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Universal Solvent or catch-all Proxy?

CAUSAL EVIDENCE ON THE EFFECT OF EDUCATION ON POLITICAL PARTICIPATION AND ATTITUDES IN GERMANY.

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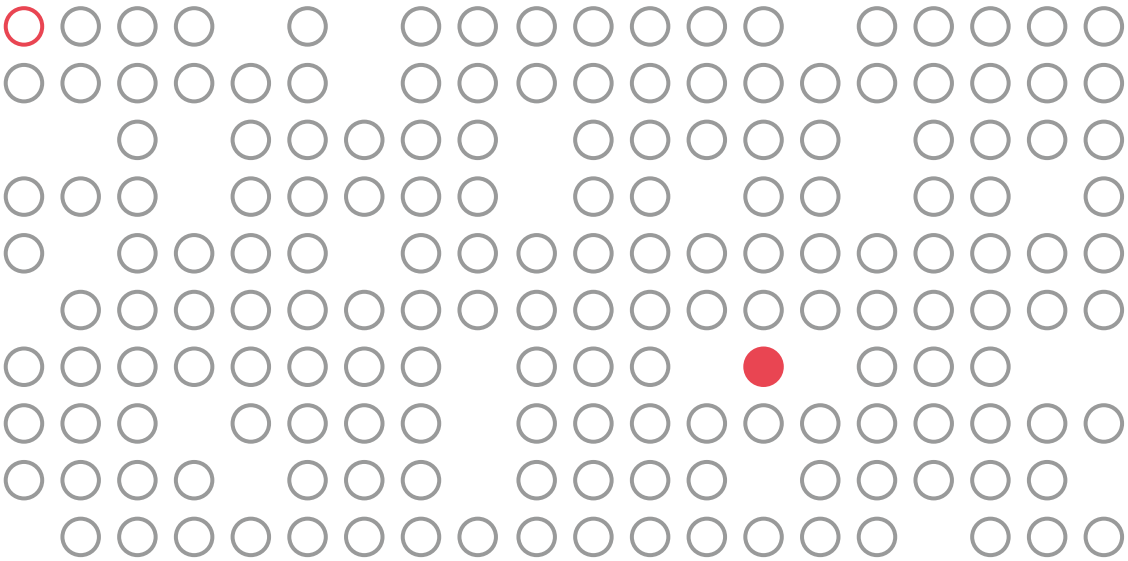

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List of Abbreviations

AfD	*Alternative für Deutschland*
AID:A	*Aufwachsen in Deutschland: Alltagswelten* (Growing up in Germany)
ALLBUS	*Allgemeine Bevölkerungsumfrage der Sozialwissenschaften* (German General Social Survey)
ALWA	*Arbeiten und Lernen im Wandel* (Working and Learning in a Changing World)
ARD	*Arbeitsgemeinschaft der öffentlich-rechtlichen Rundfunkanstalten der Bundesrepublik Deutschland* (Association of Public Broadcasting Organizations of the federal Republic of Germany)
BBSR	*Bundesinstitut für Bau-, Stadt- und Raumforschung* (Federal Institute for Research on Building, Urban Affairs and Spatial Development)
BMBF	*Bundesministerium für Bildung und Forschung* (Federal Ministry of Education and Research)
C9	extension of compulsory schooling from eight to nine years
CDU	*Christlich Demokratische Union Deutschlands*
CE	Civic Education
CIVED	Civic Education Study
COVID(-19)	Coronavirus disease 2019
CSU	*Christlich-Soziale Union*
D.	Dummy
doi	Digital Object Identifier
EACEA	European Education and Culture Executive Agency
EU(-28)	European Union (with 28 member states)
FRG	Federal Republic of Germany
G8	German academic track duration of eight years
G9	German academic track duration of nine years
GDP (p.c.)	Gross Domestic Product (per capita)
GDR	German Democratic Republic

GPA	Grade Point Average
HH	Household
ICU	Intensive Care Unit
IEA	International Association for the Evaluation of Educational Achievement
IHAD	I Have A Dream
ISEI	International Socio-Economic Index of Occupational Status
ISO	International Organization for Standardization
IV	Instrumental Variable
IZA	*Forschungsinstitut zur Zukunft der Arbeit* (Institute of Labor Economics)
KMK	*Ständige Konferenz der Kultusminister der Länder* (Standing Conference of the Ministers of Education and Cultural Affairs of the federal States)
LIfBi	*Leibniz Institut für Bildungsverläufe* (Leibniz Institute for Educational Trajectories)
MPIDR	Max Planck Institute for Demographic Research
NAEP	National Assessment of Education Progress
NEPS	National Educational Panel Study
OECD	Organisation for Economic Co-operation and Development
OLS	Ordinary Least Squares
PISA	Programme for International Student Assessment
RCT	Randomized Controlled Trial
SC3	Starting Cohort 3 (grade 5) of the National Educational Panel Study
SC6	Starting Cohort 6 (adults) of the National Educational Panel Study
SES	Socioeconomic Status
SOEP	Socioeconomic Panel
SPD	*Sozialdemokratische Partei Deutschlands*
STAR	Student Teacher Achievement Ratio
TV	Television
UK	United Kingdom
US(A)	United States (of America)
USD	US-Dollar
ZDF	*Zweites Deutsches Fernsehen*

Chapter 1

Synopsis

1.1 Introduction

Education is interfusing individual life courses over and above anything else: Already in early childhood, kids are educated in daycare facilities, later in kindergarten, and -even more intense- in school, accompanying them through their teenage years into adulthood. After general education, vocational training or tertiary education enables youth and young adults to develop further skills to gain a foothold in the labor market. Even after that, education -although less concentrated- plays an important role in maintaining employability and keeping track of innovations or technology. Also, retirement is no reason to restrain from education as there are learning opportunities specifically targeting the elderly, like university lectures for senior citizens on multifaceted topics. So, from the cradle to the grave, education can be considered as being part and parcel of life.

Resulting from this omnipresence of education, a crucial strand of research emerged focusing on the potential benefits of education in multiple areas of life. The most traditional sub-strand of this literature investigates labor market returns, for example analyzing unemployment risks, career trajectories, and earnings but also occupational prestige and status (Harmon et al. (2003); Oreopoulos and Petronijevic (2013); Psacharopoulos and Patrinos (2018)).

More recently, research interests expanded to other educational returns like health and health behavior (Galama et al. (2017); Kroh (2021)), deviance (Lochner (2011); Machin et al. (2011)) or social and political engagement (Huang et al. (2009); Lochner (2011)).

Within the latter, authors considering education as a mere proxy for other underlying factors, which simultaneously influence its attainment and political participation, can be distinguished from those interpreting education as a direct cause (see Section 1.3). Here, education is regarded as the "universal solvent" (Converse, 1972, p. 324), as it predicts multiple indicators of popular involvement and participation in politics, in terms of cognitive (level of factual information, knowledge about political concepts and its assessment) as well as motivational matters (degree of attentiveness, emotional involvement) or actual political behavior (party work, voting). As Converse (1972, p. 324) puts it: "The higher the education, the greater the 'good' values of the variable. The educated citizen is attentive, knowledgeable, and participatory, and the uneducated citizen is not."

Although this quotation seems quite extreme, it nevertheless offers a core of truth, as the educational system is regularly charged with the responsibility of educating good citizens. According to van de Werfhorst (2014), one of the tasks of education[1] is enabling students to

[1] Overall, van de Werfhorst (2014) formulates four functions of education: (1) Preparing individuals for the labor market, (2) sorting students into educational tracks fitting their talents and interests to optimize

become politically active citizens.

Although the link between education and political participation has been at the center of attention for decades, previous empirical evidence is far from conclusive. Most of the established findings in the field still heavily rely on studies only accounting for correlations and not being able to identify causal effects. These correlative studies show overall positive associations between educational attainment and outcomes like political knowledge, interest, voter turnout, and other related aspects in the field of political participation and attitudes (see for example Burden (2009); Chevalier and Doyle (2012); de Rijke (2009); Denny and Doyle (2008); Grönlund and Milner (2006); Hadjar and Becker (2006); Hauser (2000); Hillygus (2005) or Hoskins et al. (2008)).

For drawing conclusions on causal effects, it would be ideal to observe the same persons with and without being treated. As this is not possible, the second-best approach would be a randomized controlled trial (RCT). This is also very rare, for predominantly ethical, but also financial reasons. Nevertheless, there are some examples of RCTs, mainly from the US, (like *Perry Preschool*, *I Have A Dream (IHAD)*, and *Student Teacher Achievement Ratio (STAR)*), that were used to investigate political outcomes (see Sondheimer and Green (2010)). Other intervention programs operate on the level of providing additional content, such as *Student Voices* or *Kids Voting USA* in the given context. These interventions had positive effects on a range of outcomes, like discussing political issues, political knowledge, or attentiveness to news (Feldman et al. (2007); McDevitt and Chaffee (2000); McDevitt and Kiousis (2004, 2006); Pasek et al. (2008)).

Yet, most of the international evidence deriving causal claims uses exogenous variation emerging from school reforms (or pilot schemes thereof, like in Lindgren et al. (2019)), but also from changes in child labor laws or proximation to college (see Dee (2004)) interpreted as quasi-experiments. Methodologically, employing instrumental variables is most common (see for example Milligan et al. (2004)), however, there are also studies using matching approaches (like Persson (2014)) or difference-in-differences (see for example Lindgren et al. (2017)). Overall, the results of these studies can be classified as mixed, as some authors report causal effects (see Dee (2004) or Milligan et al. (2004) for the US), while others do not find any or only little evidence for causality (like Lindgren et al. (2019) for Sweden, Parinduri (2019) for Indonesia, or Pelkonen (2012) for Norway).

Because of this lack of causal evidence, especially in Germany where only one study by Siedler (2010) is available so far, in this dissertation, I pose the overall research question if education is

knowledge and skill production, (3) ensuring equal opportunities for all students and (4) socialize children into active civic engagement.

causally impacting on political participation in the German context. In the four independent articles of this cumulative work, I aim to contribute to the literature in several respects: First, I focus on appropriate methods for causal claims (for example instrumental variables or difference-in-differences) and different sources of exogenous variation (like school reforms or the reunification of Germany). Second, to get more comprehensive insights, I employ different operationalizations of both education and political participation. Overcoming the commonly used indicators –education measured in years of education and political participation proxied by voter turnout– is worthwhile as effects may be heterogenous depending on the concrete specification of the concepts used. Third, I consider both adults and adolescents as well as long- and short-term effects. The first is crucial because political attitudes and participation habits start to form early and especially political attitudes are rather stable and therefore highly predictive for political behavior later in life (Hooghe and Wilkenfeld (2008); Prior (2010)). Investigating the temporality of certain educational effects offers some valuable clues on the question of how severe or long-lasting the effects of educational policies, particular courses, or learning environments actually are. Fourth, unlike most studies in the field, I do not rely on survey data only, but enrich it by using other data sources (like official statistics or data on the provision and extent of civic education) to, at least to some extent, address the issue of confounding and to enhance reliability. Fifth, I investigate the case of Germany, for which very little evidence exists so far, although the German system provides a very interesting context. Especially, the sovereignty of the federal states, which includes that they are responsible for the funding, content, and structure of education, creates sources of variation to take advantage of in empirical research.

1.2 Concepts and Definitions

Before elaborating on the theoretical connection between education and political engagement and empirical challenges in this strand of research, in this Section, I briefly cover the basic concepts used in the following.

According to Galston (2001, p. 219) “All education is civic education”. This quote is illustrating vividly that education seems to be generally important in the field of civics, and that multiple purposes of education can arguably have civic implications, but it also points to the issue that it is still unclear *which* educational dimensions are driving the relationship under investigation. Therefore, the concept of *education* is defined rather broadly in this dissertation.

The most common approach is followed in Chapter 2, where the focus is on time spent in the

educational system. But already here, the data used is not limited to years of education typically needed to gain a certain certificate, like in many other studies lacking other indicators, but enriched with actual schooling duration from biography data.

In Chapter 3, the focus is on the macro-context of socialization, especially growing up in the repressive state of the German Democratic Republic (GDR) and the role of access to free media. As the public media has an explicit educational function (Hoffmann (2016)), which is also defined in the German Interstate Broadcasting Treaty, it serves as a crucial source of political information (Luskin (1990)), but can also be (mis)used to nurture and reinforce shared attitudes, norms, and values propagated by the particular regime (Friehe et al. (2020)).

In Chapter 4, I focus on civic education covered by subjects specially devoted to it in secondary schooling and therefore dip into educational content. In contrast to other studies, I do not rely on information on the provision and extent of civic education reported by students or school staff, but merge the corresponding data for all school types within the federal states of Germany.

Finally, in Chapter 5, I investigate the effect of an increase in schooling intensity, due to a reform shortening the duration of the highest track of secondary schooling from 9 to 8 years.

To be able to get an extensive picture in terms of outcome measures, also the concept of *political participation* is defined quite broadly, covering a variety of measures that can overall be clustered into *active political participation* and *political attitudes*. I follow Verba et al. (1995, p. 38) and define active political participation as all kinds of activities that have the effect –or at least the intent– of (directly or indirectly) influencing governmental actions or decision-making.

Summarizing all papers of this dissertation, in terms of *active political participation*, I cover participating in demonstrations, political discussions, signing petitions, and joining citizen initiatives or parties. Concerning *political attitudes*, I cover political interest, internal political efficacy, and institutional trust in traditional and new media.

Political interest is an attribute tied to individual citizens reflecting their curiosity (van Deth (1990)) or attentiveness to politics (Zaller (1992)). It is also interpreted as a prerequisite for most forms of political activity because without a minimum level of interest, individuals would not even be aware of political processes, let alone their opportunities to influence these processes and contribute to societal decisions (van Deth and Elff (2004)). Internal political efficacy is "a perceptional, subjective and psychological construct" (Rasmussen and Norgaard, 2018, p. 26) reflecting an individual's perceived competence in terms of understanding and participating in politics (Jackson (1995)). Trust in general is a multifaceted construct that is tied to specific actors (Itzenplitz and Seifferth-Schmidt (2011); Paxton (1999)), and can be specified as the

expectation of an individual that others will act in their interest or that their actions will at least not be detrimental for them (Newton, 2001, p. 202; Offe, 2001, p. 249). Institutional trust, which is sometimes also called political trust, refers to the level of trust citizens put in organizations or institutions, like the media or political institutions, and their capacity to act.

1.3 Theoretical background and empirical challenges

Reviewing theoretical considerations concerning the relationship between education and political participation shows that two main lines of reasoning can be distinguished: (1) causal theories and (2) non-causal approaches. The causal theories can be further classified as they interpret different dimensions of education as being crucial.

According to the absolute education model (Persson (2013)), education has a direct effect on political participation, so the total amount of education is most important. All effects education may have are interpreted as processes on the individual level and as being independent of the level of education prevalent in the individual's environment.

Opposing that, the relative education model, put forward by Nie et al. (1996), states that the educational effect is only indirect, that means in relative terms. Here, the level of education is always seen relative to the level of education in a particular society. Therefore, education is interpreted as a positional good, reflecting the rank an individual holds within the social order. In this context, education serves as a sorting device and is only valuable for those possessing it while others do not.

In their seminal civic volunteerism model, Verba et al. (1995) provide a synthesis of conceptual thoughts and describe three different channels through which education and civic participation are causally connected: resources, psychological engagement with politics (like motivation, norms, and values) and access to politically active social networks. Very similar considerations are also reported in the "ability-motivation-opportunity triad" by Delli Caprini and Keeter (1996, p. 179) with reference to the work of Luskin (1990) about the basis of political behavior.

Verba et al. (1995) focus on three kinds of resources, namely (1) money, (2) time, and (3) civic skills. First, highly educated individuals are more likely to possess larger amounts of money because of an advantageous labor market placement with higher income. Second, higher educational attainment does not necessarily lead to more free time to spend for political purposes, especially because opportunity costs rise with educational level, but individuals with higher education are more likely to work in positions with more flexible working hours, providing them

more freedom to accommodate to the needs of their political participation.

Third, and as also observed by Milligan et al. (2004, p. 1671), the concept of civic skill used by Verba et al. (1995) is analogous to what Becker (1964) and other economists call the skill-based approach of human capital. Educational attainment is an investment in human capital and therefore promotes the development of individuals' cognitive skills (Persson (2015)). Also, it enhances their competencies in gathering and processing politically relevant pieces of information (Brade and Piopiunik (2016)), which is crucial for gaining insights into the abstract contents of politics, and for empowering individuals to comprehend political campaigns and public discussions (Delli Caprini and Keeter (1996)). Hence, highly educated people experience themselves as being more capable of following political developments as well as being well-prepared for active participation (Hadjar and Becker (2007); Vetter (2000)). This leads to a higher probability of them being engaged in, for example, a political party or other nonviolent political activities (Uehlinger (1988)).

Next to the cultivation of general, superordinate skills, education is also essential for imparting factual knowledge about the political system, its institutions, and its mode of operation (Brade and Piopiunik (2016); Persson (2015)), for example through civic education lessons at school. This body of acquired knowledge then serves as a fundament for the sound evaluation of political issues. Consequently, attaining higher levels of education diminishes the costs of political engagement and boosts its perceived benefits for the individual by fostering democratic principles (Dee (2004)).

Via their curricula and their teaching of civics, schools themselves become proving grounds in which students can gain practical experience in applying democratic principles. They are also confronted with the shared social norms and values of the particular society, so the effect of higher educational attainment depends on the normative frame of the society (Meyer (2017)). Education is supposed to cultivate preferences or the motivation for being politically active, by, for example, integrating political discussions in class. Also, students' political awareness can be reinforced, for instance by stressing the relevance of elections, encouraging individuals to adopt being well-informed as a social norm, and granting them confidence[2] in their role as a politically well-informed citizen (Lewis-Beck et al. (2008)).

Taking a closer look at the importance of social networks requires a recap of social capital theory

[2] Rosenstone and Hansen (1993) even interpret this kind of confidence as an additional crucial resource, next to time and money.

and its connection to education (see also Helliwell and Putnam (2007)). Granovetter (1973) and Lin (1999) specify social capital as access to resources through social networks, which individuals can use to achieve certain goals. Here, networks can be characterized as being institutionalized, including civic associations or clubs, as well as informal or loose, like neighborhoods or peer groups. The relevance of education for network formation becomes apparent as soon as the principle of homophily is considered. It means that individuals are more likely to connect with others who are rather similar to themselves, and schools or other educational institutions provide opportunities to meet people with similar characteristics.

However, education may not only influence the composition of someone's peer group but also the individual's status because it functions as a social and political sorting device as it "[...] places citizens either closer or further from the center of critical social and political networks that, in turn, affect levels of political participation" (Hillygus, 2005, p. 28). Individuals holding high-rank (or more central) positions face a higher probability to be recruited by political organizations because they are more capable of contributing to the organization's advantage for example by sanitizing its public image or attracting potential new members. Additionally, they are aware of strategies to mobilize their (also high-status) social network partners, who may also be valuable as members or volunteers. High status positions are also associated with a, at least subjectively perceived, higher scope of action within society and politics (Hadjar and Becker (2007)).

The composition and structure of an individual's social network is fundamental for receiving information, for example about possibilities to participate, but also for shared opinions, values and norms. Moreover, it can also reduce constraints to joining social groups (Nie et al. (1996); Verba et al. (1995)) or affect and motivate behavior, as illustrated by Franklin (2004) who uses a group pressure argument to state that the benefits of voting (or the costs of not voting) are higher for socially integrated people because their network partners care about whether they vote or not. In the end, social capital theories indicate that the inclusion of highly educated individuals in politically active networks reinforces the individual-level effects of education. (Nie et al. (1996); Verba et al. (1995))

In contrast, non-causal approaches state that there is no causal link between the two. Instead, it is claimed that the association is spurious and caused by the fact that education serves as a proxy for other (unobserved) factors, which simultaneously influence both educational attainment and political participation and therefore act as confounding variables, i.e. common causes of the causal variable of interest (education) and the outcome variable (political participation/attitudes).

In this case, education is endogenous. There are multiple factors potentially confounding the relationship between the two, which may be grouped into (1) the macro context, (2) individual characteristics, and (3) the family background.

Examples of relevant confounding factors at the macro level are regional differences in the institutional setting that influence individual education according to the local education system characteristics (e.g. varying curricula or policies for the duration of secondary schooling), but also political outcomes (e.g. due to differences in the political systems).

Diverse individual characteristics may also affect both educational attainment and political participation, for example, intelligence (Deary et al. (2008); Luskin (1990)), genetic factors (Alford et al. (2005); Cesarini et al. (2014); Fowler et al. (2008)) or socio-demographic variables like gender, age, and ethnic origin. Personality is also interpreted as an individual-level characteristic potentially acting as a confounding variable, as certain traits, like conscientiousness, are crucial for educational attainment (O'Connor and Paunonen (2007); Poropat (2009); Richardson et al. (2012)) and also correlate to political attitudes, like political orientation (Dunn (2011)) or internal political efficacy (Rasmussen and Norgaard (2018)), as well as with a range of political activities like voting, engaging in local and national politics or participating in protest activities (see Mondak (2019); Mondak and Halperin (2008); Mondak et al. (2010, 2011)).

Finally, family background is important as parents influence the educational pathways of their children to a great extent for example by providing relevant resources. In terms of political participation, parents act as role models for their children (Grob (2006)), transmitting their political attitudes to their children (Koskimaa and Rapeli (2015)) as well as influencing their political participation as adults (Jennings et al. (2009)) for example by directly or indirectly exposing them to politics early on through, for example, the consumption of news, or reflections and discussions on political actions. Families may also converge politically because they share other influences, such as their socioeconomic status or their local political environment (see for example Andolina et al. (2003); Gidengil et al. (2016); Jennings et al. (2009); Kam and Palmer (2008); McIntosh et al. (2007); Neundorf et al. (2013); Verba et al. (1995)).

Empirically, studies relying on non-experimental observational data are confronted with the issue of separating causal from non-causal associations which are mixed up in observed associations between education and political variables (Elwert and Winship (2014)) if the method used is not adequately addressing endogeneity.

A common solution is to try to control for background characteristics to account for differences

already apparent before education kicks in. This strategy is only appropriate if these control variables are not affected by education, otherwise, this procedure leads to over-control bias in the causal effect (Elwert and Winship (2014)).

Even if controlling for observable characteristics is possible, accounting for unobservables surely is not. Omitting unobservables leads to a correlation with the error term. In this context, three types of biases resulting from unobservables can be distinguished: ability bias, self-selection bias, and measurement bias.

Ability is a crucial confounding variable as more able individuals are both more likely to reach higher educational levels and to participate politically. Omitting information on ability leads to downward biased estimates of the return to education (Card (1999)). In Chapter 4, I was able to include measures on cognitive basic skills as a proxy for ability. These measures were collected before the relevant educational input, in this case civic education in secondary schooling, started. A commonly used solution to ability bias is using instrument variable (IV) estimators, as in Chapter 2. Here, an *exogenous* source of variation in schooling, a school reform after World War II prolonging compulsory schooling for one year, is used as an instrument for years of schooling. Exposure to the reform is correlated to years of schooling but not to the political outcome measures, which is qualifying the reform to be used as a valid instrument.

In case of heterogenous returns to education, IV estimations are nevertheless biased because of the occurrence of self-selection. The problem here is that individuals are expecting different returns resulting from their investments in education and act accordingly. So, the ones with lower expectations are more likely to self-select out of education and vice versa.

Measurement bias occurs if the measurement of outcomes differs for different groups of respondents, which should be a minor issue compared to other measurement issues. These form the second methodological challenge in this research -next to endogeneity. Measurement errors in the education variable may lead to attenuation bias resulting in estimates distorted to zero. In Chapter 2, we, therefore, employ two different measures for education and find that adequate measurement is indeed crucial. Also, in Chapter 4, I do not use the information on the exposure to civic education lessons reported by students or teachers because it may be subject to socially desirable response behavior.

But also adequately covering political participation and attitudes can be a challenge as respondents may consider these as sensitive pieces of information. Therefore, if education and social desirability in the response behavior correlate the resulting estimates are biased. According to Bernstein et al. (2001), highly educated are among those who are most prone to answer according to social

desirability because they are aware of what kind of behavior is appreciated by society and are keen on acting conform to social norms. Unfortunately, participation in the majority of political activities is not officially registered, so there is just no way to avoid using survey data. The only exception is voter turnout. Therefore, in Chapter 2, we use voter turnout on the county level to proxy for individual voting, as self-reports in surveys suffer from overreporting (Bömmel et al. (2021))[3].

1.4 This dissertation

This dissertation consists of four independent scientific articles, organized in the following Chapters (for an overview see also Table 1.1). The first part (Chapters 2 and 3) investigates adult populations and the long-term effects of extending compulsory schooling, growing up in a repressive state, and (not) having access to free media on their active political participation and attitudes.

Chapter 2 "Revisiting the Causal Effect of Education on Political Participation and Interest" (co-authored by Guido Heineck) studies the long-term causal effect of an increase in compulsory schooling in West Germany. For identification, we exploit a reform after World War II in the course of which compulsory schooling was prolonged from 8 to 9 years. We use being affected by this reform as an instrumental variable to address the potential endogeneity of education and estimate the causal effect of an additional year of schooling on a range of political outcome measures over 40 years after our respondents finished general education.

Thanks to the rich data from the adult cohort of the National Educational Panel Study (NEPS), we are able to investigate a range of outcomes, containing political interest, internal political efficacy, participation in demonstrations, and signing petitions on the individual level. As self-reported voter turnout in the NEPS dataset does not match official numbers, we use additional data from BBSR (2021) to include official voting statistics on the county level in our analyses. In addition to the commonly used years of education indicator converting school-leaving certificates into schooling duration, we use the factual length of time individuals have spent in schools which NEPS collects as part of respondents' educational biographies.

Our results indicate that the well-known association between education and political participation partially reflects causal effects. Only for political interest, we are not finding a significant causal effect. In general, effects are more pronounced for the target group of the reform, namely

[3] More general problems, potentially affecting both sides of the equation, are: recall errors, misreporting, and non-response.

respondents having visited lower and intermediate tracks. Analyses that use either generated or self-reported length of schooling yield results that are roughly similar. Adding deviations between the two schooling indicators implies that faster (slower) individuals are more (less) likely politically engaged.

As the compulsory schooling reform we use here was only apparent in the western part of Germany, we are not able to draw any conclusions on respondents raised in east Germany and therefore address this issue in Chapter 3.

In "Trust in Media and COVID-19 related News - Evidence from reunified Germany and Europe", which was prepared jointly with Anica Kramer and Guido Heineck, we examine whether growing up in a socialist country, with its limited access to free media, has a lasting impact on trust in traditional media, like press and television, and new media, such as social media, decades after the socialist regime was abolished. For identification, we exploit two features of German history as natural experiments.

First, we use the separation and subsequent reunification of the two Germanies as a quasi-natural experiment to compare East and West Germans and draw conclusions about the influence of the particular environment individuals are born and raised in on later life outcomes. Second, we compare East Germans with and without access to free West German media, which is supposed to provide objective information, and thus pursue an educational function. The identifying variation arises from differences in the reception of West German television, solely based on geographical characteristics, namely the shortest distance to the nearest television tower on the East-West German border.

Again using data from the adult cohort of the National Educational Panel Study, we show that East Germans trust less in press or television than West Germans. In contrast, we find that East Germans are more likely to trust in social media. To reveal the main underlying mechanism, we compare East Germans with and without access to free West German media and show that these effects are especially pronounced for those East Germans who had no access to West German TV. Our findings are robust to several robustness checks and we rule out that, among others, the education system in East Germany, which also included a specific subject on socialist education or extreme political views, as the voting share for right-wing parties, serve as alternative explanations for our results.

Also, the results of additional analyses based on Eurobarometer data for the EU-28 countries support our main findings. The dataset also allows us to study trust in radio and the Internet. We take advantage of this data and examine the differences between former socialist and non-socialist

countries. Consistent with our earlier findings, individuals living in a former socialist country show lower levels of trust in the press, television, and radio, but put more trust in social media and the Internet.

In the second part of my dissertation (Chapters 4 and 5), I change the focus to short(er)-term effects on adolescent samples. Therefore, in Chapter 4 "Does Civic Education in Secondary Schooling have an Impact on Political Interest of Grade 8 Students? Evidence from NEPS", I investigate the correlation between civic education (CE) in secondary schooling in Germany and the political interest of students attending grade 8 (age 13/14). I use different measures of civic education, namely the mere exposure (0/1) to the subject, the timespan (number of school years of exposure), the intensity (sum of weekly hours), and the total amount of civic education in grades 5 to 8 and analyze their impact on political interest in youth. To approximate causality, I use variation in the provision of civic education between different school types and federal states, as collected by Kalina (2014). Particular attention is also paid to the heterogeneity of the student body.

Results of OLS regressions using data from the National Educational Panel Study (Starting Cohort Grade 5) show that the exposure to civic education in secondary schooling is positively related to political interest. Considering the timespan of exposure, my results reveal that the correlation is most pronounced in the first year of CE lessons, but still positive and significant in later years. Also, the intensity and the total amount of civic education correlate positively to political interest. Additionally, civic education lessons do not seem to be equally important for all students, as further heterogeneity analyses indicate differences according to gender, school type, and parental education. In detail, receiving civic education at school is more influential for the political interest of girls, students not attending higher secondary school, and students with less educated parents.

In Chapter 5, I examine whether an increase in schooling intensity affects different political outcome measures, such as political interest and participation in different kinds of political actions, like participating in political parties, citizen initiatives, discussing political issues, demonstrating and signing petitions. For identifying causal effects, I exploit exogenous variation across time and federal states induced by the so-called G8 reform. This reform was implemented by most of the German federal between 2001 and 2008 and is especially suitable for investigating my research question as it lead to a reduction of the duration of the highest track of secondary schooling from 9 to 8 years, while the overall curriculum and the total instruction time remained unchanged.

Using data from Aufwachsen in Deutschland: Alltagswelten (AID:A), I find that the reform had

a negative effect on political interest in the short run. It also negatively affected engaging in political parties but this effect did not translate into other forms of political participation. I show that these negative effects are strongly driven by the double cohort, indicating that the reform effects on political participation and interest are temporary and not permanent. Further analyses show that effects are marginally -if at all- heterogeneous according to gender and family characteristics. Investigating potential mechanisms, only a low performance level appears to be a significant channel, whereas the likelihood of receiving private tutoring, joining sports clubs, or musical free time activities and students' experiences at school remain largely unaffected by G8. Interestingly, I also find some evidence of spillover effects of children's schooling intensity on political interest and information-seeking behavior of their parents.

Overall, with this thesis, I am able to show, that, in the German context, short- and long-term causal effects of education on active political participation and political attitudes exist in adult as well as adolescent target groups. It proves to be beneficial to investigate different operationalizations of both education and political participation, to attain the most comprehensive picture. Although this dissertation contributes to the literature on Germany, multiple issues, like uncovering underlying mechanisms or digging deeper into educational content remain untouched and constitute desiderata for future research.

Table 1.1: Overview of dissertation

	Chapter 2	Chapter 3	Chapter 4	Chapter 5
Title	Revisiting the Causal Effect of Education on Political Participation and Interest	Trust in Media and COVID-19 related News - Evidence from reunified Germany and Europe	Does Civic Education in Secondary Schooling have an Impact on Political Interest of Grade 8 Students? - Evidence from NEPS	The Impact of Schooling Intensity on Political Participation and Interest - Evidence from a German Higher Secondary School Reform
Data	NEPS SC6	NEPS SC6, Eurobarometer	NEPS SC3	AID:A
Method	Instrumental variable approach	Probit	OLS	Difference-in-Differences
Co-author(s)	Guido Heineck	Anica Kramer and Guido Heineck	-	-
Own contribution	70%	45%	100%	100%

Chapter 2

Revisiting the Causal Effect of Education on Political Participation and Interest

Nadja Bömmel and Guido Heineck

A version of this study (containing less information on additional analyses) was published as:
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Bömmel, N. and Heineck, G. (2020b). Revisiting the Causal Effect of Education on Political Participation and Interest. *LIfBi Working Paper No. 92*.
Bömmel, N. and Heineck, G. (2020c). Revisiting the Causal Effect of Education on Political Participation and Interest. *BERG Working Paper No. 167*.

2.1 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 (1999, Chapters 2-3) and Levinson (1999, Chapter 4) also consider the development of democratic values and civic habits as a central objective of the educational system 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 and Hansen (1993); Verba et al. (1995)). Education also improves individuals' capacity to gather and process politically relevant information (Delli Caprini and Keeter (1996)). Social capital theories furthermore suggest that the involvement of the well-educated in politically oriented networks reinforces these individual-level effects (Nie et al. (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 about 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 and Doyle (2012); Denny and Doyle (2008); Grönlund and Milner (2006); Hadjar and 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 skeptical 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'

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

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.

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 and 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 and 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 the findings overall. The literature shows rather mixed results, and almost exclusively looks at Anglo-American or developed 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). Apart from the typical correlations, his findings do not suggest causal effects of education on political participation. He finds this using data from the German General Social Survey (ALLBUS) and ForsaBus, a program surveying public attitudes on a number of different political and social issues, which, similar to other data sources available at that time, such as the Socioeconomic Panel (SOEP) or Microcensus, were limited in that they did not provide information on where and when the person went to school. However, this is necessary to identify exactly whether the person was affected by the schooling reform that

[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 et al. (2001)).

we exploit as a source of exogenous variation in the length of schooling.

We use data from the more recent German National Educational Panel Study (NEPS), which provides accurate information on individuals' educational biographies. This does not force us to make assumptions about whether the person was affected by the schooling reform. This is relevant in that it quite likely generates measurement error in the first stage of an IV estimation with corresponding consequences for the second stage estimation. Rather, we can identify precisely, whether individuals were affected by the reform, so that there is no measurement error in the first stage.

A second, more important contribution is that we can compare analyses that do not rely exclusively on school durations generated from certificates, as is the case in many surveys that provide data on "years of education". Instead, we can use actual durations derived from individuals' educational biographies. This allows us to identify the sensitivity of the results and whether they differ, for example, for individuals who are faster or slower than their certificate implies.

Our results indicate that the well-known association between education and political participation partially reflects causal effects. This is in contrast to the results of Siedler (2010), which we can replicate, and is likely due to the differences in the data, as described. Analyses that use either generated or self-reported length of schooling yield results that are roughly similar. Adding deviations between the two schooling indicators implies that faster (slower) individuals are more (less) likely politically engaged.

Overall, our results suggest that education matters for the individual development of active political participation, in contrast to previous research. Moreover, our study also shows that precise information on individuals' educational biographies is crucial for analyses on the effects of school reforms.

2.2 Theoretical Background and Prior Research

2.2.1 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 influences 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 and 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 and 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 (Nie et al. (1996)), 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 influence 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 and Becker (2006)), which fosters the willingness to engage directly. Network composition and structure are, in addition, not only important for the availability of information, or shared opinions, values, and norms, but can also affect or motivate behavior (Klandermans and Oegema (1987); McPherson et al. (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.

2.2.2 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 (Burden (2009); Chevalier and Doyle (2012); de Rijke (2009); Denny and Doyle (2008); Grönlund and Milner (2006); Hadjar and Becker (2006); Hauser (2000); Hillygus (2005) or Hoskins et al. (2008)). 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. Another approach is taken by Persson (2013), who uses the individual's educational rank position to examine the propositions of the relative education model. His results imply that it is not absolute, but relative education that matters for voter turnout.

The few studies that aim to estimate causal effects use different sources of exogenous variation in education. Sondheimer and Green (2010) exploit exogenous variation in educational achievement from the intervention programs, 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 a proxy for the quality of civic knowledge. Using changes in compulsory schooling as an instrument, Milligan et al. (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. The latter is confirmed by Persson (2014), who also finds no causal effects once he uses matching as an identification approach. Borgonovi et al. (2010) do not find a causal relationship between years of schooling and voter turnout when employing compulsory schooling laws as instruments using data from 15 European countries. They do find a positive effect when it comes to individuals' capacity to gather information on political issues, however.

Using a compulsory schooling reform that extended schooling from 7 to 9 years as an instrument for education in Norway, Pelkonen (2012) does not find a significant causal effect on voting.

Investigating additional outcomes including political interest, participation in political discussions and demonstrations as well as other kinds of political actions, the only significant causal effect he finds is a positive effect of schooling on the probability of signing a petition. For Sweden, Lindgren et al. (2017) examine the Swedish mass education expansion that was launched in the 1950s. Results from a difference-in-differences setup imply that this reform helped to reduce the social bias, i.e. the importance of family background, in political recruitment. Again for Sweden, the study by Lindgren et al. (2019) exploit exogenous variation in education that was induced by a pilot scheme preceding a large educational reform design implemented in the early 1990s. In contrast to the findings by Lindgren et al. (2017), the results from their combined difference-in-differences and IV framework suggest no causal effect of education on political participation. In their analysis on political discontent and emigration intentions, Gevrek et al. (2021) find a positive effect of schooling on the intention to emigrate from Turkey, employing an extension from 5 to 8 years of compulsory schooling as a source of exogenous variation. Their findings indicate that increased dissatisfaction with the political situation in Turkey serves as a prime mechanism.

Similar to the mixed evidence for developed countries, results for less developed countries are not clear-cut: Dang (2019), for example, exploits a compulsory schooling reform in Vietnam that induced children younger than 15 to complete primary education. His results imply a causal effect of education on political interest and participation. Parinduri (2019) uses exogenous variation in education caused by a one-time extension of primary schooling in Indonesia at the end of the 1970s. He finds no evidence that education affects voter turnout.

As outlined before, we are aware of only one paper for Germany, in which the empirical approach allows a conclusion on causal effects. 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 types 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 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. As mentioned before, available survey data for Germany at that time did not provide information on where and when the individual went to school. This means that one could not accurately say whether the individual was affected by the reform. However, this is important in order to avoid a wrong assignment of persons to the reform and thus a potentially

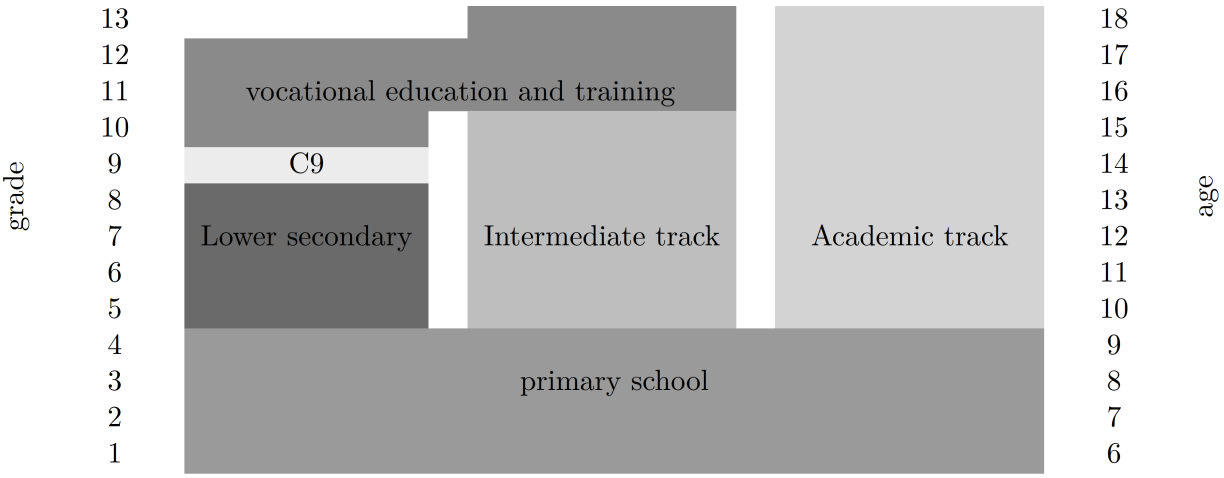
severe measurement error in this crucial variable.

We therefore add to the evidence on Germany by using data that is better suited to the analysis of the research question. Moreover, in addition to the standard information on educational attainment, as measured by certificates, the data include detailed educational biographies. This makes it possible to compare the results of analyses that use one of the indicators each as measure of the individual's length of schooling.

2.3 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 2.1), however, were in general comparable across the federal states at that time and to a large extent still are today:

Figure 2.1: West German educational system (stylized)



Source: KMK (2017), own illustration.

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 2.1 provides an overview of the variation of its implementation over time and federal state:

Table 2.1: Implementation of the 9th compulsory school year

Federal State	school year	Pischke and von Wachter (2005), Pischke and von Wachter (2008)
Bremen	Before 1949/1950	1958
Hamburg	Before 1949/1950	1949
West Berlin	Before 1949/1950	-
Schleswig Holstein	Before 1949/1950	1956
Lower Saxony*	1962/1963	1962
North Rhine-Westphalia	1966	1967
Hesse** (urban municipalities)	1966 (1962/1963)	1967
Rhineland-Palatinate	1966	1967
Baden-Wuerttemberg	1965/1966	1967
Bavaria	1968/1969	1969
Saarland***	1966	1964

Notes:
* Lower Saxony: gradual implementation of the reform between 1954/1955 and 1962/1963.
** Hesse: first implementation of the reform in urban municipalities from 1962/1963 on.
*** Saarland: gradual implementation of the reform between 1958/1959 and 1966.
Source: Helbig and Nikolai (2015); Cygan-Rehm (2022); Cygan-Rehm and Maeder (2013)

The first study in economics that exploited the extension of compulsory schooling in Germany, which has been widely used as a source of information on the reform, is that of Pischke and von 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 the Helbig and Nikolai (2015) scheme[3] because we consider their sources to be more reliable, but we also check for differences in results when using the Pischke and von Wachter (2008) scheme as a robustness check.

Table 2.1 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

[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 1968/1969 (Figure A2.1 in the Appendix), yet Pischke and von Wachter (2008), as well as Cygan-Rehm (2022) use the same timing for the reform in Bavaria.

the reform were 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 and 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 1962/1963. 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. In line with prior research (Pischke and von Wachter (2005, 2008)), we address this by using the time at which all individuals were affected by the reform, 1962/1963 for Lower Saxony, and 1966 for Saarland.

Another relevant issue is that students of lower secondary schools were the target group 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 (2022). Students in higher secondary schooling, on the other hand, were not affected directly, because they were anyway schooled for a longer time (see also Cygan-Rehm (2022); Pischke and von Wachter (2008)). We, therefore, label lower and intermediate secondary school students as the target group of the reform, and we conduct all analyses for both the full sample and the subsample of targets.

In addition to the compulsory schooling reform, a second major reform occurred in 1966/1967. All federal states except for Bavaria[4] moved the start of the school year from spring to autumn (Helbig and 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,

[4] In Bavaria, the school year already started in autumn.

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 accounting for grade-level repetition and by excluding individuals who repeated at least one grade level.
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 Cygan-Rehm (2022), Pischke and von Wachter (2005, 2008), and Siedler (2010), 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 in previous studies. Additionally, as mentioned before, we conduct comparisons for analyses in which the length of schooling is calculated based on individuals' educational certificates or educational biographies.

