ECONSTOR
Make Your Publications Visible.

A Service of

ZBW Leibniz-Informationszentrum Wirtschaft
Leibniz Information Centre for Economics

Siedler, Thomas

Working Paper

Schooling and Citizenship: Evidence from Compulsory Schooling Reforms

DIW Discussion Papers, No. 665

Provided in Cooperation with:
German Institute for Economic Research (DIW Berlin)

Suggested Citation: Siedler, Thomas (2007) : Schooling and Citizenship: Evidence from Compulsory Schooling Reforms, DIW Discussion Papers, No. 665, Deutsches Institut für Wirtschaftsforschung (DIW), Berlin

This Version is available at:
https://hdl.handle.net/10419/18397

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.

WWW.ECONSTOR.EU

Mitglied der Leibniz-Gemeinschaft

DIW Berlin

German Institute
for Economic Research

Discussion Papers

665

Thomas Siedler

Schooling and Citizenship: Evidence from Compulsory Schooling Reforms

Berlin, February 2007

Schooling and Citizenship: Evidence from Compulsory Schooling Reforms

Thomas Siedler*
University of Essex and DIW Berlin

February 22, 2007

Abstract

This paper examines whether schooling has a positive impact on individual's political interest, voting turnout, democratic values, political involvement and political group membership, using the German General Social Survey (ALLBUS). 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 law changes allow one to investigate the causal impact of years of schooling on citizenship. Years of schooling are found to be positively correlated with a broad range of political outcome measures. However, when exogenous increase in schooling through law changes is used, there is no evidence of a causal effect running from schooling to citizenship in Germany.

Keywords: Voting, civic engagement, education, externalities, instrumental variables estimation

JEL Classifications: I2; H4; H23

*Institute for Social and Economic Research, University of Essex, Wivenhoe Park, Colchester CO4 3SQ, United Kingdom. Email: tsiedl@essex.ac.uk

Opinions expressed in this paper are those of the author and do not necessarily reflect views of the institute.

IMPRESSUM
© DIW Berlin, 2007
DIW Berlin
German Institute for Economic Research
Königin-Luise-Str. 5
14195 Berlin
Tel. +49 (30) 897 89-0
Fax +49 (30) 897 89-200
http://www.diw.de

ISSN print edition 1433-0210
ISSN electronic edition 1619-4535

Available for free downloading from the DIW Berlin website.

"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 work 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).

To date, however, we know surprisingly little about whether more education has a genuinely causal impact on individuals' democratic attitudes and political behavior.[2] Moreover, whether more schooling increases political citizenship in different democratic systems and whether a positive relationship constitutes a global phenomenon are open questions. Differences in democratic institutions, forms of governments, electoral rules and historical experiences across countries may shape democratic citizenship. In particular, whether there exists a causal effect of education on political interest, participation in voting, political involvement, and democratic

[1] The terms education and schooling are used interchangeably in this study.

[2] Exceptions are the studies by Dee (2004) and Milligan et al. (2004) which investigate causal effects of education on citizenship in the United States and the United Kingdom.

values in relatively young democratic states remains unexplored.

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.

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 et al. (1978) found that with more years of schooling, students also showed increased tolerance of freedom of speech. Moreover, 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). Third, having more education increases the chances that people will learn from history. Individuals with more schooling have been found to be better informed about Communist and Nazi beliefs (Nunn et al., 1978). Finally, democratic citizenship is a compulsory subject in many school systems around the world (Holmes, 1979). 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]."[3]

In contrast, there are also plausible reasons as to 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 participation at demonstrations or being active in a citizen group. Moreover, with respect to voting participation, Dee (2004: 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 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.[4]

Using 14 waves of cross-sectional data from ALLBUS, the German General Social Survey, I find that years of schooling are positively correlated with political awareness, participation in voting, democratic values, and political group membership. Some of the effects are large. For example, increasing schooling by an additional year is associated with a four percentage point increase in individual political interest, over the average of 30 percent of individuals reporting an interest in politics. Similarly, one more year of compulsory schooling is associated with a three percent increase in having participated in an approved political demonstration, and a five percent increase of having signed a petition. However, instrumental variable (IV) estimates provide no evidence that there is a causal effect of education on democratic citizenship in

[3]See ⟨http://www.coe.int⟩ for further information.

[4]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 returns to education literature.

Germany. The IV estimates are considerably smaller than standard probit and OLS estimates; most marginal effects are negative or close to zero, and the overwhelming majority are not statistically different from zero.

The findings indicate that the positive correlations between education and democratic citizenship in Germany are driven by unobservable characteristics that are correlated with both schooling and the political outcomes under study. One potential explanation for positive correlations could be association in political citizenship and schooling between parents and children in the wake of World War II. For instance, having experienced fascism and dictatorship, better educated parents in Germany might tend to teach their children the importance of democracy, democratic participation, and the dangers of dictatorship more intensively and comprehensively than parents with fewer years of schooling, which might in turn have had a strong and long-lasting impact on their children's civic behavior. There might also exists a positive relationship between parents' and children's schooling. Indeed, using data from the German Socio-Economic Panel (SOEP) for younger cohorts, I have found suggestive evidence that part of the positive association between schooling and political citizenship in Germany might be driven by a strong intergenerational link in education and civic values.

