Revisiting the Causal Effect of Education
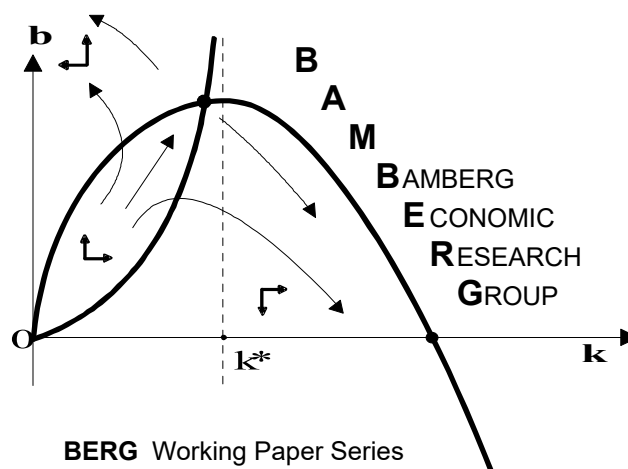# on Political Participation and Interest

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Revisiting the Causal Effect of Education on Political Participation and Interest

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Abstract

A substantial number of studies suggests a strong relationship between education and aspects of political participation and interest. Only a small body of literature, however, addresses whether these patterns represent causal effects. We add to this research and re-examine the question in the German context. For identification, we exploit an exogenous increase in lower secondary compulsory schooling between 1949 and 1969 in former West Germany, and use data from the National Educational Panel Study (NEPS) to identify individuals' educational biographies more precisely than prior research. Our results reinforce findings from Siedler (2010): multiple regression analyses first indicate a positive, statistically significant correlation between schooling and our measures of political activities. IV estimates, however, are all trivial, for both compliers and the full sample, indicating that the reform did not stimulate long-term changes in political participation and interest.

Keywords: school reform, political participation, IV estimation, returns to education, Germany

JEL-Classification: I2, H4, H23

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1. Introduction

Education is presumed to be an important -if not the most important- factor for individuals' success in life. It is a consistent predictor for outcomes in many domains, like labor market placement, occupational status, earnings or working conditions, but also beyond the labor market, affecting health, well-being, and social and political participation. The latter corresponds to what van de Werfhorst (2014) claims to be one of the functions of education[1], inasmuch as it aims at enabling individuals' civic participation. Gutmann (1987) also considers the development of "democratic habits" (Levinson 1999) as a central objective of the educational systems and its institutions.

From a theoretical perspective, education may indeed influence political participation through several causal mechanisms. Economic theories argue that education encourages the development of civic skills and knowledge, which in turn facilitate civic engagement by reducing its costs and providing strategies to participate in an effective way (Rosenstone & Hansen, 1993; Verba, Schlozman, & Brady, 1995). Education also improves individuals' capacity to gather and process politically relevant information (Delli Caprini & Keeter, 1996). Social capital theories furthermore suggest that the involvement of the well-educated in politically oriented networks reinforces these individual level effects (Nie, Junn, & Stehlik-Barry, 1996; Verba et al., 1995). Shared social norms and values may also have an impact on individuals' levels of political interest and their willingness to participate actively.

However, although theoretical insights suggest causality, there is an ongoing debate whether the patterns found in the literature reflect causal effects. The relationship between education and political behavior and attitudes is well established in the political science literature (Burden, 2009; Chevalier & Doyle, 2012; Denny & Doyle, 2008; Grönlund & Milner, 2006; Hadjar & Becker, 2006; Hauser, 2000; Hillygus, 2005), and Sondheimer and Green (2010) interpret these results as having "law-like regularity" (p.174) in the US context. They, however, also stress that there are at least two major reasons why scholars should be sceptical about the causality of this relationship on the micro level, and that there is a need to employ experimental or quasi-experimental designs.

First, schooling may be endogenous: unobserved ability or other unknown, unobservable factors may drive both educational attainment and political interest and participation. Individuals' socialization is also relevant in this context: better educated children are more likely to have a family background with more highly educated parents. Such parents are more likely to directly or indirectly expose their children to politics early on through, for example, their critical consumption of news, or their reflections and discussions on political actions. This essentially translates into children from these families growing up to have both higher awareness of politics and better skills in dealing with political information.

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[1] In addition, van de Werfhorst (2014) argues that education aims at preparing individuals for challenges on the labor market, ensuring equal opportunities with respect to the access to education, and sorting individuals into educational tracks according to their interests and talents to ensure optimal production of knowledge and skills.

The second concern of Sondheimer and Green (2010) is that the educational expansion and the related increase in average years of schooling that occurred in recent decades does not automatically translate into a similar increase in political knowledge, awareness and concern with politics (Delli Caprini & Keeter, 1996). This implies that political participation may not necessarily be increased by education if these causal pathways are not also positively affected. Additionally, Nie et al. (1996) argue that education is a sorting device differentiating by social status. Hence, "[i]f education functions as a marker of one's relative status, it is the status-associated costs and benefits of political participation that encourage those at the upper end of the distribution to participate and discourage those at the lower end." (Sondheimer & Green, 2010, p. 176)[2]

A growing body of literature addresses these concerns and aims at identifying causal effects by using experiments, quasi experiments or other appropriate empirical methods. However, there is as yet no consensus regarding findings overall. The literature shows rather mixed results, and almost exclusively looks at Anglo-American or developing countries. Because contexts are relevant for individuals' political involvement, the corresponding results may simply not be transferable to other countries with other political systems and cultural backgrounds.

We add to this literature by examining the German case for which, to the best of our knowledge, there exists only a study by Siedler (2010), which we outline in section 2 on previous research. We are able to address data-driven shortcomings of his study, as we have precise information on individuals' educational biographies that does not force us into strong assumptions about whether individuals were affected by the schooling reforms. Our results, nevertheless reinforce his findings inasmuch as our estimates do not imply a long-term effect of the exogenous increase in schooling on individuals' political participation and interest in Germany.