2.4 Data and Empirical Strategy

2.4.1 Data

We use data from the National Educational Panel Study (NEPS)[5], 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 2013/2014.
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)
- Political interest: extent of interest in political issues (4-point Likert type scale)
- Internal political efficacy: individuals' perception of (not) being able to understand politics (5-point Likert type scale)

[5] This paper uses data from the National Educational Panel Study (NEPS): Starting Cohort Adults, doi:10.5157/NEPS:SC6:9.0.1. From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network. For further information about the National Educational Panel Study see also Blossfeld and Roßbach (2019) and NEPS Network (2018).

In addition to the binary indicators (having signed a petition, having participated in a demonstration), 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[6] is measured as the frequency with which individuals have difficulties in following current political debates on a 5-point Likert type scale from (1) "never" to (5) "often", so that respondents who confirm often having such difficulties are considered as having low internal political efficacy. We inverted the scale to gain a more intuitive interpretability.

The NEPS also provides self-reported voting behavior. We do not use this in our analyses because of substantial overreporting (over 90% in the NEPS versus over 70% in official statistics (Bundeswahlleiter (2010))). Instead, we link the official voter turnout (BBSR (2021)) for the last federal election at the county level to the NEPS data.[7]

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. Kemptner et al. (2011); Pischke and von Wachter (2008)).

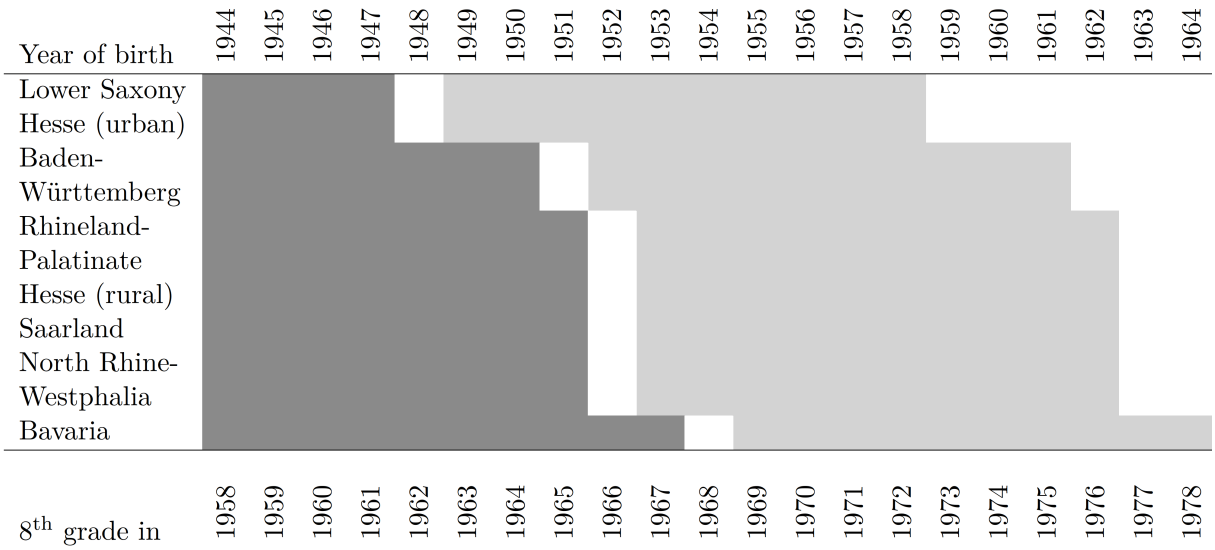
Figure 2.2 shows the sample cut of the NEPS data we use. The NEPS cohort on adults provides observations from individuals born between 1944 and 1986. The C9 reform was implemented before 1949/1950 in Schleswig-Holstein, and the city federal states of Bremen, Hamburg, and West Berlin. As there is no variation 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.[8] As the reform took place in

[6] 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).

[7] The NEPS surveys of this wave were conducted before and after the 2013 federal election, which took place on September 22, 2013. For interviews conducted before the election, we linked turnout in the previous 2009 federal election, and for interviews conducted after September 22, we linked turnout in this election. We account for this by including an additional control indicating whether the survey was conducted before or after the 2013 election. Since we are interested in voting behavior in general and control for other individual characteristics as well as for temporal and regional trends, the two different reference points should not pose a significant problem.

[8] We chose this time frame to maintain a sufficient number of observations. We conducted further analyses with narrower time bands, specifically 7 and 4 years before and after the reform, respectively. The results using the 7 year timeframe (shown in Table A2.1 in the Appendix) are very similar to our baseline results in Tables 2.3 and 2.4. For the 4 year frame however, the sample sizes get too small for reliable analyses (see Table A2.2 in the Appendix). Additionally, when using the self-reported schooling variable in the 4 year time band, F-values of the first stage do not exceed the critical value of 10 anymore.

Figure 2.2: NEPS Sample Cut



Notes: 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.
Source: Cygan-Rehm (2022); Cygan-Rehm and Maeder (2013); Helbig and Nikolai (2015), illustration based on Kroh (2021).

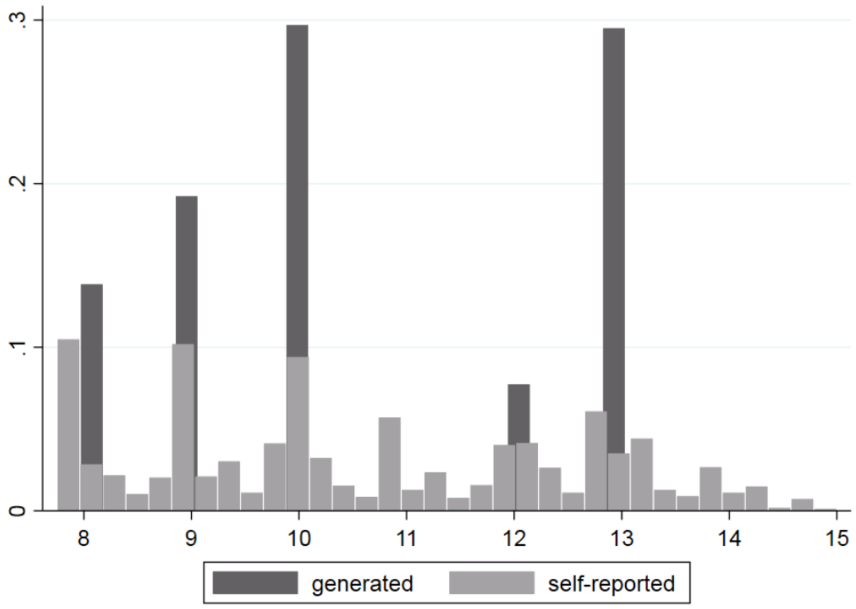
the western part of Germany only, we exclude respondents who attended grade 8 in the former GDR, as well as individuals not born in Germany. We also exclude individuals for whom the school leaving date remains unclear, for example, because of missing values or parallel biographic episodes. We also exclude individuals for whom we have information from educational biographies, but none on educational attainment.[9]

Figure 2.3 shows the distribution of the individuals' self-reported duration of schooling as well as the distribution of the schooling duration determined from their degrees. By construction, there is no variation in between the data points of the "synthetic" schooling durations. In contrast, the variation in the self-reported data around the generated indicators is quite large. The largest deviations between generated and self-reported length of schooling are visible for intermediate secondary schooling (10 years) and upper secondary schooling (13 years). The patterns may indicate that many individuals needed longer to attain either degree. However, this is not clear-cut, as there are also individuals who achieve their degrees more quickly than anticipated.

Figure 2.4 illustrates this quite clearly. It shows the deviations between the self-reported length of schooling and the one generated from educational certificates. Accordingly, the majority of individuals take up to one year more or less to complete the respective degree. This is quite

[9] This may be the case, for instance, for older individuals who went to school but did not graduate. The latter can also be the case for persons who attend a special school.

Figure 2.3: Generated and self-reported years of schooling (frequencies)



Source: NEPS SC6 9.0.1, own illustration.

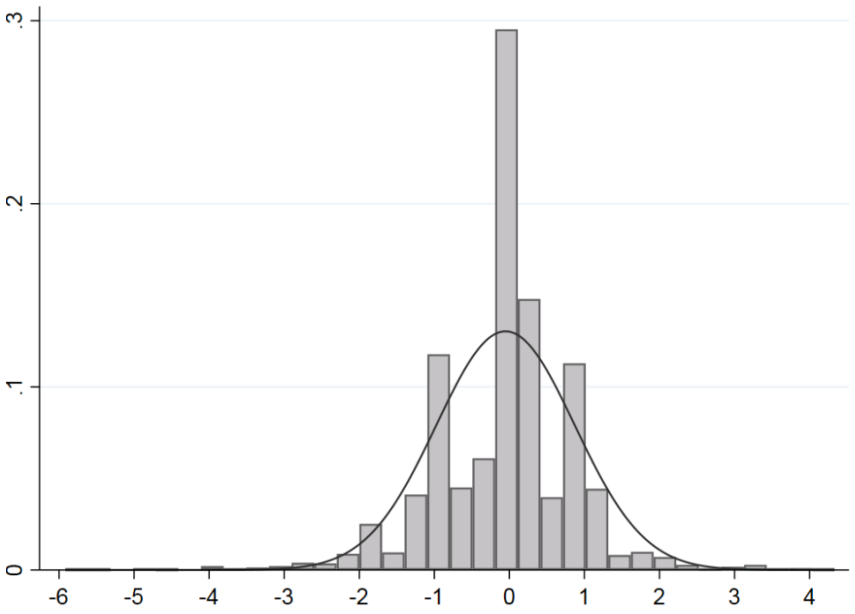
plausible, especially in the case that individuals have to repeat a grade. However, there are also individuals who are faster. Figure 2.4 also shows, that there also seem to be individuals that are much faster (up to four years) or much slower (up to six years). In these extreme cases, measurement error may play a stronger role. We account for this by trimming our sample to an absolute deviation of up to two years.[10] Applying all mentioned restrictions, around 2.700 observations remain for our analyses.

Table 2.2 shows descriptive statistics for our sample, and, in comparison, for the pre- and post-reform subsamples as well as for the target group. First, there is no general pattern of differences in political participation and interest between the pre- and post-reform groups: the post-reform individuals more likely signed petitions and have demonstrated a little more often, but they are less interested in political issues. However, pre- and post-reform individuals do not differ in whether they understand political debates, and there are no differences regarding voter turnout at the county level.

As for individuals' characteristics, the lower panel of Table 2.2 shows that the post-reform group has, on average, almost one additional year of schooling, irrespective of the indicator used (Table 2.2, column 4). The differences between the averages of the indicator, on the other hand, are only marginal. In addition, individuals in the post-reform group are about ten years younger on

[10] We lose another 102 observations doing this. We also repeated all estimations with cut-off points at one year (see Tables A2.3 and A2.4 in the Appendix) and 1,5 years (see Tables A2.5 and A2.6 in the Appendix). This does not change our main results, but the coefficients are less precisely estimated because of the smaller sample sizes.

Figure 2.4: Deviations between generated and self-reported years of schooling (frequencies)



Source: NEPS SC6 9.0.1, own illustration.

average and there are more women in this group.

There is more of a pattern when looking at the target group, i.e. individuals who were in lower or intermediate secondary school at the time of the reform (Table 2.2, column 6). On average, and compared to the individuals in upper secondary school (Table 2.2, column 5), there is less political engagement in all of the NEPS indicators.

As could be expected, the individuals also clearly differ in their average length of schooling, with about 12.6 years for upper secondary graduates and about 9.9 years for those who gratuated from lower or intermediate secondary schools. Gender composition and average age of the two groups, on the other hand, do not differ substantially.

Table 2.2: Descriptive statistics (pre-, post-reform, and target group)

	Full Sample	Pre-reform	Post-reform		Non-Target	Target	
	Mean (Std. Dev.)	Mean (Std. Dev.)	Mean (Std. Dev.)	Difference post - pre	Mean (Std. Dev.)	Mean (Std. Dev.)	Difference target - non target
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Signed a petition	0.72	0.65	0.75	0.10***	0.87	0.67	-0.19***
	(0.45)	(0.48)	(0.43)		(0.34)	(0.47)	
Demonstration participation	0.38	0.35	0.39	0.04**	0.58	0.31	-0.26***
	(0.49)	(0.48)	(0.49)		(0.49)	(0.46)	
Political interest	3.00	3.12	2.94	-0.18***	3.23	2.92	-0.29***
	(0.74)	(0.73)	(0.74)		(0.66)	(0.75)	
Internal political efficacy	3.14	3.12	3.15	0.03	3.45	3.04	-0.40***
	(0.97)	(1.00)	(0.96)		(0.87)	(0.98)	
Voter turnout	72.03	72.08	72.01	0.07	72.20	71.89	-0.23
	(3.38)	(3.44)	(3.35)		(3.42)	(3.37)	
Years of schooling (reported)	10.59	10.02	10.86	0.84***	12.57	9.93	-2.64***
	(1.91)	(2.08)	(1.77)		(1.08)	(1.66)	
Years of schooling (generated)	10.56	9.89	10.87	0.98***	12.61	9.88	-2.73***
	(1.85)	(2.00)	(1.69)		(1.02)	(1.53)	
Male	0.50	0.53	0.48	-0.05**	0.52	0.49	-0.03
	(0.50)	(0.50)	(0.50)		(0.50)	(0.50)	
Age	58.46	64.57	55.63	-8.94***	58.14	58.57	0.43*
	(5.25)	(2.76)	(3.40)		(5.35)	(5.22)	
N	2.716	860	1.856		682	2.034	

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sample sizes for voter turnout: full sample - N = 2.656; pre-reform - N = 857; post-reform - N = 1.799; target group (lower and middle secondary school) - N = 1.983; non-target group (upper secondary school) - N= 673.
Source: NEPS SC6 9.0.1, own calculations.

2.4.2 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.
The first stage of the IV regressions is as follows:

$$S_i = \gamma_0 + \gamma_1 Z_i + \gamma_2 age + \gamma_3 age^2 + \gamma_4 sex + \gamma_5 cohort + \gamma_6 state + \gamma_7 cohort * state + \gamma_8 cohort * age + \omega_i \tag{2.1}$$

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:

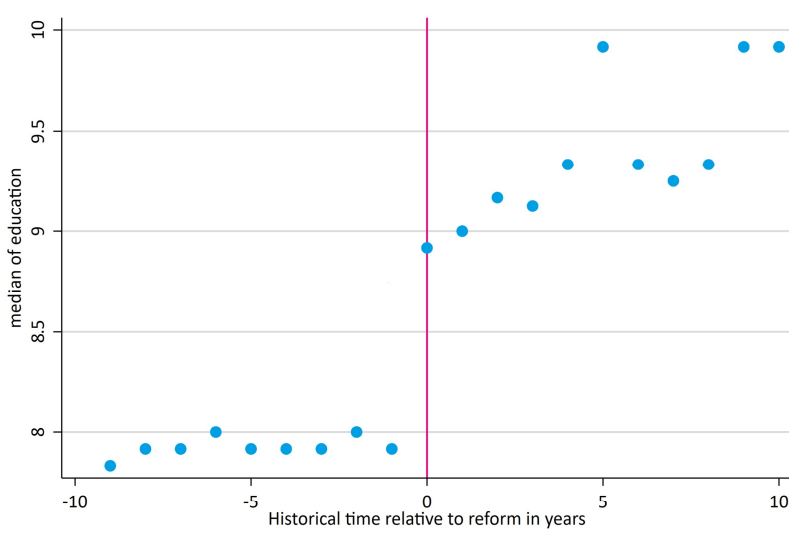
$$Pol_i = \beta_0 + \beta_1 \hat{S}_i + \beta_2 age + \beta_3 age^2 + \beta_4 sex + \beta_5 cohort + \beta_6 state + \beta_7 cohort * state + \beta_8 cohort * age + \epsilon_i \tag{2.2}$$

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 2.5. Prior to the reform, individuals on average spent close to eight years in school, which increased to nine years and more after the reform.

2.5 Results

Tables 2.3 and 2.4 provide the results of OLS and IV estimations, employing self-reported and generated education, respectively. There are several noteworthy findings: First, both sets of OLS estimations yield the well-documented positive correlation between education and political engagement as approximated by the NEPS items. This holds regardless of which education indicator is used and whether all observations or only the target individuals, i.e. individuals in the lower and the intermediate secondary schooling track, are included in the estimations (Tables

Figure 2.5: Median years of schooling before and after the reform for target students



Source: NEPS SC6 9.0.1, own illustration.

2.3 and 2.4, columns 1 and 2). Also, the size of the coefficients is roughly similar. An exception is voter turnout for the target group if self-reported education is used; there is no correlation here (Table 2.3, column 2).

These results are in line with several findings in the literature, particularly with the results of Siedler (2010), who finds quite similar patterns for political interest, signing petitions, participating in demonstrations, and (self-reported) voting behavior for the German context.

The results of the IV estimates, which represent the causal effect of schooling on political participation and interest, are less clear. There are differences in the findings between the two subsamples (all, targets) and between the sets of regressions, each using one of the two schooling indicators (self-reported, generated). First, results of the first stage regressions are in line with expectations, as well as with previous literature (Pischke and von Wachter (2005, 2008)), and in general, reinforce the pattern shown in Figure 2.5. They indicate that the compulsory schooling reform affected the average duration of schooling (Tables 2.3 and 2.4, columns 3 and 5).[11] However, the size of the coefficients, i.e. the additional time spent in school depends on whether one is referring to all students or the target group, and which educational indicator is used. The general pattern shows coefficients that are about 0.1 points larger if the generated schooling indicator instead of the self-reported information is used in the regressions. In particular, the first-stage regressions in Table 2.3 (columns 3 and 5) suggest that students who were affected by the reform stayed in school about 6.5 and 7.5 months longer, when based on self-reported

[11] In all our regressions, the F-values exceed the conventional weak instrument threshold of 10 for both groups.

Table 2.3: Political Participation and Interest, OLS and IV estimations, self-reported schooling

	OLS		IV			
	All	Targets	All		Targets	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.088***	0.081***	0.753***	0.078	0.661***	0.116**
	(0.006)	(0.008)	(0.139)	(0.051)	(0.145)	(0.058)
Signed a petition	0.068***	0.068***	0.753***	0.121**	0.661***	0.160**
	(0.005)	(0.007)	(0.139)	(0.048)	(0.145)	(0.065)
Political interest	0.104***	0.107***	0.753***	0.102	0.661***	0.053
	(0.007)	(0.011)	(0.139)	(0.073)	(0.145)	(0.105)
Internal political efficacy	0.137***	0.138***	0.753***	0.191**	0.661***	0.303**
	(0.010)	(0.014)	(0.139)	(0.097)	(0.145)	(0.139)
N	2.716	2.034	2.716	2.716	2.034	2.034
Voter turnout	0.086**	0.073	0.747***	0.540*	0.650***	0.848**
	(0.034)	(0.049)	(0.140)	(0.278)	(0.144)	(0.396)
N	2.656	1.983	2.656	2.656	1.983	1.983

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table 2.4: Political Participation and Interest, OLS and IV estimations, generated schooling

	OLS		IV			
	All	Targets	All		Targets	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.087***	0.081***	0.960***	0.061	0.862***	0.089**
	(0.005)	(0.008)	(0.140)	(0.040)	(0.136)	(0.044)
Signed a petition	0.068***	0.070***	0.960***	0.095**	0.862***	0.123**
	(0.005)	(0.006)	(0.140)	(0.037)	(0.136)	(0.048)
Political interest	0.102***	0.105***	0.960***	0.080	0.862***	0.041
	(0.007)	(0.011)	(0.140)	(0.058)	(0.136)	(0.081)
Internal political efficacy	0.136***	0.140***	0.960***	0.150*	0.862***	0.232**
	(0.010)	(0.014)	(0.140)	(0.078)	(0.136)	(0.108)
N	2.716	2.034	2.716	2.716	2.034	2.034
Voter turnout	0.095***	0.113**	0.955***	0.423**	0.853***	0.647**
	(0.033)	(0.050)	(0.142)	(0.215)	(0.137)	(0.292)
N	2.656	1.983	2.656	2.656	1.983	1.983

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

schooling episodes, but between 8.5 and 9.5 months of additional schooling if the regressions are based on the generated years of schooling (Table 2.4 (columns 3 and 5).[12] [13]

Turning to the second stage results, Tables 2.3 and 2.4 (columns 4 and 6) show that the C9 reform had a causal effect on all indicators of individuals' political interest and participation, except for political interest and for demonstration participation for the larger sample (Tables 2.3 and 2.4, column 4). We also see that, regardless of the indicator used for years of schooling, the effects are larger for students in lower and intermediate schools. In particular, the size of the coefficients implies that the additional schooling increased the individuals' likelihood of demonstrating by nine to 12 percent (Tables 2.3 and 2.4, column 6) and the likelihood of signing petitions by about 12 to 16 percent (Tables 2.3 and 2.4, column 6). Individuals' internal political efficiency increases by about 0.2 and 0.3 scale points, i.e. increases of about five to 7.5 percent. Finally, our results show that turnout at the county level did increase among the target group, but only weakly, by about 0.65 to 0.85 percentage points.

In sum, we find causal effects of education on political participation and interest. This contradicts the findings of Siedler (2010), who found no causal effects, based on ALLBUS and ForsaBus data. There is likely more than one reason for this, but we suppose that the detailed educational biographies provided by NEPS, play an important role: Unlike ALLBUS and ForsaBus data, which are limited in this regard, NEPS data allow us to tell exactly where students were living at the time of the eighth-grade reform, so we can better identify those who were affected by it. Another aspect is, that we drop individuals from our sample for which we suspect large measurement errors as proxied by the deviations between the generated and self-reported length of schooling. If there is only the possibility to work with generated years of schooling, this may lead to flawed results.[14] We also run additional analyses to replicate the findings of Siedler (2010) and will comment on this in the following Section.

In the next step, we turn to the possibility that we can use the NEPS data to address the role of the deviations between self-reported and generated years of education more precisely. Whereas

[12] The first stage coefficients in all our regressions are larger for the whole sample than for the target group. This appears counterintuitive because students in the upper secondary school track were not affected by the reform. To some extent, this may be due to the time frame -10 years before and after the reform- we have to use to obtain an adequate sample size. It is difficult to say whether social developments and especially the expansion of education that began in the 1960s, drive this finding, especially since we control for birth cohorts in the regressions. In additional analyses, we find that the first stage coefficients converge at smaller time periods around the reform date.

[13] It is unclear to us whether the different variations of the schooling duration as shown in Figure 2.3 cause the differences in coefficients. However, we will not go into more detail here, as this would go beyond the scope of this paper.

[14] In a prior version of our paper (Bömmel and Heineck (2020a,b,c)), and contrary to in Bömmel and Heineck (2022), we did not remove the observations that we suppose are likely prone to measurement error and did not find any causal effects for this sample.

we accounted for positive and negative deviations in the first regressions by using dummies, we now present results from estimations that include the (absolute) values of the deviations. We focus on the target group of lower and intermediate secondary school students and present estimates from OLS and IV, second stage only, regressions using either self-reported or generated length of schooling in Table 2.5.[15]

The OLS results repeat the patterns of positive correlations between schooling and political engagement (Table 2.5, columns 1 and 3). The coefficients are about the same as those in the OLS regressions that account for the deviations by including dummies (Tables 2.3 and 2.4). There are however more interesting findings: first, the coefficients are identical, but this is only the result of accounting for both positive and negative deviations between generated and self-reported schooling duration. It is more noteworthy that individuals who graduated faster than their achievement would need are more likely participating in demonstrations and signing petitions. In addition, people who took longer to complete their education are less likely to demonstrate, sign petitions, be politically interested, and more likely to find politics difficult. This holds only in the case of self-reported schooling (Table 2.5, column 1), but not when using the generated indicator (Table 2.5, column 3). The latter is however plausible, again because of how we constructed the deviations.

The correlations illustrate the role of education and whether individuals earned their degrees more quickly or more slowly, but do not reflect causal effects. The IV estimates then reconfirm the associations as being causal for some, but not all schooling coefficients mentioned for the OLS regressions. In particular, we see causal effects of schooling on signing petitions, internal political efficacy, and voter turnout, but no effect on participation in demonstrations (Table 2.5, columns 2 and 4). The coefficients in the IV estimates for the variances between generated and self-reported schooling are essentially the same as in the OLS regressions, implying higher (lower) political participation and interest for individuals who graduated from high school faster (slower) than required. However, we refrain from interpreting these as causal effects because we used the C9 reform to instrument school duration alone.

[15] The results for the larger sample shown in Table A2.7 in the Appendix represent the same pattern.

Table 2.5: Political Participation and Interest, OLS and IV estimations for target group indviduals, self-reported and generated schooling

	Self-reported		Generated	
Dependent variable	OLS	IV (2nd stage)	OLS	IV (2nd stage)
	(1)	(2)	(3)	(4)
Demonstration participation	0.081***	0.096	0.081***	0.096
	(0.008)	(0.059)	(0.008)	(0.059)
Positive Deviation	*0.079***	*0.079***	*-0.001*	*-0.018*
	(0.032)	*(0.032)*	*(0.033)*	*(0.072)*
|*Negative Deviation*|	*-0.048**	*-0.072*	*0.032*	*0.024*
	(0.027)	*(0.093)*	*(0.026)*	*(0.040)*
Signed a petition	0.071***	0.139**	0.071***	0.139**
	(0.007)	(0.064)	(0.007)	(0.064)
Positive Deviation	*0.062***	*0.058***	*-0.009*	*-0.081*
	(0.026)	*(0.028)*	*(0.028)*	*(0.073)*
|*Negative Deviation*|	*-0.079****	*-0.183**	*-0.007*	*-0.043*
	(0.025)	*(0.101)*	*(0.023)*	*(0.041)*
Political interest	0.108***	0.037	0.108***	0.037
	(0.011)	(0.107)	(0.011)	(0.107)
Positive Deviation	*0.055*	*0.060*	*-0.052*	*0.022*
	(0.045)	*(0.045)*	*(0.047)*	*(0.130)*
|*Negative Deviation*|	*-0.125****	*-0.017*	*-0.017*	*0.020*
	(0.043)	*(0.175)*	*(0.038)*	*(0.074)*
Internal political efficacy	0.148***	0.281**	0.147***	0.281**
	(0.014)	(0.138)	(0.014)	(0.138)
Positive Deviation	*0.035*	*0.027*	*-0.113***	*-0.254*
	(0.057)	*(0.058)*	*(0.057)*	*(0.156)*
|*Negative Deviation*|	*-0.215****	*-0.420**	*-0.067*	*-0.138*
	(0.055)	*(0.235)*	*(0.051)*	*(0.104)*
N	2.034	2.034	2.034	2.034
Voter turnout	0.107**	0.709*	0.107**	0.709*
	(0.049)	(0.395)	(0.049)	(0.395)
Positive Deviation	*0.285*	*0.254*	*0.177*	*-0.455*
	(0.184)	*(0.204)*	*(0.176)*	*(0.493)*
|*Negative Deviation*|	*-0.033*	*-0.958*	*0.074*	*-0.249*
	(0.175)	*(0.624)*	*(0.158)*	*(0.263)*
N	1.983	1.983	1.983	1.983

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), positive deviation between generated and self-reported length of schooling, negative deviation in absolute values.
Source: NEPS SC6 9.0.1, own calculations.

2.6 Robstness Checks

As a first 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. He further uses individuals' birth years to address the birth cohort first affected by the reform, with the youngest birth cohort in his sample being born in 1960. To account for the differences between the implementation in different federal states he has to use the individuals' current state of residence.[16] This means that he also needs to drop observations of individuals living in the East German federal states, even though they may have completed their schooling in former West Germany. Applying these restrictions does not change the results (see Table 2.6): We again find statistically significant and positive correlations between education and the political outcome measures, but there are no causal effects.[17]

Table 2.6: Replication of Siedler (2010) employing his restrictions

	OLS	IV	
Dependent variable		First stage	Second stage
	(1)	(2)	(3)
Demonstration participation	0.061***	0.762***	-0.023
	(0.007)	(0.175)	(0.074)
Signed a petition	0.053***	0.762***	-0.074
	(0.007)	(0.175)	(0.081)
Political interest	0.102***	0.762***	-0.087
	(0.010)	(0.175)	(0.127)
N	1.678	1.678	1.678
Voter turnout	0.094*	0.757***	0.434
	(0.042)	(0.175)	(0.557)
N	1.673	1.673	1.673

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, sex, year of birth, current state of residence, birth cohort, interaction terms between birth cohort and current state of residence.
Additional restrictions: without individuals holding a university or technical college degree, birth cohorts after 1960, and individuals currently living in East Germany.
Source: NEPS SC6 9.0.1, own calculations.

[16] As mentioned earlier, this can lead to measurement error in the first stage estimations. We perform reduced form estimations and find that the R^2 value is much lower ($R^2 = 0.0087$) compared to the R^2 values when using the information on the actual place of graduation ($R^2 = 0.0617$ for self-reported schooling, and $R^2 = 0.0422$ for the generated indicator).

[17] This overall picture further does not change if we use individuals' self-reported schooling duration or if we additionally include the deviations between generated and self-reported schooling. Results for these additional analyses are depicted in Tables A2.8 and A2.9 in the Appendix.

We run further analyses to address some of the specifics of the reform. First, we address grade repetition. 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 in general account for this in our main estimations by including a dummy on whether the respondent indicated that he or she repeated a class. We additionally rerun our analyses with a sample of target students that excludes all individuals who repeated at least one school year.

The IV results (Table A2.10 in the Appendix) are quite similar to those in Table 2.5[18]: we see causal effects of schooling on signing petitions and on internal political efficacy, but not anymore on voter turnout, which however might be a question of statistical power. The coefficients for the (absolute) deviations again imply that faster (slower) individuals are more (less) likely politically active. However, not all coefficients are statistically different from zero.

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 the timing definition by Pischke and von Wachter (2008), we rerun our analyses using their framework for another robustness check. The results are depicted in Tables A2.12 and A2.13 in the Appendix. Similar to our replication of Siedler (2010), these analyses do not yield causal effects for the overall sample. Within the sample of targets, we see causal effects on the likelihood of participating in demonstrations and signing petitions.

Finally, and as mentioned before, it is not possible to properly consider the transitional reform arrangements for the federal states of Saarland and Lower Saxony. We thus rerun our analyses without observations from these two federal states in Table A2.14 in the Appendix. Whereas the OLS estimations again show the typical association, the IV estimation only shows significant causal effects on signing petitions for the self-reported and generated schooling indicator as well as a positive effect on voter turnout if the generated schooling variable is used. However, the F-values of the first-stage regressions fail to meet the conventional weak instrument threshold of 10 for voter turnout using the self-reported schooling indicator (F = 9.95) and is also very close for the other outcomes (F = 10.43).

Overall, the robustness checks suggest that the identification of the causal effect of this particular reform depends - as is to be expected in principle - on the correct implementation of the timing of the reform and the correct identification of the individuals affected by the reform.

[18] In Table A2.11 in the Appendix, we also rerun the analyses with the full sample.

2.7 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 open, as the results of previous studies are ambiguous. We add to this literature by using the exogenous variation in schooling from a compulsory schooling reform at the lower secondary schooling level in Germany implemented in the western federal states after World War II.

Our results not only show positive correlations between education and our outcome variables - participation in political actions, political interest, internal political efficacy, and voter turnout at the county level but indicate to some extent a causal impact, particularly for the target group of lower and intermediate secondary school students. This is in contrast to prior findings for Germany which implies no causal impact (Siedler (2010)). However, we are able to employ some improvements: first, we use an improved timing scheme for the implementation of the reform as introduced by Helbig and Nikolai (2015). Second, the NEPS data provide detailed educational biographies. In this way, it is possible to determine who was affected by the reform without having to resort to assumptions. Further, it is possible to compare the results of analyses that do not have to use only the conventional indicator of years of schooling derived from educational attainment, but also the actual length of time individuals spent in school as calculated from schooling episodes.

We can therefore account for deviations between generated and self-reported years of schooling, which points to individuals who graduated faster or slower from school than their degree would imply. To some extent, this may reflect individuals' competency endowment. Our results would then additionally indicate a positive association between skills and political participation and interest.

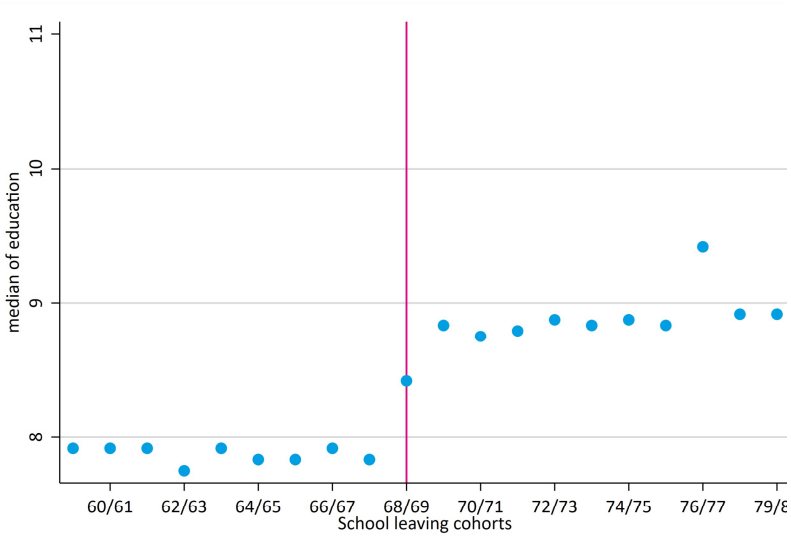
One constraint of our study is that we do not know how subject contents changed with the compulsory schooling reform, and what content was taught in the additional schooling year. According to Pischke and von Wachter (2005), the curricula for the additional 9^{th} grade were different across federal 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. This may be an interesting avenue for further analyses of the consequences of this particular schooling reform.

In general, the investigation of the qualitative dimension of education, i.e. an exploration of what

material is taught, is currently to a great extent missing in the economic 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.

Appendix

Figure A2.1: Median years of schooling for different school leaving cohorts in Bavaria



Source: NEPS SC6 9.0.1, own illustration.

Table A2.1: Political Participation and Interest, OLS and IV estimations for the full sample, restricted to time frame of 7 years before/after the reform

Dependent variable	Self-reported			Generated		
	OLS	IV		OLS	IV	
		1st stage	2nd stage		1st stage	2nd stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.090***	0.674***	0.105*	0.087***	0.866***	0.082*
	(0.006)	(0.147)	(0.059)	(0.006)	(0.150)	(0.045)
Signed a petition	0.071***	0.674***	0.135**	0.070***	0.866***	0.105**
	(0.006)	(0.147)	(0.060)	(0.005)	(0.150)	(0.046)
Political interest	0.107***	0.674***	0.144	0.104***	0.866***	0.112
	(0.009)	(0.147)	(0.101)	(0.009)	(0.150)	(0.079)
Internal political efficacy	0.139***	0.674***	0.261**	0.139***	0.866***	0.203**
	(0.011)	(0.147)	(0.120)	(0.011)	(0.150)	(0.096)
N	1.966	1.966	1.966	1.966	1.966	1.966
Voter turnout	0.087**	0.666***	0.639*	0.099**	0.860***	0.495*
	(0.039)	(0.149)	(0.352)	(0.038)	(0.151)	(0.269)
N	1.931	1.931	1.931	1.931	1.931	1.931

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.2: Political Participation and Interest, OLS and IV estimations for the full sample, restricted to time frame of 4 years before/after the reform

Dependent variable	Self-reported			Generated		
	OLS	IV		OLS	IV	
		1st stage	2nd stage		1st stage	2nd stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.098***	0.466**	0.110	0.096***	0.639***	0.080
	(0.008)	(0.191)	(0.098)	(0.007)	(0.198)	(0.071)
Signed a petition	0.076***	0.466**	0.123	0.075***	0.639***	0.090
	(0.008)	(0.191)	(0.105)	(0.008)	(0.198)	(0.075)
Political interest	0.114***	0.466**	0.223	0.111***	0.639***	0.162
	(0.011)	(0.191)	(0.179)	(0.011)	(0.198)	(0.129)
Internal political efficacy	0.124***	0.466**	0.183	0.126***	0.639***	0.133
	(0.016)	(0.191)	(0.217)	(0.015)	(0.198)	(0.162)
N	1.084	1.084	1.084	1.084	1.084	1.084
Voter turnout	0.067	0.465**	1.204	0.069	0.637***	0.880*
	(0.051)	(0.193)	(0.775)	(0.054)	(0.201)	(0.520)
N	1.072	1.072	1.072	1.072	1.072	1.072

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.3: Political Participation and Interest, OLS and IV estimations, generated schooling (absolute deviation up to one year)

	OLS		IV			
	All	Targets	All		Targets	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.084***	0.076***	1.204***	0.051	1.066***	0.083*
	(0.006)	(0.009)	(0.137)	(0.036)	(0.113)	(0.043)
Signed a petition	0.069***	0.070***	1.204***	0.095***	1.066***	0.122***
	(0.005)	(0.008)	(0.137)	(0.031)	(0.113)	(0.041)
Political interest	0.103***	0.113***	1.204***	0.080*	1.066***	0.048
	(0.008)	(0.014)	(0.137)	(0.048)	(0.113)	(0.067)
Internal political efficacy	0.137***	0.144***	1.204***	0.142**	1.066***	0.209**
	(0.011)	(0.018)	(0.137)	(0.069)	(0.113)	(0.093)
N	2.282	1.707	2.282	2.282	1.707	1.707
Voter turnout	0.102***	0.120**	1.198***	0.416**	1.054***	0.499*
	(0.034)	(0.049)	(0.140)	(0.190)	(0.115)	(0.273)
N	2.237	1.670	2.237	2.237	1.670	1.670

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.4: Political Participation and Interest, OLS and IV estimations, self-reported schooling (absolute deviation up to one year)

	OLS		IV			
	All	Targets	All		Targets	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.086***	0.075***	0.987***	0.062	0.842***	0.105*
	(0.006)	(0.009)	(0.135)	(0.044)	(0.117)	(0.054)
Signed a petition	0.069***	0.067***	0.987***	0.116***	0.842***	0.154***
	(0.005)	(0.008)	(0.135)	(0.039)	(0.117)	(0.054)
Political interest	0.105***	0.115***	0.987***	0.098*	0.842***	0.061
	(0.008)	(0.013)	(0.135)	(0.059)	(0.117)	(0.085)
Internal political efficacy	0.139***	0.143***	0.987***	0.174**	0.842***	0.265**
	(0.012)	(0.018)	(0.135)	(0.083)	(0.117)	(0.117)
N	2.282	1.707	2.282	2.282	1.707	1.707
Voter turnout	0.101***	0.105**	0.980***	0.508**	0.831***	0.634*
	(0.036)	(0.050)	(0.138)	(0.233)	(0.118)	(0.350)
N	2.237	1.670	2.237	2.237	1.670	1.670

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.5: Political Participation and Interest, OLS and IV estimations, generated schooling (absolute deviation up to 1.5 years)

	OLS		IV			
	All	Targets	All		Targets	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.087***	0.082***	0.959***	0.061	0.828***	0.087*
	(0.005)	(0.008)	(0.143)	(0.042)	(0.139)	(0.049)
Signed a petition	0.068***	0.070***	0.959***	0.092**	0.828***	0.122***
	(0.005)	(0.007)	(0.143)	(0.040)	(0.139)	(0.056)
Political interest	0.103***	0.108***	0.959***	0.076	0.828***	0.029
	(0.007)	(0.012)	(0.143)	(0.060)	(0.139)	(0.087)
Internal political efficacy	0.137***	0.143***	0.959***	0.159**	0.828***	0.254**
	(0.011)	(0.015)	(0.143)	(0.077)	(0.139)	(0.110)
N	2.584	1.929	2.584	2.584	1.929	1.929
Voter turnout	0.082***	0.098**	0.954***	0.509**	0.818***	0.732**
	(0.034)	(0.049)	(0.145)	(0.232)	(0.140)	(0.322)
N	2.530	1.884	2.530	2.530	1.884	1.884

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.6: Political Participation and Interest, OLS and IV estimations, self-reported schooling (absolute deviation up to 1.5 years)

	OLS		IV			
	All	Targets	All		Targets	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.089***	0.082***	0.767***	0.076	0.630***	0.114*
	(0.006)	(0.008)	(0.146)	(0.052)	(0.147)	(0.064)
Signed a petition	0.068***	0.069***	0.767***	0.115***	0.630***	0.160**
	(0.005)	(0.007)	(0.146)	(0.051)	(0.147)	(0.074)
Political interest	0.105***	0.109***	0.767***	0.095	0.630***	0.038
	(0.008)	(0.012)	(0.146)	(0.074)	(0.147)	(0.114)
Internal political efficacy	0.139***	0.142***	0.767***	0.199**	0.630***	0.333**
	(0.011)	(0.015)	(0.146)	(0.095)	(0.147)	(0.144)
N	2.584	1.929	2.584	2.584	1.929	1.929
Voter turnout	0.081**	0.077	0.759***	0.640**	0.618***	0.967**
	(0.035)	(0.048)	(0.148)	(0.302)	(0.147)	(0.450)
N	2.530	1.884	2.530	2.530	1.884	1.884

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.7: Political Participation and Interest, OLS and IV estimations for the full sample, self-reported and generated schooling

	Self-reported		Generated	
Dependent variable	OLS	IV (2nd stage)	OLS	IV (2nd stage)
	(1)	(2)	(3)	(4)
Demonstration participation	0.087***	0.060	0.087***	0.060
	(0.006)	(0.050)	(0.006)	(0.050)
Positive Deviation	*0.075****	*0.079****	*-0.012*	*0.019*
	(0.028)	*(0.029)*	*(0.029)*	*(0.066)*
|*Negative Deviation*|	*-0.064***	*-0.026*	*0.023*	*0.034*
	(0.025)	*(0.078)*	*(0.024)*	*(0.033)*
Signed a petition	0.068***	0.101**	0.068***	0.101**
	(0.005)	(0.047)	(0.005)	(0.047)
Positive Deviation	*0.065****	*0.061***	*-0.003*	*-0.041*
	(0.023)	*(0.025)*	*(0.024)*	*(0.059)*
|*Negative Deviation*|	*-0.077****	*-0.124**	*-0.009*	*-0.022*
	(0.020)	*(0.069)*	*(0.019)*	*(0.027)*
Political interest	0.102***	0.082	0.102***	0.082
	(0.007)	(0.073)	(0.007)	(0.073)
Positive Deviation	*0.081***	*0.084***	*-0.022*	*0.002*
	(0.038)	*(0.040)*	*(0.039)*	*(0.096)*
|*Negative Deviation*|	*-0.112****	*-0.084*	*-0.010*	*-0.002*
	(0.033)	*(0.112)*	*(0.031)*	*(0.046)*
Internal political efficacy	0.138***	0.162*	0.138***	0.162*
	(0.010)	(0.096)	(0.010)	(0.096)
Positive Deviation	*0.083**	*0.080**	*-0.055*	*-0.082*
	(0.047)	*(0.047)*	*(0.047)*	*(0.115)*
|*Negative Deviation*|	*-0.159****	*-0.192*	*-0.021*	*-0.030*
	(0.042)	*(0.149)*	*(0.040)*	*(0.062)*
N	2.716	2.716	2.716	2.716
Voter turnout	0.093***	0.485*	0.093***	0.485*
	(0.034)	(0.279)	(0.034)	(0.279)
Positive Deviation	*0.184*	*0.127*	*0.091*	*-0.358*
	(0.158)	*(0.181)*	*(0.162)*	*(0.394)*
|*Negative Deviation*|	*0.111*	*-0.441*	*0.205*	*0.044*
	(0.152)	*(0.424)*	*(0.140)*	*(0.184)*
N	2.656	2.656	2.656	2.656