The remainder of the paper is organized as follows. The next section surveys the relevant literature. Section 3 briefly describes the educational system and compulsory schooling reform in Germany. Section 4 discusses the estimation strategy. Data, sample selection, and variables used in the analysis are discussed in Section 5. Section 6 presents main results concerning the relationship between schooling and democratic citizenship. Robustness checks are presented in Section 7, 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 USA. 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 USA 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 USA, but not in the UK.[5] 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.

This paper adds to the still very limited literature on schooling and civic participation by examining causal relationships between schooling and a broad range of political outcomes. This is one of the first studies which comprehensively investigates whether there are causal effects of education on citizenship in the areas of political interest, participation in voting, democratic values, political involvement and party membership. Moreover, this is the first study to examine political returns to education in Germany, whose democratic system is considerably younger than that of the UK or the USA. Thus, it provides evidence about whether the relationships found in Anglo-Saxon countries also carry over to a younger democracy.

3 The Education System in Germany

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 college combined with an apprenticeship 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 in-

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

stitutions 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 Appendix A provides an overview of the introduction of nine years of compulsory schooling across states in Germany (taken from Pischke and von Wachter, 2005).[6]

4 Estimation Methods

I start by investigating the relationship between years of schooling and democratic citizenship, estimating standard probit and OLS regressions. In a second step, I estimate the impact of schooling on political outcomes using both IV probit and two-stage least square (2SLS) models, treating years of schooling as endogenous and the other explanatory variables as exogenous.[7] The linear specification to assess the association between years of schooling and political outcomes for each individual takes the form:

[6]Note that introduction of law changes coincided with short school years in four federal states. When 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, 2006).

[7]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 presented here.

$$P = \beta_0 + \beta_1 S + X\kappa + \epsilon, \tag{1}$$

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 state dummies, and state-specific cohort trends. Equation (1) is estimated by a probit for dichotomous outcomes and by OLS for continuous dependent variables. In a second step, I account for endogeneity of years of schooling by estimating instrumental variable regressions. 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.

As a third identifying strategy, I estimate reduced forms of compulsory schooling reform. The linear specification for each individual has the form:

$$P = \gamma_0 + \gamma_1 R + X\delta + \epsilon, \tag{3}$$

with P, R and X as defined above. The key parameter here is γ_1, which captures the average effect of compulsory schooling reform on citizenship for those affected by the legislative change. This estimation method sheds light on any direct impact of increasing compulsory schooling on civic behavior.

5 Data

I use 14 pooled cross-sectional waves from ALLBUS, the German General Social Survey, for survey years 1980, 1982, and every 2^{nd} 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, 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.[8] 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 criteria, I restrict the analysis to individuals who graduated from some form of secondary school but do not have a university or technical college degree. 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.

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).[9]

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

[9]Section 7 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.

Overall, I use one main sample and three restricted samples. The main 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.[10]

From the main sample, I extract three restricted samples: (1) individuals at the lower end of educational distribution who attended either a Hauptschule or a Realschule; (2) individuals with fathers who have 'low' (Hauptschule) or no levels of education, and (3) individuals whose fathers have an occupational prestige score below the sample's median value. Examining the impact of compulsory schooling on individuals with 'disadvantaged' family background 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.[11] 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).

In all samples, each respondent is assigned a date of introduction of the compulsory schooling reform, based on current state of residence, assuming that individuals went to school in the federal state where they currently live. Clearly, this results in some mismatches and decreases the precision of the instrument, but IV estimates will still be consistent (Lochner and Moretti, 2004). As a robustness check, I assign date of introduction according to the individual's federal

[10]Individuals currently living in West Berlin are excluded from the analysis to avoid potential problems from East-West migration. See Pischke and von Wachter (2005) for similar sample selection criteria.

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

state of birth. This results in smaller sample sizes because state of birth information is not available in all survey years but is also likely to suffer from fewer mismatches for the instrumental variable candidate.[12] The second matching procedure is based on the less restrictive assumption that individuals did not move from one federal state to another during childhood. Estimates in the robustness check section below show that the two different assignment mechanisms yield similar conclusions. It is also important to note that Germany has low levels of geographic mobility in comparison to the USA and UK and that mobility is particularly low during childhood and early adulthood (Rainer and Siedler, 2005). Moreover, mobility rates were low during the period 1950-1970 among native Germans in the Federal Republic of Germany, with an annual migration rate between states of around 0.02, as defined as the ratio of number of migrants to or from a state within one year by the population of that federal state (Hochstadt, 1999).