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[2] Two more technical concerns are: (1) attenuation bias that results from potential measurement errors in years of education, which may distort the estimates to zero, and (2) social desirability in the interview. If more highly educated individuals are more likely to give –from their perspective– the most appropriate answer, irrespective of actual behavior or underlying attitudes, the relationship between education and political participation may be overestimated (Bernstein, Chadha, and Montjoy, 2001).

2. Theoretical Background and Prior Research

Theoretical Background

The relationship between education and individuals' political participation is widely discussed in different disciplines, including not only political science, but also sociology and economics. A synthesis of theoretical approaches from these disciplines is provided by Verba et al. (1995). In short, the authors identify three factors as prerequisites for political participation. These are the availability of resources, psychological dispositions like motivation, norms and values, and involvement in recruiting social networks, discussed within the *absolute* and the *relative education model*, respectively.

The *absolute education model* posits that education has a direct effect on political participation. Hence, education has an influence on different types of skills and knowledge, which reduce the costs of political actions, enable citizens to participate in an effective way, and therefore, facilitate political behavior. Education fosters the development of cognitive and civic skills, as well as the capacity to gather and process politically relevant information. This is important for the individual's understanding of the sometimes quite abstract contents of the political discourse, and for their ability to stay informed about campaigns and political officials (Delli Caprini & Keeter, 1996). However, schooling is not only important for the formation of skills, but also for the provision of factual knowledge about the respective political system, its institutions and its mode of operation (Brade & Piopiunik, 2016; Persson, 2015). Such knowledge is needed for the sound evaluation of political issues. However, while education reduces the participation costs via knowledge and skills, individuals' increasing opportunity costs may work against increased political participation (Dee, 2004): opportunity costs of time increase with education because of better labor market options, so that the highly educated may be less likely to be active in political contexts. Dee (2004, p. 1700) further notes 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."

Contrary to the absolute education model, the *relative education model* interprets education as a positional good, which is only valuable for those possessing it if others do not. Social capital and social networks are particularly relevant in this context. According to Granovetter (1973) and Lin (1999), social capital is the accessibility of resources through social networks used to achieve different goals. The educational system is key for individuals' network formation and its extension. Individuals are likely to connect with people who are similar to themselves, and schools provide such opportunities. Education may, however, not only have an influence on the composition of someone's peer group. It may also convey democratic, pluralistic, and other political values (Dee, 2004), or interest in political issues in general (Hadjar & Becker, 2006), which fosters the willingness to engage directly. Network composition and structure is, in addition, not only important for the availability of information, or for shared opinions, values and norms, but can also affect or motivate behavior (Klandermans & Oegema, 1987; McPherson, Smith-Lovin, & Cook, 2001). Franklin (2004) introduces group pressure as a

mechanism, arguing that, for instance, the benefits of voting (or the costs of non-voting) are higher for socially connected people because their network members care about whether they vote or not.

Prior Research

There is extensive research on the association between education and political participation and interest, but as yet there exists little literature that employs identification strategies to examine causal effects. First, studies typically suggest a strong association between educational attainment and different types of political knowledge, interest, and other relevant aspects (de Rijke, 2009; Hoskins, D'Hombres, and Campbell, 2008; Hadjar and Becker, 2006; Denny and Doyle, 2008; Hauser, 2000; Hillygus, 2005; Chevalier and Doyle, 2012; Burden, 2009 or Grönlund and Milner, 2006). Going a step further, Highton (2009) notes the shortcomings of cross-sectional associations and uses panel data to address some of these concerns by accounting for individuals cognitive ability and looking at long-term changes.

Regarding studies that use appropriate identification strategies, Sondheimer and Green (2010) exploit exogenous variation in educational achievement from the intervention programmes, Perry Preschool, I Have A Dream (IHAD), and Student Teacher Achievement Ratio (STAR). Their findings suggest that the exogenously induced changes in high school graduation rates have a substantial causal effect on US voter turnout in the long term.

Also for the US, Dee (2004) uses the variation in the availability of junior and community colleges, as well as exogenous changes in the exposure of teens to child labor laws as instrumental variables. He finds positive causal effects of college attendance on voter participation, support of free speech and the frequency with which individuals read newspapers, as proxy for quality of civic knowledge. Using changes in compulsory schooling as instrument, Milligan, Moretti, and Oreopoulos (2004) find a positive effect of education on voting for the US, but not for the UK. They suspect that registration rules act as a barrier to going to the polls. Borgonovi, d'Hombres, and Hoskins (2010) do not find a causal relationship between years of schooling and voter turnout when employing compulsory schooling laws as instruments. They do find a positive effect when it comes to individuals' capacity to gather information on political issues, however.

For Germany, we are aware of only one paper that employs an identification strategy. Siedler (2010) exploits changes in compulsory schooling laws after World War II in Former West Germany, and examines whether schooling has a causal impact on different kinds of political behaviour including voting, sharing democratic values, or being involved in political actions, such as signing petitions or participating in (legally approved) demonstrations. He uses repeated cross-sectional data from the German General Social Survey (ALLBUS) and ForsaBus, a program surveying public attitudes on a number of different political and social issues. His estimates show that years of schooling and a number of political outcomes correlate positively, but there is little evidence of a causal effect. The results are, however, not fully convincing, because he uses data with several shortcomings. It lacks crucial variables

such as individuals' school leaving date or the federal state respondents lived in when going to school, which is essential for identifying whether individuals were really affected by the reform. We therefore add to the evidence on Germany by replicating Siedler's approach, but use data that is better suited to the analysis of the research question.