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: age, age^2, 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), positive deviation between generated and self-reported length of schooling, negative deviation in absolute values.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.8: Replication of Siedler (2010) employing his restrictions and our self reported schooling variable

	OLS	IV	
Dependent variable		First stage	Second stage
	(1)	(2)	(3)
Demonstration participation	0.058***	0.551***	-0.032
	(0.007)	(0.188)	(0.105)
Signed a petition	0.044***	0.551***	-0.102
	(0.007)	(0.188)	(0.114)
Political interest	0.088***	0.551***	-0.120
	(0.010)	(0.188)	(0.183)
N	1.678	1.678	1.678
Voter turnout	0.074*	0.544***	0.604
	(0.043)	(0.189)	(0.791)
N	1.673	1.673	1.673

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, sex, year of birth, current state of residence, birth cohort, interaction terms between birth cohort and current state of residence.
Additional restrictions: without individuals holding a university or technical college degree, birth cohorts after 1960, and individuals currently living in East Germany.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.9: Replication of Siedler (2010) employing his restrictions and including deviations between generated and self-reported schooling

	OLS	IV	
Dependent variable		First stage	Second stage
	(1)	(2)	(3)
Demonstration participation	0.067***	0.513***	-0.045
	(0.008)	(0.174)	(0.115)
Pos. Deviation	*-0.061***	*1.287****	*0.086*
	(0.028)	*(0.088)*	*(0.154)*
|Neg. Deviation|	*0.006*	*-0.136**	*-0.008*
	(0.015)	*(0.071)*	*(0.024)*
Signed a petition	0.054***	0.513***	-0.131
	(0.008)	(0.174)	(0.125)
Pos. Deviation	*-0.018*	*1.287****	*0.225*
	(0.028)	*(0.088)*	*(0.162)*
|Neg. Deviation|	*-0.009*	*-0.136**	*-0.034*
	(0.017)	*(0.071)*	*(0.026)*
Political interest	0.109***	0.513***	-0.152
	(0.010)	(0.174)	(0.208)
Pos. Deviation	*-0.089***	*1.287****	*0.252*
	(0.043)	*(0.088)*	*(0.290)*
|Neg. Deviation|	*0.028*	*-0.136**	*-0.062**
	(0.026)	*(0.071)*	*(0.036)*
N	1.678	1.678	1.678
Voter turnout	0.073	0.508***	0.511
	(0.045)	(0.174)	(0.822)
Pos. Deviation	*0.287*	*1.281****	*-0.284*
	(0.174)	*(0.086)*	*(1.045)*
|Neg. Deviation|	*-0.105*	*-0.138**	*0.163*
	(0.098)	*(0.071)*	*(0.174)*
N	1.673	1.673	1.673

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, sex, year of birth, current state of residence, birth cohort, interaction terms between birth cohort and current state of residence, positive deviation between generated and self-reported length of schooling, negative deviation in absolute values.
Additional restrictions: without individuals holding a university or technical college degree, birth cohorts after 1960, and individuals currently living in East Germany.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.10: Political Participation and Interest, OLS and IV estimations for target group indviduals, excluding individuals who repeated at least one school year

Dependent variable	Self-reported			Generated		
	OLS	IV		OLS	IV	
		1st stage	2nd stage		1st stage	2nd stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.075***	0.736***	0.086	0.075***	0.736***	0.086
	(0.009)	(0.149)	(0.055)	(0.009)	(0.149)	(0.055)
Pos. Deviation	*0.072***	*-0.022*	*0.072***	*-0.003*	*0.978****	*-0.014*
	(0.033)	*(0.121)*	*(0.033)*	*(0.035)*	*(0.121)*	*(0.068)*
|Neg. Deviation|	*-0.031*	*1.542****	*-0.048*	*0.044*	*0.542****	*0.038*
	(0.034)	*(0.082)*	*(0.091)*	*(0.031)*	*(0.082)*	*(0.043)*
Signed a petition	0.068***	0.736***	0.177***	0.068***	0.736***	0.177***
	(0.008)	(0.149)	(0.060)	(0.008	(0.149)	(0.060)
Pos. Deviation	*0.067****	*-0.022*	*0.065***	*-0.001*	*0.978****	*-0.112*
	(0.028)	*(0.121)*	*(0.032)*	*(0.030)*	*(0.121)*	*(0.070)*
|Neg. Deviation|	*-0.075***	*1.542****	*-0.241***	*0.007*	*0.542****	*-0.064*
	(0.032)	*(0.082)*	*(0.096)*	*(0.030)*	*(0.082)*	*(0.043)*
Political interest	0.109***	0.736***	0.068	0.109***	0.736***	0.068
	(0.012)	(0.149)	(0.096)	(0.012)	(0.149)	(0.096)
Pos. Deviation	*0.076*	*-0.022*	*0.077*	*-0.033*	*0.978****	*0.008*
	(0.051)	*(0.121)*	*(0.050)*	*(0.054)*	*(0.121)*	*(0.120)*
|Neg. Deviation|	*-0.128****	*1.542****	*-0.066*	*-0.019*	*0.542****	*0.003*
	(0.046)	*(0.082)*	*(0.158)*	*(0.041)*	*(0.082)*	*(0.069)*
Internal political efficacy	0.147***	0.736***	0.250*	0.147***	0.736***	0.250*
	(0.016)	(0.149)	(0.134)	(0.016)	(0.149)	(0.134)
Pos. Deviation	*0.027*	*-0.022*	*0.024*	*-0.120**	*0.978***	*-0.226*
	(0.060)	*(0.121)*	*(0.060)*	*(0.061)*	*(0.121)*	*(0.149)*
|Neg. Deviation|	*-0.234****	*1.542****	*-0.392**	*-0.087*	*0.542*	*-0.142*
	(0.061)	*(0.082)*	*(0.225)*	*(0.055)*	*(0.082)*	*(0.099)*
N	1.735	1.735	1.735	1.735	1.735	1.735
Voter turnout	0.115**	0.726**	0.574	0.115**	0.726***	0.574
	(0.053)	(0.150)	(0.381)	(0.053)	(0.150)	(0.381)
Pos. Deviation	*0.310*	*-0.032*	*0.304*	*0.195*	*0.968****	*-0.270*
	(0.197)	*(0.121)*	*(0.207)*	*(0.188)*	*(0.121)*	*(0.465)*
|Neg. Deviation|	*-0.024*	*1.542****	*-0.728*	*0.091*	*0.549*	*-0.154*
	(0.190)	*(0.083)*	*(0.606)*	*(0.076)*	*(0.083)*	*(0.267)*
N	1.692	1.692	1.692	1.692	1.692	1.692

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), positive deviation between generated and self-reported length of schooling, negative deviation in absolute values.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.11: Political Participation and Interest, OLS and IV estimations for the full sample, excluding individuals who repeated at least one school year

Dependent variable	Self-reported			Generated		
	OLS	IV		OLS	IV	
		1st stage	2nd stage		1st stage	2nd stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.082***	0.869***	0.047	0.082***	0.869***	0.047
	(0.006)	(0.156)	(0.045)	(0.006)	(0.156)	(0.045)
Pos. Deviation	*0.074***	*0.085*	*0.078***	*-0.008*	*1.085****	*0.031*
	(0.031)	*(0.128)*	*(0.031)*	*(0.032)*	*(0.128)*	*(0.062)*
|Neg. Deviation|	*-0.043*	*1.423****	*0.007*	*0.039*	*0.423****	*0.054*
	(0.030)	*(0.098)*	*(0.072)*	*(0.027)*	*(0.098)*	*(0.035)*
Signed a petition	0.067***	0.869***	0.129***	0.067***	0.869***	0.129***
	(0.005)	(0.156)	(0.049)	(0.005)	(0.156)	(0.049)
Pos. Deviation	*0.075****	*0.085*	*0.067***	*0.009*	*0.978****	*-0.061*
	(0.023)	*(0.128)*	*(0.027)*	*(0.025)*	*(0.121)*	*(0.063)*
|Neg. Deviation|	*-0.075***	*1.423****	*-0.262***	*-0.008*	*0.542****	*-0.033*
	(0.027)	*(0.098)*	*(0.074)*	*(0.025)*	*(0.082)*	*(0.033)*
Political interest	0.107***	0.869***	0.101	0.107***	0.869***	0.101
	(0.007)	(0.156)	(0.067)	(0.007)	(0.156)	(0.067)
Pos. Deviation	*0.096***	*0.085*	*0.097***	*-0.011*	*0.978****	*-0.004*
	(0.043)	*(0.128)*	*(0.044)*	*(0.044)*	*(0.121)*	*(0.093)*
|Neg. Deviation|	*-0.123****	*1.423****	*-0.114*	*-0.016*	*0.542****	*-0.014*
	(0.039)	*(0.098)*	*(0.107)*	*(0.036)*	*(0.082)*	*(0.048)*
Internal political efficacy	0.138***	0.869***	0.151	0.138***	0.869***	0.151
	(0.011)	(0.156)	(0.093)	(0.011)	(0.156)	(0.093)
Pos. Deviation	*0.080*	*0.085*	*0.078*	*-0.059*	*0.978***	*-0.073*
	(0.048)	*(0.128)*	*(0.049)*	*(0.049)*	*(0.121)*	*(0.114)*
|Neg. Deviation|	*-0.182****	*1.423****	*-0.201*	*-0.044*	*0.542*	*-0.050*
	(0.047)	*(0.098)*	*(0.146)*	*(0.044)*	*(0.082)*	*(0.063)*
N	2.314	2.314	2.314	2.314	2.314	2.314
Voter turnout	0.098**	0.863***	0.409	0.098**	0.863***	0.409
	(0.038)	(0.160)	(0.262)	(0.038)	(0.160)	(0.262)
Pos. Deviation	*0.252*	*0.084*	*0.212*	*0.153*	*1.084****	*-0.197*
	(0.171)	*(0.127)*	*(0.188)*	*(0.176)*	*(0.127)*	*(0.376)*
|Neg. Deviation|	*0.133*	*1.426****	*-0.305*	*0.231*	*0.426*	*0.104*
	(0.165)	*(0.099)*	*(0.411)*	*(0.153)*	*(0.099)*	*(0.192)*
N	2.263	2.263	2.263	2.263	2.263	2.263

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: age, age^2, 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), positive deviation between generated and self-reported length of schooling, negative deviation in absolute values.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.12: Political Participation and Interest, OLS and IV estimations, self-reported schooling, reform definition as in Pischke and von Wachter (2008)

	OLS		IV			
	All	Targets	All		Targets	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.088***	0.081***	0.668***	0.051	0.621***	0.143**
	(0.006)	(0.008)	(0.160)	(0.069)	(0.136)	(0.069)
Signed a petition	0.068***	0.068***	0.668***	0.061	0.621***	0.122*
	(0.005)	(0.007)	(0.160)	(0.053)	(0.136)	(0.064)
Political interest	0.104***	0.107***	0.668***	0.127	0.621***	0.108
	(0.007)	(0.011)	(0.160)	(0.078)	(0.136)	(0.101)
Internal political efficacy	0.137***	0.138***	0.668***	0.049	0.621***	0.099
	(0.010)	(0.014)	(0.160)	(0.114)	(0.136)	(0.144)
N	2.716	2.034	2.716	2.716	2.034	2.034
Voter turnout	0.086**	0.073	0.663***	0.201	0.610***	0.442
	(0.034)	(0.049)	(0.161)	(0.282)	(0.135)	(0.378)
N	2.656	1.983	2.656	2.656	1.983	1.983

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.13: Political Participation and Interest, OLS and IV estimations, generated schooling, reform definition as in Pischke and von Wachter (2008)

	OLS		IV			
	All	Targets	All		Targets	
			First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.087***	0.081***	0.829***	0.041	0.786***	0.113**
	(0.005)	(0.008)	(0.164)	(0.056)	(0.134)	(0.053)
Signed a petition	0.068***	0.070***	0.829***	0.049	0.786***	0.097*
	(0.005)	(0.006)	(0.164)	(0.043)	(0.134)	(0.050)
Political interest	0.102***	0.105***	0.829***	0.102	0.786***	0.085
	(0.007)	(0.011)	(0.164)	(0.064)	(0.134)	(0.080)
Internal political efficacy	0.136***	0.140***	0.829***	0.039	0.786***	0.078
	(0.010)	(0.014)	(0.164)	(0.093)	(0.134)	(0.116)
N	2.716	2.034	2.716	2.716	2.034	2.034
Voter turnout	0.095***	0.113**	0.826***	0.161	0.776***	0.348
	(0.033)	(0.050)	(0.166)	(0.227)	(0.134)	(0.298)
N	2.656	1.983	2.656	2.656	1.983	1.983

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Table A2.14: Political Participation and Interest, OLS and IV estimations for the full sample, excluding individuals from Lower Saxony and Saarland

Dependent variable	Self-reported			Generated		
	OLS	IV		OLS	IV	
		1st stage	2nd stage		1st stage	2nd stage
	(1)	(2)	(3)	(4)	(5)	(6)
Demonstration participation	0.086***	0.607***	0.107	0.085***	0.844***	0.077
	(0.006)	(0.188)	(0.077)	(0.006)	(0.189)	(0.054)
Signed a petition	0.070***	0.607***	0.164*	0.070***	0.844***	0.118*
	(0.005)	(0.188)	(0.088)	(0.005)	(0.189)	(0.061)
Political interest	0.105***	0.607***	0.088	0.104***	0.844***	0.063
	(0.008)	(0.188)	(0.134)	(0.007)	(0.189)	(0.097)
Internal political efficacy	0.140***	0.607***	0.171	0.140***	0.844***	0.123
	(0.011)	(0.188)	(0.174)	(0.010)	(0.189)	(0.129)
N	2.333	2.333	2.333	2.333	2.333	2.333
Voter turnout	0.103***	0.606***	0.894	0.115***	0.844***	0.642*
	(0.037)	(0.192)	(0.549)	(0.036)	(0.195)	(0.383)
N	2.277	2.277	2.277	2.277	2.277	2.277

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: age, age^2, 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), a dummy for grade level repetition, and dummies for the deviation between generated and self-reported education being larger or smaller than 0.
Source: NEPS SC6 9.0.1, own calculations.

Chapter 3

Trust in Media and COVID-19 related News - Evidence from reunified Germany and Europe

Anica Kramer, Nadja Bömmel and Guido Heineck

3.1 Introduction

A large literature documents long-lasting effects of socialist regimes on individual outcomes, among others, on preferences (Fuchs-Schündeln and Schündeln (2015), Alesina and Fuchs-Schündeln (2007), Friehe and Pannenberg (2020)), investment decisions on the stock market (Laudenbach et al. (2020)), saving behavior (Fuchs-Schündeln (2008)), gender attitudes and norms (Bauernschuster and Rainer (2012), Campa and Serafinelli (2019)), maternal labor supply (Boelmann et al. (2021)) or self-reliance (Bauernschuster et al. (2012)) and entrepreneurship (Falck et al. (2017)). Another outcome that received considerable attention is trust, which correlates positively, among others, with income (see Falk et al. (2018) for an overview).[1] The literature on the long-lasting effects of socialism on trust documents that individuals who grew up in a socialist regime display lower levels of social and institutional trust (Rainer and Siedler (2009), Heineck and Süssmuth (2013), Brosig-Koch et al. (2011)).

These differences in social and institutional trust between individuals living in former socialist versus non-former socialist countries help us understanding and explaining important challenges to the social fabric of countries around the globe, for instance, due to the 2015 "refugee crisis"[2] or due to the still ongoing COVID-19 pandemic. Concerning migrants and refugees, the existing empirical evidence shows that the lower levels of social trust of East Germans explain their higher concerns about immigration (see Lange (2021)).

In this paper, we re-visit the long-lasting effects of socialist regimes on trust but look specifically at trust in different media institutions. While this is an innovation and contribution to the literature on its own, as these outcomes have not been studied yet, our paper and findings are relevant for another current challenge: The 2019 and still ongoing COVID pandemic. In doing so, we motivate the importance of these outcomes (trust in different media institutions) by providing correlative evidence on their relationship with COVID patients in intensive care (ICU) and vaccination rates against COVID-19 at the aggregated country level. Both COVID-related measures are of high policy relevance. For instance, Ferranna et al. (2022) calculate figures that the net benefit of each additional vaccination is larger than 16,000 USD.[3] Our findings show that

[1] See for an overview of the individual determinants of trust, like age and gender, Sutter et al. (2019), for the effect of social interaction for instance Kosse et al. (2020), who investigate the participation in a mentoring program, on social preferences and trust and, for the role of culture, peers, and the environment (Ertac (2020)). Furthermore, Bjørnskov (2007) provides an excellent overview of the determinants of trust, both, on the individual and country characteristics level.

[2] The 2015 "refugee crisis" was not limited in its scope to Germany and had a huge impact on the European Union (see for instance Guild et al. (2015)) or the US (Ostrand (2015))

[3] Further, the IZA "Crisis Response Monitoring" (Eichhorst et al. (2021)) provides an excellent overview on the economic and social challenges of the COVID-19 pandemic (for instance concerning employment, earnings, and working conditions).

trust in traditional media correlates positively with COVID-19 vaccination rates and negatively with ICU patient rates. We do find the opposite for trust in social media: A higher share of the population that trusts in social media, correlates negatively with COVID-19 vaccination take-up and positively with ICU patient rates.

In detail, we investigate in this paper how the experience of growing up in a repressive state that allowed no or only limited access to free information, determines trust in different media institutions today. For our empirical analysis, we use cross-sectional data from surveys of the years 2017 and 2018 of the National Educational Panel Study (NEPS). We thereby exploit the separation and reunification of Germany as a quasi-natural experiment. This identification strategy has been frequently and successfully applied in the empirical literature (for instance by Alesina and Fuchs-Schündeln (2007), Lippmann et al. (2020), or Falck et al. (2017)). This allows us to investigate differences in trust between West and East Germans.

Further, we exploit another feature of the German case: The varying access to West German Television within East Germany. We use this second identification strategy for our analysis that can explain our findings. We follow the identification strategy of Bursztyn and Cantoni (2016), which states that solely based on geographical features, East Germans in different regions had differential access to Western television. This variation allows us to compare trust in media between West Germans and East Germans with West TV access as well as West Germans and East Germans without West TV access. We also use a sample of East Germans to identify the effect of West TV access on trust in media among East Germans.

Our results show that East Germans are less likely to trust in traditional media compared to West Germans. In detail, they are less likely to trust in the press by 12.9 percentage points and in TV by 5.9 percentage points. In contrast, East Germans are more likely to trust in social media institutions by 3.8 percentage points, such as Facebook or Twitter. These baseline findings for traditional and social media are robust across gender and rural versus urban areas. Concerning household income, we do not find any differences for traditional media. However, the finding that East Germans trust more in social media concentrates on the sample of individuals below the median household income.

Our main findings are robust to several robustness checks. First and importantly, we show that the differences between East and West Germans in trust in *different media institutions* are not just -or at least not only- a matter of lower trust of East Germans in *institutions* in general. In detail, we provide evidence on differences between East and West Germans with respect to other measures of trust, like trust in the parliament and government. The results

are partly insignificant and do not follow a clear pattern. Second, our results are robust to a number of changes to the specification, sample, and method. In detail, our results remain stable if we add regional controls to our baseline specification, code the outcome variables differently, use an ordered Probit instead of a binary Probit model as estimation method, or if we exclude outliers from our sample. Additionally, we replicate our study using Eurobarometer data for the EU-28 countries, comparing Europeans from former socialist and non-socialist countries. The corresponding results reinforce our main findings, as also here, individuals living in a former socialist country place less trust in the press and radio, but more in social media and the Internet. In general, we explain our findings by the exposure of East Germans to the socialist repressive regime they were growing up in. However, the historical setting of Germany allows us to exploit another feature: Within East Germany, individuals living in specific regions had access to West TV while others did not. The reception of West German TV was thereby solely determined by the geographical location, as telecommunications towers that were within reach of specific East German regions were built close to the inner German border in West Germany. This identification strategy has been first set up by Bursztyn and Cantoni (2016) and has then been extensively used in the literature (for instance Hennighausen (2015), Friehe et al. (2018) or Bönisch and Hyll (2015)). The empirical evidence we present documents that the effects we find in our main analysis (where we compare East against West Germans) double or even triple in magnitude if we compare West Germans with East Germans that did not have any access to West TV. In detail, the East coefficient increases from -0.129 to -0.208 in case of trust in the press, from -0.059 to -0.167 for trust in TV, and lastly, from 0.038 to 0.099 when we look into trust in social media as an outcome.

Next, analyze the sample of East Germans, even though the small sample size only allows us to provide some suggestive evidence: The findings show that East Germans without access to West TV (compared to East Germans with West TV) show lower levels of trust in the press and TV. Thus, our finding based on the East-West comparison, also holds *within* East Germany, when we compare East Germans with and without access to West TV. Our findings on trust in social media provide further suggestive evidence to our overall reasoning: East Germans without access to West TV show tentatively higher levels of trust in social media.[4]

Overall, we take this as evidence that the receipt of free media while growing up even in a

[4] When we use the sample of East Germans only, the effects for social media lack statistical significance. However, they are of a similar economic effect size as previous findings. Overall, the lack of statistical significance can be attributed to the sample size and the overall low share of individuals who trust in social media in general. For instance, in our sample, the mean of the control group of West Germans, who state that they trust in social media, is 3.1%.

repressive regime can, to some extent, out-weight the harmful effect of growing up in this regime. Furthermore, we can rule out that East-West movers, the educational system in East Germany, which also included a specific subject on socialist education, the severe economic situation after reunification, extreme political views, manifesting in the voting share for right-wing parties, or the inflow of refugees in 2015, are alternative explanations for our results.

Our paper relates to three different strands of the literature. First, the literature on the formation of trust as a non-cognitive[5] skill and thereby specifically, the effects of the environment, in our case growing up in socialism. The second strand deals with the question of how media affect behavior. The third strand of literature our paper relates to is on the take-up of immunizations, especially the recent literature on the determinants of vaccinations against COVID-19.

Ertac (2020) summarizes, that there are three different sets of determinants that make up the formation and malleability of preferences and non-cognitive skills: Individual characteristics, like age or gender, family characteristics, like parental behavior and intergenerational transmission, and lastly, the environment, which covers peers, culture, and exposure. Our paper relates to the latter. As outlined earlier, the effects of socialism, e.g. having grown up in a socialist country, have been studied intensively (for instance on preferences (Alesina and Fuchs-Schündeln (2007)), maternal labor supply Boelmann et al. (2021) or financial risk-taking Laudenbach et al. (2020)).

Within this literature, our paper is closely related to two existing ones. The first is that of Rainer and Siedler (2009). Using different waves of the ALLBUS data[6] as of 1991, 1994, and 2002, they are able to investigate differences between East and West Germans concerning various dimensions of social and institutional trust. Further, they can supplement their analysis with the 2003 wave of the SOEP, the largest micro-data set on households and individuals in Germany that runs since 1984, which provides measures of social trust. As their data range from the immediate time after reunification (1994 ALLBUS wave) to the early 2000s (2003 SOEP wave) they are able to investigate potential differences between East and West Germans over time. Their main result is that East Germans showed a lower likelihood of social and institutional trust right after reunification. However, over time, East Germans' trust in institutions converged to the level of West Germans. However, this is not the case for all dimensions of trust, as East Germans, even a decade after reunification, show lower levels of social trust.

While Rainer and Siedler (2009) do not find differences by gender, they can show that their effects concentrate on those East Germans worrying about their economic situation, being unemployed,

[5] or a socio-emotional skill

[6] ALLBUS is the German General Social Survey, a long-standing survey conducted in Germany.

and receiving a low income. The results of this paper are in line with the ones of Rainer and Siedler (2009), as we also find that East Germans are less likely to trust others (social trust). The latter can for example be explained by the socialist system, as Lichter et al. (2021) showed that the spying activities of the GDR causally affected the level of trust of East Germans, i.e. made them less trustful in fellow humans. In line with the findings of Rainer and Siedler (2009), we present results indicating that East Germans trust less in fellow humans, i.e. have a lower level of social trust compared to West Germans. Further, we show that East Germans do not differ systematically in terms of trust in institutions in general compared to West Germans. This is again in line with the findings of Rainer and Siedler (2009) and serves as an important robustness check for our analysis on differences between East and West Germans in terms of trust in different media institutions.

The second closely related paper is by Heineck and Süssmuth (2013). The authors use SOEP data (waves of 2003 and 2008) and look, among others, into outcomes of social trust, measured for example by questions on *if you can trust people* or *that you need to be careful with strangers*. They also find that East Germans are less likely to trust other people using the wave 2003, which is in line with the findings of Rainer and Siedler (2009). However, employing the 2008 wave, they do only find small differences between East and West Germans in terms of social trust. Further, they suggest full convergence in the 2010s. At first glance, this seems to contradict our results, as we still find pronounced differences between East and West Germans in terms of social trust in 2017 and 2018. However, it is plausible that East and West Germans converged between 2003 and 2008, but, given the many events, such as 9/11, the Arabic spring, the rise of a new right-wing party in Germany (Alternative für Deutschland (AfD)), and the refugee crisis in 2015, East Germans displayed a recall in terms of their social trust, which can explain our findings.

We contribute to the literature on the formation and malleability of preferences and non-cognitive skills, the long-lasting effects of socialism in general (see Alesina and Fuchs-Schündeln (2007), Lippmann et al. (2020), Falck et al. (2017) or Boelmann et al. (2021)), and the specific literature that already investigated differences between East and West Germans in terms of social and institutional trust (Rainer and Siedler (2009) and Heineck and Süssmuth (2013)) and its reasons (Lichter et al. (2021)) in the following way. First, we give an update on potential differences between East and West Germans with respect to different measures of trust for the years 2017 and 2018. This allows us to investigate if these differences are still existing, which is of special interest, as we have these measures available for a point in time, which is beyond events or developments that may influence levels of trust, like the rise of the AfD or the refugee crisis in

2015. Second, we investigate outcomes that have not been studied yet, namely trust in different media institutions. As we will show in Section 3.2 these outcomes vary quite a lot among European countries and are of particular importance for instance with respect to combatting the current COVID-19 pandemic.

The second strand our paper relates and contributes to is the literature on how media affects behavior. This literature has for example established that media, and news spread by it, causally affect migration decisions (Wilson (2021) or Farré and Fasani (2013)) and fertility (La Ferrara et al. (2012)) (see Campante et al. (2022) for an excellent overview on this literature). Specifically in the context of (East) Germany, the seminal paper of Bursztyn and Cantoni (2016) showed that the different reception of West German TV across East German regions, affected consumption behavior. East Germans with access to West German TV, which was quasi-randomly solely driven by geographic features, have a higher preference for goods that were advertised in pre-unification West TV (Bursztyn and Cantoni (2016))[7]. From this, a large literature followed this identification strategy and showed that East Germans with access to West TV are more likely to believe that effort rather than luck determines success in life (Hennighausen (2015)), are less likely to vote for right-wing parties (Hornuf et al. (2020)), show lower levels of fertility (Bönisch and Hyll (2015)), a higher probability to become entrepreneurs (Slavtchev and Wyrwich (2017)) and higher levels of consumption aspirations (Hyll and Schneider (2013)). At the regional level, East German regions with access to West TV display lower rates of violent crime and sex crime, but more fraud (Friehe et al. (2018)) and, similar to findings at the individual level, have lower vote shares for the extreme left and right-wing parties (Friehe et al. (2020)).

We contribute to this literature by investigating outcomes that come intuitively worth looking at but have not been studied so far: The effect of the differential effect of West TV, and thus, free media, on trust in different media institutions, namely trust in traditional media (the press and TV) and new media (social media). These are important outcomes to look at as they are likely to predict actual media consumption, which in turn has a high impact on outcomes, such as fertility or migration.

The third strand our paper relates to focuses on the determinants of vaccination take-up. Schmitz and Wübker (2011) - albeit their analysis concentrates on elderly people - show that age, health status, lifestyle, and for example labor force status are important. Maurer (2009) contributes to this and finds that in addition, education and health behavior are crucial factors. Steinert et al. (2022) turn to the current COVID-19 pandemic and illustrate the huge differences among

[7] We will explain this identification strategy in more detail in Section 3.2.2

European countries in vaccination take-up: While only 6.4 percent of people in Spain are hesitant to get a shot against COVID-19, this figure is close to 62 percent in Bulgaria. They show, especially for Germany, that information about the medical benefits and privileges that are connected with having the status of being vaccinated positively influence the take-up of a shot against COVID-19.[8] We contribute to this literature by providing evidence that trust in media plays an important role as well, a factor that has, to the best of our knowledge, not been studied yet.

The remainder of the study is organized as follows. In Section 3.2, we show motivating evidence on trust in media and COVID and explain the historical context of Germany after World War II and the two quasi-natural experiments that it provides for our analysis. In the following Section 3.3, we present the NEPS data, our main micro data set, and regional information we gathered for our analyses. For both, we discuss the respective descriptive statistics. This is followed by the outline of our empirical strategy. Section 3.4 covers our main results, heterogeneity analyses, and robustness checks. In Section 3.5 our main mechanisms and alternative explanations that we can rule out are depicted. In Section 3.6 we conclude.

3.2 Trust in media, historical background and identification strategies

3.2.1 Trust in media: Relevance, determinants & variation across EU-28 member states

Motivating evidence - Trust in media institutions and COVID-19 measures: We begin this Section by providing some motivating evidence on why trust in media is an important outcome to look at. To take a recent example, we illustrate the relationship between trust in different media institutions and the rate of COVID intensive care patients (ICU) in Figure 3.1 and with vaccination rates in Figure 3.2.[9] Respectively, we correlate these COVID measures

[8] For an overview of country characteristics, that determined the rollout and availability of vaccinations against COVID, see for instance Deb et al. (2022).

[9] The data for trust in different media institutions are from the Eurobarometer (European Commission (2018)) and the data on COVID cases and vaccination rates from Our World in Data (Mathieu et al. (2021, 2022)). We aggregate the individual-level data provided by the Eurobarometer (European Commission (2018)) on the national level (see Figure 3.3 for further details). The raw data of Our World in Data (Mathieu et al. (2021, 2022)) are provided at the *daily* level for each country. Here, we aggregate the information on annual figures and shares for the years 2020, 2021, and 2022. In Figure 3.1 and Figure 3.2 we correlate the 2022 COVID information with the trust data from the Eurobarometer (European Commission (2018)) as of 2017. Further illustrations for the correlation of trust in different media institutions and ICU patients and vaccination rates for the years 2020 and 2021 are in a similar vein as those presented in this paper for 2022. These illustrations are available upon request.

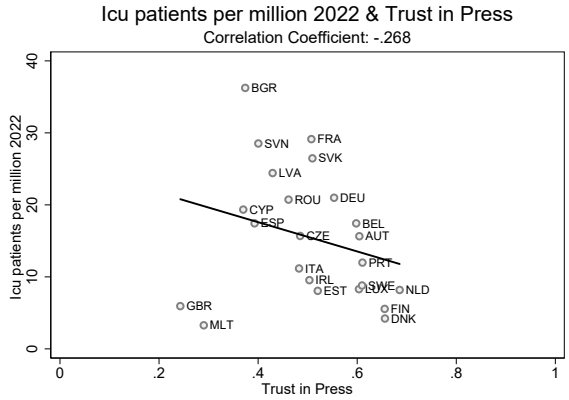
with trust in five different media institutions (for Figure 3.1 and Figure 3.2) alike: trust in press (Sub-Figure (a)), trust in radio (b), trust in TV (c), trust in the Internet (d) and trust in social media (e). Throughout this paper, we categorize press, radio, and TV as *traditional media*, which are very well established and provide, on average, objective and high-quality information, for instance, about COVID vaccinations and challenges due to the crisis. Further, we group the Internet and social media as *new media*. *New media* provide a helpful way to receive objective and high-quality information alike *traditional media*, however, they at least have a higher likelihood of providing *false information* or even *fake news* in an environment that is not open to everyone. Within social media, false information diffuses significantly farther, faster, deeper, and more broadly than the truth, irrespective of the kind of information (political news[10], science, finance, natural disasters) as shown in an example of 126,000 Twitter stories by Vosoughi et al. (2018). In the given context, Kumar et al. (2016) analyze hoax articles distributed via Wikipedia. The good news of their research is that most of these articles containing fake information are detected and deleted quickly, having little impact thus. On the other hand, they show that those few articles remaining on the platform are well-cited across the web, spreading their potentially harmful content. Finding out which psychological effects online disinformation has on political opinions and opinion certainty is the core of the study by Zerback et al. (2021). They focus on astroturfing, which is a strategy creating the impression that a given opinion shared online, in this case pro-Russian propaganda, is widely supported by the public. Their results show that exposure to astroturfing comments alters the opinions of their subjects and increases uncertainty about it. This also applies when subjects have been warned before exposure, illustrating how hard fighting disinformation on social media actually is.

Turning to the correlations of these trust in media institutions and COVID ICU patients, Figure 3.1 displays a strong negative correlation between *traditional media* and ICU patients (Figure 3.1 (a) to (c)). For instance, this correlation is -0.32 for trust in TV (c). In contrast, the number of ICU patients correlates positively with trust in *new media*, as visible for the Internet with a correlation coefficient of 0.22 (d) and 0.4 for trust in social media (e). We are aware of the fact that these are just raw correlations, however, first note that these correlations are for the *same country*, but are presented separately for different media institutions. Second, mind that country-specific characteristics (GDP p.c., the share of elderly persons, or the share of groups that might be especially vulnerable to COVID) most probably *do not drive this suggestive evidence* as

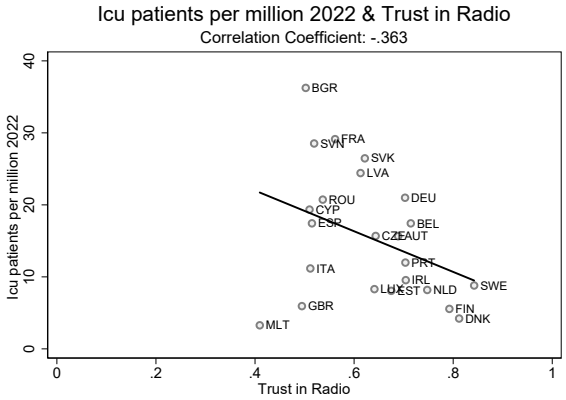
[10] See also Grinberg et al. (2019) who investigate fake news on Twitter during the presidential election of 2016 in the US.

Figure 3.1: Trust in different media institutions (2017) & COVID-19 patients in intensive care (ICU) (2022) among EU-28 member states

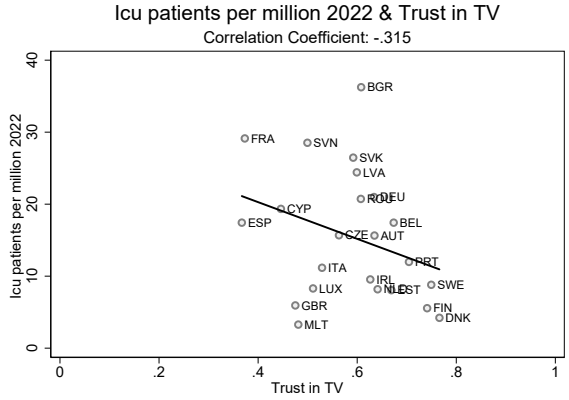
(a) Press



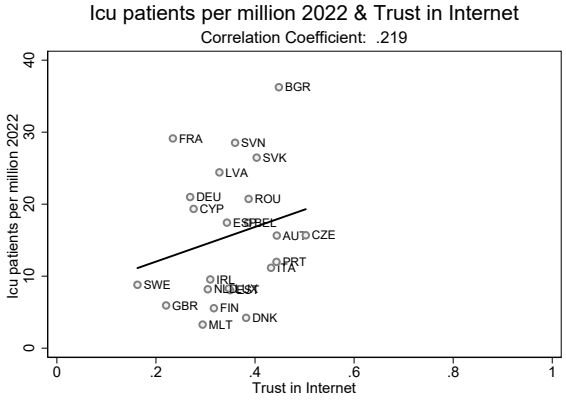
(b) Radio



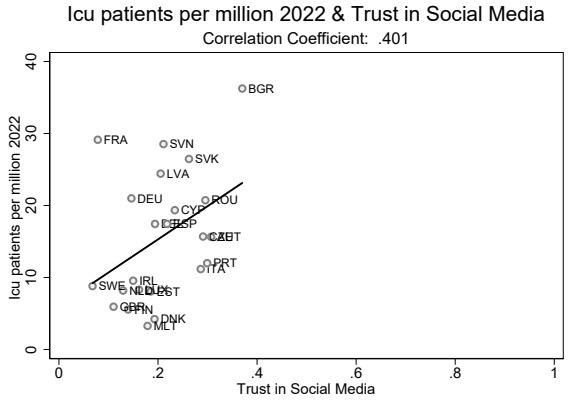
(c) TV



(d) Internet



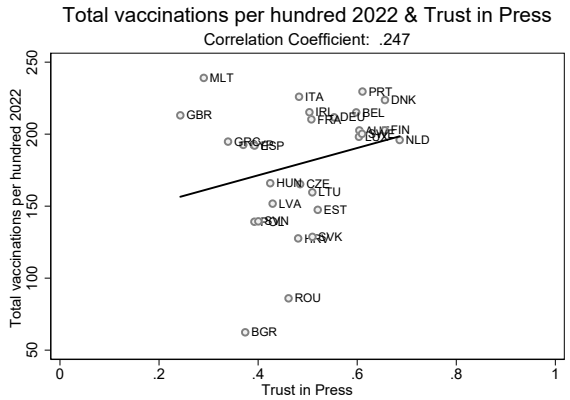
(e) Social Media



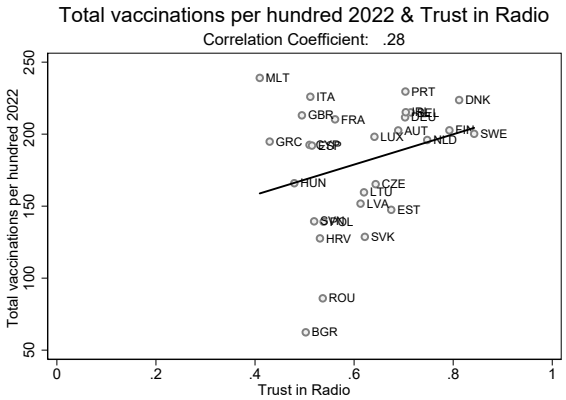
Notes: The figure shows a scatter plot and a fitted line for trust in different media institutions (x-axis) and ICU patients per million (y-axis). The names of the countries in our EU-28 sample are displayed according to the ISO-Code abbreviations. The Figure shows that the rate of ICU patients correlates negatively with trust in (a) press, (b) radio and (c) TV, which we classify as *traditional media* and positively with the Internet (d) and social media (e). We take this as suggestive evidence that *traditional media* provide more reliable information on COVID, which may induce people to increase their awareness of the pandemic. In contrast, *new media* may - or at least show a higher probability to do so - provide more *false information or even fake news.*
Source: Data on trust are from the Eurobarometer (European Commission (2018)) for the year 2017 and from Our World in Data (Mathieu et al. (2021, 2022)) for the year 2022.

Figure 3.2: Trust in different media institutions (2017) & vaccination rates (2022) among EU-28 member states

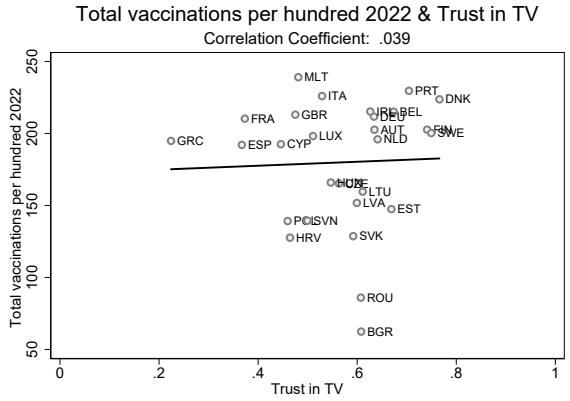
(a) Press



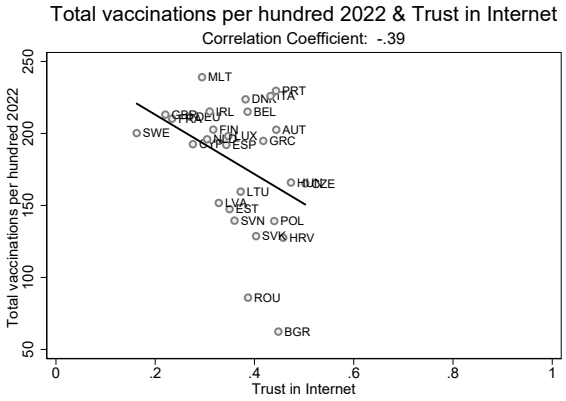
(b) Radio



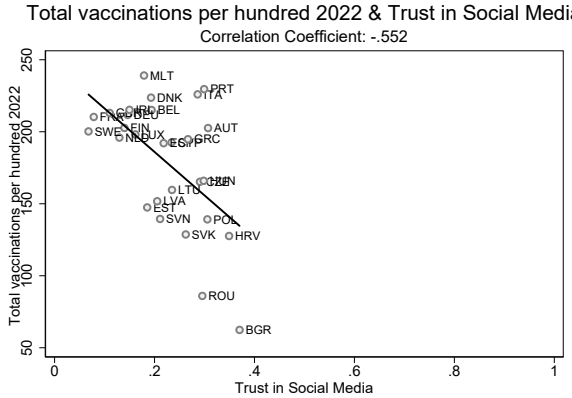
(c) TV



(d) Internet



(e) Social Media



Notes: The figure shows a scatter plot and a fitted line for trust in different media institutions (x-axis) and vaccination rates (y-axis). The names of the countries in our EU-28 sample are displayed according to the ISO-Code abbreviations. The Figure shows that the rate of ICU patients correlates positively with trust in (a) press, (b) radio and (c) TV, which we classify as *traditional media* and negatively with the Internet (d) and social media (e). We take this as suggestive evidence that *traditional media* provide more reliable information on COVID, which may induce people to get vaccinated at all or at least earlier. In contrast, *new media* may - or at least show a higher probability to do so - provide potentially *false information or even fake news* on the gains and risks of getting vaccinated. Source: Data on trust are from the Eurobarometer (European Commission (2018)) for the year 2017 and from Our World in Data (Mathieu et al. (2021, 2022)) for the year 2022.

long as the corresponding omitted variable bias is *not different for the different trust measures*. We argue that this is hardly the case.