There are five types of measures of democratic citizenship:

- **Political Awareness:** whether interested in politics; whether expressed personal importance of politics and public life;
- **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. In addition to dichotomous outcome

[12]State of birth information is available in ALLBUS surveys 1991, 1992, 1994, 2000, 2002 and 2004.

measures, I construct four political scales: political awareness, democratic values, political involvement, and political group membership. Each scale is the sum of the dichotomous political outcomes in the corresponding category. For example, the political involvement scale ranges from 0 (respondents who were 'not politically active') to 3 (respondents who answer that they were active in a citizen group, signed a petition and participated in a demonstration). The scales provide additional measures of individuals' democratic citizenship. 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. A detailed definition of all outcome variables and political scales is given in Table 2 in Appendix A. Summary statistics, broken down by whether respondents were affected by compulsory schooling reform, are presented in Table 3, Appendix A.

6 Schooling and Citizenship

6.1 Main Sample

I run regressions for each of the outcomes first using probit or OLS and second using IV probit or 2SLS to account for endogeneity. Moreover, average treatment effects of compulsory schooling are estimated. Table 1 presents the findings for the relationship between years of schooling and political outcomes for the main sample. 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. 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 political interest by four percentage points, compared to an average of 30 percent of individuals who expressed an interest in politics.

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 said that they had at some point participated in a political demonstration. The standard errors for both outcomes are 0.002, which implies that estimates are statistically different from zero at the one percent level (standard errors are corrected for state and year of birth clustering).[13]

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 and OLS 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

[13]In unreported regressions, I also controlled for non-linearity in state-specific time trends by including federal 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.

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 ALLBUS, clearly suggesting that voting participation is overreported in ALLBUS. Higher self-reported turnout rates compared to official figures are also found by Milligan et al. (2004) in both the USA and UK. If years of schooling are indeed positively linked to report socially acceptable political behavior, probit and OLS estimates might be upward 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, OLS 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 probit and OLS 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 become negative. Similarly, 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 express an interest in politics or to have voted in the last general election. There is also no evidence that the average effect of compulsory schooling reform for those who were assigned to it improved democratic values, political involvement or the likelihood of political group membership. The results are in line with Touya (2006) who also reports positive relationships between years of schooling and citizenship outcomes in Spain, but finds no evidence for causal effects from schooling on civic engagement.

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 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. This indicates that the positive association is likely to be driven by factors that are correlated with both schooling and people's political behavior and attitudes.

6.2 Restricted Samples

Table 2 provides further evidence of the relationship between years of schooling and political behavior for the three restricted samples: (1) individuals at the lower end of educational distribution; (2) individuals with fathers who have 'low' or no levels of education, and (3) individuals whose fathers have an occupational prestige score below the sample's median value.[14] 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 size in each of the five citizenship categories: political awareness, participation in voting, democratic values, political involvement and political group membership. Similarly, I only present results for the scale political membership.

The estimates shown in Table 2 point to positive significant relationship between years of schooling and the majority of political outcomes. There are no huge differences in magnitude or significance of the marginal effects across the three samples. Similarly, there are no major

[14]I also estimated the models on a sample of individuals with both parents having basic schooling or less. This did not change the results.

differences between these and the results in Table 1, suggesting no systematic variation in political returns to schooling among individuals with different family backgrounds. Similarly, turning to the IV results, many are negative or close to zero, further indicating that there is no empirical evidence of a causal relationship between years of schooling and democratic citizenship in Germany.[15]

Why might there be a positive relationship between schooling and citizenship in Germany but little evidence of causality? One potential explanation could be a strong link in political citizenship and schooling between parents and children in the wake of World War II. For example, having experienced the Third Reich, better educated parents in Germany might have taught their children about the importance of democracy and democratic participation more comprehensively and in greater depth than parents with fewer years of schooling. This might have had an important influence on children's democratic behavior. It is also likely that parents with more education have children with more years of schooling.

I provide some suggestive evidence favoring this hypothesis based on data from the German Socio-Economic Panel (SOEP) for younger cohorts. The SOEP household panel survey is representative for the entire German population residing in private households.[16] I use SOEP wave 21 (year 2004) to construct an intergenerational data set covering children born between 1950 and 1986 and their parents. The SOEP asks respondents how interested they are in politics on a four-point scale.[17] For both parents and children, I generate a dummy variable for political interest which equals one if they say that they are "extremely" or "fairly" interested in politics, and zero otherwise.

The estimates in Table 3 point to a positive relationship between years of schooling and political interest (column 1). This is in line with the results in Table 1 for older cohorts.

[15]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.

[16]For further information on the SOEP see Haisken-DeNew and Frick (2005) and Burkhauser et al. (1997).

[17]The question reads: "Generally speaking, how much are you interested in politics?" Respondents could answer: "extremely", "fairly", "not very" or "not at all".