3. The West German educational system and the compulsory schooling reforms

Before elaborating on the data, we sketch the schooling system in former West Germany after World War II. The West German school system was -and still is- characterized by the sovereignty of the federal states, meaning that the states are responsible for the funding, content and structure of schooling. The main features of the educational system (Figure 1), however, were in general comparable across the federal states at that time and to a large extent still are today:

Figure 1: West German educational system (stylized)
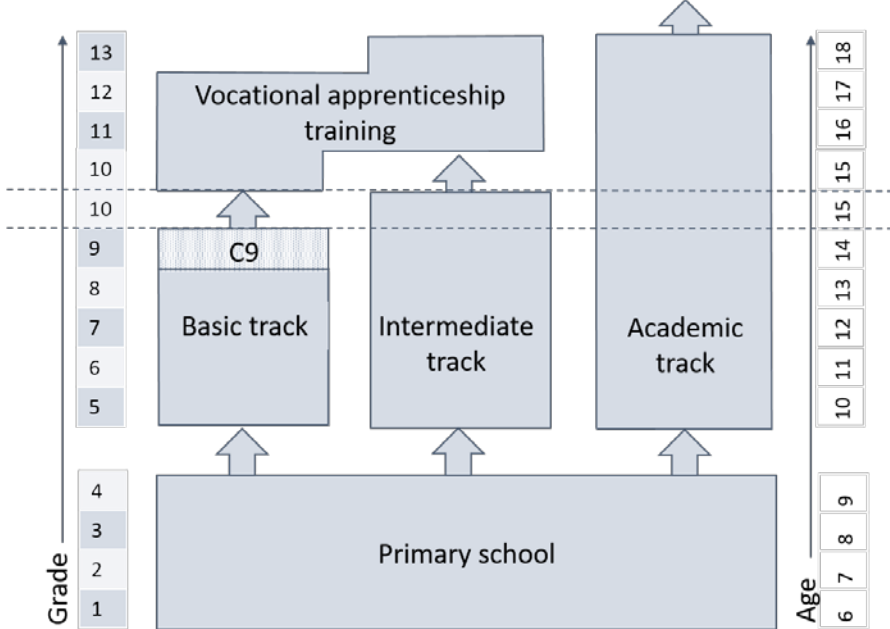
Source: Sekretariat der Ständigen Konferenz der Kultusminister der Länder in der Bundesrepublik Deutschland (2017), illustration by Susanne Elsas.

Children start compulsory schooling at around the age of six. From grade one to four, i.e. from age six to ten, primary school provides training in basic reading, writing, and mathematical skills. Tracking into secondary schooling occurs for most children at about age ten. Children are tracked in three different types of secondary schools: lower secondary school (basic track), intermediate secondary school (intermediate track), and upper secondary school (academic track). The tracks differ in duration and cognitive requirements, and prepare students for different educational and vocational paths. At the time of the reform, most students attended lower or intermediate secondary schools, whereas higher secondary schools became more common in the course of the educational expansion in the 1970s.

Prior to World War II, compulsory schooling ended after eight years in most federal states. After the war, between 1949 and 1969, two major reforms took place: the extension of compulsory schooling from eight to nine years (C9), and the shift of the beginning of the school year from spring to autumn. For the extension of compulsory schooling, Table 1 provides an overview of the variation of its implementation over time and federal state:

Table 1: Implementation of the 9th compulsory school year

Federal State	School year	Pischke & Wachter (2005, 2008)
Bremen	Before 1949/50	1958
Hamburg	Before 1949/50	1949
West Berlin	Before 1949/50	-
Schleswig-Holstein	Before 1949/50	1956
Lower Saxony*	1962/63	1962
North Rhine-Westphalia	1966	1967
Hesse** (urban municipalities)	1966 (1962/63)	1967
Rhineland-Palatinate	1966	1967
Baden-Wuerttemberg	1965/66	1967
Bavaria	1968/69	1969
Saarland***	1966	1964

Source: Helbig & Nikolai (2015); Cygan-Rehm (2018); Cygan-Rehm & Maeder (2013).

* In Lower Saxony the reform was implemented successively between 1954/55 and 1962/63.
** In Hesse the reform was first implemented in urban municipalities from 1962/63 on.
*** In Saarland the reform was implemented successively between 1958/59 and 1966.

The first study in economics that exploited the extension of compulsory schooling in Germany, which has been widely used as source of information on the reform, is that of Pischke and Wachter (2008). Only recently, in 2015, did another source become available, provided by Helbig and Nikolai (2015). The authors made a great effort to even better document the changes in the educational system in the different federal states of Germany. The two sources are similar, with some smaller, but relevant differences between them. We use Helbig and Nikolai's (2015) scheme[3] because we consider their sources to be more reliable, but we also check for differences in results when using Pischke and Wachter's (2008) scheme as a robustness check.

Table 1 shows that, generally, the northern federal states were early reformers, whereas the southern federal states implemented the reform later. The first federal states to introduce the reform were

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[3] We, however, depart from Helbig and Nikolai (2015) in one case. For Bavaria, our data strongly suggests that the reform was implemented in the school year 1968/69 (see Appendix – Figure A1), yet Pischke and Wachter (2008), as well as Cygan-Rehm (2018) use the same timing for the reform in Bavaria.

Hamburg, Bremen and Schleswig-Holstein. The last state to introduce the compulsory 9th grade was Bavaria.

There are several specifics of the reform. First, because of a lack of teachers, some federal states implemented a transitional arrangement (Helbig & Nikolai, 2015). In Hesse, such an arrangement was set up in which urban municipalities implemented the reform earlier than the rest of Hesse. In contrast to prior research, we are able to identify who went to school in urban municipalities in Hesse, where the extension to nine years of compulsory schooling was already introduced in 1962/63. For both Saarland and Lower Saxony, gradual implementation processes took place, and there is a lack of institutional information on the exact timing of the implementation[4]. We address this by using the time at which all individuals were affected by the reform, 1962/63 for Lower Saxony, and 1966 for Saarland.

Compliance to the reform is another relevant issue. Students of lower secondary schools were targets of the reform, making them continue school for one additional school year. This may also have affected their subsequent educational or even occupational choice, because the relative cost of achieving intermediate secondary schooling decreased, as they had to invest only one additional year compared to two years before the reform was implemented. In reaction to the reform, students sorted differently into the lower and intermediate track, as shown by Cygan-Rehm (2018). Students in higher secondary schooling, on the other hand, were not affected directly, because they were anyway schooled for a longer time (see also Pischke & Wachter, 2008; Cygan-Rehm, 2018). We therefore employ lower and intermediate secondary school students as compliers of the reform, and we conduct all analyses for both the full sample and for the subsample of compliers.