In Figure 3.2 we do the same exercise, but this time, correlate the trust measures with vaccination rates against COVID. The picture we get is the opposite as for ICU patients: In countries, where we observe a *high level of trust in traditional media* we find a *high and positive correlation with vaccination rates*, which is for instance 0.28 for trust in radio (Sub-Figure (b) of Figure 3.2). In contrast, we observe in countries with a *high level of trust in social media a strong, high, and negative correlation with vaccination rates*, which is -0.39 for trust in the Internet and -0.55 for trust in social media ((d) and (e) in Figure 3.2). In sum, we argue that trust in different media institutions (*traditional versus new media*) is an important outcome to look at, as they differ in the *type and quality of information* they provide. This, in turn, is *likely to affect the behavior of individuals*, such as the awareness of the consequences of COVID (a low awareness might explain higher rates of ICU patients) or trust and beliefs (a high COVID-19 vaccination rate may display belief and trust in medicine and the information the government provides).

What connects trust in different media institutions and COVID-19 measures? We argue that higher levels of trust in specific media also increase the probability of consuming this media. Next, a higher level of media consumption also comes along with a higher likelihood of changing behavior due to it (see, among others, Tompson (2016)). de Beaumont (2021) documents that inhabitants in the US, who receive most of their information on COVID-19 through social media show a higher level of misinformation and lower vaccination rates.[11]

Determinants of trust in media: Given this high relevance of trust in media, what determines trust in media institutions? At the individual level, trust in media is positively correlated with age, income, internal political efficacy (Kalogeropoulos et al. (2019)) as well as the political proximity between individuals and journalists and political participation (Splendore and Curini (2020).[12] At the same time, on the more aggregated level, trust in media correlates with the level of democracy, freedom of the press, and corruption (Splendore and Curini (2020). Conceptionally, the relationship of these outcomes (democracy, corruption, and freedom of the press) with trust in media can go in both directions, respectively, rising the problem of reverse causality. In this paper, we attempt to provide evidence that the exposure to democracy and thus, a free

[11] See Strömbäck et al. (2020) for a general overview of the connection between trust in media and media consumption and Vosoughi et al. (2018), Zerback et al. (2021) for the relationship between false information and fake information in general, Grinberg et al. (2019) for the role of Twitter during the 2016 elections in the US, and Kumar et al. (2016) for the role of the web in providing false information.

[12] On a more disaggregated level, the literature also points to differences in trust in media by topics, facts, the accuracy of depictions, and journalistic assessment, see Kohring and Matthes (2007) for an overview.

press, has a long-lasting causal effect on trust in media. Before we proceed with our strategy to identify this causal relationship, we provide some further motivational correlative evidence in the next paragraph on the differences in trust in various media institutions for the EU-28 countries. We thereby specifically point out crucial differences between post-socialist countries and other European countries, in which media was free.

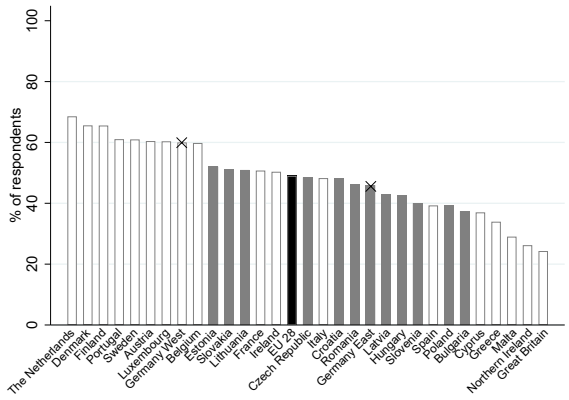
Motivating evidence - Trust in different media institutions among EU-28 countries: In Figure 3.3 we present levels of trust in different media institutions among the EU-28 member states. We make use of the Eurobarometer, provided by the European Commission (2018), which includes questions on the level of trust respondents place in the press (Figure (a)), radio (Figure (b)), TV (Figure (c)), the Internet (Figure (d)) and social media (Figure (e)). Respondents are asked to state if they tend to trust or not tend to trust in the respective media institution. From this, we generate binary indicators that take the value one if an individual states to tend to trust. Next, we calculate the mean of this variable on the country level. Thus, each bar represents the average share of inhabitants of a certain country that tend to trust, and therefore, higher values correspond to higher levels of trust. In each Figure, we highlight the bars of post-socialist countries in grey (white otherwise) and the EU-28 mean in black.

Figure (a) of Figure 3.3 provides information on trust in the press. Overall, the mean of the European Union (EU-28) is roughly 50 percent. However, as clearly visible in Figure (a) of Figure 3.3, this share varies enormously across European countries, ranging from 25 percent in Slovenia to 80 percent in the Netherlands. Most of the post-socialist countries range below the EU-28 mean. For trust in radio (Figure b) and TV (Figure c), this pattern is less clear. However, except for Estonia in terms of trust in TV, none of the former socialist countries ranges on top of the respective country rankings. In sum, respondents living in former socialist countries seem to trust less in the press, radio, and TV compared to respondents in non-socialist countries.

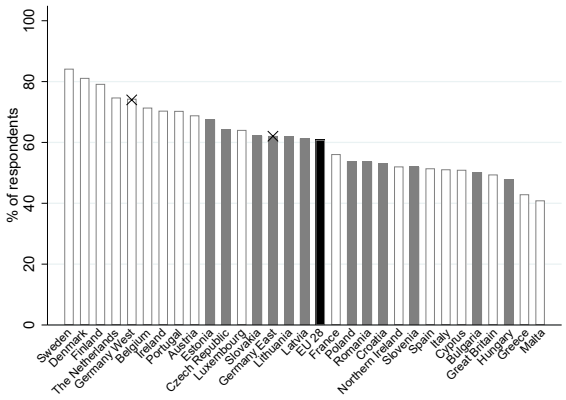
The pattern we observe for *traditional media* is reversed for trust in the Internet (Figure (d)) and social media (Figure (e)). Here, most of the post-socialist countries range above the EU average and take top positions in the rankings as well. For instance, respondents in the Czech Republic, Hungary, Croatia, and Bulgaria show the highest share of trust in the Internet and are far above the EU average of 38%. In contrast, in countries like Sweden or Northern Ireland, less than 20 % trust in the Internet. The same holds for trust in social media, for which the country levels again vary enormously from over 35% in Bulgaria to less than 10% in Sweden. In sum, respondents living in post-socialist countries seem to trust less in *traditional media* such as press, radio, and TV whereas trust levels in the Internet and social media are higher compared

Figure 3.3: Trust in different media institutions (2017) among EU-28 member states for former socialist versus non-socialist countries

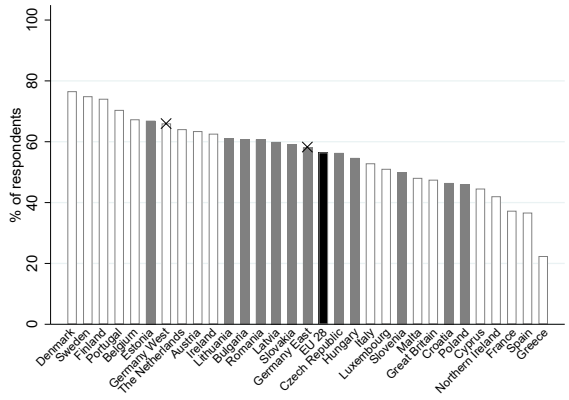
(a) Press



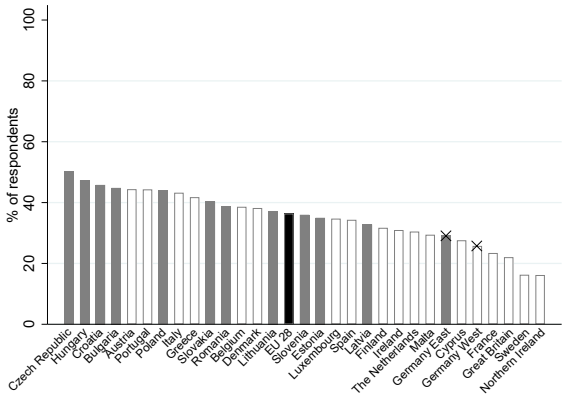
(b) Radio



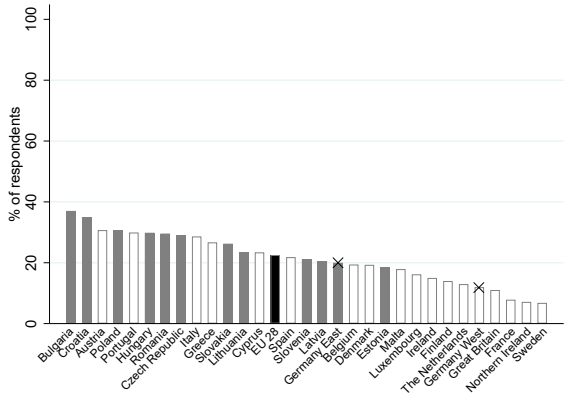
(c) TV



(d) Internet



(e) Social Media



Notes: The figures show the level of trust among EU-28 member states in different media institutions. For this country sample, we highlight former socialist countries with bars in gray (otherwise white) and the EU-28 average by a bar in black. The data include information on trust of European residents in different media institutions as indicated. Respondents state if they tend to trust, tend not to trust or deny an answer to these questions. From this, we generate binary indicators at the individual level that take the value one if an respondent states to tend to trust in the respective media institution. Next, we generate from this the mean on the country level. Therefore, the figures indicate the share of respondents who stated to tend to trust in the different media institutions. In line with our main findings, individuals living in post-socialst countries trust less in traditional media (as press or TV) and are, in contrast, more likely to trust in new media, as social media. Source: Eurobarometer (European Commission (2018))

to non-post-socialist countries. We argue that the experience of growing up in a socialist regime explains these differences even today and employ the context of the separation and reunification of Germany to support this reasoning.

The descriptive evidence we provided in Figure 3.3 gives us first hints on the question if the observed patterns also hold *within* Germany. In detail, the Eurobarometer allows us to differentiate between East and West Germany, like for other countries. In each of the different panels of Figure 3.3, we label the values of East and West Germany with a cross (x). In line with the previous patterns for post-socialist versus non-post-socialist, East Germany always ranks behind West Germany in terms of trust in the press (a), radio (b), and TV (c). In terms of trust in the Internet (d), both former parts of Germany are quite similar. However, the pattern again changes in terms of trust in social media (e): East Germany ranks remarkably higher compared to West Germany, which indicates that East Germans place trust more in social media than West Germans.

3.2.2 Historical background and identification strategies

The separation and reunification of Germany as a quasi-natural experiment: After World War II, in 1945, Germany was occupied and divided into four different zones: the American, the French, the British, and the Soviet. Figure 3.4 provides a map of Germany and its respective zones.

Out of these zones, the separation of Germany into the Federal Republic of Germany (FRG) and the German Democratic Republic (GDR) took place in 1949. While the American, British and French zones were merged into the democratic Federal Republic of Germany, out of the Soviet zone the German Democratic Republic was founded. The following decades shaped huge differences between the formerly united Germans until Germany was reunified in the course of the fall of the iron curtain in 1990.

As outlined in Section 3.1, the separation and reunification of Germany has been extensively used in the empirical literature as a quasi-natural experiment (see, among others, Fuchs-Schündeln and Schündeln (2015), Alesina and Fuchs-Schündeln (2007), Laudenbach et al. (2020), or Brosig-Koch et al. (2011)).

Recently, Becker et al. (2020) made an important contribution to the usage of the respective historical episode as a quasi-natural experiment: They argue, that first, East and West Germany were far from being highly comparable before the separation, second, that the different occupying forces affected East and West Germany differently and third, that the East Germans remaining

Figure 3.4: Germany by zones of occupations (pre- World War II boundaries)

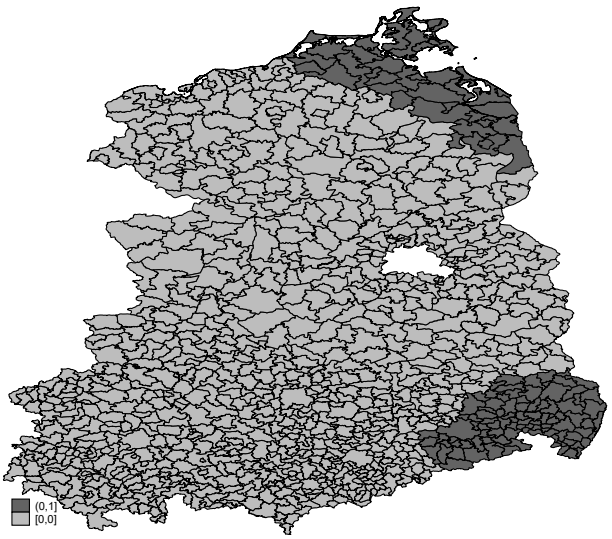
Notes: The division of Germany and German territorial losses after World War I and II.
Source of Basemap: MPIDR and CGG (2011), own illustration.

in the GDR are a highly selective group, as about one fifth (and especially highly educated) of the initial population of the GDR fled to West Germany. However, in their paper, they also evaluate the severity of this problem with respect to the considered outcomes and state that for example trust and preferences are less affected by their concerns (see Becker et al. (2020), p. 164). In detail, they are also pointing out that the paper by Heineck and Süssmuth (2013) is not affected by the raised concerns. As our paper is closely related to it, we translate this into the reasoning that our paper is less affected by the outlined concerns and, that our strategy to identify the causal effect of growing up in a socialist regime is reliable.

Varying access to West German TV across regions in East Germany: As outlined previously, the separation of Germany led to huge and intense differences between both states. One prominent feature was the setup of media in general and public TV in particular. While TV in the Federal Republic was free and guided by the motive of democracy, TV in the GDR was controlled by the socialist regime, trying to influence the population via media. However, West German TV (ARD and ZDF) - transmitted via air - was also accessible in certain areas in East Germany, based on geographical features like distance to the nearest West TV station at the inner-German border. While basically, this signal strength varied in a continuous way, it divided East Germany into regions *with access to West TV* versus regions *without access to West TV*.

Figure 3.5 maps East Germany and highlights the areas in the North-East and South-East of

Figure 3.5: East German counties with and without access to West - TV



Notes: Figure 3.5 maps counties in East Germany. County borders are marked in black and Berlin is not indicated. Counties in East Germany, where individuals could not recieve any West TV are displayed in dark gray. To differentiate East German counties with and without access to West TV, we follow the classification of counties according to Campa and Serafinelli (2019).
Source: Own illustration.

Germany in which inhabitants could not receive West TV in dark grey. Overall, we argue that the reception of West TV gave certain East Germans the opportunity to access free and democratic West TV, while others had to solely rely on the news they received from the controlled and censored GDR TV.[13]

This setting was first exploited by Bursztyn and Cantoni (2016) for the investigation of consumption behavior and used many times later on (as a determinant of success in life (Hennighausen (2015)), voting behavior (Hornuf et al. (2020)), or fertility (Bönisch and Hyll (2015)). Importantly, this literature established the regionally different receptions of West German TV across East German regions as a reliable identification strategy. The existing empirical evidence showed convincingly, for example, that i) East German regions with and without access to West TV did not differ systematically in other dimensions apart from access to West TV (treatment and control regions are highly comparable), ii) mobility within East Germany was low (there was no selection into treatment), and iii) East Germans who had access to West TV also consumed West TV (which goes beyond an intention-to-treat-effect).[14]

[13] See Crabtree et al. (2015), among others, for a detailed description of the West and East German TV program and content as well as their development from separation to reunification.

[14] However, East Germans did not predominantly watch West TV for news, but for entertainment and advertisement (see Bursztyn and Cantoni (2016)). Note that this does not exclude their exposure to free and democratic media, which follows the literature on this setting that established a causal link on fertility (Bönisch and Hyll (2015)). The latter plausibly calls for behavior changes by watching West TV. In sum, the purpose of East Germans watching West TV is not against our reasoning - at least not at the extensive margin.

3.3 Data and and Empirical Strategy

3.3.1 Individual level data (NEPS)

In the empirical analysis, we make use of data from the adults' cohort of the National Educational Panel Study (NEPS SC6).[15] The NEPS SC6 consists of the original sample and an additional refreshment sample of adults born between 1944 and 1986 and provides detailed information about adults' educational biographies, their vocational training, (un-)employment episodes, and a rich set of socio-demographic variables (Blossfeld and Roßbach (2019); NEPS Network (2020a)). Of particular importance for the analysis, NEPS provides rich retrospective information about the locations where the respondents' were born, went to school, worked, and currently live. Taking advantage of these data, we define East Germans as individuals who were born in the former GDR[16]. This is a major advantage of NEPS data compared to SOEP data, where only the place of living in 1989, or the Microcensus, where only the current place of living is available. The NEPS interviews take place annually, with some basic contents being surveyed every year and others being part of the questionnaires less frequently. Our outcomes of interest - trust measures - were part of the survey in 2017/2018 (wave 10). Consequently, we restrict our sample to respondents who participated in that particular wave. Further, we only consider native Germans who were born either in Western or Eastern Germany and cases for which we have full information on outcomes as well as control variables. Applying these restrictions leaves us with a total of 5943 individuals, of which 4616 were born in West Germany and 1327 in East Germany. The outcomes we consider in our analysis cover trust measures in different media institutions, which we classify as *traditional media* and *new media*. *Traditional media* cover the institutions press and TV, whereas *new media* covers social media.[17] All of them were measured by asking the respondents for an evaluation of their level of trust in these on a 4-point Likert scale ranging from 1 "very much trust" to 4 "no trust at all". We recoded the measures into binary indicators taking the value zero if the respondent opted for the two scale points indicating a lower level of trust and the value one if the respondent rated his level of trust on the two points of the original scale that are indicating a higher level of trust.[18]

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[15] This paper uses data from the National Educational Panel Study (NEPS): Starting Cohort Adults, doi:10.5157/NEPS:SC6:11.0.0. From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS has been carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network.

[16] or the corresponding territories earlier the foundation of the GDR in 1949

[17] The exact wording used in NEPS is provided in Appendix Table A3.1.

[18] We follow Rainer and Siedler (2009) in coding the outcome variables.

As outlined in Section 3.1, the purpose of this paper is to investigate differences in trust levels between East and West Germans in different media institutions. However, to revisit earlier findings of the literature and to mitigate the concerns that *trust in media* does not capture overall *trust in institutions*, we also investigate other dimensions of institutional and social trust.[19] For institutional trust, the NEPS also includes the dimensions of trust in the federal government and parliament. Alike trust measures in media institutions, they were measured at a 4-point Likert scale and were accordingly coded into binary indicators. We also make use of social trust, which covers the general level of trust the respondents attribute to other people on a scale ranging from 0 "you can never be careful enough" to 10 "you can trust most people". We again recode this measure of social trust into a binary indicator, which takes the value zero if respondents rated their general level of trust between 0 and 6 on the original scale and the value one for ratings between 7 and 10 on the original scale.

Descriptive Statistics: Table 3.1 presents descriptive statistics for outcomes and control variables for respondents being born in East and West Germany as well as for the overall sample. Column (1) displays the mean values for the whole sample, column (2) for the subgroup of East Germans and column (3) for West Germans, respectively. Column (4) shows the raw differences between East and West Germans.

In the upper part of Table 3.1, the mean values of our outcome measures are reported. To provide a comprehensive overview of the data, descriptive statistics are on the original scale as well as the binary indicators of the trust outcomes. While in the whole sample, 41 percent declare to trust in the press, this share makes up only 29 percent in the East sample, but 44 percent in the West German sample. This difference of 15 percentage points is statistically significant at the 1 percent level. The same pattern emerges for trust in TV, for which the trust gap between East and West Germans is 10 percentage points. Turning to social media, only 5 percent of the overall sample trust in this media institution. Interestingly, albeit the difference is not statistically significant, among East Germans 7 percent declare to trust in social media. In contrast, this share is only about 4 percent in the West sample. These raw differences suggest an important pattern that we are going to analyze in greater detail in Section 3.4: East Germans trust less in traditional media, such as Press and TV. The higher share of East Germans that trust in new media, here social media, may suggest that they trust media from other institutions than traditional media. Turning to the intermediate part of Table 3.1 in which mean values of our independent variables are tabulated shows that the East and West German subsamples differ in some of their demographics.

[19] For summary statistics on these indicators, see Table A3.2 in the Appendix.

Table 3.1: Descriptive statistics - Individual level (NEPS)

	(1) All	(2) East Germans	(3) West Germans	(4) Δ(East-West)
Panel A. Dependent variables				
Trust in media (original scale 1-4)				
Press	2.40	2.21	2.45	-0.24***
Television	2.26	2.14	2.30	-0.16***
Social Media	1.68	1.75	1.66	0.09**
Trust in media (Dummy 0/1)				
Press	0.41	0.29	0.44	-0.15***
Television	0.32	0.24	0.34	-0.10***
Social Media	0.05	0.07	0.04	0.03
Panel B. Independent variables (baseline)				
Age	51.39	49.73	51.89	-2.16***
Male	0.51	0.53	0.50	0.03
East	0.23	1	0	
Years of education	13.21	13.44	13.14	0.30***
employment status				
Fulltime	0.46	0.55	0.44	0.11***
Parttime	0.25	0.17	0.28	-0.11***
Retired	0.22	0.21	0.22	-0.01
Unemployed	0.03	0.05	0.03	0.02
Other	0.03	0.03	0.04	-0.01
Household income	3288.59	2986.28	3380.03	-393,75***
Married	0.64	0.57	0.65	-0.08***
religous affiliation				
none	0.40	0.80	0.27	0.53***
Catholic	0.29	0.04	0.36	-0.32***
Protestant	0.29	0.15	0.34	-0.19***
Other	0.03	0.02	0.03	-0.01*
Left wing political orientation	0.20	0.21	0.20	0.01
Right wing political orientation	0.24	0.23	0.24	-0.01
N	5943	1327	4616	

Notes: * (**, ***) denotes significant differences of t-Tests at 10% (5%, 1%). The Table displays weighted sample means of main outcomes and baseline covariates in the respective samples. The differences in means of the outcome variables between East and West Germans (Panel A.) indicate that East Germans show a lower probability to trust in traditional media, such as the press and TV compared to West Germans. In contrast, East German tend to trust more in social media. However, these differences are at a smaller magnitude compared to differences in means in traditional media.
Source: NEPS SC6 11.0.0, own calculations.

The East Germans, which constitute 23% of our sample, are two years younger on average and spent slightly more years in education. The share of males is almost equal in both subsamples. The East and West Germans in our sample also differ with respect to their employment status. A higher share of East Germans works full-time and accordingly the share of part-time workers is lower among East Germans. This pattern is plausible and has historical reasons, as full-time employment was the default for men and women in the GDR. To a certain extent, this is still true today, also because the provision of childcare, especially for very young children is on a higher level compared to the western parts of Germany, where the application of the male breadwinner model was (and still is) more common. The shares of retired, unemployed, and people with other work arrangements instead do not differ between East and West Germans in our sample. The household income is significantly lower in East Germany, which corresponds to official statistics provided by Statistisches Bundesamt (2022). A lower share of East Germans is married and reports a religious affiliation. Being less bonded to religion is also a relict of the GDR history, as the GDR regime established a non-religious worldview and actively restricted the influence of religious institutions.

The political orientation of East and West Germans does not differ in our sample. Here, we define respondents as having a left-wing political orientation when they rate themselves between 0 and 3 on a left-right scale ranging from 0 to 10. Respondents rating themselves as 6 and higher are considered as having a right-wing political orientation.

3.3.2 Regional data

Further, we use several regional indicators at the district level to capture differences in the environments the respondents live in. We gathered these data from BBSR (2020) at the level of districts (*Regierungbezirke*), of which the NEPS classifies 38 different regions (7 in current East Germany). If not otherwise indicated, the data are from the year 2017. From this data source, we collected the following information: The *unemployment rate* and *GDP per capita* to proxy for the economic situation. Further, the *share of migrants*, which is the share of people holding a citizenship other than German, proxies for the amount of contact to dissimilar others. Next, the *population density* (number of inhabitants living on one square kilometer) and *level of rurality*. The latter is the share of inhabitants in an area living in cities with a population density lower than 150 people per square kilometer. In the course of our analysis, we also investigate the role of the immediate post-reunification situation and two more recent developments: The *inflow*

Table 3.2: Descriptive statistics - Regional level

	(1) All	(2) East Germany	(3) West Germany	(4) Δ (East-West)
Panel A. District level indicators (2017)				
Unemployment rate (in %)	5.80	7.04	5.43	1.61***
GDP per capita (in 1000 €)	38.83	32.22	40.83	-8.61***
Share of migrants (in %)	11.24	7.01	12.52	-5.51***
Population density (inhabitants per km^2)	508.33	522.45	504.05	18.4
Level of rurality (share of inhabitants in areas with <150 inhabitants per km^2)	21.96	32.82	18.68	14.14***
Panel B. Post-reunification indicators (1991-1995				
Unemployment rate 1991 (in %)	7.16	9.63	6.40	3.23***
GDP per capita 1992 (in 1000 €)	20.56	13.03	22.87	-9.84***
Net outflow 1995	-0.03	-1.0	0.26	-1.26***
D. District lost population	0.37	0.62	0.30	0.32***
Panel C. Recent developments (2017)				
Share of refugees (in %)	2.01	1.68	2.11	-0.43***
AfD vote share (in %)	12.87	19.59	10.84	8.75***

Notes: * (**, ***) denotes significant differences of t-Tests at 10% (5%, 1%). The Table displays key regional level data with respect to current regional and economic characteristics (Panel A.), economic indicators soon after the German reunification (Panel B.) and indicators of more recent events and developments (AfD vote shares and refugees) in Panel C.. Overall, districts in East Germany still perfom less successfully than the average of West German districs (i.e. in terms of unemployment rate or GDP p.c.), host a lower share of migrants and are more rural. The descriptive statistics of Panel B., which relate to the immediate years after reunification indicate that the economic shock directly after reunification was quite strong, with for example an average unemloyment rate in East German districts of 9.6 percent and, that these differences between East and West Germany are still visible in Panel A. were more pronounced in the early 1990s than today (for instance, the difference with respect to unemployment rates was 3.2 percentage points higher in East Germany, compared to 1.6 percentage points today). Lastly, Panel C. documents that the share of refugees in East Germany is much lower, but that the AfD recieved much more votes in 2017 compared to West German districts.

Source: NEPS SC6 11.0.0, own calculations.

of refugees in 2015 and the *rise of votes for the AfD.*[20] Therefore, we additionally collected the following information: *GDP per capita* in 1992 at the district level (Statistisches Bundesamt (2021)), the *unemployment rate* in 1991 at the federal state level (Bundesanstalt für Arbeit (1992)) and the *net outflow of inhabitants* from a district due to internal migration in 1995 (BBSR (2020)). In the analysis, we use the latter binarily, indicating whether a district lost population due to internal migration.

Table 3.2 reveals the pronounced regional differences between East and West Germany. The share of migrants for example is almost twice as high in West Germany, whereas the level of unemployment is higher in the Eastern part of Germany. Both do not differ in their population density on average, but the share of inhabitants living in rural areas is significantly higher in East Germany. East and West Germany also differed in several respects in the post-reunification period. The GDP per capita was significantly lower in Eastern Germany in 1992. The unemployment rate in East Germany in 1991 exceeded the West German one by about 3 percentage points. With 62% more than twice as many East German districts lost population due to internal migration in 1995 compared to 30% of West German districts. Concerning more recent developments, Table 3.2 shows that the share of refugees is significantly lower in East Germany. At the same time, the share of votes for AfD in the federal election in 2017 is about 9 percentage points higher in the Eastern part of Germany.

3.3.3 Empirical Strategy

To investigate potential disparities between East and West Germans in trust in different media institutions, we run Probit regressions of the following form:

$$Pr(Y_i = 1) = \Phi(\beta_1 East_i + X_i), \tag{3.1}$$

where the outcome Y_i is one of the trust in media institutions measures on press and television (traditional media) as well as social media (new media). $East_i$ is an indicator that takes the value one if an individual i was born in the former GDR[21] and zero for individuals born in West Germany. β_1 is the coefficient of interest: It displays if East Germans still show different levels of trust in 2017, almost 30 years after reunification.

X_i is a vector of control variables that covers individual, demographic, and household character-

[20] AfD stands for "Alternative für Deutschland" and is a comparatively new right-wing populist party, which is increasingly successful in recent elections in Germany on the federal level as well as in the states.

[21] or the corresponding territories earlier the foundation of the GDR in 1949

istics. In detail, it includes age, age squared and gender of the respondent, years of education, employment status, where we further differentiate using information about working hours, net household income[22], marital status, religious affiliation, and political orientation. Here, we use two dummy indicators representing both extremes on the original left-right scale, running from 0 to 10. We define those rating their political orientation as 3 or lower as left-wing and 6 or higher as right-wing.

We cluster standard errors at the district of living level and weight regressions with weights provided in the NEPS data, which were calibrated based on the German Microcensus 2017.

3.4 Results

3.4.1 Main Findings and Heterogeneous Effects

Main Findings: Table 3.3 presents our main findings on potential differences between East and West Germans regarding trust in traditional and new media. In terms of traditional media, we find that East Germans are 12.9 percentage points less likely to trust in the press. In a similar vein, East Germans trust less in TV by 5.9 percentage points. This is in stark contrast to new media, for which we find that East Germans are 3.8 percentage points more likely to trust in. We argue that the differences for traditional media are attributable to the fairly limited opportunity to access free media in the GDR. In the course of our analysis (Section 3.5.1), we will provide empirical evidence supporting this reasoning. The higher trust of East Germans in social media may counteract this and provide a source of information that seems to be more credible to them.[23]

Heterogeneous Effects: Are these baseline results robust across different socio-demographic groups? In this Section, we provide evidence on this question on the dimensions of gender, rural versus urban regions, household income, birth cohorts, and education.

In Panel A. of Table 3.4, we first provide our baseline results on the full sample. Next, we conduct the analyses separately for males (Panel B.) and females (Panel C.). As evident, our baseline results do not concentrate on any gender. The same holds for the degree of urbanity. Recall from Section 3.3.2 that the BBSR (2020) database provides a measure of rurality, which is defined as the share of the population in a district living in a municipality with less than 150

[22] In our models, we include information about net household income as the log of net household income divided by the square root of the number of individuals in the household.

[23] In addition to the coefficient on the East-West dummy, all coefficients of the covariates we included in the model (see Section 3.3.3) are reported in Table 3.3. All of them show the expected sign.

Table 3.3: Baseline Results - Trust in different media institutions

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
East German	-0.129***	-0.059*	0.038**
	(0.030)	(0.031)	(0.016)
Demographic controls			
Age	-0.011	0.018**	-0.004
	(0.012)	(0.008)	(0.005)
Age squared/100	0.011	-0.013	0.003
	(0.012)	(0.008)	(0.005)
Male	0.092***	0.042	0.012
	(0.020)	(0.027)	(0.012)
Years of education/10	0.252***	-0.000	-0.078***
	(0.070)	(0.063)	(0.027)
Married	0.013	0.015	-0.022*
	(0.026)	(0.027)	(0.011)
Employment status and Income			
Other	-0.005	-0.021	0.011
	(0.056)	(0.048)	(0.039)
Parttime	0.020	0.029	-0.019
	(0.027)	(0.029)	(0.013)
Unemployed	0.039	0.022	0.036
	(0.057)	(0.062)	(0.035)
Retired	0.005	0.053	-0.020
	(0.042)	(0.034)	(0.016)
Household income	0.081**	0.075**	-0.004
	(0.033)	(0.032)	(0.012)
Religious affiliation and political preferences			
Catholic	0.052	0.052*	0.032***
	(0.033)	(0.027)	(0.011)
Protestant	0.056*	0.014	0.023*
	(0.030)	(0.027)	(0.012)
Other	-0.010	-0.024	-0.000
	(0.083)	(0.064)	(0.018)
Left-wing political orientation	0.102***	0.021	0.021
	(0.025)	(0.023)	(0.014)
Right-wing political orientation	-0.078***	-0.061**	0.011
	(0.026)	(0.025)	(0.013)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). Here, we present our main findings following equation 3.1, which we discussed in Section 3.3.3. It shows that East Germans show significantly less trust in traditional media (press (column 1) and TV (column 2) whereas they tend to trust more in new social media (column 3) compared to their West German counterparts. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%). The reference category of employment status is *fulltime* and for religious affiliation it is *none*.
Source: NEPS SC6 11.0.0, own calculations.

inhabitants per square kilometer. Table 3.2 provides the summary statistics on this measure. As the sample mean is 21.96, we split the sample into rural (at least 25 on the rurality measure) and urban (below 25) districts. For trust in the press, the baseline coefficient is virtually similar if we only use the rural (Panel D. of Table 3.4) or urban (Panel E. of Table 3.4) subsample. In a similar vein, we do not find that the differences between East and West Germans in trust in TV or social media concentrates on urban or rural districts. The sizes of the coefficients are comparable to those of the baseline sample, albeit they are not statistically significant in urban districts throughout.

Lastly, we split the sample into individuals below and above the unweighted median household income in the sample, which is 3500 Euro. Again, the differences in trust in traditional media between East and West Germans does not concentrate on any of these groups. Interestingly, we find pronounced patterns for trust in social media. Our result for the full sample of 3.8 percentage points higher trust of East Germans, seems to be driven by individuals below the median household income. In detail, we find for this group that they are close to 5 percentage points more likely to trust in social media, whereas the coefficient in the above median household income is 0.02 and far from being statistically significant at conventional levels.

Next, we investigate differences by birth cohorts. The intensity an individual experienced the GDR system is potentially captured by the length of having experienced the respective system. We follow Lichter et al. (2021) and define the following cohorts: Individuals born between 1944 to 1961, 1962 to 1971, and 1972 to 1986.[24] The oldest cohort (1944 to 1961) was born before the Berlin Wall was built and thus, had the chance to move to West Germany. This is different for the second cohort (1962 to 1971). This cohort could not move to West Germany until 1989 and spent their whole socialization in the socialist system. This group was between 18 and 27 years old at reunification in 1989. As the formation of trust takes place during adolescence and is mostly stable after the age of 21 (Sutter and Kocher (2007)), we expect the largest effects for this group. The youngest cohort in our sample - individuals born between 1972 and 1986 - was still born in the GDR, however, they were at most 17 years old in 1989. Consequently, only parts of their socialization took place in the GDR, which means that they got influences from GDR as well as reunified Germany. Against the background of the literature that documents high levels of inter-generational correlations of preferences (see for instance Ertac (2020)), which has also been documented for the East-West German case by Heineck and Süssmuth (2013), we expect

[24] 1944 is the oldest birth cohort in the NEPS data. Thus, we are unfortunately not able to investigate individuals that were born earlier, for instance, East Germans that did not grow up in the GDR.

Table 3.4: Heterogenous Effects by Sociodemographic groups

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. Baseline Results			
East German	-0.129***	-0.059*	0.038**
	(0.030)	(0.031)	(0.016)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel B. Males			
East German	-0.123***	-0.038	0.031*
	(0.030)	(0.036)	(0.018)
Mean West Germans	0.558	0.381	0.039
N	2926	2926	2926
Panel C. Females			
East German	-0.128***	-0.074	0.039*
	(0.048)	(0.047)	(0.022)
Mean West Germans	0.446	0.323	0.023
N	3017	3017	3017
Panel D. Rural Districts			
East German	-0.127***	-0.083*	0.040*
	(0.048)	(0.043)	(0.022)
Mean West Germans	0.486	0.362	0.033
N	2232	2232	2232
Panel E. Urban Districts			
East German	-0.133***	-0.087	0.029
	(0.044)	(0.054)	(0.021)
Mean West Germans	0.508	0.347	0.030
N	3711	3711	3711
Panel F. Below median HH income			
East German	-0.107**	-0.060	0.049**
	(0.047)	(0.041)	(0.025)
Mean West Germans	0.455	0.345	0.037
N	2878	2878	2878
Panel G. Above median HH income			
East German	-0.143***	-0.054	0.021
	(0.047)	(0.045)	(0.023)
Mean West Germans	0.538	0.357	0.026
N	3065	3065	2942

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). In Panel A., we analyze the full sample, whereas in Panel B. (males) and C. (females) we split the sample by gender. Based on a value above 25 on the ruality measure, we conduct the analysis in Panel D. for rural districts and in contrast on urban districts in Panel E.. Panel F. (below) and Panel G. (above) split the sample by the unweighted median of household income in the sample. Overall, the results of this exercise show that in general, our results do not concentrate on a specific socio-demographic group. The only exception is that the finding that East Germans tend to trust more in social media concentrates on the sample of East and West Germans below the median household income. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

lower levels of trust of East Germans in this cohort as well. However, as they partially grew up in reunified Germany, this difference should be smaller compared to earlier cohorts.

Figure 3.6 presents the differences between East and West Germans of the predictive margins at the dimension of the outlined birth cohorts.[25]

Sub-figure (a) of Figure 3.6 visualizes these differences for the outcome *trust in the press*. In line with our expectations, East Germans of the birth cohorts 1962 to 1971 show the largest gap compared to West Germans. In detail, the difference is about 13 percentage points for the oldest cohort (1944 to 1961), about 17 percentage points for the cohorts 1962 to 1971, and 11 percentage points for the youngest cohort (1972 to 1986). Given the confidence intervals displayed, these differences are highly statistically significant for the two oldest groups and on lower levels also for the youngest group. While we do see these pronounced patterns for trust in the press, findings presented in Sub-figure (b) of Figure 3.6 on trust in TV do not follow this pattern. Here, the differences between East and West Germans, ranging from 8 percentage points in the oldest cohort to 4 percentage points in the youngest cohort, decrease by cohorts. That is, we find the largest differences between East and West Germans for the cohorts born between 1944 to 1961, being also the group for which the difference is statistically significant. Results on social media (Sub-figure (c) of Figure 3.6) indicate that in the youngest group (cohorts 1972 to 1986) East Germans are almost on par with their West German counterparts. The finding that East Germans trust more in social media essentially concentrates on the cohorts completely socialized in the GDR.

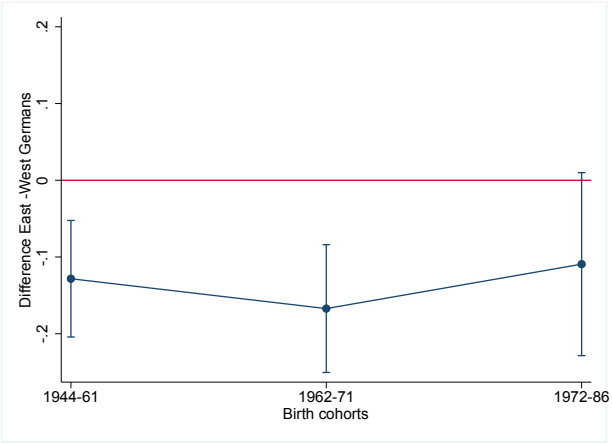
In sum, the results of this exercise show that the trust gap for the press and new media concentrates on those East Germans that spent their whole socialization in the GDR. For trust in TV, this is especially pronounced if individuals belong to the birth cohorts 1944 to 1961.

Lastly, we investigate education as a potential dimension of heterogeneous effects. Overall, the literature documents that the level of education is an important determinant of trust (Alesina and La Ferrara (2002)). Furthermore, the educational system of the GDR and West Germany differed in many aspects (see for instance Fuchs-Schündeln and Masella (2016)). Apart from socialist education in secondary schooling which affected all students (see Section 3.5.2), for instance, to obtain a high school degree that allows entering a university (*Abitur*), East Germans had to comply with the GDR system. Also, the curricula of vocational training or tertiary education consisted of several elements of socialist education. In sum, East Germans with more years of education were also more confronted with the socialist system. Therefore, they might

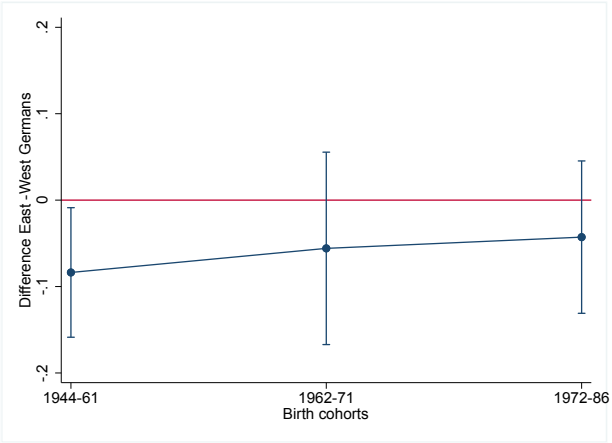
[25] Similar information is also provied in Table A3.4 in the Appendix.

Figure 3.6: Heterogenous Effects by Birth Cohorts

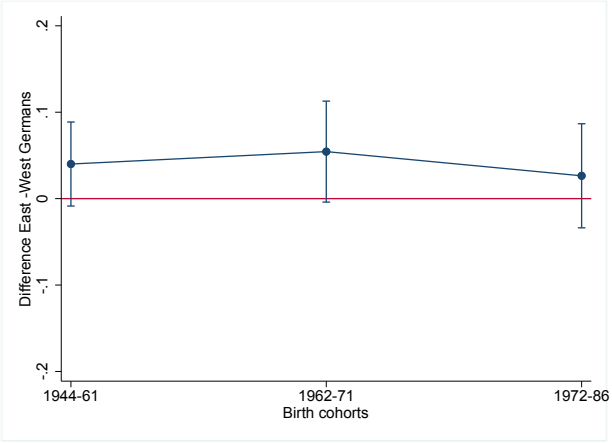
(a) Trust in Press



(b) Trust in TV



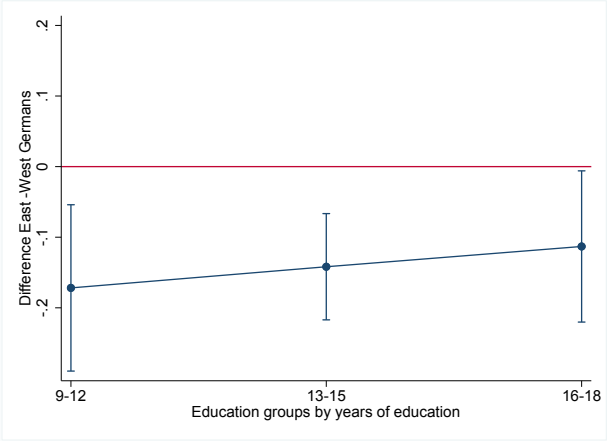
(c) Trust in Social Media



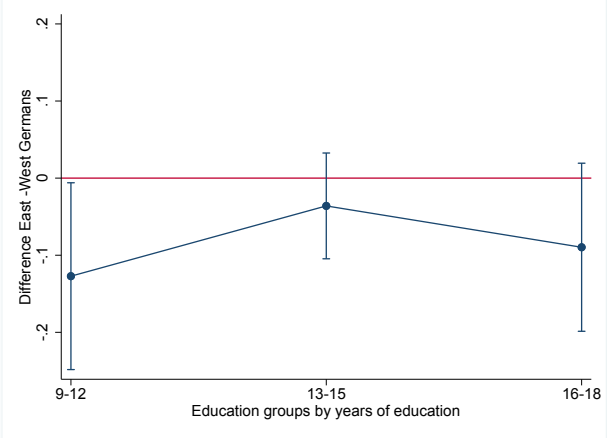
Notes: The Figures show the differences between East and West Germans on the indicated outcome variables (contrast of predictive margins). While the dots indicate point estimates, the bars show 95% confidence intervals. The Figures show that our findings for trust in the press (Figure 3.6 (a)) are robust across birth cohorts, but highest for the birth cohorts 1962 to 1971, who grew up solely in the GDR as well as the oldest cohorts, born between 1944 to 1961. For trust in TV (Figure 3.6 (b)) our results concentrate on the oldest birth cohorts, for whom TV has been the most innovative (compared to somewhat usual as for later cohorts), which might explain our findings. Panel (c) of Figure 3.6 clearly shows that our findings on trust in social media are again driven by the 1944 to 1961 and 1962 to 1971 cohorts, who spent a large part or even their whole socialization in the GDR.
Source: NEPS SC6 11.0.0, own calculations.