Furthermore, there exists a strong positive association between the political interest of parents and adult children in Germany. For instance, having a mother who reports being interested in politics is associated with adult children's likelihood of also expressing political interest by 13 percentage points. In comparison, one more year of schooling is associated with an increase in respondents' political awareness by five percentage points. Moreover, once parents' political interest and highest educational attainment are controlled for, the association between adult children's education and political interest weakens (columns 2 and 3). The estimates provide suggestive evidence as to why the positive correlation between schooling and democratic citizenship among Germans born between 1930 and 1960 might not be causal. The positive link could be driven by generational inheritance of both political attitudes and schooling, which might have been even stronger among older cohorts (those born between 1930 and 1960) in Germany due to parents' experience of the Third Reich and World War II.

7 Robustness Checks

This section provides various 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 for each citizenship category.

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. I distinguish between respondents who are still 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 5 percent moved between the four states that introduced the reform law in the same year. Estimates of these restricted samples are reported in Panel A of Table 4. 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 law 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 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 actually increased compulsory schooling in 1959 rather than in 1958 when it was officially introduced, as indicated in Table 1, Appendix A. 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, probit and OLS 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 schools per 10,000 inhabitants[19], percentage of displaced persons (Vertriebene) in the

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

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

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 Table 4, Appendix A. 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]

World War II 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, casualties among family members, eviction of teachers, and demolition of infrastructure. To account for potential offsetting effects of the war when using compulsory laws as exogenous variation for schooling, Panel D in Table 4 presents evidence for Germans born between 1940 and 1960 only. With the exception of IV estimates for voter turnout, the results display similar patterns to previous estimates.[21]

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 survey years 1980-1988. Estimates are reported in Panel E of Table 4. Again, it is apparent that the results do not change the general conclusions.

Definition of Outcomes I also checked sensitivity of results to the definition of outcome

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

[21] Physical destruction by the war was more severe in cities than in the countryside (Ichino and Winter-Ebmer, 2004). As additional sensitivity analysis that World War II is no confounding factor in the present estimates, I also run regressions for those living currently in the countryside. Again, this does not change the results.

variables used. Five dichotomous outcomes were derived from categorical ALLBUS variables. For example, the variable interest in politics takes the value 1 if respondents report having a "strong" or "very strong" interest in politics, and zero otherwise.[22] I re-estimated the models using different definitions for these outcome variables, i.e., defining political outcome as one if respondents express the strongest expression of agreement only, and zero otherwise. Panel F shows the results for the outcome interest in politics. It is apparent that this leads to similar conclusions to the previous estimates.

Alternative Measures of Explanatory Variable Finally, I use two alternative measures of years of schooling. Estimates are based on the main sample extracted from ALLBUS. First, I estimated regressions using years of schooling as reported by 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, 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) in Table 5 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 provide little evidence of causality.[23]

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

[22] See Table 2, Appendix A for further categorical ALLBUS variables.

[23] 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 and OLS, suggesting higher political returns to education, but are not precisely estimated.

little evidence of causality of education on a broad range of political outcome measures.

8 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. However, there exists surprisingly little empirical evidence that more years of schooling has a true causal impact on citizenship.

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 and OLS 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. The results are robust to the choice of political outcome measure and to sample selection. Furthermore, estimates are robust to controlling for geographic mobility, potential historical influences of World War II, German reunification, and political, economic and social circumstances in the federal states around the time that compulsory schooling laws went into effect.

The finding of no causal impact may arise from the specifics of the German educational system or recent German history. Changes in compulsory schooling in Germany affected a large proportion of the population (Pischke and von Wachter, 2005), in contrast to the United States, where a smaller proportion of the population was affected (Oreolopolous, 2006). This implies that the political returns to schooling in Germany when estimating instrumental variable regressions are closer to average returns to education in the population than those obtained by

Dee (2004) and Milligan et al. (2004) for the United States. The effect of more schooling on democratic citizenship when a large part of the population is affected by an increase in compulsory schooling might be lower than when a more selected group of the population (with higher political returns to education) receives more schooling due to legislative changes. Another potential explanation for these differences are individual experiences during the Third Reich and World War II. The impact of these events on Germans born during or in the aftermath of the war may have been so severe that it outweighed any effect of years of schooling.

9 References

Acemoglu, D. and Angrist, J. D. (2000), 'How large are human capital externalities? Evidence from compulsory schooling laws', in: Bernanke, B. S. and Rogroff, K. (eds.) NBER Macroeconomics Annual 2000. Cambridge, MA.: MIT Press, 9-59.

Acemoglu, D., Johnson, S., Robinson, J. A. and Yared, P. (2005), 'From education to democracy?', *American Economic Review Papers and Proceedings*, 95(2), 44-49.

Almond, G. A. and Verba, S. (1963) The civic culture: Political attitudes and democracy in five nations. Princeton: Princeton University Press.

Amemiya, T. (1978), 'The estimation of a simultaneous equation generalized probit model', *Econometrica*, 46(5), 1193-1205.

Angrist, J. D. and Krueger, A. B. (1991), 'Does compulsory school attendance affect schooling and earnings?', *Quarterly Journal of Economics*, 106(4), 979-1014.

Angrist, J. D., Imbens, G. W. and Rubin, D. B. (1996), 'Identification of causal effects using instrumental variables', *Journal of the American Statistical Association*, 91(434), 444-455.