In addition to the compulsory schooling reform, a second major reform occurred in 1966/1967. All federal states except for Bavaria[5] moved the start of the school year from spring to autumn (Helbig & Nikolai, 2015). To implement this reform, the majority of West-German federal states (Baden-Württemberg, Bremen, Hesse, Lower Saxony, North Rhine-Westphalia, Rhineland-Palatinate, Saarland and Schleswig Holstein) used two short school years, from April to November 1966 and from December 1966 to June 1967. West Berlin and Hamburg, however, implemented one long school year, from April 1966 to August 1967. Pischke (2007) exploited this reform and showed that the shortened school years increased the risk of repeating classes. Because the two reforms coincide in Rhineland Palatinate, rural Hesse, Saarland and North-Rhine Westphalia, the effect of the compulsory schooling reform risks being underestimated if we do not consider the potential effects of the parallel reform. We address this in our robustness checks by excluding individuals who repeated at least one class.

The reform(s) initiated exogenous variation in education over time and federal states, and we exploit this variation as an instrument for years of schooling in our analyses. In contrast to Pischke and Wachter

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[4] We exclude observations from Lower Saxony and Saarland but utilize them as a robustness check in Section 6.
[5] In Bavaria, the school year already started in autumn.

(2005; 2008), Siedler (2010), and Cygan-Rehm (2018), the data we use provides detailed information about individuals' educational biographies. Not only do we have start and end dates of schooling episodes, but we also have information about both the states and municipalities where individuals went to school, and the school track individuals attended when they were in grade 8. We can thus identify the exogenously induced change in schooling, and whether and to what degree the individual was affected by the reform more precisely than previous studies.

4. Data and Empirical Strategy

Data

We use data from the National Educational Panel Study (NEPS)[6], which provides detailed information about adults' educational biographies, their vocational training, (un-)employment episodes, and a rich set of sociodemographic variables. The most comprehensive information on individuals' political behavior and interest is available for the survey year 2013/14.

The central outcome variables on individuals' political participation and interest used in the following analyses are:

- Political actions: having ever signed a petition or having ever participated in a legally approved demonstration (0/1)
- Voting: having voted in the last general election (0/1)[7]
- Political interest: extent of interest in political issues (4-point Likert type scale)
- Internal political efficacy[8]: individuals' perception of (not) being able to understand politics (5-point Likert type scale)

In addition to the binary indicators (having signed a petition, having participated in a demonstration, having voted), responses to individuals' political interest range from (1) 'not at all interested' in political issues to (4) 'very interested' on a 4-point Likert type scale. Internal political efficacy is measured as the frequency with which individuals have difficulties in following current political debates on a 5-point
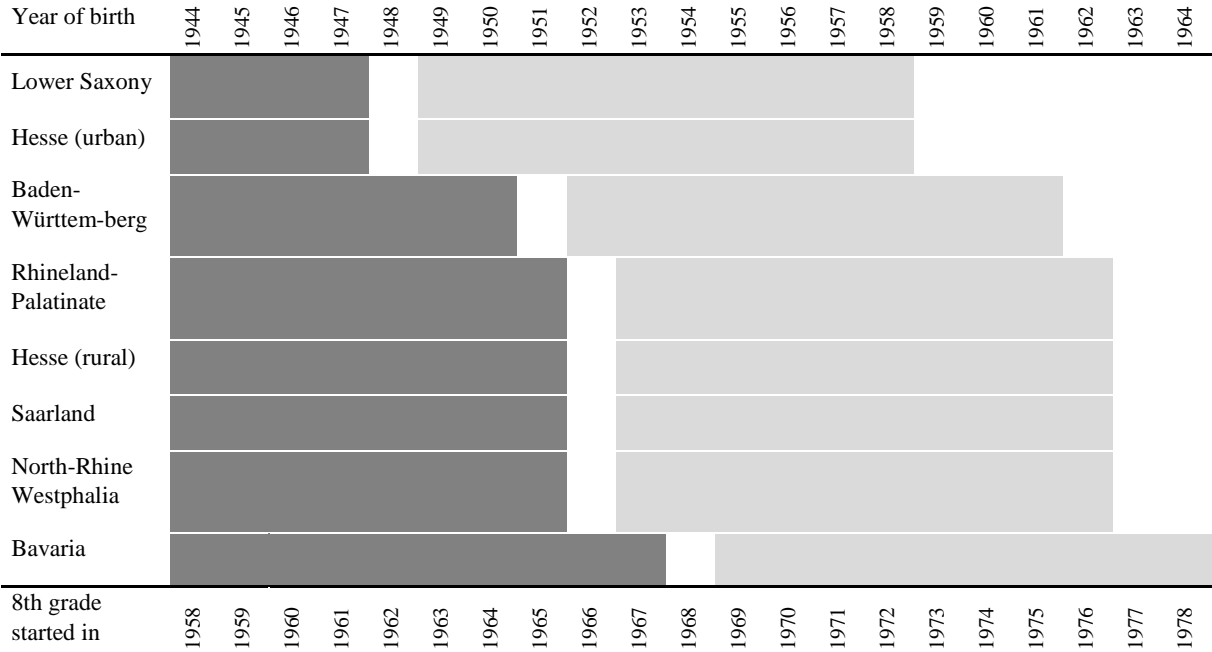
[7] Responses to this question refer to two elections. The 2013 election to the German the federal parliament, the Bundestag, took place on September 22, 2013. NEPS interviews were conducted before and after that date. Respondents interviewed before the election thus had to refer to the prior election to the Bundestag in 2009, whereas respondents interviewed after the September 22 referred to the 2013 election. We account for this by including an additional control indicating whether the interview was before or after the 2013 election. Because we are interested in voting in general, and control for further individual characteristics, as well as time and regional trends, the two different reference points are not expected to pose a significant problem.
[8] Political efficacy can be defined as "both the belief that the potential voter can influence what the government does (external efficacy), and the belief that the potential voter has the competence to understand and participate in politics (internal efficacy)" (Jackson 1995, p. 280).