Figure 3.7: Heterogenous Effects by Education Groups

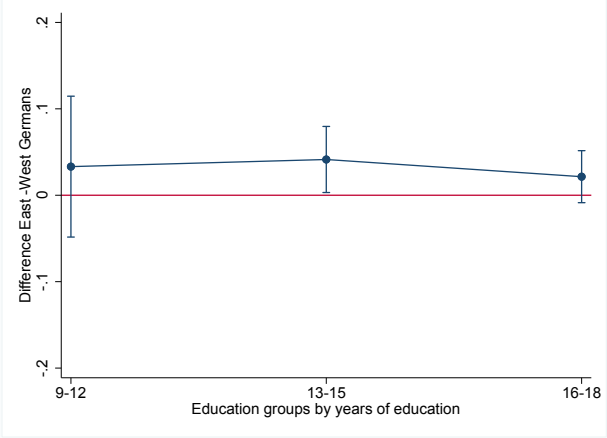
(a) Trust in Press



(b) Trust in TV



(c) Trust in Social Media



Notes: The Figures show the differences between East and West Germans on the indicated outcome variables (contrast of predictive margins). While the dots indicate point estimates, the bars show 95% confidence intervals. Here, we look into heterogeneous effects by educational groups (9-12 years, 13-15 years, and 15 to 18 years). Overall, our results - irrespectively of the outcome Panel (a) Trust in Press, (b) Trust in TV or (c) Trust in social media of Figure 3.7 concentrate on the lowest and medium-skilled groups. Overall, these groups might not have had the chance to make new experiences with free media after reunification or could not fully benefit from it, compared to highly educated East Germans who only show differences for trust in the press (Panel (a) of Figure 3.7).
Source: NEPS SC6 11.0.0, own calculations.

show different levels of trust compared to West Germans that acquired the same amount of education.

To analyze this issue, we define the following education groups: 9 to 12, 13 to 15 and 16 to 18 years of education.

Figure 3.7 visualizes our findings. We find the clearest pattern for trust in the press. Among the group holding 9-12 years of education, East Germans trust less in the press by 17 percentage points, those with 13 to 15 years by 14 percentage points, and the ones with 16 to 18 years of education (university degree) the difference makes up 11 percentage points. Overall, these results are against the hypothesis that the negative effect of growing up in a socialist regime might concentrate on the more educated, as they were subject to the regime during their education. A potential explanation for this might be that highly educated individuals, especially after reunification, had a higher cognitive ability to reflect on the limited access to information in the GDR system. Another explanation might be that highly educated East Germans are more likely to sort into free media after they got the chance to do so. Consequently, their potentially positive experience with the free press lowers their general concerns or distrust of media.

For the other dimensions of trust in media, results for trust in TV concentrate on the 9 to 12 years of education group and differences between East and West Germans are not statistically significant for the other groups. For social media, we do not find very pronounced differences by educational levels.

3.4.2 Robustness Checks

Changes to specification and sample: Table 3.5 presents evidence documenting that our results are robust to a series of changes to the specification, sample, and method. To ease the comparison, Panel A. reports our baseline findings. First, we augment our baseline specification with economic control variables at the regional level. In detail, we additionally account for population density, GDP per capita, unemployment rate, and the share of migrants (all measured in 2017). As outlined in Section 3.3.2, regions in East Germany are still less economically strong and show higher unemployment rates. As we assume that economic activity correlates positively and unemployment negatively to trust in different media institutions, our baseline results might be upward biased. Results presented in Panel B. of Table 3.5 document that this is not the case, as the *East* coefficient remains virtually unchanged. For the regional factors themselves, the positive correlation between the unemployment rate and trust in TV is counter-intuitive in the first place. Potentially, this finding indicates a sorting into TV consumption by socioeconomic

Table 3.5: Robustness Checks - Trust in different media institutions

	Traditional Media		New Media
	Press	TV	Social media
Panel A. Baseline Results			
East German	-0.129***	-0.059*	0.038**
	(0.030)	(0.031)	(0.016)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel B. Adding regional controls to baseline (all of 2017)			
East German	-0.127***	-0.094***	0.045**
	(0.032)	(0.035)	(0.019)
Pop.density/100	0.002**	-0.001	-0.001**
	(0.001)	(0.002)	(0.001)
GDP p.c./100	0.083	0.327	-0.065
	(0.216)	(0.323)	(0.101)
Unemployment rate/10	-0.092	0.167**	-0.001
	(0.072)	(0.067)	(0.031)
Share of migrants	-0.003	-0.008	0.002
	(0.005)	(0.006)	(0.002)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel C. Alternative coding			
East German	-0.019*	0.002	0.025**
	(0.011)	(0.012)	(0.011)
Mean West Germans	0.068	0.029	0.005
N	5943	5943	5760
Panel D. Ordered Probit (Pred. prob.)			
East German	-0.333***	-0.145*	0.247***
	(0.080)	(0.081)	(0.080)
Mean West Germans	2.525	2.320	1.636
N	5943	5943	5943
Panel E. Sample without potential outliers			
East German	-0.128***	-0.057*	0.040**
	(0.031)	(0.031)	(0.017)
Mean West Germans	0.510	0.357	0.029
N	5780	5780	5780

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted) for Panels A., B., C., and E.. Panel D. reports the predicted probabilities of an ordered Probit model. Panel A. presents our baseline findings. In Panel B. we additionally control for regional factors measured in 2017. In Panel C. we use an alternative coding of the dependent variable, where our binary dependent variables only take the value of one if an individual stated to *very much trust* in the respective media institutions. In Panel E. we exclude individuals that report on all three outcomes to trust them *very much* or *not at all*. Overall, our main results are robust against these changes to the specification, sample, and modifications to code the outcome variables. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

status (SES) (see for example Mackenbach et al. (2019)).

Next, we modify the coding of the dependent variables. Recall from Section 3.3 that our binary outcome variables stem from original answers on a four-point Likert scale.[26] These binary indicators take the value one if an individual opted for the two highest categories, corresponding to the answers "pretty much trust" and "very much trust". In Panel C. of Table 3.5, we present results based on an alternative coding where the binary indicator variables only take the value of one if an individual answered the respective questions on the highest scale point, namely "very much trust". While for trust in the press and social media, our baseline results hold, this is not the case for trust in TV. This speaks against our result on differences in trust in TV between East and West Germans. However, only 167 individuals (of which 32 are East German) indicated to trust very much in TV. Therefore, our data at hand provide only a limited opportunity to investigate the outcomes using alternative coding. At the same time, we can rule out that we just coincidentally find differences between East and West Germans.

Adding to that, we use the inversion of the original Likert scale ranging from 1 "no trust at all" to 4 "very much trust" and run an ordered Probit model. We reversed the scale to accomplish a more intuitive interpretation of higher values indicating higher levels of trust. Panel D. of Table 3.5 shows that this does not alter the pattern of results with respect to sign and statistical significance for all our trust measures. Thus, our baseline results are robust concerning the empirical model as well as the coding of the dependent variables (for 2 out of 3 outcomes).

Finally, we exclude individuals from the sample either reporting "very much trust" or "no trust at all" in all three media institutions, as these might be extreme cases that drive our baseline findings. However, our results of Panel E. are highly comparable to our baseline findings if we exclude those extreme cases.

In sum, our baseline results hold if we additionally control for regional factors, use an ordered Probit model or exclude any extreme cases in terms of respondent behavior from the sample. Using an alternative coding of the dependent variables results in terms of trust in the press and social media hold. This is not the case for trust in TV. However, the latter can be explained by a low number of individuals who state to "very much trust" in TV.

Other dimensions of trust: In this paper, we are interested in the effect of growing up in a repressive regime on trust in traditional and social media later in life. This might call the concern that our measures on trust in different media institutions pick up differences in *trust in*

[26] Figure A3.1 in the Appendix provides histograms of the frequencies on the original answers by the different trust in media outcomes.

institutions in general between East and West Germans, rather than specifically trust in different *media institutions*. The findings of Rainer and Siedler (2009) mitigate this concern. Their results as of 2002 indicate that East Germans still show lower levels of social trust compared to West Germans. However, and in stark contrast to social trust, East Germans are on par with West Germans in terms of institutional trust, measured by trust in the legal system and trust in parliament. Thus, East and West Germans converged in terms of institutional trust, which supports our approach that our measures of trust in media institutions do not pick up trust in institutions in general.

Table 3.6: Robustness Checks - Trust in fellow humans (social trust) and different institutions (institutional trust)

	Social Trust	Other dimensions Institutional Trust	
	(1)	(2)	(3)
	Fellow humans	Government	Parliament
East German	-0.103***	-0.018	-0.049*
	(0.031)	(0.027)	(0.028)
Mean West Germans	0.539	0.554	0.566
N	5943	5943	5943

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). Compared to the main analysis, here, we report differences between East and West Germans in other trust dimensions, which have partly been investigated in the previous literature, namely social trust (column 1) and other measures of institutional trust (columns 2 and 3). In line with previous findings of Rainer and Siedler (2009), East Germans, still in 2017 and 2018, trust less in fellow humans but are on par with West Germans in terms of institutional trust. The results of our analysis are in a similar vein for measures of trust in the government and parliament, which is, however, marginally significant at the 10 % level. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%). Source: NEPS SC6 11.0.0, own calculations.

Apart from findings of the existing literature that supports our reasoning, the data at hand allow us to investigate other measures of trust directly. As outlined in Section 3.3, NEPS data also includes information on social trust and institutional trust. Table 3.6 presents the results. In line with previous findings, East Germans are 10.3 percentage points less likely to trust fellow humans. Concerning institutional trust, we find that East Germans are on par with West Germans in terms of trust in the government. For trust in parliament, our results show that East Germans in our sample place slightly less (5 percentage points) trust in this institution. This difference is significant at the 10 percent level. Overall, our results on alternative dimensions of institutional trust do not show a clear pattern and we, therefore, argue that our baseline results do not only pick up East-West differences in institutional trust per se.

Analysis on the European level: We perform our analyses with another data set: The Eurobarometer (European Commission (2018)), which provides data for the sample of EU-28 countries. We do so for several reasons. First, we aim to cross-check our results generated from NEPS data. Second, in addition to replicating our analyses with the same outcome variables, Eurobarometer allows us to investigate additional information on trust in radio, representing traditional media and the Internet as part of the new media. Third, by using data from other country contexts, we make sure that our baseline findings are not just reflecting the unique case of Germany, but are also apparent in the more general setting of examining the differences between former socialist and non-socialist countries within EU-28. The results generated from the Eurobarometer data are depicted in Panel C. of Table 3.7 (for ease of comparison, Panel A. shows the baseline results with the full set of controls as in our main analysis and Panel B. the results were we only control for demographic controls age, age squared, a male dummy, years of education, and marital status as indicated in Table 3.3 using the NEPS data). Consistent with our baseline findings, individuals living in a former socialist country show less trust in the press (-10.8 percentage points) and TV (-4.6 percentage points), but more in social media (35.3 percentage points).[27] Investigating additional outcomes that are not covered in the NEPS data reveals that individuals from former socialist countries trust less in the radio and more in the Internet. These results support our main findings. Overall, this last check hints at the conclusion that our results are externally valid and transferable to other country contexts beyond Germany.

3.5 Mechanisms

In this Section, we discuss several explanations for our finding that East Germans trust less in traditional media institutions. The most likely mechanism is the thread of East Germans with the repressive regime of the GDR. In Section 3.5.1 we further illustrate that those East Germans with no access to West TV show even higher levels of distrust in traditional media. East Germans without access to West German TV are considered as being more exposed to GDR propaganda, as GDR media was controlled by the regime. Further, we show that within the sample of East Germans, those without access to West TV place significantly less trust in these media institutions. We argue that the receipt of West German TV gave East Germans the chance to consume other kinds of media than the GDR media. This diverse experience of TV may

[27] The fact that only 3.1 percent of West Germans (our comparison group in Panel A. using the NEPS), but 21.4 percent of Western and Southern Europeans trust in social media (Panel C. using the Eurobarometer) most probably explains the coefficient of 0.353.

Table 3.7: Robustness Checks - Eurobarometer

	Traditional Media			New Media	
	(1)	(2)	(3)	(4)	(5)
	Press	TV	Radio	Social media	Internet
Panel A. Baseline (NEPS SC6 Data)					
East German	-0.129***	-0.059*		0.038**	
	(0.030)	(0.031)		(0.016)	
Mean West German	0.501	0.352		0.031	
N (Sample)	5943	5943		5943	
N (Sample East Germans)	1327	1327		1327	
Panel B. Baseline with only demographic controls (NEPS SC6 Data)					
East German	-0.165***	-0.087***		0.021	
	(0.021)	(0.026)		(0.015)	
Mean West German	0.501	0.352		0.031	
N (Sample)	5943	5943		5943	
N (Sample East Germans)	1327	1327		1327	
Panel C. Eurobarometer Data (2017)					
Inhabitant of a former	-0.108***	-0.046***	-0.120***	0.353***	0.213***
socialist country	(0.004)	(0.004)	(0.005)	(0.004)	(0.004)
Mean Western & Southern					
Europeans	0.523	0.572	0.631	0.214	0.345
N (Sample)	10071	10071	10071	10071	10071
N (Sample Eastern Europeans)	3891	3891	3891	3891	3891

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). To ease comparison, we show the baseline results in Panel A. and the baseline results with only demographic controls as indicated in Table 3.3 in Panel B.. For Panel A. and Panel B., the results are based on the NEPS data for Germany. In Panel C. we use the Eurobarometer and conduct a similar analysis in this sample of European countries. The coefficient of interest is now if an individual lives in a former socialist country. In this specification, we control for the main demographic controls age, age squared, male, years of education, and married, which is in line with the NEPS specification we use in Panel B.. The Table shows clearly that our main results hold in the European sample as well, and, given the much larger sample, are now statistically significant for all outcomes. Furthermore, the Eurobarometer allows us to add another traditional media: radio. Again, inhabitants of post-socialist countries are less likely to trust in this media source by 12 percentage points. Furthermore, we can also look into another new media with the Eurobarometer: Trust in the Internet. In a similar vein as for social media, people living in post-socialist countries trust more in the Internet by 21.3 percentage points. Robust standard errors are clustered at the district level in Panels A. and B. when we use the NEPS as the data source and at the country level, when we use the Eurobarometer as the data source (Panel C.) in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

have transmitted also to other media, such as the press. In Section 3.5.2 we rule out alternative explanations, among others the economic downturn of East Germany after reunification or the recent refugee inflow.

3.5.1 Main explanation: Access to West TV

As outlined in Section 3.2, parts of the former GDR had access to Western German TV. We follow Campa and Serafinelli (2019) in coding East German counties[28] that did not receive West German TV. Figure 3.5 displays East German counties by the possibility to receive West TV. In our East German sample, 11% of respondents did not have access to West German TV (see Table A3.3 in the Appendix).

We begin by replacing our main variable of interest with another one indicating whether an individual born in East Germany *had access to West German TV* and therefore could potentially consume both: GDR TV and West TV. Then we additionally identify those East Germans *without access to West TV* as we hypothesize that they show even higher gaps in the different trust measures. We compare both groups of East Germans to West Germans, for whom our indicator always takes the value of zero.

Table 3.8 presents the results. Panel A. of Table 3.8 reports our baseline results based on the comparison of individuals born in East versus West Germany. The results of Panel B., based on a comparison of West Germans to *East Germans with access to West TV* are close to Panel A.. This is in stark contrast to our findings taking East Germans *without access to West TV* into account, which are also shown in Panel B. of Table 3.8. This group is 21 percentage points less likely to trust in the press and 17 percentage points less likely to trust in TV. Concurrently, this group is 10 percentage points more likely to trust in social media. Overall, the effects are almost twice as large as in our baseline set of results (Panel A. of Table 3.8).[29]

Another way to explore the varying access to West TV in the former GDR is to concentrate on the sample of East Germans only. In this setting, the variable of interest is a binary indicator of *No West TV*. Here, we compare East Germans *without* access to West TV to East Germans *with* West TV. Results of Panel C. of Table 3.8 suggest a negative effect, however, the respective coefficient on trust in TV is only statistically significant at the 10 percent level. For trust in the

[28] These are the counties Bautzen, Dresden, Görlitz, Sächsische Schweiz-Osterzgebirge, Vorpommern-Greifswald and Vorpommern-Rügen.

[29] We are aware of the fact that the comparison group in Panel B. might be problematic, as not only West Germans are included but also East Germans without West TV or East Germans with West TV, respectively. Therefore, in Table A3.5 in the Appendix, we only take West Germans as the comparison group. The results are not altered.)

Table 3.8: Mechanisms - Exposure with West TV and different access to free media among East Germans

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. Baseline			
East German	-0.129***	-0.059*	0.038**
	(0.030)	(0.031)	(0.016)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel B. West Germans versus East Germans ...			
with West TV	-0.118***	-0.086**	0.039**
	(0.033)	(0.036)	(0.019)
without West TV	-0.208***	-0.167***	0.099*
	(0.063)	(0.049)	(0.059)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel C. Sample of only East Germans...			
without West TV	-0.094	-0.067*	0.043
	(0.060)	(0.039)	(0.051)
Mean East Germans with West TV	0.304	0.266	0.064
N	1133	1133	1133
Panel D. ... and district controls (2017)			
East without West TV	-0.101*	-0.079**	0.035
	(0.053)	(0.033)	(0.037)
Mean East Germans with West TV	0.304	0.266	0.064
N	1133	1133	1133
Panel E. ... and district fixed - effects			
East without West TV	-0.230***	-0.173***	0.011
	(0.038)	(0.027)	(0.050)
Mean East Germans with West TV	0.304	0.266	0.064
N	1133	1133	1133

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). To ease comparison, we show the baseline results in Panel A. In Panel B. we compare West Germans with East Germans with and without access to West TV. In Panel C. we compare East Germans with and East Germans without access to West German TV. In Panel D. we add the economic control variables as in Table 3.5 to the baseline specification. In Panel E. we account for time-constant differences between districts and include district fixed effects. This allows us to exploit variation between counties in the same district.
The results we present in Panels A. and B. are based on the sample of West and East Germans, which we use for the main part of our analysis. In contrast, for the analysis presented in Panel C. to E., we only leave East Germans in our sample. Each Panel presents the results from separate regressions. The main takeaway of these exercises is that East Germans with West TV (Panel B.) also trust less in the press and TV compared to West Germans. However, if we compare East Germans without West TV to West Germans (Panel B.) the coefficients of being East German for all outcomes of interest grow in magnitude: for trust in the press the coefficient nearly doubles (column 1), from -0.129 to -0.208, for TV it is three times larger as in our baseline findings (from -0.059 to -0.167) and for trust in social media, it again almost triples from 0.038 to 0.099. This suggests that the non-exposure to West TV (East Germans without West TV), drives the results. Results of Panel C., D., and E. further support this explanation. Here, we concentrate on the sample of East Germans and show, that also within the sample of East Germans our main results hold. Our results are not statistically significant throughout. However, we devote this to the much smaller sample here (N=1133) compared to our baseline sample (N=5943). Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

press and social media, the coefficients are not statistically significant at conventional levels. This changes if we additionally control for the regional characteristics of population density, GDP per capita, unemployment rate, and share of migrants.[30] Now, East Germans without access to West TV are 10.1 percentage points less like to trust in the press and 7.9 percentage points less likely to trust in TV, compared to East Germans with access to West TV. This finding also holds if we replace the regional characteristics with district fixed effects (Panel E. of Table 3.8). Overall, this exercise reveals some important insights. East Germans, who have been treated with West TV, but also GDR TV, show lower levels of trust in traditional media. This suggests that the persistent exposure to GDR media could not be outweighed by their access and potential take-up of West TV. However, for those who had no access at all to West TV and their information receipt was solely based on the GDR media, the trust gap is even larger. We take this as suggestive evidence that East Germans who had access to West TV would have had even higher trust gaps compared to West Germans if they would not have had access to West TV. In this vein, access to free media has the potential to mitigate the negative consequences of a repressive regime on trust in media. In making the general comparison of East versus West Germans (Panels A. and B.), we find that East Germans trust more in social media, which is even more pronounced if we contrast East Germans without West TV to West Germans (Panel B.). However, we do not find such an effect in the only East sample (Panels C. to E.). These findings suggest that the *general exposure to the GDR system* makes East Germans more trustful in social media, rather than access to West TV.

3.5.2 Ruling out alternative explanations and sensitivity analysis

In this Section, we discuss and check alternative explanations and present further results supporting our main findings.

East-West migrants: In our main analysis, the East identifier takes the value of one if an individual was born in East Germany. This also includes East Germans who migrated to West Germany.[31] This group of individuals was less exposed to the GDR system and thus, should have higher levels of trust in traditional media compared to East Germans who did not leave. If this is the case, our baseline results are downward biased and the gap between East and West Germans would be even larger. We account for this in several ways and present the respective

[30] Which corresponds to the robustness check of Panel B. of Table 3.5 in Section 3.4.2.

[31] According to Table A3.3 in the Appendix, 6% of our East German sample moved to the West before October 1989 and 27% afterwards. See Hunt (2000) and Fuchs-Schündeln and Schündeln (2009) for an overview on migration from East to West Germany after reunification.

results in Table 3.9.

To ease the comparison of results we again show our baseline effects in Panel A. of Table 3.9.[32] As already mentioned above, a substantial share of our East German sample moved to the West before or after reunification. In Panel B. we exclude all respondents who were born in the Eastern part of Germany and moved to the West and compare them to the West German part of our sample. The respective coefficients for trust in traditional media grow somewhat in terms of magnitude, however, they do not differ substantially from our baseline results, in which we include East-West movers. The same pattern emerges if we only exclude a specific sample of East-West movers, i.e. those who moved to West Germany after 1989 (Panel C.) and those who left before 1989 (Panel D.). We suggest that individuals who left East Germany are more comparable to West Germans, and, that our baseline results may be downward biased if we include movers. However, the sample of movers we are available to identify with the NEPS (around 400) is too small to investigate this in depth and, consequently, to be a major threat to our analysis.[33]

GDR school system and exposure to socialism: The repressive regime of the GDR was visible in all domains of life and did not only concentrate on media. A prime area of the regime's influence and impact was the educational system, aiming to raise "socialist personalities". In detail, the regime set up a specific course at school, called *Staatsbuergerkunde*. This course was established to provide regime-approved information on Marxism-Leninism, the political system of the GDR as well as an introduction to the principles of socialist production and central planning. Teachers were instructed to focus on knowledge provision and to suppress diverging opinions, critical thinking, discussion, and student initiative (Fuchs-Schündeln and Masella (2016)). *Staatsbuergerkunde* was, next to state-run free time activities, one of the most important means of indoctrination and preservation of socialist values for children.

Findings from previous literature document that exposure to socialist education has long-lasting effects on educational as well as labor market outcomes. For example, Fuchs-Schündeln and Masella (2016) find that one additional year of socialist education decreases the probability

[32] In Panel A. of Table A3.6 in the Appendix, we reproduce our baseline only using the initial sample of NEPS SC6 (ALWA). The reason for this exercise is that we only have biographical information on all places of living for this sample. For the rest of our sample for whom this detailed information is not available, we proxy it by using the place of school or work in October 1989. The corresponding results show that this procedure seems to work sufficiently well, as the results do not differ substantively. Mind, however, that the coefficient on trust in TV does not reach conventional levels of statistical significance here, probably due to the lower number of cases and the corresponding loss of power.

[33] We further, albeit the sample of East Germans is limited with respect to its size, conducted the analysis on this respective sample and checked if East German movers differ from East German stayers. The results we present in Table A3.6 in the Appendix show that this is not the case.

Table 3.9: Ruling out alternative explanations - East West Movers

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. Baseline			
East German	-0.129***	-0.059*	0.038**
	(0.030)	(0.031)	(0.016)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel B. Sample excluding East German movers			
East German	-0.132***	-0.062	0.037***
	(0.041)	(0.040)	(0.012)
Mean West Germans	0.501	0.352	0.031
N	5549	5549	5549
Panel C. Excluding East German movers who moved after 89			
East German	-0.137***	-0.069*	0.040***
	(0.039)	(0.039)	(0.012)
Mean West Germans	0.501	0.352	0.031
N	5624	5624	5624
Panel D. Excluding East German movers who moved before 89			
East German	-0.125***	-0.055*	0.026**
	(0.032)	(0.033)	(0.012)
Mean West German	0.501	0.352	0.031
N	5842	5842	5842

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). In Panel A. we report our baseline results. In Panel B. we exclude East Germans that moved from East to West Germany (at any point in time). Then, we exclude East Germans having moved after October 1989 in Panel C. and those having moved before that in Panel D. It shows that effect sizes are larger when comparing East Germans who moved before 1989 (Panel C.) to West Germans. Overall, taking moving into account does not change our findings and therefore does not explain our main results. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

of obtaining a college degree by about 2 percentage points compared to West German levels. Long-term effects on labor market outcomes differ by cohort, are only visible for men, and show, for example, that there are negative effects on working hours and wages. Potentially, higher exposure to this course leads to lower levels of trust in media as well.

Table 3.10: Ruling out alternative explanations - Socialist education

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. Extensive Margin			
Any socialist education	0.026	-0.024	-0.081
	(0.081)	(0.075)	(0.066)
Mean East Germans			
without socialist education	0.374	0.155	0.071
N	1327	1327	1327
Panel B. Intensive Margin			
Years of socialist education/10	0.109	0.090	0.075
	(0.276)	(0.218)	(0.102)
Mean East Germans			
without socialist education	0.374	0.155	0.071
N	1327	1327	1327

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). For this exercise, we restrict the sample to East Germans only and regress our outcomes of interest on different measures of socialist education. In Panel A. we present results comparing East Germans who had the subject *Staatsbuergerkunde* with East Germans who did not have this course in their curriculum (extensive margin). In Panel B. (intensive margin) we consider potential treatment intensity and compare the years of socialist education with zero years of socialist education. In both cases, the measures of socialist education are neither economically nor statistically significant. Against this backdrop, we can rule out socialist education as an alternative explanation for our findings. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

Table 3.10 presents results if we regress the trust in media measures on a binary indicator displaying whether an individual attended the socialist education course, which is true for about 79% of our East German sample (Panel A., extensive margin) and the number of years of exposure (3.16 on average in our East German sample, Panel B., intensive margin). We conduct this analysis on the sample of East Germans only. Neither for the extensive nor intensive margin, the coefficients are economically meaningful and statistically significant.

Economic downturn post-reunification: Starting in 1990 and during the first years after reunification, East Germany has undergone an economic recession, manifesting for instance in high levels of unemployment. The paper of Rainer and Siedler (2009) explains the lower levels of *social trust* of East Germans with their harmful economic experiences after reunification. Possibly, the economic downturn in the early 1990s had a moderating effect on trust in media as

well.

To check for this, we augment our baseline specification i) with unemployment experiences in the aftermath of reunification on the individual level and ii) with the following information: GDP per capita in 1992 at the district level, the unemployment rate in 1991 at the federal state level, and a dummy variable indicating if a district lost population due to internal migration in 1995. First, we consider individual-level unemployment experiences. Thanks to the rich NEPS data, we can identify unemployment episodes of our respondents as well as their length. In Panel B. of Table 3.11 we use both kinds of information separately. In Panel B1. we include an indicator showing if someone was unemployed between 1990 and 1995. In our sample, this is the case for 12% of our respondents (19% of East and 10% of West Germans, see Table A3.2). Compared to the baseline, there are hardly any changes in the *East* indicator. The coefficients of the individual unemployment variable are small and not statistically significant throughout. Turning to the duration of unemployment between 1990 and 1995 the same pattern emerges. In our sample, the average duration of unemployment at this time is 8.58 months. For East Germans this number is substantially higher with a mean value of 10.47 months compared to West Germans with a mean of 7.5 months (see Table A3.2). This is in line with expectations as well as with numbers from the official statistics (see Table 3.2). In Panel B2., the results for the *East* indicator do not change compared to the baseline in Panel A. and the coefficients of the unemployment duration are small and insignificant.

Second, if we compare the *East* coefficient of our baseline analysis (Panel A. of Table 3.11) to that based on the model augmented with regional information (Panel C.), we do not find any large differences. Note, however, that the coefficient for trust in TV is quite similar in magnitude but not statistically significant here.

We conclude from these analyses that the economic shock after reunification does not explain the East-West trust gap in traditional media or the higher probability of East Germans to trust social media.

Inflow of refugees and AfD votes: In a last step, we examine if the inflow of refugees in 2015 and the rise of the AfD are omitted variables in our baseline specification. In detail, we additionally control for the share of refugees and AfD vote share in a district as of 2017. The share of refugees is significantly lower in East Germany (1.68%) compared to the western part of Germany (2.11%). The contrary is true for AfD vote share in the federal election of 2017: here, East German districts score significantly higher (19.59%) compared to West German districts (10.84%).

Table 3.11: Ruling out alternative explanations - Economic situation after reunification

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. Baseline			
East German	-0.129***	-0.059*	0.038**
	(0.030)	(0.031)	(0.016)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel B1. adding unemployment (extensive margin) between 1990 & 1995			
East German	-0.132***	-0.059*	0.037**
	(0.030)	(0.031)	(0.016)
... was unemployed	0.035	0.004	0.020
	(0.034)	(0.028)	(0.020)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel B2. adding unemployment (intensive margin) between 1990 & 1995			
East German	-0.129***	-0.056*	0.037**
	(0.030)	(0.031)	(0.016)
... number of months in unemloyment (month/10)	-0.007	-0.025	0.005
	(0.021)	(0.020)	(0.008)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel C. Adding regional controls at the district level (post-reunification time)			
East German	-0.101***	-0.061	0.040**
	(0.034)	(0.038)	(0.019)
GPD p.c. 1992/100	0.184	0.258	-0.059
	(0.212)	(0.259)	(0.078)
Unemployment rate 1991	-0.006	0.008	-0.003
	(0.004)	(0.005)	(0.002)
Lost population 1995 (Dummy)	0.018	0.021	0.003
	(0.020)	(0.027)	(0.011)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). Panel A. reports our baseline results. In Panel B., we add variables to our model that capture the experience of unemployment in the immediate time after reunification. In detail, in Panel B1., we consider the period 1990 to 1995 and add a binary variable that takes the value one if an individual was unemployed in the defined years (extensive margin). In Panel B2., we replace this variable with another capturing the number of months of unemployment between 1990 and 1995. Lastly, in Panel C., we add variables at the district level to our baseline model, which account for the economic situation in Germany during the immediate post-reunification period (GDP p.c. and unemployment rate) and an indicator that takes the value one if a district lost population. For all of these different modifications that proxy the bad economic situation of East Germany after reunification, we can observe that our point estimates virtually do not change. The only difference is trust in TV, which is not statistically significant anymore (but was also only significant at the 10 percent level in our baseline findings) and concurrently, it does not change in terms of the magnitude of the coefficient. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%). Source: NEPS SC6 11.0.0, own calculations.

Table 3.12: Ruling out alternative explanations - AfD voting shares and share of refugees at district level (2017)

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. Baseline Results			
East	-0.129***	-0.059*	0.038**
	(0.030)	(0.031)	(0.016)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel B. Adding share of refugees to baseline			
East	-0.136***	-0.054	0.033*
	(0.031)	(0.035)	(0.017)
Share refugees 2017	-0.166	0.109	-0.095
	(0.267)	(0.270)	(0.085)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943
Panel C. Adding AfD vote share to baseline			
East	-0.118***	-0.037	0.038*
	(0.037)	(0.040)	(0.021)
AfD vote share 2017	-0.014	-0.027	-0.000
	(0.032)	(0.035)	(0.011)
Mean West Germans	0.501	0.352	0.031
N	5943	5943	5943

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). Panel A. shows the baseline results, in Panel B. we additionally control for the share of refugees at the district level in 2017, and in Panel C. we control for the AfD vote share in the federal election in 2017. For Panel A. and B. our main results remain stable and virtually unchanged. Thus, they do not seem to explain our results in an important way. The estimate for trust in TV is stable in Panel B. (from 0.059 to 0.054) but changes meaningfully in Panel C. (from 0.059 to 0.037). In both cases, the coefficient is not statistically significant anymore. However, note that we took our results for trust in TV as suggestive evidence during this paper as it is also only statistically significant at the 10 percent level in our baseline results. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

Table 3.12 presents the results. For means of comparison, the baseline results are depicted in Panel A. of Table 3.12. In Panel B. the estimation results additionally controlling for the share of refugees in a district indicate that it does not influence our results. The overall pattern and size of the *East* coefficients are mainly preserved although the coefficient of trust in TV is barely insignificant. The share of refugees is insignificant for all our trust outcomes.
A similar picture can be drawn from Panel C. which shows estimations additionally controlling for AfD vote share. The main results concerning the *East* indicator are preserved and the share of AfD votes in a district is insignificant in all estimations.
Overall, we conclude from this last exercise that new developments such as the inflow of refugees and the shift to the political right is not capable to explain the trust differences between East and West.

3.6 Conclusion

A long-standing strand of economic literature documents the persistent effects of growing up and living in a socialist country, for example concerning preferences, saving rates, social norms, and trust. The latter has been investigated as an outcome in terms of trust in fellow humans (social trust) and trust in the parliament or related (institutional trust). For instance, for the case of Germany the literature provides evidence that East Germans, compared to West Germans, show lower levels of social trust, even decades after reunification.
We contribute to this literature by investigating potential differences between East and West Germans in different types of media institutions. Trust in media is an important outcome to look at. In our paper, we provide correlative evidence that trust in traditional media correlates positively with vaccination rates against COVID-19 and negatively with the share of patients in intensive care. In contrast, trust in new media, such as social media, correlates negatively with vaccination and positively with ICU patient rates. Most probably, traditional media are more likely to provide free and reliable information on COVID-related measures, and false and fake information are more likely to make their way and get distributed via social media.
Our results show that East Germans trust less in traditional media, such as the press and TV but more in new media, such as social media. This result holds for a series of robustness checks and, most importantly, we find similar patterns between post-socialist and democratic countries in the EU-28 sample. Next, we provide evidence that the different receipt of West German TV across regions in East Germany, and thus, access to free, democratic media, plays a key role

in trust in media. Our results show that the gap between East and West Germans in trust in different media institutions is more than twice as large as in our baseline analysis.

Our paper has two main implications. First, the experience of having access to free media and therefore, consuming independent content, while growing up seems to be a strong predictor of trust in media later in life. Second, trust in media determines the consumption of specific media, which vary in their degree of information and reliability. Providing sound information on COVID-19 via the Internet and social media might be a promising way to increase vaccination rates and finally, combat the pandemic.

Appendix

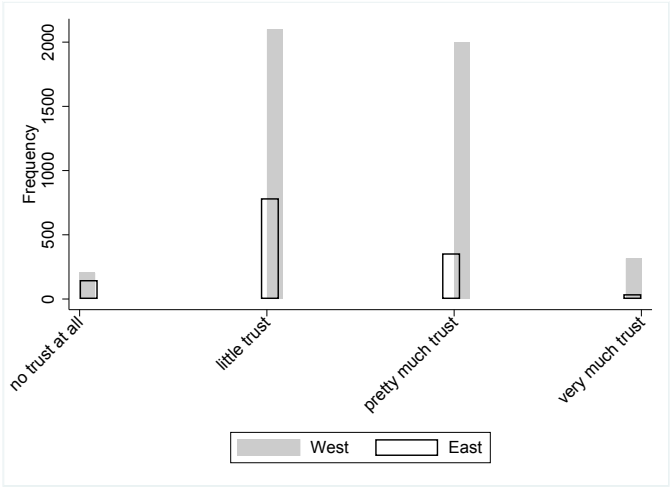
Table A3.1: Item wording in NEPS SC6 (wave 2017/18)

Traditional and new media	Now I'll give you some names of institutions. Tell me whether you have very much, pretty much, little or no trust at all in these institutions. The Press Television Social media, such as Facebook or Twitter
Institutional trust	Now I'll give you some names of institutions. Tell me whether you have very much, pretty much, little or no trust at all in these institutions. The Federal Government The Parliament of the Federal Republic
Social trust	Generally speaking: Do you think that you can trust most people, or that you can't be careful enough when dealing with other people? Please answer on a scale from 0 to 10, where 0 means "you can't be too careful" and 10 means "you can trust most people".

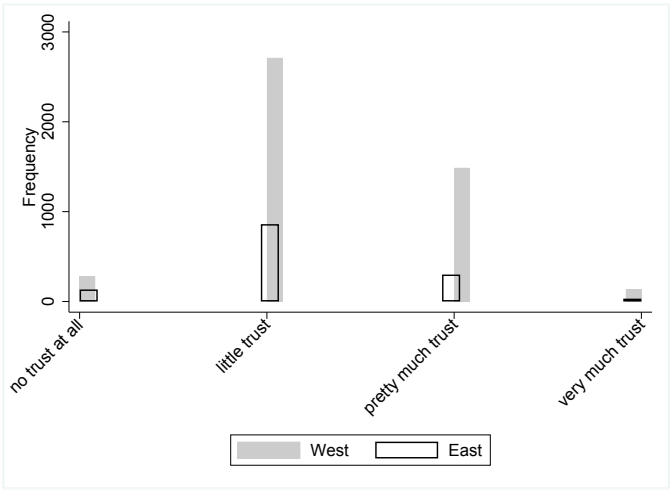
Source: Leibniz Institute for Educational Trajectories (2019). Own illustration.

Figure A3.1: Frequencies of answers of respondents on outcome variables

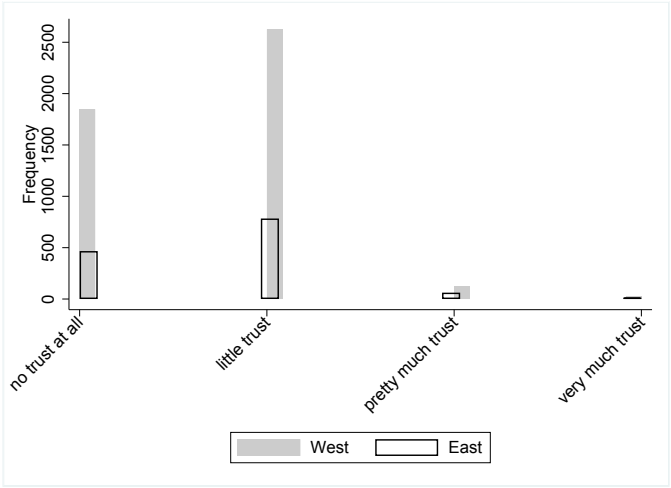
(a) Trust in Press



(b) Trust in TV



(c) Trust in Social Media



Notes: The Figures show the frequencies of answers of respondents in our data on the respective trust measures, separated by East and West Germans.
Source: NEPS SC6 11.0.0, own calculations.

Table A3.2: Further Summary statistics

	(1) All	(2) East Germans	(3) West Germans	(4) Δ(East-West)
Panel A. Further dependent variables				
Institutional trust (original scale 1-4)				
Government	2.43	2.30	2.47	-0.17***
Parliament	2.44	2.32	2.48	-0.16***
Social trust (original scale 0-10)	5.77	5.25	5.93	-0.68***
Institutional trust (Dummy 0/1)				
Government	0.46	0.39	0.48	-0.09***
Parliament	0.46	0.38	0.48	-0.10***
Social trust (Dummy 0/1)	0.44	0.33	0.47	-0.14***
Panel B. Further independent variables				
Individual unemployment between 1990 & 1995				
D. unemployed	0.12	0.19	0.10	0.09***
Unemployment duration (in months)	8.58	10.47	7.50	2.97**
Individual unemployment between 1990 & 1999				
D. unemployed	0.21	0.34	0.17	0.17***
Unemployment duration (in months)	13.56	18.23	10.75	7.48***
N	5943	1327	4616	

Notes: The Table displays weighted sample means of additional outcomes and covariates in the respective samples. Here, we present key summary statistics for the robustness checks presented in Table 3.6 and Table 3.11. * (**, ***) denotes significant differences of t-Tests at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

Table A3.3: Summary statistics - Sample of only East Germans

	(1) East Germans
Independent variables	
No West TV	0.11
East - west Movers (before 10/89)	0.06
Wast - west Movers (after 10/89)	0.27
Any socialist education	0.79
Years of socialist education	3.16
N	1327

Notes: The Table displays weighted sample means for different robustness checks we conduct with the sample of East Germans, see Tables 3.8, 3.9, and 3.10. * (**, ***) denotes significant differences of t-Tests at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

Table A3.4: Heterogenous Effects by Birth Cohorts and Education Groups

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. East - West contrasts of predictive margins by birth cohorts			
1944-61	-0.128***	-0.084**	0.040
	(0.039)	(0.038)	(0.025)
Wald test p-value	0.001	0.029	0.107
1962-71	-0.167***	-0.056	0.054*
	(0.042)	(0.057)	(0.030)
Wald test p-value	0.000	0.326	0.068
1972-86	-0.109*	-0.043	0.026
	(0.061)	(0.045)	(0.031)
Wald test p-value	0.072	0.341	0.390
Panel B. East - West contrasts of predictive margins by educational groups			
9-12 years of education	-0.172**	-0.127**	0.033
	(0.060)	(0.062)	(0.042)
Wald test p-value	0.004	0.040	0.426
13-15 years of education	-0.142***	-0.036	0.041**
	(0.038)	(0.035)	(0.020)
Wald test p-value	0.000	0.303	0.034
16-18 years of education	-0.113**	-0.090	0.021
	(0.55)	(0.056)	(0.015)
Wald test p-value	0.039	0.107	0.162
N	5943	5943	5943

Notes: The Table reports the point estimates underlying the analysis in Figures 3.7 and 3.6, where we report East-West contrasts in predictive margins by birth cohorts (Panel A. of Table A3.4) and educational groups (Panel B. of Table A3.4). Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

Table A3.5: Mechanisms - Exposure to West TV (adjusted comparison group)

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. West Germans versus East Germans ...			
East German with West TV	-0.116***	-0.083**	0.038**
	(0.034)	(0.037)	(0.018)
Mean West Germans	0.501	0.352	0.031
N	5768	5768	5768
Panel B. West Germans versus East Germans ...			
East German without West TV	-0.208***	-0.161***	0.120*
	(0.065)	(0.056)	(0.072)
Mean West Germans	0.501	0.352	0.031
N	4791	4791	4791

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). The main takeaways of these exercises are that East Germans with West TV (Panel A.) also trust less in the press and TV compared to West Germans. However, if we compare East Germans without West TV to West Germans (Panel B.) the coefficients of being East Germans for all outcomes of interest grow in magnitude: for trust in the press the coefficient nearly doubles (column 1), from -0.129 to -0.208, for TV it is almost three times as high as in our baseline findings (from -0.059 to -0.161) and for trust in social media, it also triples from 0.038 to 0.120. This suggests that the non-exposure to West TV (East Germans without West TV), drives the results. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

Table A3.6: Ruling out alternative explanations - Futher analyses concerning East West Movers

	Traditional Media		New Media
	(1)	(2)	(3)
	Press	TV	Social media
Panel A. Only Sample of Germans with full residential history and born until 1956 (ALWA)			
East German	-0.135***	-0.061	0.053***
	(0.049)	(0.052)	(0.020)
Mean West Germans	0.509	0.339	0.028
N	2727	2727	2727
Panel B. Only East German Sample ...			
East German Movers	-0.014	-0.006	-0.002
	(0.054)	(0.044)	(0.021)
Mean East German stayers	0.296	0.263	0.060
N	1327	1327	1327
Panel C. Only East German Sample ...			
Moved before 89	-0.090	-0.064	0.132
	(0.065)	(0.047)	(0.097)
Moved after 89	0.008	0.030	-0.022
	(0.058)	(0.048)	(0.019)
Mean East German stayers	0.296	0.263	0.060
N	1327	1327	1327

Notes: The Table reports marginal effects of weighted Probit regressions (control group means are unweighted). In Panel A. we reproduce our baseline results but only use the initial sample of the NEPS (ALWA). This sample includes individuals born until 1956 for whom their whole residential history is available. In Panels B. and C. we restrict the sample to East Germans only and compare East Germans that stayed in the GDR with i) all East German movers (irrespective of when they moved), ii) East Germans that moved before 1989 to West Germany, and iii) with East Germans that moved after 1989 to West Germany. Overall, neither movers nor the potentially negative selection of stayers, change our findings and therefore do not explain our main results. Robust standard errors clustered at the district level are in parentheses. * (**, ***) denotes significance at 10% (5%, 1%).
Source: NEPS SC6 11.0.0, own calculations.