Barro, R. J. (1999), 'The determinants of democracy', *Journal of Political Economy*, 107(6), S158-S183.

Bernstein, R., Chadha, A. and Montjoy, R. (2001), 'Overreporting voting: Why it happens and why it matters', *Public Opinion Quarterly*, 65, 22-44.

Bound, J. D., Jaeger, D. A. and Baker, R. M. (1995), 'Problems with instrumental variables estimation when the correlation between the instrument and the endogenous explanatory variable is weak', *Journal of the American Statistical Association*, 90(430), 443-450.

Burkhauser, R. V., Kreyenfeld, M. and Wagner, G. G. (1997), 'The German Socio-Economic Panel: A representative sample of reunited Germany and its parts', *Vierteljahreshefte zur Wirtschaftsforschung*, 66(1), 7-16.

Card, D. (2001), 'Estimating the return to schooling: Progress on some persistent econometric problems', *Econometrica*, 69(5), 1127-1160.

Corbett, M. (1982), Political tolerance in America: freedom and equality in public attitudes. New York: Longman.

Dee, T. S. (2004), 'Are there civic returns to education?', *Journal of Public Economics*, 88, 1697-1720.

Del Bono, E. and Galindo-Rueda, F. (2006), 'The long term impacts of compulsory schooling: Evidence from a natural experiment in school leaving dates', ISER working paper 2006-44, Colchester: University of Essex.

Dewey, J. (1916), Democracy and education: an introduction to the philosophy of education. New York: The Macmillan Company.

Dustmann, C. (2004), 'Parental background, secondary school track choice, and wages', *Oxford Economic Papers*, 56, 209-230.

Franklin, M. N. (1996), Electoral participation, in: LeDuc, L., Niemi, R. G., Norries, P. (eds.) *Comparing democracies: Elections and voting in global perspectives.* CA: Sage Publications, 216-235.

Glaeser, E. L., LaPorta, R., Lopez-de-Silanes, F. and Shleifer, A. (2004), 'Do institutions cause growth', *Journal of Economic Growth*, 9(3), 271-303.

Haisken-DeNew, J. P. and Frick, J. R. (2005), 'Desktop companion to the German Socio-

Economic Panel (SOEP)', Berlin: DIW Berlin.

Harmon, C. and Walker, I. (1995), 'Estimates of the economic return to schooling for the United Kingdom', *American Economic Review*, 85(5), 1278-1286.

Hochstadt, S. (1999), Mobility and modernity: Migration in Germany 1820-1989. Ann Arbor: University of Michigan Press.

Holmes, B. (1979), International guide to education systems. Paris: Unesco.

Ichino, A. and Winter-Ebmer, R. (2004), 'The long-run educational cost of World War Two', *Journal of Labor Economics*, 22(1), 57-86.

Jackman, M. R. (1978), 'General and applied tolerance: does education increase commitment to racial integration?', *American Journal of Political Science*, 22(2), 302-324.

Lipset, S. M. (1976), Political man. London: Heinemann.

Lochner, L. and Moretti, E. (2004), 'The effect of education on crime: Evidence from prison inmates, arrests, and self-reports', *American Economic Review*, 94(1), 155-189.

Milligan, K., Moretti, E. and Oreopoulos, P. (2004), 'Does education improve citizenship? Evidence from the United States and the United Kingdom', *Journal of Public Economics*, 88, 1667-1695.

Newey, W. K. (1987), 'Efficient estimation of limited dependent variable models with endogenous explanatory variables', Journal of Econometrics, 36(3), 231-250.

Nie, N. H., Junn, J., and Stehlik-Barry, K. (1996), Education and democratic citizenship in America. Chicago: University Chicage Press.

Nunn, C. Z., Crockett, H. J., and Williams, J. A. (1978), Tolerance for nonconformity. San

Francisco: Jossey-Bass Publishers.

Oreopoulos, P. (2006), 'Estimating average and local average treatment effects of education when compulsory schooling laws really matter', *American Economic Review*, 96(1), 152-175.

Pischke, J.-S. and von Wachter, T. (2005), 'Zero returns to compulsory schooling in Germany: Evidence and interpretation', IZA discussion paper no. 1645, Bonn: IZA.

Pischke, J.-S. (2006), 'The impact of length of the school year on student performance and earnings: Evidence from the German short school years', London: London School of Economics.

Przeworski, A., Alvarez, M., Cheibub, J. A. and Limongi, F. (2000), Democracy and development: Political institutions and well-being in the world, 1950-1990. Cambridge: Cambridge University Press.

Rainer, H. and Siedler, T. (2005), 'O Brother, where art thou? The effects of having a sibling on geographic mobility and labor market outcomes', ISER working paper 2005-18. Colchester: University of Essex.

Staiger, D. and Stock, J. H. (1997), 'Instrumental variables regression with weak instruments', *Econometrica*, 65(3), 557-586.