Likert type scale: respondents who confirm often having such difficulties are considered as having low internal political efficacy.

As control variables, we use the gender of the respondents, age and age-squared to account for non-linear age effects, birth cohort, the federal state in which they went to school in grade 8, an interaction effect between birth cohort and federal state, and an interaction term between birth cohort and age. This approach corresponds to prior studies that used the reform for analyses of different outcomes (see e.g. Pischke & Wachter, 2008; Kemptner, Jürges, & Reinhold, 2011).

Table 2: NEPS sample cut

Year of birth	1944	1945	1946	1947	1948	1949	1950	1951	1952	1953	1954	1955	1956	1957	1958	1959	1960	1961	1962	1963	1964
Lower Saxony																					
Hesse (urban)																					
Baden-Württem-berg																					
Rhineland-Palatinate																					
Hesse (rural)																					
Saarland																					
North-Rhine Westphalia																					
Bavaria																					
8th grade started in	1958	1959	1960	1961	1962	1963	1964	1965	1966	1967	1968	1969	1970	1971	1972	1973	1974	1975	1976	1977	1978

Source: Helbig & Nikolai, 2015; Cygan-Rehm, 2018; Cygan-Rehm & Maeder, 2013; illustration by Jacqueline Lettau.

Note: white cells in between the shaded areas mark the years when the C9 reform was implemented. The shaded cells mark the period of time we cover in our sample prior to, and after the reform.

Table 2 shows the sample cut of the NEPS data we use. The NEPS cohort on adults provides observations from individuals born between 1944 and 1986. The C9 reform was implemented before 1949/50 in Schleswig-Holstein, and the city federal states of Bremen, Hamburg, and West-Berlin. As there is no variation in schooling because of the reform, we exclude observations from these federal states. We follow Cygan-Rehm and Maeder (2013), and trim our sample of age groups for each federal state to avoid comparing individuals with very large age differences. Where possible, we therefore restrict the sample to individuals who attended the 8th grade in periods between 10 years before and 10 years after the reform. As the reform took place in the western part of Germany only, we exclude respondents who attended grade 8 in the former GDR, as well as individuals not born in Germany. We also exclude individuals for whom the school leaving date remains unclear, for example, because of

missing values or parallel biographic episodes. To account for extreme outliers, we trim 1% of all values on the upper and lower end of the distribution of years of schooling. Applying these restrictions, around 3.000 individuals remain for our analyses.

Table 3 shows descriptive statistics for the full sample, and compares pre and post reform sub-samples. There is no clear pattern of differences in political participation and interest between the pre and post reform groups: based on t-tests, the younger, post reform individuals report having signed petitions or demonstrated a little more often, and are somewhat more interested in political issues. The pre and post reform individuals however do not differ in whether they have voted[9] or whether they understand political debates. Normalized differences, however, do not indicate any difference between pre- and post-reform subsamples in our main indicators.

Table 3: Descriptive statistics

Variable	Full Sample		Pre reform		Post reform		Diff.	Norm. Diff.
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.		
Signed a petition	0,71	(0,45)	0,65	(0,48)	0,74	(0,44)	0,09***	-0.14
Participated in a demonstration	0,37	(0,48)	0,35	(0,48)	0,38	(0,49)	0,03*	-0.05
Has voted	0,94	(0,23)	0,95	(0,22)	0,94	(0,24)	-0,01	0.03
Political interest	3,00	(0,75)	3,11	(0,74)	2,94	(0,75)	-0,17***	0.16
Internal political efficacy	3,13	(0,98)	3,12	(1,01)	3,13	(0,97)	0,01	-0.01
Years of schooling	10,56	(1,92)	9,96	(2,06)	10,86	(1,77)	0,9***	-0.33
Male	0,51	(0,50)	0,54	(0,50)	0,49	(0,50)	-0,05**	0.06
Age	58,66	(5,31)	64,60	(2,78)	55,65	(3,41)	-8,95***	2.03
Affected by C9 reform	0,66	(0,47)						
N	3.063		1.031		2.032			

Source: NEPS SC6 9.0.1, own calculations; Norm. Diff.: normalized differences, as suggested by Imbens & Wooldridge (2009), with the critical threshold at |0.25|.

Empirical Strategy

We first estimate multiple regression models (OLS), as a baseline for comparison to the results from instrumental variable (IV) estimations. We then use the IV approach, in which we exploit the exogenous change in schooling as induced by the C9 reform to identify causal effects.

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[9] Note that the reported voting incidence is higher than that reported by official statistics (over 90% vs. over 70% in official statistics (Bundeswahlleiter, 2010)). Overreporting is a plausible cause for that, but this is not an issue specific to the NEPS data, but rather a phenomenon occurring in many surveys. Siedler (2010) and Milligan et al. (2004) also note the risk of overreporting. According to Bernstein et al. (2001) overreporting is most likely for those who are most expected to, and therefore feel the most pressure to vote. The authors claim that educated, partisan and religious people are those who overreport the most, because they are aware of socially accepted behavior and seek to act conform to social norms.

The first stage of the IV regressions is as follows:

$$S_i = \gamma_0 + \gamma_1 Z_i + \gamma_2\, age + \gamma_3\, age^2 + \gamma_4 sex + \gamma_5 cohort$$

$$+ \gamma_6 state + \gamma_7 cohort * state + \gamma_8 cohort * age + \omega_i,$$

i.e., we estimate years of schooling, S_i, based on the instrumental variable, Z_i, and control for age, and sex, as well as cohort and state specific trends.

In the second stage, we use the predicted years of schooling to estimate their influence on the political outcome variables, Pol_i:

$$Pol_i = \beta_0 + \beta_1 \hat{S}_i + \beta_2\, age + \beta_3\, age^2 + \beta_4 sex + \beta_5 cohort$$

$$+ \beta_6 state + \beta_7 cohort * state + \beta_8 cohort * age + \varepsilon_i ,$$

applying the same control variables as in the first stage.