Chapter 4

Does Civic Education in Secondary Schooling have an Impact on Political Interest of Grade 8 Students? Evidence from NEPS

Nadja Bömmel

4.1 Introduction

Political attitudes of adolescents and their manifestation in participatory activities have been at the center of attention repeatedly since the 1960ies (with the first empirical contribution by Langton and Jennings (1968)), especially in political science. Recently, with the emergence of the "Fridays for Future" movement, scientific and public interest in the question of how youths can be socialized into becoming active citizens is revitalizing. It seems to be beyond dispute that political attitudes are not innate but products of multifaceted learning and socialization processes. Looking at adolescence is especially reasonable in this context because this is a crucial period in which individuals develop their political identity as well as participatory habits that tend to remain stable in adulthood (Campbell (2008); Neundorf et al. (2013); Prior (2010)). Youths start to explore political facts and circumstances and form their own opinions on multiple societal and political questions (Fend (1991); Schmid (2003)). Niemi and Hepburn (1995) narrow the crucial period for political socialization to the interval from about age fourteen to the mid-twenties. These processes take place within and in interaction with different social contexts, all influencing the issue in multiple ways. Several authors, such as Dostie-Goulet (2009), Koskimaa and Rapeli (2015), and Stadelmann-Steffen and Sulzer (2018) stress the importance of certain socialization agents in this context, namely parents, peers, and schools. Besides the parental home, as the first and most immediate source of contact with politics, schools are frequently charged with the responsibility of pursuing civic education goals (Torney-Purta (2002)). Schools represent the public environment in which children and youths are obligated to spend most of their time, irrespective of their social background, and, therefore, serve as a unique setting in which students can be universally reached and equally and systematically supplied with civic education.

Therefore, I focus on the role secondary schools play in educating students to become politically interested citizens. In the German system, this is realized by teaching civics either as a separate subject (which is often called "Politische Bildung" or "Sozialkunde") or combined with other contents within the field of social sciences such as law or economics. By providing formal civic education (CE), schools are supposed to positively influence political knowledge and civic skills, which will, in turn, foster youths' capacities to understand and reflect upon political issues and thus enhance their interest in politics. Moreover, civic education can boost political interest by fostering motivation or a sense of civic duty for politics and active participation, since the educational system serves as a context in which social norms are shared and a lab in which students can practice democratic principles.

One of the major attitudes that typically emerges during the formative phase of adolescence is political interest. In the literature, it is described as a measure of individual cognitive and motivational activation and is key for developing a political identity in adolescence (Schmid (2003)). Political interest serves as a "switch from passivity to participation" (Armingeon, 2007, p. 363) and is important for the individual willingness to participate, as it constitutes the motivational basis for any kind of politically relevant behavior such as seeking information or possibilities to become active (Claes and Hooghe (2017)). Therefore, political interest, or the lack of it can be interpreted as a precursor and crucial predictor of subsequent political (non-)participation. According to Stadelmann-Steffen and Sulzer (2018) using political interest in the target group of adolescents is most appropriate as other outcomes such as self-reported or hypothetical participation, systematically overestimate actual behavior and may not be as reliable.

Although a substantial number of empirical studies exist focusing on the relationship between schooling e.g., formal civic education (Gainous and Martens (2012)(US), Hooghe and Dassonneville (2011)(Belgium), Neundorf et al. (2016)(US and Belgium)) or classroom climate (Campbell (2008)(US)) and different aspects of political involvement, such as political knowledge (Hooghe and Dassonneville (2011)(Belgium), Niemi and Junn (1998)(US)), generalized and political trust (Witschge et al. (2019)(Netherlands)) or participatory attitudes (Claes and Hooghe (2017)(Belgium), Dassonneville et al. (2012)(Belgium), Hoskins et al. (2012)(Finland, England, Germany, Italy, Poland)), authors like, for example, Dostie-Goulet (2009), Stadelmann-Steffen and Sulzer (2018) or Prior (2010) still name political interest and its development as being understudied in the existing literature.

Therefore, I focus on political interest and investigate the relationship between the mere exposure, timespan, intensity, and total amount of civic education in secondary schooling and political interest in youth. I use data on political interest collected in grade 8 since one conclusion from prior literature is that education in early adolescence can be expected to be more influential than in later years (Persson and Oscarsson (2010)). Additionally, at that time, all students are in a similar phase of compulsory schooling irrespective of school type and also irrespective of individual further life courses, which, in turn, have the potential to influence political interest.

Results of OLS regressions using data from the National Educational Panel Study (NEPS) show that exposure to civic education in secondary schooling is positively related to political interest. Also, the timespan, intensity, and total amount of civic education correlate positively with political interest. Additionally, heterogeneity analyses, as a second focus of the study at

hand, reveal that civic education lessons are not equally important for all students, but indicate differences according to gender, school type, and parental education.

With this, I contribute to the literature in several respects. On a conceptual level, the study at hand is more finely graded than many others because I seek to disentangle which features of civic education are related to political interest in German secondary schools. Gaining insights about the impact of mere exposure to civic education, its intensity, total amount, and the timespan CE is taught is helpful for the development of an in-depth understanding of how political socialization can be realized in schools in the most effective way. A second focus is the heterogeneity of the student body. As students largely differ in their characteristics and social backgrounds, the impact of (civic) education may not be the same for everybody, as most of the existing literature suggests. Digging deeper on this point seems worthwhile, as existing inequalities in political interest and participation between groups of, for example, different social and educational backgrounds may be reproduced and transmitted to the next generation. Counteracting these trends is only possible with a profound understanding of these heterogeneities.

On the empirical level, using German data is something that has not yet been done extensively (except for Hoskins et al. (2012) and Schmid (2003)), although using the German context for getting insights into the relationship between different aspects of civic education and political interest seems to be fruitful due to the responsibility of the federal states in educational matters. The German educational system consists of many different approaches and thus provides a unique setting for investigating the impact of civic education taught at school. Unlike course-oriented systems, such as in the US, in Germany all subjects to be covered in secondary schooling are compulsory, so (within federal state and school track) self-selection into civic education is no issue that needs to be considered. Therefore, this study is better able to approximate causal evidence compared to many other studies in the field, although the variation I use here is not purely exogenous (for discussion see Section 4.9).

In addition, this study adds value, as the data used differs significantly from the data previously employed in other studies. Former literature largely relies on survey data as a single data source, containing civic education indicators reported by either students or school staff (e.g., Claes et al. (2009); Dassonneville et al. (2012)). Both may be subject to bias, as students who are more interested or active in politics are more likely to recall their civic education received at school. Both students and school staff may display socially desirable response behavior, as covering social and political topics at school is highly valued and even expected by the government and public. Therefore, I combine rich survey data from NEPS, which has not been used in this context

before, with information about the provision and extent of civic education in all 16 federal states of Germany, collected by Kalina (2014), which is an alternative and perhaps a more reliable approach compared to the exclusive use of survey data. Before turning to the empirical part, I provide a short overview of the hypothesized relation between civic education and political interest and prior research in this context.

4.2 Theoretical links between civic education and political interest in adolescence

To illustrate the connection between civic education in secondary schools and political interest, I elaborate on schools as environments in which political processes can be practiced, schooling as a general investment in human capital, and the relevance of civic education lessons in particular as well as the importance of both for political attitudes and behavior.

Schools (but also student councils, boards, or classes) can be interpreted as "democratic living environment[s]" (Hooghe and Dassonneville, 2011, p. 329) in which students can practice implementing democratic principles, preparing them to play a significant role in society. In this context, students are confronted with the shared social norms and values of society. Lewis-Beck et al. (2008) point out that schools are supposed, for example, to raise students' awareness of the importance of elections, the civic duty of staying well-informed, and the self-confidence to act as good citizens. Civic education provides these norms on the individual level, but they may also spread indirectly via social networks[1] within classes or schools. The structure and composition of individual networks is important for obtaining useful information and for adapting shared opinions, norms, and values, which may affect or motivate behavior (Klandermans and Oegema (1987); McPherson et al. (2001)). For example, Franklin (2004) argues that group pressure within social networks leads to changes in the cost-benefit structure of certain political actions. The perceived benefit of showing interest in political issues is higher for individuals within a politically interested network, such as a school or class. Dostie-Goulet (2009) argues that the main reason for the high importance of peers for shaping political opinions is to be accepted by other members of the group. In the same vein, Stadelmann-Steffen and Sulzer (2018) state

[1] Following the notions of social capital theory, with social capital being defined as the accessibility of resources through social networks that can be used to achieve individuals goals (Granovetter (1973); Lin (1999)), education is highly important for the creation of social capital (Helliwell and Putnam (2007)). This is because educational institutions are crucial for network formation, as they provide opportunities to connect with individuals that are rather similar to oneself (for example, referring to own or parental characteristics, such as education or socioeconomic status) and therefore are more likely to constitute a social network (principle of homophily).

that the willingness or motivation to get involved in politics is related to expectations in the environment or norms and tradition.

Secondly, education, in general, can be interpreted as an investment in human capital following the skill-based approach of human capital theory. Verba et al. (1995) outline similar arguments in their influential civic voluntarism model. According to this perspective, education trains certain politically relevant skills. Following Persson (2015), schooling fosters the development of cognitive skills, critical thinking, and problem-solving abilities (Humphries et al. (2013)) as well as the capability to gather and process information (Brade and Piopiunik (2016)). This is relevant to understand the rather abstract contents of politics and the ability to display political interest by following political campaigns and the political agendas of certain politicians and political parties (Delli Caprini and Keeter (1996); Nie et al. (1996); Verba et al. (1995); Wolfinger and Rosenstone (1980)).

Further, education directly impacts on students' knowledge, which reduces the costs and raises the perceived benefits of following political topics, as education supports the rehearse of democratic principles (Dee (2004)). Schooling and especially lessons on political education impart factual knowledge about the democratic system, political institutions, the functioning of political processes, possibilities of civic participation, and so forth, as this "traditional" kind of civic education is intended to have cognitive outcomes, namely increasing students' political knowledge (Hooghe and Dassonneville (2011); Kahne et al. (2006)). This body of knowledge constitutes the basis for appropriately evaluating daily politics and facilitates being interested in political topics. Stadelmann-Steffen and Sulzer (2018, p. 556) state that "[p]olitical knowledge helps citizens to understand what their political interests are, how their political decisions impact these interests, and how their own interests can be promoted in the political process". By incorporating discussions or raising awareness of political issues in class, civic education serves its task of encouraging students to become politically interested and actively participating citizens (Stadelmann-Steffen and Sulzer (2018)). Therefore, civic education provides early access and a point of contact with the most important political topics and reduces the complexity and the level of abstractness as well as it improves the basic familiarity with political issues.

Taken together, I assume that general exposure to civic education relates positively to self-rated political interest. The impact of timespan and intensity of civic education is a more open question as both may compensate each other. In general, both are supposed to have a positive impact as well, but not necessarily a linear one because marginal returns may be unequal. Being a combination of the two, also the total amount of CE received in grades 5 to 8 is supposed to

have a positive impact on political interest in grade 8.

A longer timespan of exposure may be beneficial because the continuous engagement in political issues in the long term fosters evolving an in-depth understanding of these matters. Being exposed to civic education for a longer period of time supports the appreciation of politics as a tangible part of everyday life.

Also, starting to teach civic education at an early stage in the educational process bears the potential to prepare the lessons in an age-appropriate manner and to establish political content step-by-step. Applying this procedure makes it easier for teachers to address students' disparities in their level of development and to reach out to every individual student irrespective of where they stand. Like this, the alienation and political apathy of particular children or groups may be prevented at an early stage and students' social background may be less important. At the same time, the growth in political interest is supposed to be strongest in the first years when initial levels of interest are lower and the potential for increase is highest. Additionally, the amount of new information and unfamiliar political positions is high which is supposed to boost political interest stronger than providing more details and in-depth discussion of well-known topics. Therefore, the correlation is expected to be positive and probably flatten after some years of civic education.

A higher intensity, as well as a higher total amount of CE, should boost political interest as the potential to keep in mind political facts and internalize democratic values is higher when students are intensively confronted with these topics. Having little time to cover the basics of political education, because the curriculum is only offering a small number of school hours for it, may lead teachers to focus on providing the most important facts about the functioning of the German political system and its institutions. Focussing on the transmission of political knowledge seems rational in this case and is important, but only one issue civic education is supposed to cover. Other components of comprehensively teaching civic education like fostering motivation to engage politically, practicing democratic principles, discussing daily politics in class, exchanging conflicting opinions and negotiating compromises may be as important. Covering all these issues is only possible if enough time is available in CE classes. It can be assumed that a certain amount of lessons is necessary to accurately address the manifold elements of CE, but it may not be expected that every additional hour will equally increase political interest. More technically speaking, I would assume the relationship between the intensity of civic education to be positive in general, but not necessarily linear, as the marginal returns of CE lessons are supposed to decrease.

As a second focus of this paper, I investigate heterogeneities in the relationship between civic education and political interest by differentiating the sample according to gender, school type, and parental education.

First, considering gender seems reasonable because politics remains a male-dominated field into which boys and girls are socialized differently, leading to diverging attitudes, orientations, and dispositions (Bilden (1991); Schmid (2003)). It can therefore be hypothesized that children enter civic education under different preconditions which may lead to a differential impact of civic education in secondary school on individual political interest. Additionally, teachers may treat girls and boys differently, for example, by trying to motivate girls to express their political opinions, so the relationship under investigation could be gender-specific.

Second, checking for school type distinctions seems fruitful as different school types vary in multiple respects, such as cognitive requirements, educational content, academic focus, the composition of the student body, and so forth. Baumert et al. (2006) even sees different types of schools as differential milieus for individual learning and development. This means that students attending different school types are facing dissimilar chances irrespective of their personal, intellectual, cultural, social, or economic resources, caused by institutional work and learning conditions as well as specific pedagogic and didactic traditions. Also, the importance of CE within curricula and teaching methods used in class are highly different in diverse school types. This is leading to the hypothesis that receiving civic education in different types of schools may have a differential impact on political interest.

Third, parental education is important because civic education in school may compensate for a lack of parental civic education at home and vice versa (Claes et al. (2009)). Parents with higher education are more likely to expose their children to political issues and discuss them at home, and adolescents from these backgrounds are more likely to identify with democratic values and to consider themselves as part of the political process (Campbell (2008)). So, it can be expected that civic education at school is more influential for children from lowly educated backgrounds providing low levels of political socialization (Dostie-Goulet (2009); Stadelmann-Steffen and Sulzer (2018)).

4.3 Review of existing empirical evidence

In the following, I will summarize international findings of (1) purely correlative studies, (2) studies using panel data and (3) studies reporting causal effects of different aspects of civic education on political outcome measures for youth and young adults.[2] Then, I turn to the evidence for Germany.

Concerning the exposure to formal CE received in grades 10 to 12 in US high schools, Langton and Jennings (1968) were the first to open up this field of research by showing that civic education courses are not significantly related to political interest, political knowledge, political efficacy, and other outcomes, except for the subsample of black students.

Later findings challenge these results: For the US, Niemi and Junn (1998) show that choosing a course in social studies comes along with an increase in the 1988 National Assessment of Education Progress (NAEP) civics exam score, whereas Gainous and Martens (2012) find that, using data from the 1999 Civic Education Study (CIVED), the frequency of receiving social studies is positively correlated with political knowledge, external political efficacy, and intent to vote among 14-year-olds. Also using CIVED data, Campbell (2008) indicates that an open classroom climate is positively related to the political knowledge and voting intention of ninth graders, but the frequency of social studies instruction is not.

Investigating cross-sectional data from a Canadian Youth Survey of 2006, Claes et al. (2009) show that receiving classes about politics correlates positively with political knowledge but not with the intent to join conventional political participation and social movements in their sample of 15-17-year-old students.

For the swiss case, Stadelmann-Steffen and Sulzer (2018) investigate the relationship between three dimensions of political instruction in higher secondary schools (knowledge, skills, and arousing interest in politics) by conducting content analyses of respective curricula and find that only the skill dimension[3] is positively related to youths' political interest.

Going a step further, several authors acknowledge the shortcomings in the informative value of cross-sectional associations and employ panel data.[4] Using the two-year Belgian Political Panel Survey, Dassonneville et al. (2012) and Hooghe and Dassonneville (2011) reveal that receiving

[2] Of course, the literature focusing on adult target groups is quite substantial but beyond the scope of this paper. Studies dealing with educational content in the tertiary education system are for example provided by Hillygus (2005) and Niemi and Hanmer (2010) for the US as well as Paterson (2009) for Britain.

[3] The skill dimension covers the development of different skills like competencies to judge, competencies to act, and factual competencies.

[4] Unfortunately, panel datasets containing relevant information about the political participation of youth and their exposure to civic education are rare, so the majority of the studies described in the following use the same dataset from Belgium.

classes about politics at age 16 is positively related to political interest and internal political efficacy, but not to political knowledge at age 18. Making use of a third wave of the Belgian Political Panel Survey, Claes and Hooghe (2017) report that civic classroom instruction, school council membership, and open classroom climate are impacting on the growth of political trust and that receiving civics and engaging in the school board are associated with the formation of political interest. Adding on that, Neundorf et al. (2016) show that formal civic education positively influences the starting level of their political engagement index at age 14 containing the level of political media consumption, political interest, and the frequency of political discussions, but not its development as respondents grow older.

Only in the last years, first approaches using identification strategies or intervention programs to allow conclusions on causal effects emerge in this field of research. Persson and Oscarsson (2010) exploit the extension of social science courses initiated by a reform in Swedish high schools as a natural experiment in a sample of 18 to 29-year-olds and find no effect on political knowledge, party activities, voting, and political interest.

The US "Student Voices" program[5] was evaluated as a quasi-experiment by Feldman et al. (2007) and Pasek et al. (2008). Feldman et al. (2007) indicate that the program has a significant, positive effect on following and discussing political issues, political knowledge, and internal political efficacy for students attending "Student Voices" for the entire school year, whereas Pasek et al. (2008) conclude that 1.5 years after the end of the program, these effects vanished to some extend. Investigating an intervention program by "Kids Voting USA" in California[6], McDevitt and Chaffee (2000) and McDevitt and Kiousis (2004, 2006) show immediate, as well as partly persistent, positive effects on the frequencies of student-parent discussion, newspaper reading, TV news watching, and attention to the news in general.

For Germany, only very little is known about the relationship between CE and political participation and interest.

In a multi-country analysis, including Germany, Hoskins et al. (2012) report that the number of hours of social sciences classes is not associated with cognition about democratic institutions[7],

[5] "Student Voices" is a 10-session per semester supplement to regular civic education that was implemented in the 2002/2003 school year in 26 public high schools in Philadelphia (US).

[6] In almost half of the schools in San Jose, California, students in grade 5 to 12 received civic education for at least 6 weeks prior to an election in 1994. The curriculum was designed by "Kids Voting USA" and combined information about the upcoming election, the candidates and their programes, positions, and advertisements with participatory activities to practice civic skills, analyze political positions and argue for either side in debates.

[7] Cognition about democratic institutions contains information about content knowledge, and skills in interpreting material containing civic or political content

attitudes towards democracy, and participatory attitudes[8]. Unfortunately, the cross-sectional CIVED data used here was collected already in 1999. Therefore, the authors advise their readership to be cautious and avoid overinterpreting their results as well as their relevance to current educational practices.

For the German federal state of Brandenburg, Schmid (2003) finds that engagement in lessons on politics has a positive influence on the level of political interest. Although Schmid (2003) contributes to the understanding of the German case, her panel dataset only covers 558 students from one federal state (Brandenburg) and one school type (higher secondary school), diminishing the external validity of her study. Additionally, with active participation in civic education lessons, she uses a measure that is scarcely relatable to international literature.

Relating to my research question, the existing international literature overall provides mixed evidence. However, transferring these results to the German context is hardly possible, as the political system, the cultural background, and other country-specific characteristics are important for individuals' political involvement.

Unfortunately, also the very few studies looking at the German context are not able to enlighten the role of civic education in secondary schooling. Therefore, I add to the evidence on Germany by using rich survey data that is well suited to the analysis of the research question. Moreover, instead of relying on self-reported CE measures, I use additional data on the provision and extent of civic education in all 16 federal states of Germany. This innovative approach enables me to rule out potential bias due to recall error or socially desirable response behavior. Further, the fragmented responsibility for the educational system and the corresponding differences across federal states (see Section 4.4) allow me to approximate causal effects more closely than other studies before.

4.4 The German educational system and differences in the provision of civic education

To get an impression of the context the provision of civic education is embedded in, I sketch the German educational system. The German public school system is highly affected by the sovereignty of the 16 federal states, as these are responsible for the basic structure and funding of schooling as well as the curriculum.

[8] Participatory attitudes incorporate internal political efficacy, expected participation in community services, political activities, voting, and participation in school

Although the differences are appreciable, the basic features are quite comparable (see Figure A4.1 in the Appendix): children enter compulsory schooling at around the age of six. They first visit a primary school for four years in most federal states (only Brandenburg and Berlin have a six-year primary school), where they develop some basic skills in reading, writing, and mathematics. At the age of about ten, the division into three different types of secondary schools takes place: lower secondary, intermediate secondary, and higher secondary school. The educational paths vary in duration and cognitive demands and pave the way for different educational and occupational careers. Some federal states also offer comprehensive schools[9] or multiple track schools combining two or all three branches within one school. In all federal states, schooling is compulsory for ten years.

As the federal states are responsible for their school system, variation also exist regarding the subjects taught in the different types of secondary schools. Most importantly for the analysis at hand, the provision and the extent of civic education differ across federal states and school types. Nevertheless, all federal states agreed upon a common goal to pursue by teaching CE. Young people shall develop the necessary capabilities to orient in modern society, assess political, societal, and economic issues competently, engage in public affairs, and take responsibility in a fair, solidary, and democratic manner. Civic education aims at overall empowerment, motivation, and willingness to engage in all areas of life. Therefore, CE contributes to the preservation, enhancement, and modernization of democracy (KMK (2005)).

As depicted in Table 4.1, deviations are not only visible between federal states but also within states across diverse school types. Some states, such as Saxony, do not cover civic education explicitly until grade 8.[10] Others, such as Saarland or Bavaria, teach civic education only within certain types of schools. Another option, implemented by Hamburg or Bremen, for example, is to include civic education in all types of schools but at different grade levels.

[9] Hesse, Hamburg, Bremen, Mecklenburg-Western Pomerania, North Rhine-Westphalia, and Saarland (see Table 2.1).

[10] Saxony starts teaching CE in grade 9 in all school types, Bavaria in grade 10 in intermediate and higher secondary schools, Saarland in grade 9 in higher secondary schools, and Thuringia in grade 9 in higher secondary schools (Kalina (2014)).

Table 4.1: Civic education in the German federal states

Federal state	school type	Grade 5	6	7	8	Intensity	Total
Bavaria	lower secondary					3.33	506.16
	intermediate secondary					0.00	0.00
	higher secondary					0.00	0.00
Baden-Württemberg	lower secondary					3.40	533.80
	intermediate secondary					3.33	522.81
	higher secondary					3.11	488.27
Berlin	multiple track					5.00	785.00
	higher secondary					5.00	785.00
Brandenburg	multiple track					3.67	578.03
	higher secondary					4.00	630.00
Bremen	multiple track					4.00	634.00
	higher secondary					1.67	264.70
	comprehensive					2.33	369.31
Hamburg	multiple track					3.47	544.79
	intermediate secondary					2.67	419.19
	higher secondary					0.67	105.19
	comprehensive					4.00	628.00
Hesse	lower secondary					1.50	234.75
	intermediate secondary					1.67	261.36
	higher secondary					1.50	234.75
	comprehensive					1.60	250.40
Mecklenburg-West Pomerania	multiple track					7.00	1109.50
	higher secondary					6.00	951.00
	comprehensive					6.00	951.00
Lower Saxony	lower secondary					6.00	951.00
	intermediate secondary					2.00	317.60
	higher secondary					1.00	158.5
	multiple track					4.33	686.31
North Rhine-Westphalia	lower secondary					4.00	640.00
	intermediate secondary					4.50	720.00
	higher secondary					4.67	747.20
	comprehensive					4.00	640.00
Rhineland-Palatinate	lower secondary					3.89	622.40
	intermediate secondary					2.33	372.80
	higher secondary					2.00	320.00
	multiple (2) track					4.00	640.00
	multiple (3) track					3.83	612.80
Saarland	multiple track					1.00	156.50
	higher secondary					0.00	0.00
	comprehensive					3.33	521.15
Saxony	multiple track					0.00	0.00
	higher secondary					0.00	0.00
Saxony-Anhalt	multiple (2) track					1.00	156.00
	higher secondary					1.00	156.00
	multiple (3) track					1.33	207.48

Table 4.1 continued

Federal state	school type	Grade 5	6	7	8	Intensity	Total
Schleswig-Holstein	multiple track					2.83	452.80
	multiple track (lower track)					6.83	1092.80
	multiple track (intermediate track)					6.75	1080.00
	higher secondary					5.25	840.00
Thuringia	multiple track					1.00	158.00
	higher secondary					0.00	0.00

CE as single subject
CE as combined subject

Notes: Intensity denotes the sum of weekly hours of CE cumulated for grades 5 to 8. *Total* is the total amount of CE lessons received in grades 5 to 8. It is calculated by multiplying the intensity measure with the number of school weeks in the respective federal state in the school years 2010/11 to 2013/14 (152 weeks in Bavaria, 156 in Saxony-Anhalt, 156.5 in Saarland and Hesse, 157 in Baden-Württemberg, Berlin, and Hamburg, 157.5 in Brandenburg, 158 in Thuringia, 158.5 in Bremen, Mecklenburg-West Pomerania, Lower Saxony and finally 160 weeks in North Rhine-Westphalia, Rhineland-Palatinate, and Schleswig-Holstein). The number of school weeks per year equals 52 weeks per year - the sum of weeks on vacation according to the official overview of vacations by federal state (KMK (2010, 2011, 2012, 2013)) and is then summed up for the 4 school years under consideration. Source: Kalina (2014), Table 13 containing information from 2013, own illustration.

Another disparity is the point in time at which civic education begins and therefore the timespan of exposure to CE. Some states start teaching in grade 5 (such as Schleswig-Holstein), whereas others start in grade 7 (such as Lower Saxony) or grade 8 (such as Saxony-Anhalt), or even later (such as Saxony). A further difference originates in the fact that the intensity of civic education varies, as shown in the penultimate column of Table 4.1. Connected with the intensity, also the total amount of CE serves as a source of variation. As depicted in the last column of Table 4.1, the same intensity of CE does not necessarily translate into the same total amount of CE lessons received in grades 5 to 8. This may seem odd at first glance, but the reason is the simple fact that federal states are also responsible for arranging their vacancies. In a federal state with a higher amount of vacations, like Bavaria, the same intensity of CE results in a lower total amount of CE compared to states with fewer holidays, like Baden-Württemberg. In this example, both lower secondary schools in Bavaria and intermediate secondary schools in Baden-Württemberg have 3.33h of CE per week, adding up to a total amount of 522.81h in Baden-Würtemberg and only 506.16h in Bavaria.

In addition, there are differences in how the implementation of civic education looks like, which is illustrated by the grey-shaded cells in Table 4.1. Some states establish a separate subject that is especially devoted to teaching civic education (often called “Politische Bildung” or “Sozialkunde”), while others combine it with other contents within the field of social sciences such as law or

economics. Some place it within a bigger subject group containing geography or history.
Given that these wide-ranging differences implement some variation within civic education in secondary schooling across the federal state and school type, I use this variation to have a fine graded look at the different features (exposure to CE, timespan, intensity, total amount) of civic education and their relation to political interest.

4.5 Data and methods

For the analyses, I use data from Starting Cohort Grade 5 of the National Educational Panel Study (NEPS)[11], providing detailed information about children's educational biographies, their competencies, characteristics of different learning environments, and a rich set of sociodemographic variables of themselves and their parents. Although NEPS provides a panel dataset, I use it as a wave 4 (grade 8) cross-section, in which one-shot information on self-rated political interest is available and enrich it with some pieces of information collected in previous waves.[12]
According to Steinhauer and Zinn (2016) the propensity to participate in wave 4 is influenced by age, gender, participation in previous waves, and being surveyed outside the institutional context of schools. I argue that this is indeed uncritical for my analyses as I control for age and gender. Further, as long as all information on relevant control variables is available, it is also not crucial whether respondents participated in (all) previous waves or not. Only those not participating in wave 4 cannot be included altogether, as in this case data on the outcome variable is missing. As the regular way of collecting data in NEPS SC3 is asking respondents to answer the questions in school, a majority of respondents is surveyed in this setting, which has the advantage of a generally higher participation probability.
The NEPS dataset consists of children who entered grade 5 in 2010. Therefore, most of the respondents were born in 1999 or 2000. I only consider students without track changes between grades 5 and 8 to ensure correct civic education assignment across federal state and school type. I exclude students visiting a school for students with special educational needs, because information on political interest is not available for them, and students being part of the migrant

[11] This paper uses data from the National Educational Panel Study (NEPS): Starting Cohort Grade 5, doi:10.5157/NEPS:SC3:10.0.0. From 2008 to 2013, NEPS data was collected as part of the Framework Program for the Promotion of Empirical Educational Research funded by the German Federal Ministry of Education and Research (BMBF). As of 2014, NEPS is carried out by the Leibniz Institute for Educational Trajectories (LIfBi) at the University of Bamberg in cooperation with a nationwide network. For further information, see Blossfeld and Roßbach (2019); Leibniz Institute for Educational Trajectories (2020); NEPS Network (2020b).

[12] For example, basic demographic information, like gender and date of birth, are measured in the first interview only.

oversample within NEPS SC3, as the sampling procedure is different compared to the main sample (Steinhauer and Zinn (2016)). Similarly, students are excluded from the sample when the school type cannot be clearly assigned, or information on other control variables is missing. After restricting the sample as described above, N=2.278 observations remain for my analyses.

The central outcome variable is the individuals' self-rated political interest. The corresponding question in NEPS is quite straightforward, asking how much one is interested in politics. Possible responses range from (1) "not at all interested" to (4) "very interested" in political issues on a 4-point Likert scale. Using a single-item measurement for political interest with a 4-point response scale is quite common in the literature in different country contexts (see for example Claes and Hooghe (2017); Dassonneville et al. (2012); Neundorf et al. (2013); Persson and Oscarsson (2010)) ensuring connectivity of my study with existing literature. Also following previous studies in the field using linear models (for example Claes and Hooghe (2017); Dassonneville et al. (2012)), I compute OLS regressions. According to Breen et al. (2018) and Mood (2010) using linear models is an appropriate alternative also for non-metric outcomes[13], as difficulties in interpreting coefficients from nested models fitted to the same sample and coefficients of the same model fitted to different groups do not occur and thus these models have an easy, straightforward interpretation. To account for the nested data structure, I use clustered standard errors. Clustering on more aggregate levels is recommended as being a more conservative approach when different levels of clustering are imaginable (Cameron and Miller (2015)).[14] Therefore, I cluster on the federal state level in all models.

The civic education indicator of main interest enters the regressions as a dummy variable taking into account the federal state and the school type and equals one if a student was exposed to any kind of civic education before the data on the outcome (political interest) was collected in grade 8 and zero otherwise. The timespan of exposure to civic education is also assigned to every student depending on the federal state of schooling and the school type attended and varies between one (if started in grade 8) and four (if started in grade 5) school years. It is implemented by using multiple dummy variables indicating (1) one year, (2) two or three years and (3) four years of CE, the reference category is zero years of CE. Two and three years are combined here, because only a small number of observations, those going to school in Mecklenburg-West Pomerania, are assigned three years of civic education lessons. The advantage of using dummies instead of a

[13] Although Mood (2010) proposes using linear probability models for binary outcomes, Breen et al. (2018) explicitly stress that this reasoning also applies to ordered outcomes. Additionally, in Table A4.1 in the Appendix, I show that the basic pattern of results is not sensitive to the method, although results using different methods are not directly comparable.

[14] In my data, students are nested in classes, in schools, in school types, and in federal states.

continuous variable is a maximum of flexibility in modeling the functional form of the relationship under investigation. Following this reasoning, also the intensity of CE is incorporated using multiple dummies. This measure is the sum of weekly school hours in civic education a student in the given school type and federal states receives in grades 5 to 8 and varies from zero if someone did not receive any lessons in this subject until grade 8 and a maximum of seven hours if someone attends a multiple track school in Mecklenburg-West Pomerania. Uneven numbers occur because I assume that the number of hours in combined subjects will be equally distributed to all sub-subjects covered.[15] Here, the dividing lines for generating the dummies correspond to the 25%, 50%, and 75% percentile. More precisely, I account for four dummy variables: (1) Up to 2 hours of CE, (2) more than 2 and up to 3.11 hours, (3) more than 3.11 hours and up to 4.5 hours, and (4) more than 4.5 hours. Again, the reference is receiving zero hours of civic education, and the information is assigned to every individual based on the federal state and school type.

As a combination of timespan and intensity, I further consider the total amount of civic education lessons students have received in grades 5 to 8. Therefore, I multiply the intensity measure (which is the sum of weekly hours cumulated for grades 5 to 8) with the number of school weeks in the respective federal state in the schoolyears 2010/11 to 2013/14. Again, I model the most flexible functional form by using dummy variables that are assigned to every individual based on the federal state and school type. As for the intensity measure, I take the 25%, 50%, and 75% percentile as dividing lines for generating the dummies. In detail, I account for four dummy variables: (1) Up to a total of 158.5 hours of CE, (2) more than 158.5 hours and up to 488.27 hours, (3) more than 488.27 hours and up to 720 hours, and (4) more than 720 hours in total. Again, the reference is receiving zero hours of civic education.

In all regressions, I control for a set of micro-level indicators to account for selection processes into different types of secondary schools and to rule out compositional differences in the student body of diverse federal states. These indicators are gender, year of birth, migration background (at least 2nd generation)[16], the Big Five personality traits[17], cognitive basic skills (measured in

[15] For example students attending lower secondary school in Bavaria receive 2 hours per week in grade 6, 2 in grade 6, 3 in grade 7, and 3 in grade 8, which is a total of 10 hours per week in grades 5 to 8. As this is a combined subject, covering contents of civic education, geography, and history, I assign 10/3=3.33 hours to CE.

[16] For the link between migration background and tracking in secondary school, see for example Lüdemann and Schwerdt (2013) and for the link between migration background and political participation in Germany, see for example Müssig (2021).

[17] For the link between Big Five and educational attainment, see for example O'Connor and Paunonen (2007) and Poropat (2009) and for the link between Big Five and political attitudes and behavior, see for example Dunn (2011); Mondak and Halperin (2008); Mondak et al. (2010, 2011); Rasmussen and Norgaard (2018).

grade 5)[18], and parents' highest educational level in years of education.

Ideally, controlling for the federal state of schooling would be appropriate to additionally account for unobserved confounding variables on the federal state level. Unfortunately, given that the variation in the provision of civic education lessons lies only on the federal state and school type level, this is not feasible here. The reason behind this is that the within variation is just too low, as there are only 16 federal states with two to five school types which partly show little or even zero variation in their civic education. Therefore, I control for the type of school attended and whether the federal state of schooling is in east or west Germany, as this distinction is expected to be most relevant out of historical reasons.[19]

In the robustness Section, I show that varying models (not) controlling for school type and federal state do not lead to substantially different results.

4.6 Results

For a first glance at the data at hand, some descriptive statistics are provided in Table 4.2. Relying on t-statistics, my outcome measure, political interest[20], is significantly higher for respondents being exposed to civic education. In addition, students exposed to CE are more often female, do not attend higher secondary school, and go to school in western Germany. These differences may represent composition effects in the student body of different types of secondary schools and the divergent provision of civic education in terms of timing as well as extent in different school types (as discussed in Section 4.4). Students with and without exposure to civic education in secondary school do not differ with respect to their migration background and in 4 out of 5 personality traits. Students having received CE also show significantly lower levels of cognitive basis skills (measured as perceptual speed and reasoning), which is due to the fact that highly skilled children are most likely to attend higher secondary schools which in turn provide less CE

[18] To measure cognitive basic skills, NEPS provides data on reasoning and perceptual speed, both being overall measures of domain-general cognitive functioning (see Haberkorn and Pohl (2013); Leibniz Institute for Educational Trajectories (2018)).

[19] After World War II, Germany was separated into the GDR (German Democratic Republic) in the East and the FRG (Federal Republic of Germany) in the West. Until reunification in 1989, both German states consisted of very different economic (central planning versus free market economy) and political systems (socialism versus democracy) leading to the fact that people coming from the eastern or western territories still differ in multiple respects, like their political participation. For example, voter turnout in general elections is lower in Eastern Germany (74% versus 77,2% in the general election 2021 (Bundeswahlleiter (2022))) and East Germans still value socialism, exhibit less social trust, a higher likelihood of abstaining from voting and a lower likelihood to engage in public meetings (Campbell (2011)). Also, the provision of civic education in secondary schooling differs between East and West, as all states are responsible for their educational system. As visible in Table 4.1 the general trend seems to be less CE in east German federal states. For example, no civic education (until grade 8) is provided in Saxony, and very little in Thuringia and Saxony-Anhalt.

[20] In Figures A4.2 and A4.3 in the Appendix, I additionally illustrated the mean values of political interest by federal state and school type.

Table 4.2: Descriptive statistics by exposure to Civic Education

	(1) overall mean	(2) mean without CE	(3) mean with CE	(4) Δ (without-with)
Outcome				
political interest (scale 1-4)	2.25	2.09	2.28	0.19***
Student characteristics				
male	0.50	0.58	0.49	-0.09***
higher secondary school (0/1)	0.59	0.70	0.56	-0.14***
school in west (0/1)	0.93	0.78	0.95	0.17***
migration background (0/1)	0.09	0.08	0.09	0.01
highest parental education (in years)	14.81	14.82	14.81	-0.01
extraversion[21]	3.80	3.86	3.79	-0.07
agreeableness	3.67	3.63	3.68	0.05
conscientiousness	3.66	3.59	3.67	0.08*
neuroticism	3.20	3.14	3.21	0.07
openness	3.88	3.85	3.86	0.01
Perceptual speed[22]	44.45	47.53	43.87	-3.66***
Reasoning[23]	7.45	7.80	7.39	-0.41**
Regressors				
Civic Education (0/1)	0.84	0	1	
Timespan of CE	2.88	0	3.42	
1 year	0.11	0	0.13	
2 or 3 years	0.09	0	0.11	
4 years	0.65	0	0.77	
Intensity of CE	2.77	0	3.29	
Up to 2h	0.23	0	0.28	
2h up to 3.11h	0.20	0	0.24	
3.11h up to 4.5h	0.22	0	0.27	
More than 4.5h	0.18	0	0.22	
Total amount of CE	440.30	0	522.40	
Up to 158.5h	0.11	0	0.13	
158.5h up to 488.27h	0.33	0	0.39	
488.27h up to 720h	0.22	0	0.27	
More than 720h	0.18	0	0.22	
N	2278	358	1920	

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Source: NEPS SC3 10.0.0, own calculations.

compared to other types of secondary schools.

Having a first look at the regressors, it is important to note that only 358 (16%) of our respondents have not been exposed to any civic education in secondary schooling so far. For those exposed to civic education before grade 8, the average timespan of exposure is 3.42 school years, and the average intensity is 3.29 hours per week, summed up for grades 5 to 8.[24] It is also visible

[21] The scale for all Big Five measures reaches from 1 to 5.
[22] The perceptual speed scale covers the values 1 to 93.
[23] The reasoning scale spans from value 0 to 12.
[24] For the entire sample, the average timespan of exposure is 2.88 school years, whereas the average intensity

that the distribution of the timespan dummies is quite uneven: only 11% of affected students received only one year of civic education, 9% faced two or three years and the huge majority of 65% were exposed to four years of CE lessons. By construction, the share of students in every single intensity dummy is almost equal at about 20%. The same applies to the total amount of CE dummies. Here, only the two dummies indicating less civic education receipt are more unevenly distributed.

For the multivariate investigation of the relationship between exposure to civic education and its timespan, intensity, and total amount before grade 8 and self-reported political interest, I run multiple regression models. I first run bivariate models (results are depicted in Table A4.2 in the Appendix) before adding my set of controls for every regressor separately. Then, I run a model accounting for the mode of implementation as a single or combined subject. Additionally, I check for possible heterogeneities in the association between exposure to civic education and political interest by running models separated by gender, school type, and parental education. Lastly, I do some robustness checks.