Statistisches Bundesamt (2006), Wahlbeteiligung nach Geschlecht und Altersgruppen seit 1983. ⟨http://www.destatis.de⟩.

Touya, D. M. (2006), 'Can we teach civic attitudes?', DT 06-03, Vigo: Universidade de Vigo.

Van Buuren, S., Boshuizen, H. C. and Knook, D. L. (1999), 'Multiple imputation of missing blood pressure covariates in survival analysis', *Statistics in Medicine*, 18, 681-694.

Winkelmann, R. (1996), 'Employment prospects and skill acquisition of apprenticeship –

trained workers in Germany', *Industrial and Labor Relations Review*, 49(4), 658-672.

Table 1: The Impact of Schooling on Citizenship (Main Sample)

Outcomes	Probit and OLS	IV	Average effect of reform	First-stage F statistic	N
	(1)	(2)	(3)	(4)	(5)
Political awareness					
Interest in politics	0.041***	-0.096***	-0.060***	*82.84*	*14,007*
	(0.002)	(0.021)	(0.015)		
Importance of politics and public life	0.014***	-0.070*	-0.040**	*74.39*	*7,775*
	(0.003)	(0.027)	(0.016)		
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 citizen group	0.002***	-0.003	-0.001	*91.06*	*10,973*
	(0.000)	(0.008)	(0.003)		
Member of political party	0.004***	-0.021	-0.009	*95.25*	*16,005*
	(0.001)	(0.016)	(0.005)		
Political Scales					
Political awareness	0.058***	-0.187**	-0.107***	*60.65*	*7,015*
	(0.006)	(0.065)	(0.032)		
Democratic values	0.043***	-0.067	-0.033	*12.14*	*2,672*
	(0.010)	(0.097)	(0.046)		

Continued on next page

Table 1 – continued from previous page

Outcomes	Probit and OLS	IV	Average effect of reform	First-stage F statistic	N
	(1)	(2)	(3)	(4)	(5)
Political involvement	0.116***	-0.082	-0.03	*13.19*	*4,679*
	(0.008)	(0.143)	(0.049)		
Political membership	0.009***	-0.023	-0.009	*91.06*	*10,973*
	(0.002)	(0.015)	(0.005)		

Notes: 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. Results are marginal effects from IV probit estimations for dichotomous political outcomes and 2SLS estimates for political scales. Standard errors corrected for state-year of birth clustering are shown in parentheses. ***, **, * indicate significance at 1-, 5-, and 10-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 Sample)

	Individuals with basic or middle track schooling[a]		Father has basic school certificate or less		Father with occupational prestige score below median	
	Probit and OLS	IV	Probit and OLS	IV	Probit and OLS	IV
Political awareness						
Interest in politics	0.029***	-0.185***	0.037***	-0.117***	0.046**	-0.096***
	(0.003)	(0.032)	(0.002)	(0.024)	(0.004)	(0.042)
	N = 11,818		*N = 10,551*		*N = 5,603*	
Participation in voting						
Voted in last general election	0.010***	-0.013	0.007***	-0.004	0.012***	-0.028
	(0.002)	(0.034)	(0.002)	(0.020)	(0.003)	(0.047)
	N = 8,560		*N = 7,768*		*N = 4,089*	
Democratic values						
Demonstrating is civil right	0.020*	0.026	0.013	0.008	0.030	-0.028
	(0.011)	(0.091)	(0.009)	(0.064)	(0.016)	(0.300)
	N = 2,464		*N = 2,177*		*N = 1,174*	
Political involvement						
Signed a petition	0.042***	-0.175***	0.041***	-0.097*	0.039***	0.073
	(0.005)	(0.071)	(0.005)	(0.050)	(0.007)	(0.089)
	N = 4,198		*N = 3,784*		*N = 2,065*	
Political membership						
Member of political party	0.004***	-0.046	0.003***	-0.001	0.002***	-0.001
	(0.001)	(0.032)	(0.001)	(0.013)	(0.001)	(0.009)
	N = 13,666		*N = 12,187*		*N = 6,492*	
Political scale						
Political membership	0.008***	-0.046	0.006***	-0.006	0.008***	-0.025
	(0.002)	(0.025)	(0.002)	(0.016)	(0.002)	(0.027)
	N = 9,667		*N = 8,377*		*N = 4,604*	

Notes: [a] Individuals who successfully finished Hauptschule or Realschule. 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. Results are marginal effects from IV probit estimations for dichotomous political outcomes and 2SLS estimates for political scales. Standard errors corrected for state-year of birth clustering are shown in parentheses. ***, **, * indicate significance at 1-, 5-, and 10-percent level, respectively.