The IV approach depends on two critical assumptions: the instrument must be exogenous to the outcome, and it has to be highly correlated to the endogenous variable. If the two assumptions hold, only the exogenously induced changes in schooling drive the effects. We suppose that both assumptions are met. It is plausible that an adult's political participation and interest is not driven by having been affected by a reform as an adolescent except for the effect that the additional time spent in school may have had on political outcomes. That the reform had an effect on individuals' average years of schooling -so that it is correlated- is illustrated in Figure 2: prior to the reform, individuals on average spent close to eight years in school, which increased to nine years and more after the reform.

Figure 2: Median duration of schooling before and after the reform for compliers
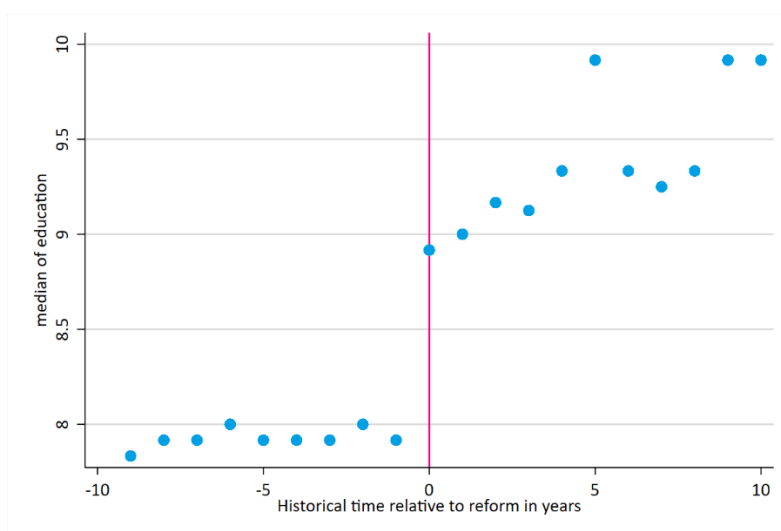
Source: NEPS SC6 9.0.1, own calculations

5. Results

We first report the results of the OLS estimations, which serve as benchmark for the IV estimates. The results are shown in Table 4, columns 1 and 2. Each estimate represents the result of a separate regression.

Table 4: Political Participation and Interest - Results of OLS and IV estimations

Dependent variable	OLS		IV			
	Full sample	Compliers	Full Sample		Compliers	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Participated in demonstration	0.076***	0.066***	0.528***	0.033	0.373**	0.040
	(0.005)	(0.006)	(0.156)	(0.075)	(0.161)	(0.108)
Signed a petition	0.057***	0.053***	0.528***	0.113	0.373**	0.152
	(0.004)	(0.005)	(0.156)	(0.073)	(0.161)	(0.120)
Political interest	0.087***	0.085***	0.528***	0.132	0.373**	0.093
	(0.007)	(0.009)	(0.156)	(0.101)	(0.161)	(0.174)
Internal political efficacy	0.113***	0.103***	0.528***	0.167	0.373**	0.361
	(0.009)	(0.011)	(0.156)	(0.121)	(0.161)	(0.247)
Has voted	0.014***	0.012***	0.533***	-0.047	0.383**	-0.108
	(0.002)	(0.003)	(0.156)	(0.048)	(0.162)	(0.085)
N	3.063	2.347	3.063	3.063	2.347	2.347

Note: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age², sex, birth cohort, federal state in grade 8, interaction terms between birth cohort and federal state in grade 8 and birth cohort and age (for voting: interview before or after the 2013 election)
Source: NEPS SC6 9.0.1, own calculations

The results of the OLS regressions for both the full sample and the compliers, i.e. individuals in the lower and the middle secondary schooling track, show the well-documented association between education individuals' political participation and interest (Table 4, columns 1 and 2). In particular, years of schooling correlate with participation in demonstrations, signing petitions and voting. A similar correlation is found for political interest and perceived difficulties in understanding political debates – our measure for internal political efficacy. Results for compliers show the same patterns, and the size of the coefficients is almost the same. These results are in line with several findings in the literature, and particularly with the results of Siedler (2010), who finds quite similar patterns for political interest, signing petitions, participating in demonstrations and voting behavior for the German context.

Results of the IV estimations, which represent the causal effect of schooling on political participation and interest, are given in Table 4, columns 3 to 6.

Results of the first stage regressions reinforce the pattern shown in Figure 2, and indicate that the compulsory schooling reform affected the duration of schooling (Table 4, column 3 for the full sample, column 5 for compliers). Compared to those leaving school before the reform took place, students affected by the reform remained at school for almost 6 months more on average (column 3). The first stage results are statistically significant at the 1% level and are in line with expectations, as well as previous literature (Pischke & Wachter, 2005; 2008). The first stage estimates are smaller and less statistically significant for the subsample of compliers (column 5), indicating about four months of additional schooling. F-values of the first stage are around 12, and therefore exceed the conventional weak instrument threshold of 10 in the first stage estimations for both groups.

The basic patterns of the OLS results can be reproduced qualitatively in most cases in the second stage of the IV estimations. However, although some of the *t*-values are relatively close to the 10% significance threshold, none of the estimates are statistically different from zero. In addition to the results for the full sample, we also report results for the target group of the C9 reform, i.e. individuals that attended lower and intermediate secondary schools. Again, all IV coefficients are not statistically significant.

Our IV results do not suggest causal effects in terms of statistical significance, yet this might be driven by the typical inflation of standard errors which is inherent to the IV method. As only the exogenous part of the variations is used for the estimations, standard errors increase, at times substantially, and even more for small sample sizes, as in our case.

6. Robustness Checks

To address some of the specifics of the reform, we run additional analyses. First, as mentioned earlier, it is not possible to properly consider transitional arrangements for Saarland and Lower Saxony. We thus rerun our analyses without observations from these two federal states. The corresponding results are shown in Table 5, and do not differ substantially from our main results.[10]

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[10] Note that this specification fails to meet the conventional weak instrument threshold of an F-value of 10 in the first stage.

