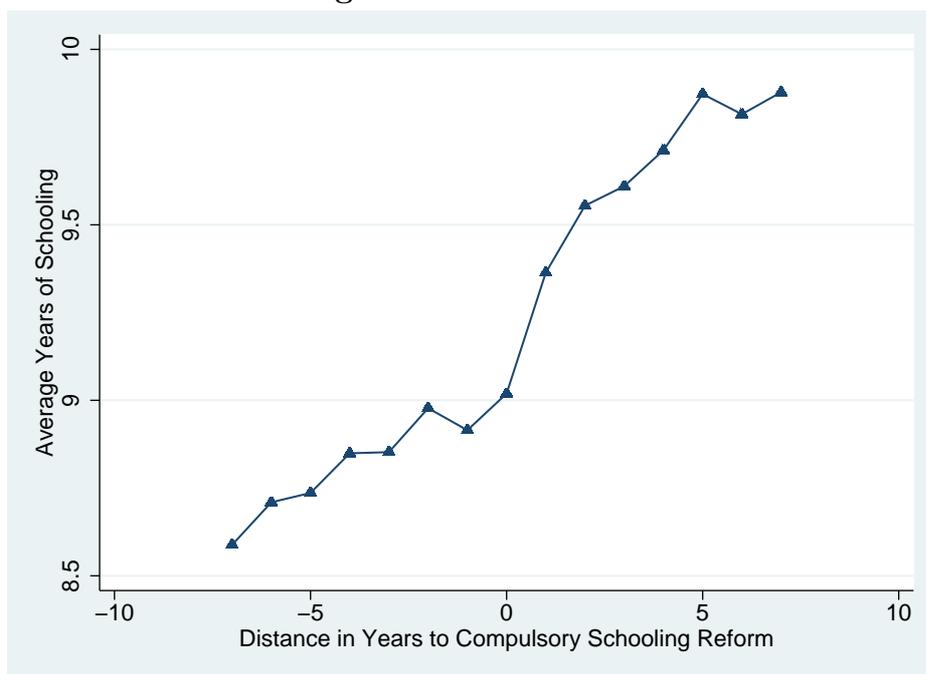**Figure 1: Effect of Compulsory Schooling Reform on Years of Schooling**

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	7.3
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 member-ship	Interest in politics	Voted in last general election
Age	0.087**	0.075**	0.066	0.125**	0.072**	0.099**
	(0.021)	(0.015)	(0.034)	(0.019)	(0.029)	(0.005)
Age-squared	-0.001**	-0.001*	-0.001	-0.001**	-0.001**	0.000
	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Year of birth	0.032	0.094**	0.085**	0.062**	0.103**	0.135**
	(0.019)	(0.008)	(0.046)	(0.017)	(0.024)	(0.002)
Female	-0.146**	-0.149**	-0.132*	-0.139**	-0.403**	-0.375**
	(0.032)	(0.042)	(0.055)	(0.017)	(0.024)	(0.012)
Dummy for Compulsory Schooling Reform	0.538**	0.522**	0.392**	0.520**	0.480**	0.434**
	(0.074)	(0.085)	(0.117)	(0.067)	(0.041)	(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. **, * indicate significance at 1- and 5-percent level, respectively.

Table 3: The Effect of Compulsory Schooling Law on Educational Outcomes (ALLBUS)

Outcome variable	Coefficient	Standard error	Number of observations
Years of schooling[a]			
Lower levels of schooling	0.272**	0.047	7,383
Higher educational schooling	0.146	0.162	1,954
Highest educational certificate[b]			
Technical college entrance qualification (Fachhochschulreife)	0.008	0.006	9,337
University entrance qualification (Abitur)	0.005	0.007	9,337
Technical college degree (Fachhochschulabschluss)	-0.000	0.007	9,337
University degree(Universitätsabschluss)	-0.001	0.009	9,337

Notes: [a] Estimated coefficients of compulsory schooling law dummy (R) on educational outcomes using OLS. [b] Marginal effects of compulsory schooling law dummy (R) on educational outcomes using probit. **, * indicate significance at 1- and 5-percent level, respectively.