Table 3: Intergenerational Relationship in Political Interest (Dependent variable: Interested in politics)

Key explanatory variables	(1)	(2)	(3)	(4)
Years of schooling	0.062***	0.050***	0.047***	0.047***
	(0.006)	(0.006)	(0.006)	(0.006)
Mother interested in politics		0.134***	0.127***	0.125***
		(0.028)	(0.028)	(0.028)
Father interested in politics		0.103***	0.099***	0.098***
		(0.022)	(0.022)	(0.022)
Dummies for mother's and father's schooling degree[a]			yes	yes
Dummies for federal states				yes
Number of observations		*1,860*		

Notes: Estimates are marginal effects from probit regressions using SOEP wave 21 (year 2004). The dependent variable equals one if adult children answer that they are "very much" or "much" interested in politics, and zero otherwise. Covariates capturing parents' interest in politics are defined accordingly. Regressions also include a dummy for female, age, age-squared and parents' age. Standard errors allowing for clustering at mother's identification number are in parentheses. Sample comprises adult children aged 17-54, living in West Germany with parents who are also SOEP respondents. [a] 4 groups (secondary general school certificate, intermediate school certificate, grammar school certificate, no or other school certificate). N denotes number of individuals. ***, **, * indicate significance at 1-, 5-, and 10-percent level, respectively.

Table 4: Robustness Checks

Outcomes	Interest in politics		Voted in last general election		Member of political party		Political membership scale	
	Probit	IV	Probit	IV	Probit	IV	OLS	IV
Panel A (Geographic mobility)								
Individuals with valid	0.046***	-0.113***	0.016***	0.022	0.007***	0.003	0.022***	-0.014
state of birth information	(0.003)	(0.030)	(0.002)	(0.035)	(0.001)	(0.024)	(0.005)	(0.047)
	N = 4,745		*N = 2,814*		*N = 4,737*		*N = 1,359*	
Non-movers[a]	0.045***	-0.112***	0.014***	0.041	0.008***	-0.008	0.008***	-0.024
	0.004	(0.031)	(0.003)	(0.039)	(0.002)	(0.024)	(0.002)	(0.015)
	N = 4,206		*N = 2,479*		*N = 4,136*		*N = 8,377*	
Panel B (Timing of reform)								
Individuals affected by law change								
in 2 years before and after	0.041***	-0.098***	0.008***	0.003	0.004***	-0.023	0.009***	-0.024
introduction are excluded	(0.002)	(0.020)	(0.001)	(0.019)	(0.001)	(0.017)	(0.002)	(0.015)
	N = 13,725		*N = 9,885*		*N = 15,185*		*N = 10,973*	
Panel C (Environment controls)								
Controls for social, political and								
economic federal state characteristics	0.048***	-0.107**	0.017***	0.014	0.007***	0.004	0.023***	-0.065
during 1950s	-0.002	(0.045)	(0.003)	(0.108)	(0.002)	-0.065	(0.004)	(0.033)
	N = 3,872		*N = 2,298*		*N = 3,868*		*N = 1,240*	
Panel D (World War II)								
Individuals born between 1940-1960	0.042***	-0.118***	0.010***	0.046	0.004***	-0.022	0.010***	-0.036
	-0.002	-0.032	(0.002)	(0.213)	(0.001)	(0.018)	(0.002)	(0.029)
	N = 9,856		*N = 7,128*		*N = 11,248*		*N = 7,686*	
Panel E (German reunification)								
Outcomes measured during	0.031***	-0.062	0.005***	-0.033	0.001	-0.027	0.002*	-0.012
survey years 1980 1988	(0.004)	(0.055)	(0.002)	(0.063)	-0.001	-0.031	(0.001)	(0.020)
	N = 4,879		*N = 3,989*		*N =6,882*		*N = 6,882*	
Panel F (Alternative definitions)								
Alternative definition of political outcome	0.013***	-0.053**						
	-0.001	(0.018)						
	N = 14,007							

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. Results are marginal effects from IV probit estimations for dichotomous political outcomes and 2SLS estimates for political scales. Standard errors corrected for state-year of birth clustering are shown in parentheses. ***, **, * indicate significance at 1-, 5-, and 10-percent level, respectively.

Table 5: Alternative Measures of Years of Schooling (ALLBUS)

	Years of schooling observed in surveys 1990-1992 (1)		Generated variable years of schooling[b] (2)	
	Probit and OLS	IV	Probit and OLS	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*	
Democratic values				
Demonstrating is civil right[b]			0.035***	-0.014
			(0.007)	(0.058)
			F = 41.15	
			N = 2,683	
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 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*	

Political scale				
Political membership	0.013***	0.021	0.015***	-0.016
	(0.003)	(0.058)	(0.002)	(0.010)
	F = 5.57		*F = 251.14*	
	N = 2,736		*N = 10,973*	

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 are estimated by ordinary probit, OLS and instrumental variable (IV) and 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. Results are marginal effects from IV probit estimations for dichotomous political outcomes and 2SLS estimates for political scales. Standard errors corrected for state-year of birth clustering are shown in parentheses. ***, **, * indicate significance at 1-, 5-, and 10-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.

[b] Information about democratic values is only available in survey years 1982 and 1988.