Table 5: Results without observations from the federal states Saarland and Lower Saxony

Dependent variable	OLS		IV			
	Full sample	Compliers	Full Sample		Compliers	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Participated in demonstration	0.074***	0.063***	0.271	0.088	0.156	-0.876
	(0.005)	(0.007)	(0.185)	(0.183)	(0.192)	(11.30)
Signed a petition	0.058***	0.054***	0.271	0.246	0.156	4.150
	(0.005)	(0.006)	(0.185)	(0.215)	(0.192)	(49.55)
Political interest	0.086***	0.083***	0.271	0.110	0.156	-1.520
	(0.007)	(0.009)	(0.185)	(0.283)	(0.192)	(19.86)
Internal political efficacy	0.114***	0.103***	0.271	0.263	0.156	5.760
	(0.009)	(0.012)	(0.185)	(0.348)	(0.192)	(69.39)
Has voted	0.013***	0.012***	0.276	-0.030	0.020	-1.033
	(0.003)	(0.003)	(0.184)	(0.120)	(0.190)	(10.14)
N	2.629	1.991	2.629	2.629	1.991	1.991

Note: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age², sex, birth cohort, federal state in grade 8, interaction terms between birth cohort and federal state in grade 8 and birth cohort and age (for voting: interview before or after the 2013 election)
Source: NEPS SC6 9.0.1, own calculations

Grade repetition is another concern. Pischke (2007) shows that the short school years that were implemented in 1966 and 1967 increased repetition rates in schools, which may confound our estimations. We therefore rerun our analyses with a sample that excludes all individuals who repeated at least one school year.

Table 6: Estimations excluding individuals who repeated at least one school year

Dependent variable	OLS		IV			
	Full sample	Compliers	Full Sample		Compliers	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Participated in demonstration	0.074***	0.063***	0.559***	0.023	0.362*	0.048
	(0.005)	(0.007)	(0.181)	(0.076)	(0.186)	(0.119)
Signed a petition	0.057***	0.052***	0.559***	0.154*	0.362*	0.239
	(0.005)	(0.006)	(0.181)	(0.083)	(0.186)	(0.149)
Political interest	0.091***	0.085***	0.559***	0.182*	0.362*	0.188
	(0.007)	(0.009)	(0.181)	(0.106)	(0.186)	(0.190)
Internal political efficacy	0.114***	0.101***	0.559***	0.228*	0.362*	0.471
	(0.010)	(0.013)	(0.181)	(0.130)	(0.186)	(0.315)
Has voted	0.013***	0.010**	0.566***	-0.008	0.373**	-0.038
	(0.002)	(0.003)	(0.181)	(0.037)	(0.187)	(0.061)
N	2.593	1.989	2.593	2.593	1.989	1.989

Note: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age², sex, birth cohort, federal state in grade 8, interaction terms between birth cohort and federal state in grade 8 and birth cohort and age (for voting: interview before or after the 2013 election)
Source: NEPS SC6 9.0.1, own calculations

Results in Table 6 show that excluding individuals who repeated at least one school year (about 15% of our observations) does not change the overall picture of no causal effect for compliers. However, for the full sample, we do see that the reform sparked an increase in signing petitions, political interest, and internal political efficacy, with coefficients that are statistically significant at the 10%-level and that indicate moderate changes. This is not fully convincing, but suggests that the introduction of the short school years may have thwarted the positive effects of the C9 reform. Not finding statistically significant effects for compliers may furthermore be an issue of statistical power.

As previously mentioned, we follow Helbig and Nikolai's (2015) definition of the timing of the reform implementation. As most of the prior research that employed this reform used Pischke and Wachter's (2008) definition of the reform timing, we rerun our analyses using their framework for another robustness check.

Table 7: Estimations using the Pischke and Wachter reform implementation scheme

Dependent variable	OLS		IV			
	Full sample	Compliers	Full Sample		Compliers	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Participated in demonstration	0.076***	0.066***	0.480***	0.001	0.400**	0.075
	(0.005)	(0.006)	(0.159)	(0.096)	(0.161)	(0.110)
Signed a petition	0.057***	0.053***	0.480***	0.033	0.400**	0.082
	(0.004)	(0.005)	(0.159)	(0.073)	(0.161)	(0.095)
Political interest	0.087***	0.085***	0.480***	0.156	0.400**	0.158
	(0.007)	(0.009)	(0.159)	(0.109)	(0.161)	(0.148)
Internal political efficacy	0.113***	0.103***	0.480***	-0.001	0.400**	0.072
	(0.009)	(0.011)	(0.159)	(0.142)	(0.161)	(0.198)
Has voted	0.014***	0.012***	0.482***	-0.046	0.407**	-0.100
	(0.002)	(0.003)	(0.160)	(0.047)	(0.162)	(0.075)
N	3.063	2.347	3.063	3.063	2.347	2.347

Note: Clustered standard errors in parentheses; $^*\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$
Controls: age, age², sex, birth cohort, federal state in grade 8, interaction terms between birth cohort and federal state in grade 8 and birth cohort and age (for voting: interview before or after the 2013 election)
Source: NEPS SC6 9.0.1, own calculations

Using the Pischke and Wachter reform implementation scheme, the results do not differ substantially compared to our main results. Following up on the prior robustness check in which we exclude individuals that repeated at least one year, we rerun similar analyses employing the Pischke and Wachter scheme (see Table A1 in the Appendix). We again find that individuals' political interest increases when using the full sample, but that there is no further evidence for causal effects for our other indicators in the full sample, nor for the subsample of compliers.

With another robustness check, we address the differences between our implementation strategy and that of Siedler (2010). We reproduce his estimations as closely as possible with the NEPS data, restricting the sample to individuals having graduated from any type of secondary school, but not holding a university or technical college degree. As the NEPS data does not provide a variable consisting of reported years of schooling, we deduct the number of school years calculated using the educational degree attained, as Siedler does in his robustness checks. In his reform implementation strategy, he uses individuals' birth years to address the birth cohort first affected by the reform. To account for the differences between the implementation in different federal states, he uses the current state of residence of the respondents. Applying these restrictions does not change the results (see Table A2 in the

Appendix): We again find significant and positive correlations between education and the political outcome measures, but there are no causal effects.