First, I report the main results as displayed in Table 4.3. Before including my set of control variables, I run a bivariate model (column (1) in Table A4.2 in the Appendix). Here, self-reported political interest turns out to be positively related to having received civic education at school. This positive association can also be found after adding the control variables (column (1) in Table 4.3), indicating that civic education still correlates to political interest when important confounders are considered in the model. For evaluating the size of this, and the following results, recall that self-rated political interest was measured on a 4-point scale, with a mean value of 2.25 and a standard deviation of 0.80.

To account for some of the multiple differences in the provision of civic education in secondary school that occurs because the German federal states are in charge of the organization and content of their educational system, I additionally consider how long and how intense students have already been exposed to CE. Looking at the timespan students were treated with CE, the bivariate model (column (3) in Table A4.2 in the Appendix) shows a positive and significant correlation of all timespans with self-rated political interest. This also holds when control variables are added (column (2) in Table 4.3). Compared to receiving zero years of civic education, the correlation with political interest is biggest for those having had just one year of civic education lessons.[25] This may be the case, as these lessons are new to the students and may therefore be

adds up to 2.77 hours per week in grades 5 to 8.

[25] Statistical testing reveals that the differences between the dummies are not statistically significant (Wald tests: 1 year versus 2 or 3 years, p-value 0.26; 1 year versus 4 years, p-value 0.11; 2 or 3 years versus 4 years,

Table 4.3: Main results

	political interest				
	(1)	(2)	(3)	(4)	(5)
Civic Edcuation	0.229***				
	(0.0273)				
Timespan of CE (Ref.: no civic education)					
1 year		0.314***			
		(0.0611)			
2 or 3 years		0.226**			
		(0.0823)			
4 years		0.208***			
		(0.0232)			
Intensity of CE (Ref.: 0h of civic education)					
Up to 2h			0.264***		
			(0.0545)		
2h up to 3.11h			0.219***		
			(0.0299)		
3.11h up to 4.5h			0.264***		
			(0.0336)		
More than 4.5h			0.148***		
			(0.0229)		
Implementation of CE as... (Ref.: no civic education)					
...single subject				0.261***	
				(0.0728)	
...combined subject				0.223***	
				(0.0209)	
Total amount of CE lessons until grade 8 (Ref.: 0h of civic education)					
Up to 158.5h					0.303***
					(0.0684)
158.5h up to 488.27h					0.227***
					(0.0279)
488.27h up to 720h					0.249***
					(0.0443)
More than 720h					0.152***
					(0.0275)
Controls	X	X	X	X	X
N	2278	2278	2278	2278	2278

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: gender dummy, migration background dummy, year of birth, parental education, school type dummies, schooling in West Germany, Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns represend the results of seperate regressions.
Source: NEPS SC3 10.0.0, own calculations.

more effective in triggering political interest.

Turning to the intensity of civic education, the bivariate model (column (5) in Table A4.2 in the Appendix) reveals a positive correlation to political interest of almost all intensity levels. As soon as the control variables are added (column (3) in Table 4.3), the positive correlation can also be reproduced. Here, the same tendency as for the timespan of exposure is visible: compared to having had zero hours of CE, the relationship to political interest is biggest for those receiving the least amount of civic education, namely up to 2 hours. Amounts of more than 2 hours up to 3.11 hours and 3.11 hours up to 4.5 hours correlate quite similarly to my political interest measure, whereas receiving more than 4.5 hours of CE shows a lower impact.[26]

As stated above, the federal states and school types also differ in how they provide civic education in secondary schools. Some implement subjects that are specially devoted to CE into their curriculum while others consider CE as a part of a larger group of subjects (such as history, geography, law, and economics). Here, I would expect, that fostering political interest is more pronounced when civic education is taught as a single subject because in this case, it is possible to cover a higher range of topics enhancing political knowledge and having more in-depth discussions about current political events. Having a single subject for CE requires that a fixed amount of time is devoted to civic education, whereas combined subjects offer more freedom to decide how much time is actually spent on CE. In this case, it is possible that the disposable time will be distributed unequally across the different subjects and that less time will be spent teaching CE compared to the other topics. The results in column (4) in Table 4.3 are in line with this expectation, as the positive and statistically significant correlation under investigation is bigger for students receiving civic education as a single subject than for those receiving combined subjects covering CE embedded in a group of other social sciences. However, it has to be noted that the difference is small and not statistically significant (Wald test with p-value 0.55).

Column (5) in Table 4.3 shows the results for the total amount of civic education received in grades 5 to 8. Here, a similar pattern as for the intensity measure is visible: receiving civic education lessons is always positively correlated to political interest irrespective of its total amount. Again, the highest correlation shows for the lowest category of up to 158.5 hours in total and the lowest for having more than 720 hours. The fact that the results are quite similar to the ones illustrated above is indeed no surprise, as the intensity measure is used for the calculation of the total amount of CE lessons received in grades 5 to 8. Nevertheless, it is still interesting

p-value 0.83).

[26] Performing Wald tests shows that only the coefficients of the dummies up to 2h and more than 4.5h (p-value 0.03), as well as 3.11h up to 4.5h and more than 4.5h (p-value 0.02), are significantly different.

to do this exercise as the total amount of CE is not just an upward shift of the intensity by a certain number of school weeks but the federal states differ in their amount of vacancies and hence also in their amount of weeks in school. Therefore, the intensity of teaching CE is reflected differently in the summary measure depending on the holiday arrangements of the federal states. Nevertheless, the consequences of the dissimilar amount of vacancies across federal states are not severe enough to change the general pattern of results.

4.7 Heterogeneity analyses

For the main analyses, as in many other studies, I treated the students I looked at as a homogeneous group, assuming that the association between civic education lessons in secondary school and political interest is equally pronounced for everybody. As this procedure may not be sufficient, I do some subgroup analyses looking for heterogeneities in the association under investigation. The fact that political participation among youth is a complex, multifaceted and group-specific field is also emphasized by Henn and Foard (2014) and Stadelmann-Steffen and Sulzer (2018).

First, the relationship between CE and political interest is reconsidered, separating the sample by gender. As the results in columns (2) and (3) in Table 4.4 show, the relationship under investigation indeed slightly differs by gender. For boys, the association between exposure to CE and political interest is smaller than for girls. This indicates that teaching civics is of special importance for girls, as it fosters their interest in political issues more. The smaller relationship for boys may be due to the greater relative importance of other socialization agents, such as parents or peers, or the fact that boys usually show higher levels of political interest in the first place[27] and thus the potential to increase their interest in politics is smaller. In line with my findings, Geboers et al. (2015) show that in a sample of 12-16-year-old Dutch students, girls in general developed a higher interest in societal issues, more pronounced prosocial ability and a larger body of societal- and interpersonal knowledge during lower secondary education than boys.

Second, I separate the sample by school type and compare students visiting higher secondary schools with those attending other school types. As depicted in columns (4) and (5) in Table 4.4, the results separated by school type show that civic education is positively related to self-reported political interest for students attending higher secondary education and also students from all other school types. The higher coefficient for students attending other school types than higher

[27] Descriptive statistics show that this is also the case in my sample as the average political interest score for boys (2.34) is significantly higher than for girls (2.15).

Table 4.4: Correlation of civic education and political interest separated by gender, school type and parental education

	political interest						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	boys	girls	Higher secondary school	other school type	parental education below average	parental education above average
Civic Education	0.229***	0.206***	0.248***	0.184***	0.279***	0.320***	0.154***
	(0.0273)	(0.0446)	(0.0515)	(0.0319)	(0.0230)	(0.0417)	(0.0373)
Controls	X	X	X	X	X	X	X
N	2278	1141	1137	1336	942	1004	1274

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: gender dummy (model 1, 4-7), migration background dummy, year of birth, parental education (model 1-5), school type dummies (model 1-3, 6, 7), schooling in West Germany, Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns represend the results of seperate regressions.
Source: NEPS SC3 10.0.0, own calculations.

secondary school may be due to the fact that civic education appears to be more important in the curricula of these schools, as visible in Table 4.1. In almost all federal states the amount of CE is lower (and teaching starts later) in higher secondary schools compared to the other school types and therefore may not impact as much on students political interest. On the other hand, children attending higher secondary school are more likely to stem from high SES families. Therefore, it can be assumed that they already show higher levels of political interest before entering civic education lessons at school because of political socialization at home, leading to a lower potential to further enhance interest in political issues. Investigating this explanation descriptively shows that the mean value of political interest for students from higher secondary schools (2.35) is significantly higher compared to 2.11 for children attending other types of secondary schools.

The last heterogeneity I look at is supposed to shed further light on the influence of childrens' family background. More precisely, I separate the sample by highest parental education[28] (below and above average years of education in the sample).

As visible in columns (6) and (7) in Table 4.4 the correlation between being exposed to civic education lessons at school and political interest is significantly positive for both groups but appears to be lower for students with highly educated parents than for those with parents with an educational level below the mean of the sample. This finding suggests that civic education in school can play a role in compensating for missing political socialization at home and help close the gap in political interest between children with less educated parents (with a mean of 2.09)

[28] Unfortunately, NEPS data does not contain variables about parental political interest or participative behavior. As a robustness check, I compare parental ISEI and parental education as proxies for parental socialization.

and students with highly educated parents (with a mean value of 2.37).[29]

A similar result is reported by Gainous and Martens (2012) who investigate the question of whether the home environment influences the effectiveness of civics instruction using the US version of the 1999 Civic Education Study by the International Association for the Evaluation of Educational Achievement (IEA). They find that the frequency of civic education lessons is only influencing political knowledge, external political efficacy, and intent to vote of grade 9 students coming from families scoring below the mean of their home environment index which combines information on expected own education, parental education, number of books at home and frequency of political discussions. They, therefore, conclude that providing civic education is not equally effective for all student groups and argue that stimulating external influences from students' homes may lead to higher gains in their political outcomes compared to extending teaching civics at school (Gainous and Martens, 2012, p. 254).

Also, Neundorf et al. (2016) ask how family and school interact in Belgium and the US, using long-standing panel data from the Belgian Political Panel Study (2006–2011) and the US Youth-Parent Socialization Panel Study (1965–1997). They show that civic education at school compensates for dissimilar political socialization in the families in the US and Belgium and that this conclusion can be drawn for different points in time (the 1960s and the 2000s) as well as for varying length of observation windows from youth to late adulthood and impressionable years only. Differentiating level- from developmental effects reveals that while "high levels of parental socialization and civic education boost starting levels of political engagement at the age of 14, civic education —especially in form of group-work— affects the development of political engagement for respondents from less political families more." (Neundorf et al., 2016, p. 946)

4.8 Robustness checks

To address some of the specifics that have not been considered so far, I run additional robustness analyses. First, as already mentioned before, the variance in the provision of civic education lies on the level of federal states and types of schools, which makes controlling for both unfeasible. In Table 4.5, I therefore compare three models with different strategies to deal with this issue: in model (1) neither type of school, nor federal states are controlled for, in model (2) only school types are included, and finally model (3) includes type of school dummies and an east indicator

[29] Descriptive analyses of our data show that children having parents with an educational level above the sample average report a significantly higher frequency of discussions about political and social issues at home. In this context, Dostie-Goulet (2009) shows that children of parents often discussing politics are more interested in political issues.

(like in the main specification above). It is evident that the results do not differ substantively and it can be concluded that my decision for the model (3) specification is not driving my main results.

Table 4.5: Model comparison

	political interest		
	(1) Without type of school and without federal state control	(2) With type of school and without federal state control	(3) With type of school and schooling in West Germany dummy
Civic Education	0.220***	0.242***	0.229***
	(0.0257)	(0.0265)	(0.0273)
Controls	X	X	X
N	2278	2278	2278

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: gender dummy, migration background dummy, year of birth, parental education, school type dummies (model 2, 3), schooling in West Germany (model 3), Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns represend the results of seperate regressions.
Source: NEPS SC3 10.0.0, own calculations.

Next, I compare using parental ISEI and parental education as a proxy for political socialization within the family.

The results are shown in Table 4.6 and indicate that the correlation between parental education and political interest is less pronounced compared to parental ISEI. The differences with respect to the influence on the link between civic education and political interest are neglectable instead. Another issue that may influence the findings is the degree of politicization in the environment. It is reasonable to assume that politics is high on many people's agenda in an election year. Both students' political interest and political discussion in class are supposed to be affected, and thus upcoming elections might be relevant in the given context. The majority of measures in my analyses have been surveyed in 2013 (and the beginning of 2014) when the election to the Bundestag was close (22.09.2013). As this election was held on the federal level and equally affected students and their parents from all states, I expect that my results are not biased by this. Another, more relevant side of the same coin is elections on the federal state level, which are considered to be part of the unobserved heterogeneity on the federal state level. In Bavaria, Hesse, and Lower Saxony, the respective inhabitants were called to vote for new parliaments in 2013. As these elections are not equally affecting all of Germany, I separate my sample by the presence of an election in 2013.

[30] Mind that the number of cases is slightly lower here because parental ISEI consisted of more missing data. To base the comparison on the same sample, it was cut according to available data on parental ISEI.

Table 4.6: Comparison of parental ISEI and parental education

	political interest					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	parental ISEI below average	parental ISEI above average	All	parental education below average	parental education above average
Civic Edcuation	0.247***	0.333***	0.160***	0.240***	0.312***	0.177***
	(0.0272)	(0.0334)	(0.0349)	(0.0273)	(0.0380)	(0.0438)
parental ISEI	0.119***					
	(0.0267)					
parental education				0.038***		
				(0.0088)		
Controls	X	X	X	X	X	X
N[30]	2130	1113	1017	2130	939	1191

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: gender dummy, migration background dummy, year of birth, parental ISEI (model 1), parental ISEI below average dummy (model 2), parental ISEI above average dummy (model 3), parental education (model 4), parental education below average dummy (model 5), parental education above average dummy (model 6), school type dummies, schooling in West Germany, Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns represend the results of seperate regressions.
Source: NEPS SC3 10.0.0, own calculations.

Table 4.7: Correlation of civic education and political interest separated federal states with and without an election to federal-state parliament in 2013

	political interest	
	(1)	(2)
	federal states with election in 2013	federal states without election in 2013
Civic Education	0.310*	0.234***
	(0.0849)	(0.0398)
Controls	X	X
N	762	1516

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: gender dummy, migration background dummy, year of birth, parental education, school type dummies, Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns represend the results of seperate regressions.
Source: NEPS SC3 10.0.0, own calculations.

The results in Table 4.7 show that it is indeed important to take into account elections to federal state parliaments. For both groups of federal states (with and without elections in 2013) the correlation between civic education at school and political interest is positive and statistically significant. The correlation is bigger in federal states, with an imminent election. This may be because politics is omnipresent these days, which may lead teachers to take the opportunity and make the upcoming election part of their lessons, take some extra time for this or discuss diverging positions of the parties running for office in the particular federal state. These efforts may then contribute to students showing increased levels of political interest.

In addition to self-rated political interest, NEPS provides two other measures that are close to the concept of political interest but focus on slightly different facets. The first asks how often students talk about political issues to classmates, friends, or parents aside from class. According to Hahn (1999), talking about political affairs is an indicator of political interest. The second measure covers the frequency of following political issues in the media (TV, internet, newspaper, radio) and can be interpreted as an activity revealing interest in the topic.

Generally, the results do not change substantially and similar patterns become evident when using these indicators (see Tables A4.3 and A4.4 in the Appendix) instead of self-rated political interest as outcome variable. Almost all relationships under investigation are significantly positive. The most remarkable deviations from the main results are briefly pointed out: The correlation between receiving civic education as a single subject (compared to no CE) and the frequency of talking about political issues outside class is not statistically significant (see column (8) in Table A4.3 in the Appendix). Thus, the positive overall relationship seems to be driven by those attending combined subjects covering contents of civic education. Supposedly, triggering students to go on talking about those issues outside class is more successful, if CE is embedded in a bigger context of history, economics, or law. For both talking about politics and following it in the media, the highest correlation with the intensity of civic education lessons shows for the category more than 3.11 up to 4.5 hours (see column (6) in Tables A4.3 and A4.4 in the Appendix, respectively).[31] Accordingly, also the correlation between both alternative outcome measures is highest with a total amount of CE of 488.27h up to 720h (see column (10) in Tables A4.3 and A4.4 in the Appendix, respectively). Apart from that, the overall pattern of results is similar for following politics in the media and only slightly different for talking about politics. The smallest correlation is visible for the highest amount of CE for all outcomes.

[31] Just like in the main results only the difference between up to 2h and more than 4.5h of CE (p-value 0.01) and between 3.11h up to 4.5h (p-value 0.09) is statistically significant. The differences in the timespan of CE coefficients and its mode of implementation are not statistically significant.

Summing up, these results indicate that my main results are not driven by using the self-rated political interest measure, but can be reproduced with other indicators being close to the concept of political interest.

4.9 Conclusion

In conclusion, with this study, I add to the scarce literature on civic education in German secondary schools by having a fine-graded and explorative look into the relationship between different facets of civic education provision and youth political interest. I show that receiving civic education classes, in general, is positively related to self-rated political interest of grade 8 students. Then, I conclude that it seems valuable to consider different features of civic education to get a more nuanced picture of what is actually happening in a system as complex as the German one by further disentangling different levels of CE intensity and timespans of exposure. Also, by acknowledging students as a heterogeneous group, some interesting insights become evident. The correlation between being exposed to civic education classes is higher for girls, students not visiting higher secondary schools, and having parents with fewer years of education than the average in the sample. As these are the ones normally showing less interest in political issues, civic education at school bears the potential of reducing pre-existing inequalities in political behavior. Although showing these differences is an important first step, further efforts are needed to explain these group-specific disparities in more detail.

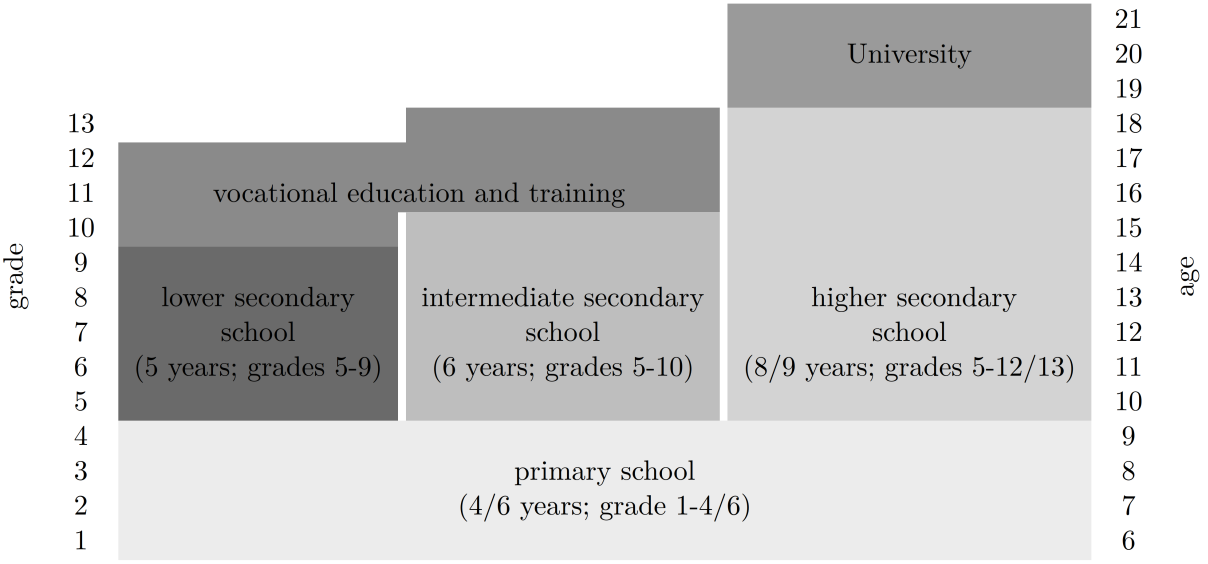
Nevertheless, this study is a valuable first glance into civic education in German secondary schools but also suffers from limitations that must be kept in mind when classifying the results. First and foremost, these results only approximate causal effects, as I do not have (quasi-)experimental data and the variation in civic education that I use here cannot claim to be purely exogenous. Nevertheless, I argue that I ruled out as many sources of potential endogeneity as possible. By implementing a set of control variables, I account for some important confounding variables. In doing so, I attempt to eliminate the most likely source of bias, namely the lack of random assignment of children's school type and the place of schooling (and the exposure to the corresponding state-specific educational system). At the same time, I argue that (self-) selection would cause disproportionately high costs as moving across federal state borders with the entire family because schools in other federal states provide more (or less) civic education does not seem reasonable as parents can more easily compensate for this themselves. Hence, switching school type is very likely to be accompanied by another secondary school degree, which is crucial

for access to apprenticeships, tertiary education, and the labor market. Thus, bearing all these implications for the entire life course because of civic education lessons would not be rational. Nevertheless, selection processes cannot be ruled out here either, so using a proper identification strategy to account for causal effects remains crucial for future research. Also, this study may suffer from unobserved confounders on the federal state level, as it was not possible to include a full set of federal state controls, because of little variance in the data at hand.

Another limitation is that I can only account for civic education that is provided within the subjects devoted to teaching political content. It is indeed possible that other subjects also cover some aspects of civic education, such as the functioning of other countries' political systems in foreign language classes. Even within the share of civic education that could be implemented here, it is not possible to learn about the contents covered. Investigating the level of content that means taking into account the civic education curricula, therefore, remains for future research. Doing this could be highly relevant, as it would be possible to provide guidelines based on empirical evidence for those in charge of setting up revised curricula to make civic education as effective as possible. In addition to the exposure to politics in school and the corresponding timespan as well as intensity, it would be very interesting for future research to further explore their interrelations. Like that, it would be possible to shed light on the question if a higher amount of civic education is more influential in lower or higher grade levels. Also, more research is needed to take a profound look at the mechanisms being at work here. Given the heterogeneous correlations of civic education and political interest for divergent student groups, the question of how they can be explained is a further step towards a better understanding of the issue. Lastly, expanding the outcomes under investigation would be very worthwhile. As political interest is often seen as a first approximation to the political field, it would be very interesting to examine empirically, if teaching induced political interest actually translates into participating in politically motivated activities, like demonstrating or even joining a political party.

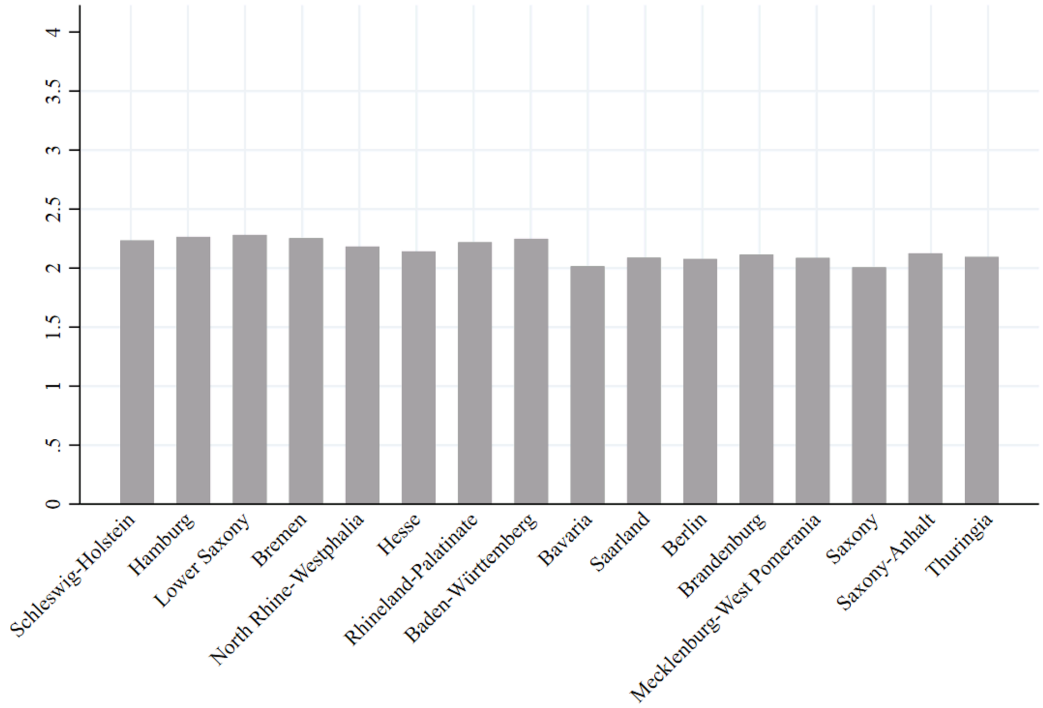
Appendix

Figure A4.1: The German educational system (stylized)



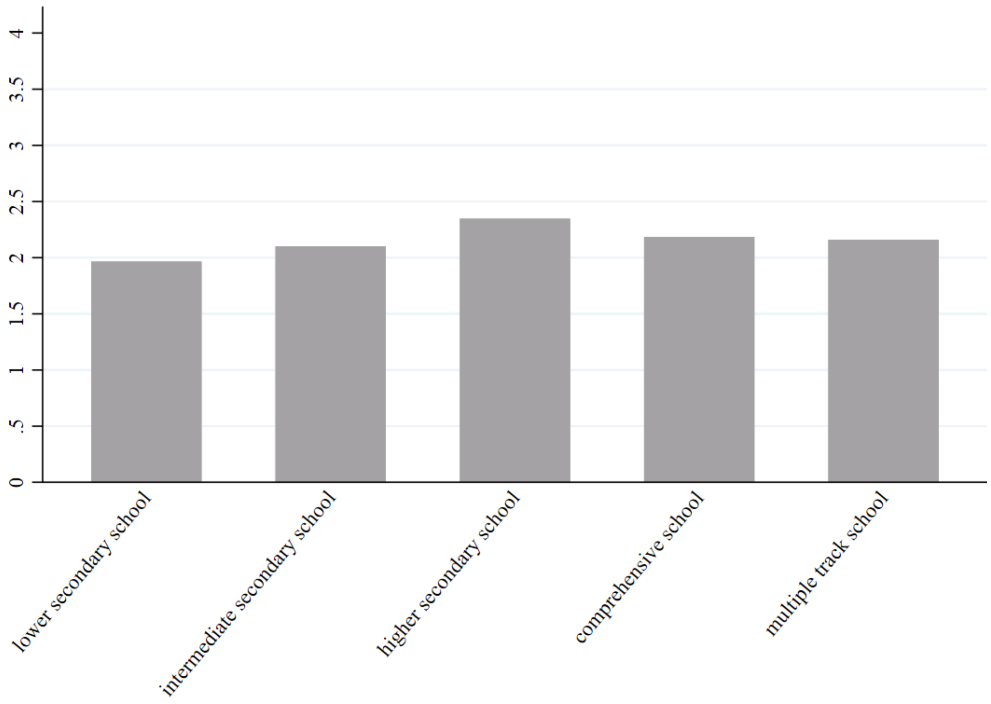
Source: European Commission/EACEA/Eurydice (2019), own illustration.

Figure A4.2: Mean of political interest by federal state



Source: NEPS SC3 10.0.0, own calculations.

Figure A4.3: Mean of political interest by school type



Source: NEPS SC3 10.0.0, own calculations.

Table A4.1: Comparison OLS and (ordered) probit models

	(1) very interested	(2) fairly interested	(3) hardly interested	(4) not interested at all
Panel A. OLS models for dummies				
Civic Education	0.030***	0.081***	-0.021	-0.089***
	(0.0073)	(0.0173)	(0.0159)	(0.0141)
N	2278	2278	2278	2278
Panel B. Probit models for dummies				
Civic Education	0.268***	0.262***	-0.054	-0.361***
	(0.0580)	(0.0545)	(0.0407)	(0.0651)
N	2278	2278	2278	2278
Panel C. Average Marginal Effects of Ordered Probit model				
Civic Education	0.041***	0.074***	-0.039***	-0.076***
	(0.0064)	(0.0091)	(0.0085)	(0.0074)
N	2278	2278	2278	2278

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Controls: gender dummy, migration background dummy, year of birth, parental education, school type dummies, schooling in West Germany, Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns in Panel A. and B. represend the results of seperate regressions.
Source: NEPS SC3 10.0.0, own calculations.

Table A4.2: Correlation of civic education and political interest

	political interest									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Civic Edcuation	0.190***	0.229***								
	(0.0377)	(0.0273)								
Timespan of CE (Ref.: no civic education)										
1 year			0.341***	0.314***						
			(0.0892)	(0.0611)						
2 or 3 years			0.125***	0.226**						
			(0.0331)	(0.0823)						
4 years			0.174***	0.208***						
			(0.0435)	(0.0232)						
Intensity of CE (Ref.: 0h of civic education)										
Up to 2h					0.249***	0.264***				
					(0.0509)	(0.0545)				
2h up to 3.11h					0.292***	0.219***				
					(0.0464)	(0.0299)				
3.11h up to 4.5h					0.029	0.264***				
					(0.0406)	(0.0336)				
More than 4.5h					0.199***	0.148***				
					(0.0403)	(0.0229)				
Implementation of CE as... (Ref.: no civic education)										
... single subject							0.072*	0.261***		
							(0.0410)	(0.0738)		
... combined subject							0.202***	0.223***		
							(0.0430)	(0.0209)		
Total amount of CE lessons until grade 8 (Ref.: 0h of civic education)										
up to 158.5h									0.341***	0.303***
									(0.0892)	(0.0684)
158.5h up to 488.27h									0.246***	0.227***
									(0.0543)	(0.0279)
488.27h up to 720h									0.029	0.249***
									(0.0406)	(0.0443)
more than 720h									0.199***	0.152***
									(0.0403)	(0.0275)
Controls		X		X		X		X		X
N	2278	2278	2278	2278	2278	2278	2278	2278	2278	2278

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: gender dummy, migration background dummy, year of birth, parental education, school type dummies, schooling in West Germany, Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns represend the results of seperate regressions.
Source: NEPS SC3 10.0.0, own calculations.

Table A4.3: Correlation of civic education and talking about politics outside class

	Talk about politics[32]									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Civic Edcuation	0.138**	0.181***								
	(0.0564)	(0.0346)								
Timespan of CE (Ref.: no civic education)										
1 year			0.218*	0.194**						
			(0.1075)	(0.0671)						
2 or 3 years			0.065	0.195***						
			(0.0889)	(0.0455)						
4 years			0.136*	0.175***						
			(0.0643)	(0.0343)						
Intensity of CE (Ref.: 0h of civic education)										
Up to 2h					0.195***	0.212***				
					(0.0546)	(0.0430)				
2h up to 3.11h					0.247***	0.166***				
					(0.0764)	(0.0419)				
3.11h up to 4.5h					-0.017	0.238***				
					(0.0677)	(0.0408)				
More than 4.5h					0.136**	0.087**				
					(0.0503)	(0.0353)				
Implementation of CE as... (Ref.: no civic education)										
...single subject							-0.067	0.153		
							(0.0620)	(0.0994)		
...combined subject							0.159**	0.186***		
							(0.0610)	(0.0269)		
Total amount of CE lessons until grade 8 (Ref.: 0h of civic education)										
up to 158.5h									0.218*	0.178**
									(0.1075)	(0.0713)
158.5h up to 488.27h									0.220***	0.201***
									(0.0681)	(0.0323)
488.27h up to 720h									-0.017	0.236***
									(0.0677)	(0.0414)
more than 720h									0.136**	0.093**
									(0.0503)	(0.0317)
Controls		X		X		X		X		X
N	2278	2278	2278	2278	2278	2278	2278	2278	2278	2278

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01

Controls: gender dummy, migration background dummy, year of birth, parental education, school type dummies, schooling in West Germany, Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns represend the results of seperate regressions.

Source: NEPS SC3 10.0.0, own calculations.

[32] Talking about politics was measured on a 4-point Likert scale, has a mean value of 2.00 and a standard deviation of 0.75.

Table A4.4: Correlation of civic education and following politics in the media

	Follow politics in the media[33]									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Civic Edcuation	0.184***	0.272***								
	(0.0611)	(0.0419)								
Timespan of CE (Ref.: no civic education)										
1 year			0.343***	0.332***						
			(0.1018)	(0.0576)						
2 or 3 years			0.081	0.262*						
			(0.0474)	(0.1260)						
4 years			0.172**	0.259***						
			(0.0737)	(0.0398)						
Intensity of CE (Ref.: 0h of civic education)										
Up to 2h					0.273***	0.306***				
					(0.0596)	(0.0496)				
2h up to 3.11h					0.279**	0.171*				
					(0.1201)	(0.0891)				
3.11h up to 4.5h					-0.034	0.417***				
					(0.0891)	(0.0939)				
More than 4.5h					0.231***	0.166**				
					(0.0574)	(0.0608)				
Implementation of CE as... (Ref.: no civic education)										
... single subject							0.027	0.353***		
							(0.0639)	(0.0774)		
... combined subject							0.199**	0.258***		
							(0.068)	(0.0449)		
Total amount of CE lessons until grade 8 (Ref.: 0h of civic education)										
up to 158.5h									0.343***	0.300***
									(0.1018)	(0.0705)
158.5h up to 488.27h									0.254***	0.239***
									(0.0841)	(0.0590)
488.27h up to 720h									-0.034	0.394***
									(0.0891)	(0.0986)
more than 720h									0.231***	0.180**
									(0.0574)	(0.0610)
Controls		X		X		X		X		X
N	2278	2278	2278	2278	2278	2278	2278	2278	2278	2278

Notes: * p < 0.10, ** p < 0.05, *** p < 0.01

Controls: gender dummy, migration background dummy, year of birth, parental education, school type dummies, schooling in West Germany, Big Five, perceptual speed, reasoning; Robust standard errors in parentheses, clustered at the federal state level. All columns represend the results of seperate regressions.

Source: NEPS SC3 10.0.0, own calculations.

[33] Following politics in the media was measured on a 5-point Likert scale, has a mean value of 2.76 and a standard deviation of 1.22.

Chapter 5

The Impact of Schooling Intensity on Political Participation and Interest - Evidence from a German Higher Secondary School Reform

Nadja Bömmel

5.1 Introduction

Effects of increased schooling intensity have been at the center of attention at least since policy-makers discuss it as a means to encounter demographic change and skilled labor shortage. A manifold literature emerged shedding light on the impact of schooling intensity on differential outcomes, predominantly student performance indicators (Anderson and Walker (2015); Andrietti and Su (2019); Büttner and Thomsen (2015); Camarero Garcia (2022)) but also other school-related outcomes like repetition rates (Huebener and Marcus (2017); Pischke (2007)), the likelihood of attending higher secondary school (Pischke (2007)), graduation rates (Huebener and Marcus (2017)), university enrollment (Büttner and Thomsen (2015); Marcus and Zambre (2019); Meyer and Thomsen (2016); Meyer et al. (2019)) and university performance (Dörsam and Lauber (2019); Kühn (2014)). Long-term effects of an increase in schooling intensity on labor market outcomes and earnings (Pischke (2007)) and mental and physical health (Braakmann (2010)) as well as short-term effects on crime rates and substance abuse (Westermaier (2016)) have also been investigated.

Within the schooling intensity literature, also political outcome measures have been examined in the short (Krekel (2017)) as well as in the long run (Pischke (2003)). In this paper, I re-visit the effect of an increase in higher secondary schooling intensity on political interest. I further expand the range of outcomes by also looking at diverse forms of political participation. The latter covers participating in political parties, citizen initiatives, discussing political issues, demonstrating and signing petitions. Gaining further insights into how the increase in schooling intensity affects students' engagement in the political sphere is relevant, as adolescence is a crucial period in which individuals develop their political identity as well as participatory habits. During this time, youths start to explore political facts and circumstances and form their own opinions on multiple societal and political questions (Fend (1991); Schmid (2003)) with the help of and in interaction with multiple socialization agents like parents, peers, and schools (Dostie-Goulet (2009); Koskimaa and Rapeli (2015)). This formative phase is highly important, as these attitudes tend to remain rather stable in adulthood. For the case of political interest, Prior (2010) examines eleven different panel studies collected in four countries over a period of 40 years to show that political interest is exceptionally stable in the short as well as in the long run. These findings stress the conclusion that all influences that may stimulate or distract these formation processes in adolescence bear the risk of having consequences in the long run, as they are highly predictive for political interest and participation in adulthood. Arguably, the increase in schooling intensity

may be a disruptive factor in this process.

In my empirical strategy, I take advantage of a higher secondary schooling reform (the so-called G8 reform) that was implemented in most of the German federal states between 2001 and 2008, as a natural experiment to study this question. Former literature shows that this reform is well suited for providing exogenous variation in schooling intensity across time and federal state in German higher secondary schools (e.g. Dahmann (2017)). In the course of the implementation of the G8 reform, the intensity of schooling had to be increased, manifesting in two distinct ways namely the extension of school days into the afternoon and the increase in the intensity of instruction. Theory suggests that both may have detrimental effects on students' political engagement. Prolonged school days lead to less time for other activities outside school such as participating in political actions like demonstrating or joining a youth organization of a political party. Increasing the intensity of instruction means that the same curriculum has to be covered at a faster pace, followed by a higher perception of stress. This in turn leads students to have a smaller amount of free mental capacities to develop other interests apart from school content, like concerns with political issues.

For my empirical analysis, I use cross-sectional data from Aufwachsen in Deutschland: Alltagswelten (AID:A). My results show, that the increased schooling intensity had an overall negative effect on political interest in the short run. It also negatively affected one of the facets of political participation covered, namely engaging in a political party. The results for all other political activities also show negative signs, but could not reach conventional levels of statistical significance. These findings are quite robust with respect to the implementation schemes in different federal states, the sample composition as well as selection out of treatment. Further analyses show that effects are slightly heterogeneous according to gender and non-intact family background and seem to be strongly driven by the double cohort. Checking for the potential mechanisms of free time and intensity of instruction reveals only suggestive evidence as none of these channels reaches conventional levels of statistical significance. Instead, students' performance level could be identified as a significant mechanism being at work here.

Tackling the issue of the importance of timing, I further investigate the long-term effects of higher schooling intensity on the very same outcomes in another sample consisting of respondents aged 25 to 30. The results show that the reform effects on political participation and interest vanish. This leads to the conclusion that political participation and interest are only negatively influenced in the short but not in the long run.

Further analyses show that schooling intensity is not only impacting on children but also on

their parents, as their political interest and information behavior is affected by the increase in schooling intensity.

With this study, I add to a still small body of literature on the causal effects of education on political participation. Additionally, I add to the strand of literature investigating the short- and long-term effects of increases in schooling intensity and the unintended consequences of the German G8 reform as a quasi-natural experiment. Most aspects of political participation have not been investigated in this context so far, which is addressed here by expanding the range of political outcome measures under investigation in addition to reexamining the case of political interest. By including measures covering actual participation in diverse political actions, it is possible to shed light on a broader picture and to see if the effects on political interest that Krekel (2017) already showed actually translate into active behavior. For the research at hand, the AID:A dataset that I use is especially suitable because it contains data on the household as well as on the individual level and covers a substantial range of birth cohorts. This enables me to investigate (1) short-term effects on young respondents while still in school (starting from age 13), (2) long-term effects on older respondents who already left the educational system (age 25 to 30) as well as (3) spillover effects on parents of children being targeted by higher intensity schooling.

First, expanding the age range to younger ages (compared to age 17 in former studies using SOEP data) appears to be interesting, as the reform did not only influence older students close to graduation but the whole track of higher secondary schooling. Usually, increasing weekly class hours and prolonging school days most heavily affected students in grades seven to ten (Dahmann (2017); Huebener and Marcus (2015)), leading to the most immediate effects of intensified schooling starting at age 12. Huebener et al. (2017, p. 768) also argue that it would have been necessary to collect data in grades five to nine/ten (Sekundarstufe I in the German system) in the course of a reform evaluation because additional demands coming along with G8 potentially are especially apparent here. In my context, this is essential, as the formative phase in which political attitudes are developed and the foundations for future participative behavior are laid starts in early adolescence, so it is important to be able to include younger students as well.

Second, having a look at the long-term effects of a higher schooling intensity is of particular importance as the implications of long-term effects on political participation and interest would be more far-reaching compared to the immediate effects intensified schooling may have while still in school or shortly after graduation. Investigating long-term effects gives insights into the

question whether immediate effects of high schooling intensity can be compensated later on or if they persist also after leaving the educational system.

Third, AID:A household-level data allows me to investigate potential spillover effects also on the parents of children being targeted by higher-intensity schooling. To the best of my knowledge, this perspective has not been taken before but is interesting as it may uncover if educational policies have far-reaching consequences on the political interest of the parent generation.

The remainder of this paper is structured as follows. Section 5.2 provides some theoretical thoughts on the impact of schooling intensity on political participation and interest as well as a brief review of related literature. Some background about the German school system in general and the G8 reform, in particular, is given in Section 5.3. Section 5.4 describes the data and outlines the empirical strategy. Section 5.5 provides descriptive statistics, baseline results, and further heterogeneity analyses. Robustness checks, evidence on potential channels, the persistence of G8 effects in the long run, and spillover effects on parents are reported in Sections 5.6, 5.7, and 5.8 respectively. Section 5.9 concludes.

5.2 Theoretical background and literature review

In the following, some theoretical thoughts will be brought up to be able to develop hypotheses on the question of how a higher schooling intensity affects political participation and interest. As a starting point, the direct consequences of the G8 reform are twofold. First, as the total amount of hours spent in school is almost constant, but students have one school year less time until graduation, school days have to be prolonged into the afternoon and second, students have to cover the curriculum at a faster pace. Arguably, both can have implications for the likelihood to get involved in political actions and the level of political interest to be developed in adolescence. Prolonged school days make children spend more time in school in the afternoon, which was free for other activities before the reform was implemented. Therefore, under the G8 system, students have less time for free time activities, like doing sports, joining music and theater groups, or meeting their friends outside school. In the literature, the evidence for this crowding-out effect is mixed (Hübner et al. (2017); Krekel (2017); Meyer and Thomsen (2015)). As engaging in the political field can also be interpreted as an important free time activity, students may cut back on their political engagement as well. Here, G8 effects may be unequal depending on the time requirement and flexibility of an activity. The strongest negative effects may be suspected on those kinds of political actions that are both very time-consuming and inflexible, like engaging

in a political party or citizen initiative. On the other end of the continuum, signing petitions may be less affected as it is fastly done and often even possible online, providing a maximum of flexibility.[1]

On the other hand, going through the curriculum at a faster pace leads to the fact that "time available for absorbing the relevant material, for accomplishing the necessary homework, for comprehending the essentials and reiterating the pertinent subject matter declines, while the academic requirements remain the same" (Büttner and Thomsen, 2015, p. 82). Therefore, the learning intensity, defined as the quantity of material to be covered per time unit is rising, which leads students to experience more stress in coping with the requirements of school. Former studies, for example, by Quis (2018), and Marcus et al. (2020) have shown, that perceived stress is higher among G8 students. This and the pressure of keeping track of all topics covered in class may lead to the fact that not only time for activities outside school is getting scarce but also mental capacities to deal with extracurricular fields. Thus, students may focus on topics that are directly related to those covered at school and devote fewer capacities to developing their own interests. As curricula in secondary schooling typically do not cover high amounts of political education (Gökbudak and Hedtke (2020)), it may be suspected that it is less feasible for G8 students to get involved with political topics outside school. Especially low performing students are at risk of losing sight of political issues, as there are most likely struggling with the increased requirements of intensified schooling. Of course, political participation and interest are strongly interrelated, as being interested in political issues is often described as the most crucial prerequisite for getting involved (Armingeon, 2007, p. 363). Reversely, participating in political actions may provide new insights and different opinions or perceptions which may deepen attention to certain topics or trigger interest in others.