10 Appendix A

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)

Table 2: Definition of Outcome Variables

Political Outcome	Question in Allbus reads:	Variable in Allbus	Years information is available	Definition of political outcome variable
Political awareness				
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 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.
Participation in voting				
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.
Democratic values				
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.
Political involvement				
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.
Political group membership				
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 citizen's action group, and is zero otherwise.
Member of a political party	"Are you currently a member of a political party?"		Biennial from 1980-1990, 1991, biennial from 1992-2004.	Variable equals one if a respondent said "yes", and zero if answered with "no".
Political scales				
Political awareness	Scale is the sum of the two outcome variables in the category political awareness and ranges from 0 to 2.			
Democratic values	Scale is the sum of the two outcome variables in the category democratic values and ranges from 0 to 2.			
Political involvement	Scale is the sum of the two outcome variables in the category political involvement and ranges from 0 to 3.			
Political group membership	Scale is the sum of the two outcome variables in the category political group membership and ranges from 0 to 2.			

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
Outcomes			
Political awareness			
Interest in politics	0.304	0.289	14,007
Importance of politics and public life	0.268	0.240	7,775
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 citizen group	0.010	0.011	10,973
Member of political party	0.050	0.038	16,005
Political scales			
Political awareness	0.568	0.514	7,015
Democratic values	1.452	1.585	2,672
Political involvement	0.666	0.880	4,679
Political membership	0.050	0.040	10,973
Covariates			
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	8.54	9.63	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

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.

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 were not available. Sources: Regional statistical offices of federal states and Statistisches Jahrbuch fr die Bundesrepublik Deutschland 1954.

11 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 rational 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

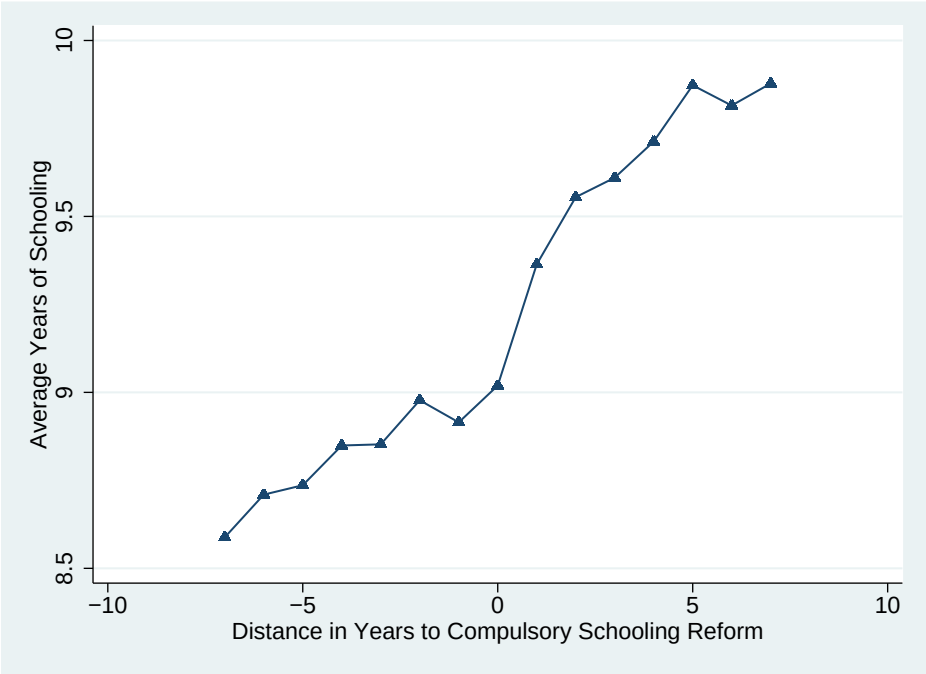

Table 1: Distribution of Years of Schooling before and after Compulsory Schooling Reform

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	Interest in politics	Voted in last general election	Demon -strating is civil right	Signed a petition	Party member-ship	Political membership scale
Age	0.087***	0.075***	0.229***	0.066*	0.125***	0.153***
	(0.021)	(0.015)	(0.057)	(0.034)	(0.019)	(0.023)
Age-squared	-0.001***	-0.001**	-0.002***	-0.001	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Year of birth	0.032	0.094***	0.047	0.085***	0.062***	0.080***
	(0.019)	(0.008)	(0.046)	(0.046)	(0.017)	(0.018)
Female	-0.146***	-0.149***	-0.028***	-0.132**	-0.139***	-0.090***
	(0.032)	(0.042)	(0.047)	(0.055)	(0.017)	(0.032)
Dummy for Compulsory Schooling Reform	0.538***	0.522***	0.493***	0.392***	0.520***	0.550***
	(0.074)	(0.085)	(0.093)	(0.117)	(0.067)	(0.073)
Partial-R^2	0.006	0.004	0.008	0.003	0.005	0.007
Sample size	*14,007*	*10,100*	*2,683*	*4,929*	*16,005*	*10,973*

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-, 5-, and 10-percent level, respectively.

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

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-, 5-, and 10-percent level, respectively.