7. Conclusions

The investigation of the relationship between education and political participation has been subject to much research across different disciplines. However, the question of causality remains unanswered, as results of previous studies are ambiguous. We add to this literature by using the exogenous variation in schooling from a compulsory schooling reform in Germany after World War II for an instrumental variable approach. Our results show a positive correlation between education and our outcome variables - participation in political actions, political interest, internal political efficacy, and voting. Using IV estimation techniques, however, implies that the associations cannot be established as being causal effects.

Although the NEPS data allows several data shortcomings to be overcome which have plagued previous studies, our paper also has its limitations. One of these issues is that the casting of votes in national elections may be selectively overreported in survey data by highly educated people, leading to biased results on educational effects. Another constraint is that we do not know how subject contents changed with the compulsory schooling reform, and what content was taught in the additional schooling year. According to Pischke and Wachter (2005), the curricula for the additional 9th grade were different across federal states. For example, Berlin used the additional time for political education, Bremen stressed general knowledge and Lower Saxony wanted to strengthen basic skills, to give students an understanding of the adult world, responsibility and the working environment. The investigation of the qualitative dimension of education, i.e. an exploration of what material was taught, is currently to a great extent missing in the empirical literature. Accounting for the heterogeneity in this is yet another source of variation that needs to be addressed in future research, as the information value of the amount of time spent in school, as a quantitative indicator of education is limited.

8. Appendix

Figure A1: Median of education for different school leaving cohorts in Bavaria
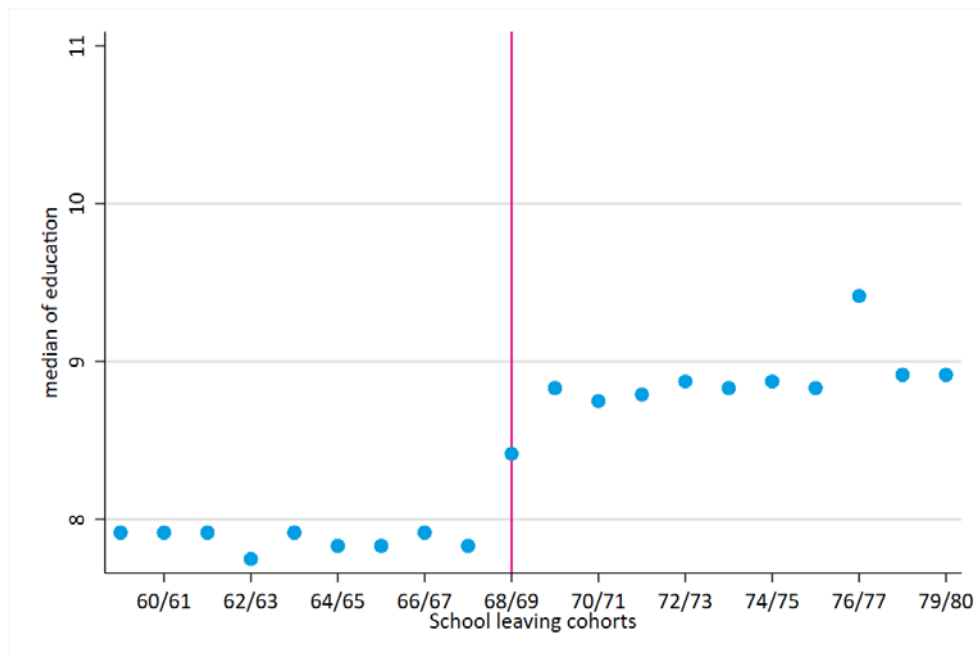
Table A1: Using the Pischke and Wachter reform implementation scheme and excluding individuals who repeated at least one school year

Dependent variable	OLS		IV			
	Full sample	Compliers	Full Sample		Compliers	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Participated in demonstration	0.074***	0.063***	0.538***	-0.031	0.379**	0.055
	(0.005)	(0.007)	(0.187)	(0.098)	(0.186)	(0.129)
Signed a petition	0.057***	0.052***	0.538***	0.065	0.379**	0.145
	(0.005)	(0.006)	(0.187)	(0.073)	(0.186)	(0.112)
Political interest	0.091***	0.085***	0.538***	0.188*	0.379**	0.220
	(0.007)	(0.009)	(0.187)	(0.109)	(0.186)	(0.177)
Internal political efficacy	0.114***	0.101***	0.538***	0.058	0.379**	0.144
	(0.010)	(0.013)	(0.187)	(0.135)	(0.186)	(0.233)
Has voted	0.013***	0.010***	0.541***	-0.007	0.389**	-0.042
	(0.002)	(0.003)	(0.188)	(0.035)	(0.186)	(0.061)
N	2.593	1.989	2.593	2.593	1.989	1.989

Note: Clustered standard errors in parentheses; $^{*} p < 0.10$, $^{**} p < 0.05$, $^{***} p < 0.01$
Controls: age, age², sex, birth cohort, federal state in grade 8, interaction terms between birth cohort and federal state in grade 8 and birth cohort and age (for voting: interview before or after the 2013 election)
Source: NEPS SC6 9.0.1, own calculations

Table A2: Replication of Siedler (2010)

Dependent variable	OLS	IV	
		First stage	Second stage
	(1)	(2)	(3)
Participated in demonstration	0.054***	0.625***	-0.018
	(0.006)	(0.146)	(0.057)
Signed a petition	0.063***	0.625***	0.013
	(0.008)	(0.146)	(0.062)
Political interest	0.109***	0.625***	0.162
	(0.011)	(0.146)	(0.115)
Has voted	0.021***	0.625***	-0.049
	(0.004)	(0.146)	(0.043)
N	2.224	2.224	2.224

Note: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age², sex, year of birth, current state of residence, birth cohort, interaction terms between birth cohort and current state of residence (for voting: interview before or after the 2013 election)
Source: NEPS SC6 9.0.1, own calculations

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