These arguments lead to the conclusion, that the higher schooling intensity implemented alongside the G8 reform is supposed to have a negative impact on political participation and interest. But there is also reason to believe that the opposite effect may arise. The implementation of G8 can be experienced as the first occasion in which adolescents realize that political decisions actually impact daily life. Particularly when students and their parents experience or fear the negative consequences of the reform, interest in the further development of educational policy may rise. Also, the willingness to support petitions or demonstrations directly related to the G8

[1] Other activities that are covered in the data, namely boycotting products out of ethical, political, or environmental reasons and protesting online are not considered in this study, as, a priori, there is no reason to believe that the G8 reform should affect these activities. Also, participation in elections is not part of this analysis, as Germans are eligible to vote at age 18 and huge parts of the sample are younger.

implementation is supposed to rise among the affected.

In the last decades, a substantial body of literature on the effects of G8 on diverse outcomes emerged (for summary articles see for example Huebener and Marcus (2015) or Thomsen (2015)). Focusing on immediate effects already visible while still in school, Huebener and Marcus (2017) show that G8 leads to a reduction of the average graduation age by 10 months, but also to a slightly higher probability of grade repetition, especially in the three years before graduation and lower grades at graduation (Büttner and Thomsen (2015); Huebener and Marcus (2017)). Concerning the impact on skills, the results of different studies are mixed. Dahmann (2017) finds a positive effect on crystallized intelligence for males but not on fluid intelligence while Huebener et al. (2017) show no differences in competencies in mathematics and physics but lower competencies of G8 students in Biology and English reading in Baden-Württemberg. Effects on PISA test scores are positive on average (Andrietti and Su (2019)).

Apart from these effects directly related to school careers, skills, and test scores, there is also evidence on the (unintended) impact of G8 on other dimensions. Dahmann and Anger (2014) as well as Thiel et al. (2014) show mixed results on adolescents' personality traits, locus of control, and self-control.

Focusing on health effects, Hofmann and Mühlenweg (2018), Hübner et al. (2017), Marcus et al. (2020) and Quis (2018) find an increase in mental health problems (partly only for girls) and in perceived stress for both genders. Existing evidence on physical health is inconclusive, as Hofmann and Mühlenweg (2018) cannot find any effect on physical health, whereas Quis and Reif (2017) show a positive effect of the reform on the Body-Mass-Index.

This increase in health problems may also be related to changes in leisure time activities. Students affected by the G8 reform spent less time on activities like jobbing, volunteering, or watching TV (Hübner et al. (2017); Krekel (2017); Meyer and Thomsen (2015)). There is mixed evidence for doing sports, reading, surfing the internet, or meeting friends whereas playing music is not affected. Interestingly, also no effects can be found when looking at scholastic involvement, for example, participation in a drama or dance group, choir or orchestra at school, or engaging as a student or class representative (Krekel (2017)).

Within the same paper, therefore being most related to my study, Krekel is also covering the effect of a higher schooling intensity on political interest using the same reform as I do and data from the Socioeconomic Panel (SOEP). Although this is not the core of his research question, he shows that the G8 reform has a differential impact on political interest in his sample of 17-year-olds. As Krekel (2017, p. 17) puts it: "there is a depolarisation at both ends of the

spectrum", as the reform has a significantly positive effect on being weakly interested and a significantly negative effect on not being interested at all and being moderately interested[2] in political issues. Unfortunately, SOEP data does only offer information on political interest and no related indicators.

Another closely related study comes from Pischke (2003), in which he exploits the exogenous increase in schooling intensity caused by the short school years that were implemented in the course of standardizing the school starting date in Germany in the 1960s. He uses repeated cross-sectional data from the German General Social Survey (ALLBUS) and shows that there is no systematic impact of the short school years on potential voting, actual voting, and political interest. He further reports that those affected by the short school years are more likely to sympathize with more radical parties by 4.4 percentage points, but also this difference does not reach conventional levels of statistical significance. Also, Pischke (2003) admits that this sympathy does not translate into voting for radical parties. His voting indicators may be misreported and not be representing actual behavior, as only very few respondents, less than one might expect when looking at official voting statistics, reported potential or actual non-voting[3] and having voted for a radical party.

I, therefore, add to the evidence on Germany by using data that is better suited to shedding light on the effect of higher schooling intensity on political participation and interest.

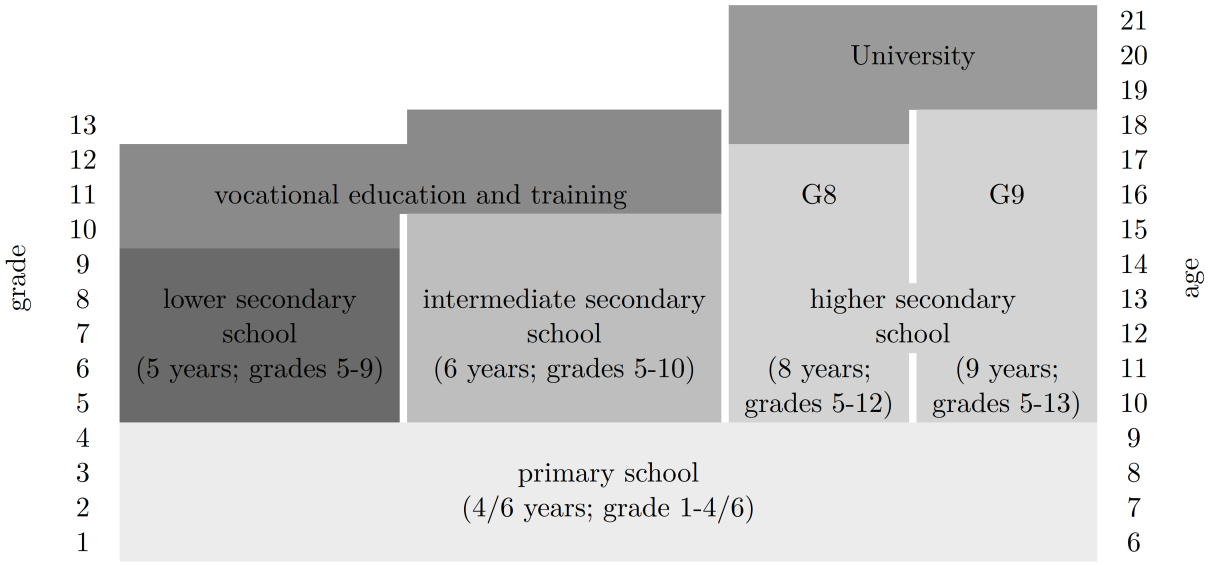
5.3 The G8 reform in the German school system

As the 16 German federal states are responsible for the content and structure of the educational system in their particular state, schooling in Germany is not completely uniform. A stylized overview is provided in Figure 5.1.

[2] Krekel (2017) combined the categories being "strongly interested" and "fairly interested" and a formed a new one that he called being "moderately interested" in politics.

[3] See also Bömmel et al. (2021) for further remarks on why self-reported voting may be a problematic indicator.

Figure 5.1: The German educational system (stylized)



Source: European Commission/EACEA/Eurydice (2019), own illustration.

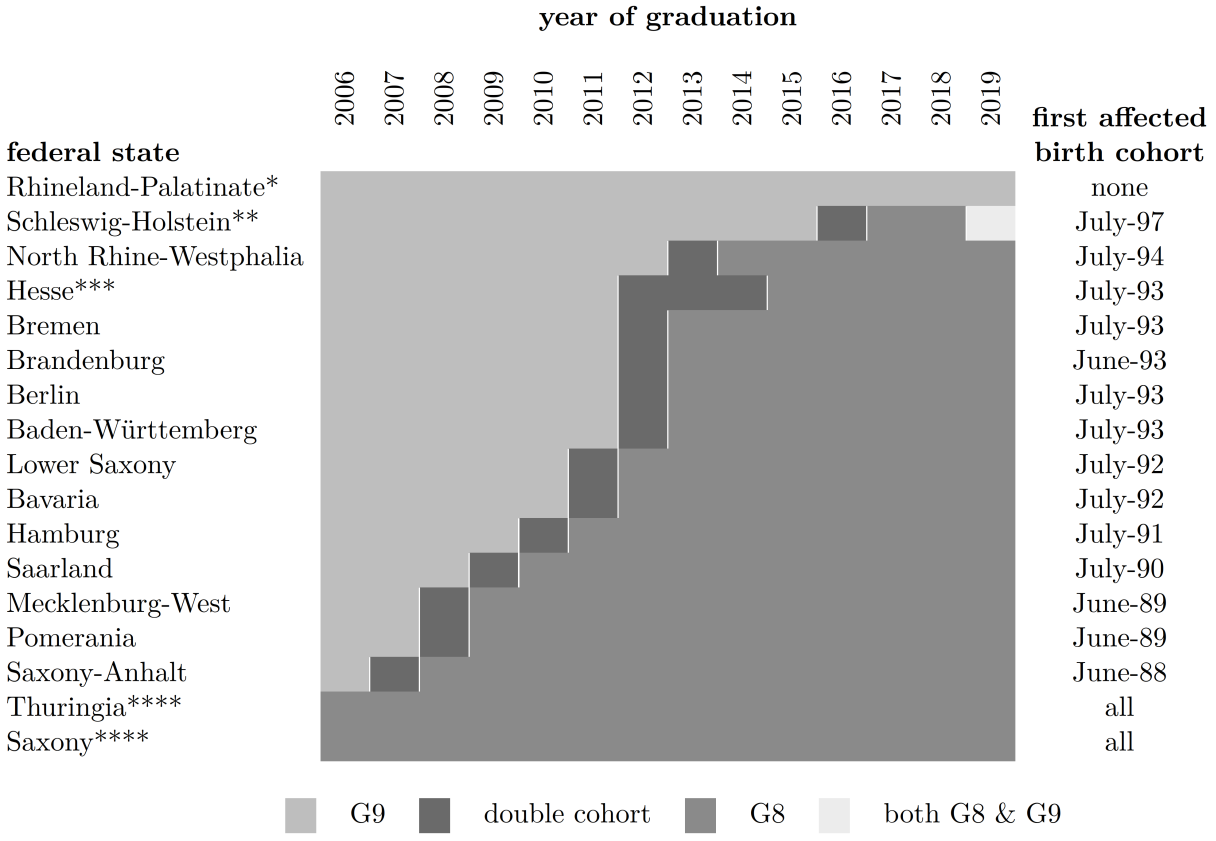
Usually, children enter primary school at the age of 6 years and are schooled together for four (in some states six) years of primary school. At about age 10 students change over to one of the three types of secondary school. Tracking is based on achievement in primary school and teacher recommendations, which are binding in some states. Lower and intermediate tracks qualify students for vocational education and training whereas only the upper (academic) track leads to the university entrance qualification (*Abitur*). Higher secondary school (*Gymnasium*) lasts from grade 5 to grade 12 (G8) or 13 (G9), depending on the fact if a federal state already reduced the duration of the track.

The need for speeding up higher secondary education was based on the change in the demographic structure of the population, which is considered one of the major challenges of the present age. In Germany, as well as in many other countries, the mean age of the population is rising as the number of younger citizens is shrinking, while older ones live longer. These developments come along with several problems, especially on the labor market, like skilled labor shortage, and concerning the financing of social security systems and pensions. The fact that German graduates are older than in many other countries further exacerbates the situation. According to OECD (2005), German students typically are 19 years old when they leave the educational system with a university entrance qualification, compared to 17-18 years in the Netherlands, 16 years in Turkey, or 17 years in Russia, leading to a comparative disadvantage in the international context. Younger graduates enter the labor market or tertiary education at a younger age, are

qualified for skilled positions earlier, and start contributing to the social systems at a younger age and also for a longer period of time. Additionally, concerns about the organization of education in Germany rose as soon as the results of the first round of the Programme for International Student Assessment (PISA) were published. Overall, PISA test scores of German students turned out to be lower than expected, whereas students in Saxony and Thuringia, where the highest educational track lasts only eight years for historical reasons[4], performed comparatively well.
To overcome these challenges, German policymakers in almost all federal states agreed upon shortening the highest (and most long-lasting) track of secondary education from 9 years to 8 years. The corresponding reform was then implemented between 2001 and 2008. Although the federal states are generally responsible for their educational system, the Standing Conference of the Ministers of Education and Cultural Affairs (KMK (2021)) predefines binding requirements to ascertain that German schools and degrees are comparable to a certain extent. One of those, set in the course of the implementation of the G8 reform, is that the demands for graduation remained the same for G9 and G8 students. That means that the overall curriculum remained unchanged and that both groups have to complete the same total instruction time from grade 5 to graduation (in grade 12 or 13) in order to obtain the university entrance qualification. As a consequence, the intensity of higher secondary schooling rose, as these requirements could only be met by increasing the weekly instruction time for G8 students, which was realized by prolonging the school day into the afternoon. Overall, the implementation of G8 resulted in an increase of 3.7 instruction hours per week (12,5%) across all subjects whereas the overall curriculum remained unaffected. This feature of the reform of not changing educational content but the intensity of teaching is one of the reasons why G8 is particularly interesting to use as a natural experiment. The second is, that the federal states are responsible for the organization of schooling which caused a staged implementation process, leading to variation across regions and time (see Figure 5.2).

[4] After reunification, Saxony and Thuringia restrained from adapting their school system to West German standards and, therefore, kept the 8-year track they already had before.

Figure 5.2: Implementation of the G8 reform in the German federal states



Notes:
* In Rhineland-Palatinate the G8 reform was only implemented in 19 out of 148 schools, all other schools kept 12,5 years.
** In Schleswig-Holstein graduating from G9 is possible again starting in 2019, as 11 of 99 higher secondary schools switch back to G9 and in 4 additional schools both G8 and G9 exist in parallel.
*** Hesse implemented G8 over a time span of 3 years.
**** Thuringia and Saxony never changed their system and always had an 8-year higher secondary school.
Source: Huebener and Marcus (2015); KMK (2022); Marcus et al. (2020), own illustration.

The federal state-specific implementation schemes are illustrated by the different coloring of the cells: medium gray stands for G9, darker grey for G8 and light grey for a system in which G8 and G9 exist in parallel. The darkest grey cells mark the double cohort, e.g. the point in time when the last cohort of G9 and the first of G8 graduate in the same year.[5]

In sum, 13 out of 16 federal states reduced the duration of higher secondary schooling, but as the implementation schemes vary between the federal states, some further specifics have to be pointed out. In the case of Rhineland-Palatinate, the G8 reform was never fully implemented, Hesse implemented G8 in a 3-year timespan, and Thuringia as well as Saxony always had an 8-year higher secondary track for historical reasons. In Schleswig-Holstein, the possibility to switch back to G9 was granted in 2011, so from 2019 graduates from both G8 and G9 are apparent

[5] As this is a very special issue I also deal with the double cohort in the robustness checks.

again[6]. How I deal with this is outlined in the next Section.

5.4 Data and empirical strategy

For the empirical analyses, I use data from Aufwachsen in Deutschland: Alltagswelten (AID:A) from the German youth institute. AID:A provides data for research on the development and behavior of children, youth, and adults within their daily life contexts. As a special focus is set on families, AID:A provides data not only on the individual but also on the household level. For my analyses, I take into account the AID:A waves from 2009, 2014, and 2019 (Kuger et al. (2020)) as a pooled cross-section. AID:A relies on a resident registration office sample of the population of all persons living in German private households aged 0 to 55 (wave 2009) or 32 (waves 2014 and 2019) and their household members (Aust et al. (2010, 2015)).

For my study, I only consider individuals currently attending the highest track of secondary school, because students in this track are the main targets of the G8 reform. For older respondents who already left school, I only take the ones holding a university entrance qualification as their highest educational degree. Although I cannot make sure that this degree was earned at a higher secondary school, it appears to be very likely as this is true for a huge majority of the university entrance qualifications in Germany. Between 2004 and 2010 less than 7% of university entrance qualifications were completed each year at comprehensive schools and around 1% each at Waldorf schools, different kinds of adult education colleges (Kollegs, Abendgymnasium), and vocational schools (Autorengruppe Bildungsberichterstattung (2012)). Lastly, I only consider complete cases concerning outcomes as well as control variables.

As outcome variables, I look at political interest, which is available for all respondents older than 13 years. AID:A operationalizes it by asking how much the respondent is interested in politics on a 5-point Likert Scale from (1) “very interested” to (5) “not interested at all”. This is a very common question that is implemented in almost all large-scale surveys containing politics. I inverted the scale to gain a more intuitive interpretability of higher values standing for a higher level of political interest. Aside from political interest, I investigate the participation in certain political activities in the last 12 months[7] as dummy variables. Here, I consider participating in public discussions, citizen initiatives, political parties, demonstrations, and signing petitions,

[6] Meanwhile, it is evident that the implementation of G8 is not persisting, as almost all federal states are switching back to G9 or opening up possibilities to run G8 and G9 in parallel. As already pointed out, in this study this is only relevant for Schleswig-Holstein, because in the other federal states the first graduates from the re-reformed systems will only appear after my obervation period.

[7] For the exact wording of both see Table A5.1 in the Appendix.

using data available for respondents older than 16. Because of data availability, I look at two different samples, namely from age 13 to 20 for political interest and from age 16 to 20 for participation in political activities. The upper bound of 20 years is oriented on existing literature in the field (Krekel (2017)) to avoid any confounding associated with higher tertiary education or labor market entrance.

The variable of main interest is the reform indicator, which takes the value one if an individual was affected by the G8 reform and zero otherwise. Whether someone is affected is assigned to every individual according to the federal state of living and year and month of birth[8] (if still in school) or year of graduation. In the case of Rhineland-Palatinate, the G8 reform was never fully implemented as only 19 out of 148 high-track schools had G8. As it is not possible to identify students from these particular schools, I adopt the approach proposed by Marcus et al. (2020) and treat all students from Rhineland-Palatinate as G9 students. The federal state of Hesse implemented G8 in a 3-year timespan. In the empirical part, I treat all graduates of 2013 or later as being affected by G8 as 70% of schools in Hesse implemented the reform by then.[9] Thuringia and Saxony always had an 8-year higher secondary track for historical reasons. Here, I adopt the strategy proposed by Dahmann (2017) and treat students from Thuringia and Saxony as being treated. In Schleswig-Holstein graduates from G8 and G9 may be apparent in the graduation year of 2019 (N=8 in my sample). As only 11 schools out of 99 higher secondary schools in total in Schleswig-Holstein switched back to G9 and additional 4 schools run both systems in parallel, I treat all 2019 graduates from Schleswig-Holstein as having attended G8.[10]

For estimating the causal effect of a higher schooling intensity trough the G8 reform as a quasi-experiment, I apply a difference-in-differences estimator of the following form:

$$Y_i = \alpha + \beta_1 G8_i + \beta_2 state_i + \beta_3 year\,of\,birth_i + \beta_4 X_i + \epsilon_i \tag{5.1}$$

Employing OLS regressions, I estimate the average treatment effect on the treated for outcome Y_i, based on the reform indicator $G8_i$ and control for a full set of birth year dummies, $year\ of\ birth_i$, as well as a set of individual, parental and family controls X_i (gender, age, migration background, has graduated, place of residence in east Germany, place of residence in a small town, parental education, parental employment status, single parent, only child). As there may be some state

[8] In all federal states the same birth cohorts between 1988 and 2006 (2003) are covered.

[9] According to the Standing Conference of the Ministers of Education and Cultural Affairs (KMK (2022)) 10% of all schools in Hesse implemented G8 in the first year of the implementation phase, an additional 60% in the second year, and the remaining 30% in the third year.

[10] In the robustness Section, I check the implications of these decisions for my results.

specifics related to the timing of the implementation of the reform, I further include a full set of federal state dummies, $state_i$. The selection of control variables broadly follows former empirical studies by Dahmann (2017), Dahmann and Anger (2014) and Krekel (2017). Standard errors are clustered on the federal state level. According to Breen et al. (2018) using linear models is also appropriate for non-metric outcomes and offers the additional advantage of an easy and straightforward interpretation of the results compared to logistic or ordered models.[11]

Interpreting the G8 reform as a quasi-experiment, as I do here, depends on three critical assumptions: (1) common trends assumption, (2) ignorability assumption, and (3) stable unit treatment value assumption. The common trend assumption is not directly testable and implies that the outcome measures would have evolved in the same way in treatment and control group in the absence of the G8 reform. Therefore, I check if my sample is at least balanced on observables by descriptively comparing treatment and control group, as covariate imbalances could be an indication for a violation of the common trends assumption (Krekel (2017)).

Table A5.3 in the Appendix shows the means of all controls, overall and separated for treatment and control group as well as the scale-free normalized difference. According to Imbens and Wooldridge (2009) a deviation of more or than |0.25| can be interpreted as covariate imbalance. The differences between the means of treatment and control group are below this critical threshold for most of my control variables in both samples. For age and share of graduates, this is not the case. Both are lower in the treatment group, which is indeed no surprise as one of the explicit goals of the reform was reducing the number of school years and therewith the graduation age. Here, Huebener and Marcus (2017) show empirically that G8 indeed leads to a reduction of the average graduation age by 10 months. Another imbalance is visible in the share of respondents living in east Germany. Here, the share is higher in the treatment group. This appears to be plausible and is due to the reform implementation. With Thuringia and Saxony, two east German states are completely dedicated to the treatment group, leading to a higher share of east Germans in the treatment group. The shares of respondents living with a single parent and as an only child in the household are also slightly above the threshold, which might hint at changes in family formation in the younger sample of 13- to 20-year-olds. All in all, covariate imbalance seems to be somewhat critical here. Even facing the imbalances described above, the identification strategy used is not necessarily violated as a rich set of observables is controlled for, which nets out potential systematic differences for example between federal states (Krekel

[11] In Table A5.5 in the Appendix, a replication of the baseline results using logit models is shown. Overall, the results do not differ.

(2017)). To further investigate the common trend assumption, I run a placebo test in which I analyze the G8 reform effect in all other school tracks (except for the higher secondary track) that are not targeted by the reform. With this, I rule out that other factors that might have changed simultaneously with the implementation of the G8 reform drive my results. Table A5.4 in the Appendix shows the expected picture of no significant reform effects for respondents attending other school types. This result is adding plausibility to the common trends assumption to hold. As another assumption, ignorability implies that treatment assignment is not dependent on the outcome. The stable unit treatment assumption requires that the treatment status of a student is independent of the outcome of another student. Both are likely to hold, as the exposure to a higher schooling intensity because of the G8 reform is independent of the political interest and participatory behavior of a certain student and it does also not depend on the political outcomes of another student.

Additionally, I show in Table A5.2 in the Appendix that the timing of implementation across federal states is unsystematic concerning the state government at that time, the voter turnout in the last federal election before G8 was implemented and the economic situation, approximated by GDP per capita. For example, the early reforming states (implementation before 2004) represent one West German state (Saarland), one East German state (Saxony-Anhalt), and one city-state (Hamburg), have the highest (Saarland) as well as the lowest (Saxony-Anhalt) voter turnout and the highest (Hamburg) as well as the lowest (Saxony-Anhalt) GDP per capita in the implementation year. At first glance, it seems like all of those states have been led by CDU (the main conservative party in Germany) at that time, which is true, but taking a closer look at the timeline of deciding on the reform and actually implementing it, reveals that at least in Saxony-Anhalt CDU was not the driving force behind G8. Taken together I argue that ignorability is sufficiently given.

A more critical point is the additional requirement of no treatment variation. Especially in the federal states of Saxony-Anhalt and Mecklenburg-West Pomerania affected students of the first G8 cohort possibly faced a higher treatment intensity, as the reform was first implemented for students in grade 9. Those students had to catch up with a disproportionally higher share of the additional instruction time. In the other federal states, G8 was implemented with the transition of the first affected cohort into secondary schooling. For them, treatment intensity can be assumed to be equal, as the reform explicitly aimed at a higher schooling intensity without implementing further changes e.g. in the curriculum simultaneously. The case of Saxony-Anhalt and Mecklenburg-West Pomerania as well as other issues related to differences in the

implementation of the reform are further addressed in the robustness checks.

5.5 Results

For a first glance at the outcome measures, some descriptive statistics are provided in Table 5.1. It shows the overall mean, the means separated for treatment and control group as well as raw and normalized differences.

Table 5.1: Descriptive statistics

	(1) overall mean (Std. Dev.)	(2) mean treatment group (Std. Dev.)	(3) mean control group (Std. Dev.)	(4) Δ (treatment-control)	(5) normalized difference
Panel A. Sample 13-20 years					
political interest	3.18	3.14	3.37	-0.23***	-0.17
(scale 1-5)	(0.96)	(0.95)	(0.97)		
N	3959	3258	701		
Panel B. Sample 16-20 years					
discussion	0.18	0.17	0.25	-0.08**	-0.15
	(0.38)	(0.37)	(0.44)		
citizen initiative	0.03	0.03	0.04	-0.01	-0.07
	(0.16)	(0.16)	(0.20)		
party	0.03	0.02	0.07	-0.05***	-0.16
	(0.16)	(0.15)	(0.26)		
demonstration	0.21	0.21	0.24	-0.03	-0.06
	(0.41)	(0.41)	(0.43)		
petition	0.36	0.35	0.43	-0.08**	-0.11
	(0.48)	(0.48)	(0.50)		
N	2072	1843	229		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, normalized differences, as suggested by Imbens and Wooldridge (2009), with the critical threshold at |0.25|.
Source: AID:A 2009, 2014, 2019, own calculations.

Panel A. depicts the sample aged 13 to 20 with the respective outcome self-rated political interest. The overall mean is 3.18 on the scale ranging from 1 to 5, the mean for the treatment group including individuals affected by the increase in schooling intensity is 3.14 and for those not affected, the control group, the mean is 3.37. As expected, those affected by G8 show lower levels of political interest on average. Although the raw difference between both groups is statistically significant at the 1% level, the normalized difference does not exceed the critical threshold of |0.25| as suggested by Imbens and Wooldridge (2009). In this sample, 701 (18%) respondents are not exposed to higher secondary schooling under G8. In Panel B., which is providing the descriptive statistics for the sample aged 16 to 20 this share is only 11% (229 individuals). Also here, the overall picture shows that a higher share of non-affected respondents (control

group) reports having participated in politically motivated activities in the past 12 months. The most common one is signing petitions, as 35% in the treatment group and 43% in the control group have done this. Joining citizen initiatives and political parties is rarely reported, as only 2% (3%) of the treated and 7% (4%) of the non-treated individuals worked in a party (citizen initiative). Demonstrating and discussing political issues is ranging in between with a share of 21% and 17% in the treatment group and 24% and 25% in the control group. Overall, the raw differences are small and only statistically significant for joining a political party, demonstrating, and participating in political discussions. Also, the normalized differences are highest for the activities mentioned before, but do not exceed the critical value of |0.25|.

Turning to the multivariate analyses, the baseline results emerging from OLS regressions[12] are depicted in Table 5.2. They show that the increase in schooling intensity through the G8 reform negatively affected political interest and participation of youth and young adults. Also, the probability of joining a political party was significantly reduced in the course of G8 in the sample of 16- to 20-year-old respondents. All other indicators measuring participation in political actions also show negative signs, but do not reach conventional levels of statistical significance. Although the direction of the hypothesized effect was not crystal clear from theoretical considerations, finding overall negative effects is highly plausible. Not seeing positive effects may be due to the fact that these hypothesized positive effects on both political participation and interest may not necessarily translate into a change in actual interest or behavior if the time for pursuing increased willingness to participate and get involved is lacking. Also, the tendency to support the opponents of G8 actively may be stronger for parents than for students, as, especially in the implementation phase, parental concerns and fears were severe.[13]

To further test if the effects of higher schooling intensity on political participation and interest are driven or mitigated by subgroups, heterogeneity analyses were conducted. Methodologically, these analyses are generated by adding interaction terms to the baseline specification. The results for heterogeneous groups with respect to gender, migration background, parental education, non-intact family, and living in East or West Germany are shown in Table 5.3.

Gender – Potentially, girls and boys react differently to changes in schooling intensity, as has been shown in many related studies (e.g. Dahmann (2017); Meyer et al. (2019); Meyer and Thomsen (2016); Quis (2018)). A possible explanation for this is that female and male students prefer divergent strategies when it comes to learning (Green and Oxford (1995)) and also differences in

[12] As political participation indicators are binary, results of additional logit regressions can be found in Table A5.5 in the Appendix. As they do not differ substantively, running OLS regressions seems to be unproblematic.

[13] Spillover effects on parents will be examined in Section 5.8.

Table 5.2: Baseline Results OLS - Political interest and participation

	political interest	political participation				
	(1)	(2) discussion	(3) citizen initiative	(4) party	(5) demonstration	(6) petition
G8 reform	-0.115**	-0.039	-0.004	-0.032**	-0.007	-0.084
	(0.0526)	(0.0470)	(0.0124)	(0.0140)	(0.0274)	(0.0598)
N	3959	2072	2072	2072	2072	2072

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01
Controls: age, has graduated, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. Estimation on political interest (participation) contain individuals aged 13 to 20 (16 to 20).
Source: AID:A 2009, 2014, 2019, own calculations.

biological and mental development may lead to gender-specific effects of altering instructional time (de Bellis et al. (2001)). My results, displayed in Panel A. of Table 5.3 reveal that there are several significant gender differences in the reform effect on political participation and interest. Males are found to drive the effects elaborated on in the main results.

Migration background – Another common distinction is looking at the influence of the migration background of respondents. It can be assumed, that children with a migration background may be less able to cope with the increased challenges the reformed system is posing. They probably also get less support from their families and are at risk of having to reduce extracurricular activities, like joining political actions. On the other hand, the increase in time at school may have positive effects, as students with a migration background are better integrated into their peer group at school and improve their language skills. Both could have positive effects on political participation and interest, as peers are important socializing agents in the political field (Dostie-Goulet (2009); Koskimaa and Rapeli (2015)) and language competencies have been argued to be influential for political participation (Hillygus (2005); Müssig (2021)). My results (Panel B. in Table 5.3) indeed show that there are no significant differences in the reform effects for respondents with and without migration background. A possible explanation may be that students attending higher secondary school are a positively selected group and therefore migration background does not play a role in this particular sample.

Parental education – Heterogeneous reform effects may also occur with respect to different family backgrounds and the corresponding level of support families can provide to their children. To approximate this issue, I interact the G8 indicator with a dummy showing if at least one parent has a tertiary degree or not. As visible in Panel C. in Table 5.3, high parental education does not seem to be of importance overall. Only the G8 effect on the likelihood of joining a political

Table 5.3: Effect heterogeneity

	political interest	political participation				
	(1)	(2) discussion	(3) citizen initiative	(4) party	(5) demonstration	(6) petition
Panel A. Gender (male)						
G8 reform	-0.019	-0.011	-0.003	-0.004	-0.007	-0.078
	(0.0581)	(0.0547)	(0.0183)	(0.0144)	(0.0289)	(0.0668)
Interaction	-0.220**	-0.072**	-0.002	-0.073***	0.001	-0.015
	(0.0784)	(0.0337)	(0.0273)	(0.0141)	(0.0444)	(0.0837)
Wald test						
p-value^{+}	0.007***	0.035**	0.788	0.001***	0.889	0.269
N	3959	2072	2072	2072	2072	2072
Panel B. Migration Background						
G8 reform	-0.110**	-0.040	0.001	-0.035**	-0.013	-0.076
	(0.0474)	(0.0516)	(0.0134)	(0.0151)	(0.0217)	(0.0489)
Interaction	-0.030	0.009	-0.028	0.019	0.042	-0.049
	(0.0937)	(0.0728)	(0.0226)	(0.0396)	(0.0538)	(0.0906)
Wald test						
p-value^{+}	0.238	0.630	0.233	0.667	0.684	0.346
N	3959	2072	2072	2072	2072	2072
Panel C. Highly educated parents						
G8 reform	-0.139**	-0.042	-0.007	-0.014	-0.002	-0.113**
	(0.0631)	(0.0420)	(0.0223)	(0.0150)	(0.0291)	(0.0470)
Interaction	0.063	0.009	0.011	-0.050***	-0.013	0.084
	(0.0675)	(0.0564)	(0.0356)	(0.0101)	(0.0311)	(0.0583)
Wald test						
p-value^{+}	0.228	0.648	0.868	0.001***	0.671	0.751
N	3959	2072	2072	2072	2072	2072
Panel D. non-intact family						
G8 reform	-0.140**	-0.059	0.007	-0.037***	-0.023	-0.104
	(0.0517)	(0.0506)	(0.0089)	(0.0126)	(0.0345)	(0.0789)
Interaction	0.288*	0.155***	-0.087	0.041	0.128	0.159
	(0.1510)	(0.0502)	(0.0585)	(0.0294)	(0.0873)	(0.1506)
Wald test						
p-value^{+}	0.345	0.007***	0.211	0.914	0.146	0.502
N	3959	2072	2072	2072	2072	2072
Panel E. East-West difference						
G8 reform	-0.119**	-0.039	-0.004	-0.032**	-0.006	-0.084
	(0.0525)	(0.0468)	(0.0124)	(0.0141)	(0.0275)	(0.0597)
Interaction	0.162***	0.608***	0.200*	0.156	-0.651***	-0.092
	(0.0543)	(0.1308)	(0.1043)	(0.1180)	(0.0980)	(0.1485)
Wald test						
p-value^{+}	0.541	0.003***	0.100	0.295	0.000***	0.386
N	3959	2072	2072	2072	2072	2072

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Controls: age, has graduated, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. OLS estimations on political interest (participation) contain individuals aged 13 to 20 (16 to 20).

$^{+}$Test of the hypothesis H0: Reform (G8) + Interaction = 0.

Source: AID:A 2009, 2014, 2019, own calculations.

party is found to be driven by students with highly educated parents.

Non-intact family – To further address differences in the family environment, I additionally check the importance of non-intact families, operationalized as households with a single parent. Given the multiple demands in the private and work sphere, single parents may be less able to additionally support their children. On the other hand, the higher amount of time spent at school may provide a more stable environment and strengthen relationships with other students and also teachers. Panel D. in Table 5.3 illustrates that students with single parents benefit from the changes in schooling induced by the G8 reform only in terms of a higher likelihood of discussing political issues. These effects may come from the prolonged school days, more interaction with peers, and more occasions for exchanging thoughts on not school related issues, like when having lunch together. The importance of peers for the development of political interest and the recruitment into political participation is an established field of research, especially in the political sciences. The findings of Dahmann and Anger (2014) are adding further plausibility to this explanation, as they show that students from non-intact families got more open and more extroverted in the course of the implementation of G8.

East-West difference – Lastly, in Panel E., I account for divergences between the eastern and the western part of Germany, as eastern federal states are more experienced with shorter higher secondary school tracks because they were a feature of their system before they adapted to the western standards of having 9 years of higher secondary school after reunification. The results show, that G8 effects in east Germany in general appear to be stronger than in the west and also more volatile in sign. The interaction term shows, that G8 had a significant and positive impact on the probability of participating in political discussions and a negative one on the likelihood of joining demonstrations for east Germans.

This exercise overall shows that there are only some hints of marginal -if any- effect heterogeneities. Within the differences considered, gender seems to be most important, but still, the interaction term is only significant for half of the outcomes.

5.6 Robustness Checks

To make sure that the results reported above are not sensitive to any potentially distracting issues in terms of the implementation schemes in different federal states, the composition of my analyses samples, and selection out of treatment, the following provides an overview of the robustness checks that were carried out.

Different implementation schemes in federal states - First, I consider different uncertainties and federal state-differences in the implementation of the G8 reform, as shown in Table 5.4. To ease comparison, Panel A. again shows the baseline results. I rerun my estimations excluding all federal states without an actual change in the duration of their higher secondary school track. In Saxony and Thuringia, this track always lasted for 8 years, as these states refused to adjust their system to west German standards in the course of reunification. Contrary to that, Rhineland-Palatinate withholds from implementing G8, except for very few schools. So, the huge majority here has always been exposed to the old system with 9 years of higher secondary schooling. Estimations without these federal states show very similar patterns compared to the main results but the coefficients are slightly larger in size, which leads to the conclusion that the inclusion of these in the main specification may lead to a downward bias in my results (Panel B. in Table 5.4).

Next, I exclude the states of Mecklenburg-West Pomerania and Saxony-Anhalt, as students were educated in higher secondary schools for several years when the reform was implemented. Here, the first affected student cohort already attended grade 9 and therefore might face a higher treatment intensity. As also the following cohorts already started higher secondary schooling when G8 kicked in, I omit students from these federal states altogether in Panel C.. Even though my sample contains only few respondents from these states, the tendency that the G8 effects are less pronounced when excluding them is visible, as my effects very slightly decrease in size and are only significant on lower levels than in the baseline specification.

The next federal state worth considering in terms of a special implementation scheme is Hesse. As already reported in Section 5.3, Hesse implemented the reform within a time span of 3 years, making it virtually impossible to accurately assign the correct treatment status on the individual level. In the main analyses, I considered all students from Hesse as being treated when the majority of schools had implemented G8. Still, there is a substantial risk of misassignment of treatment status, so I exclude all respondents from Hesse[14] in Panel D. of Table 5.4. The results show that the overall pattern of negative reform effects for both political interest and certain forms of political participation remains stable. Opposed to the baseline results, the negative effect on signing petitions reaches conventional levels of significance in this specification dropping all observations of people living in Hesse.

Then I exclude all 2019 graduates from Schleswig-Holstein, to check if the potential misassignment of newly possible G9 graduates causes bias. According to Panel E. in Table 5.4, my effects only

[14] Only excluding the implementation cohorts reveals similar results.

Table 5.4: Robustness Checks - Implementation

	political interest	political participation				
	(1)	(2) discussion	(3) citizen initiative	(4) party	(5) demonstration	(6) petition
Panel A. Baseline						
G8 reform	-0.115**	-0.039	-0.004	-0.032**	-0.007	-0.084
	(0.0526)	(0.0470)	(0.0124)	(0.0140)	(0.0274)	(0.0598)
N	3959	2072	2072	2072	2072	2072
Panel B. Without Saxony, Thuringia and Rhineland-Palatinate						
G8 reform	-0.167**	-0.047	0.003	-0.046***	-0.003	-0.093
	(0.0642)	(0.0523)	(0.0120)	(0.0075)	(0.0287)	(0.0602)
N	3574	1883	1883	1883	1883	1883
Panel C. Without Mecklenburg-West Pomerania and Saxony-Anhalt						
G8 reform	-0.108*	-0.051	-0.001	-0.028*	-0.016	-0.096
	(0.0570)	(0.0497)	(0.0116)	(0.0155)	(0.0279)	(0.0579)
N	3837	2018	2018	2018	2018	2018
Panel D. Without Hesse						
G8 reform	-0.121*	-0.036	-0.006	-0.031*	-0.031	-0.110**
	(0.0566)	(0.0562)	(0.0129)	(0.0154)	(0.0233)	(0.0431)
N	3616	1903	1903	1903	1903	1903
Panel E. Without 2019 graduates from Schleswig-Holstein						
G8 reform	-0.109**	-0.027	-0.002	-0.030*	-0.004	-0.076
	(0.0501)	(0.0364)	(0.0119)	(0.0149)	(0.0287)	(0.0648)
N	3951	2064	2064	2064	2064	2064
Panel F. Without all the aforementioned						
G8 reform	-0.235***	-0.040	0.003	-0.040***	-0.036	-0.124**
	(0.0558)	(0.0507)	(0.0114)	(0.0074)	(0.0321)	(0.0495)
N	1652	1652	1652	1652	1652	1652

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Controls: age, has graduated, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. OLS estimations on political interest (participation) contain individuals aged 13 to 20 (16 to 20).
Source: AID:A 2009, 2014, 2019, own calculations.

very slightly decrease in size and level of significance.

Lastly, in Panel F., I exclude all observations, that were just excluded separately, together, to get rid of all these state-specific issues coming along with different implementation approaches. Although this restriction leads to a huge loss of observations, the coefficients are larger in size and partly reach higher levels of significance. Most noticeably, the reform effect on political interest more than doubles compared to the baseline specification. Like in the model omitting observations from Hesse, signing petitions is also significant here. Overall, how the reform is implemented in different states and the adequate capturing of it in the data appears to be important for the effects on signing petitions, but the pattern of the main effects seem quite robust.

Differences in Sample composition and selection out of treatment - A second set of robustness checks shown in Table 5.5 focuses on the sample composition, taking into account that reform effects may differ for students and graduates as well as the double cohort and subsequent cohorts. Additionally, out-of-treatment selection will be addressed.

Restricting the analyses to the student sample only leads to a substantial reduction in sample size, especially for my political participation outcomes. But still, the result depicted in Panel B. of Table 5.5 hold some interesting insights, as they show that the effect on political interest gets very small and insignificant and therefore seems to be driven by graduates. Opposingly, the effects on political participation are more pronounced in the student sample, as all coefficients rise in magnitude and also gain in statistical significance. Students affected by a higher schooling intensity are significantly less likely to join political discussions, citizen initiatives, and parties and have a significantly lower probability of signing petitions. These stronger effects for students are plausible, as this group is affected by the reform most immediately.

The double cohort, which is the last cohort of G9 and the first of the G8 system, is a very special one and therefore deserves special treatment. It can be suspected, that this cohort may be especially affected by the reform. As both will graduate at the same time the number of graduates in a given federal state will be much higher than in regular cohorts, which is leading to an increase in competition e.g. for positions in higher education or apprenticeships. Also, the lack of experience in making a good transition from the 9-year to the 8-year system is most important for this special cohort. Linking this discussion to the issue of selection, which will be addressed below, for the double cohort also skipping (or repeating) a grade is an additional matter to select out of (or into) treatment. To have the possibility to skip a grade is very unlikely, as it requires a very good academic performance and therefore is a quite rare event. Grade repetition instead

is more common. The share of students repeating a grade was between 2.2% and 2.9% in the schoolyears 1993/94 to 2018/19 (Autorengruppe Bildungsberichterstattung (2020)). To get rid of these issues I exclude the double cohort in Panel C. of Table 5.5. Compared to the baseline estimates, the results for the sample without the double cohort[15] predominately show negative effects, but none is reaching a conventional level of significance. This can be interpreted as an indication of the reform effects being strongly driven by students from the double cohort. In other words, what I find above seems to be a transition effect that is only significantly impacting student cohorts right on the edge between the 9-year and the 8-year system. A further check on the issue of timing and persistence is provided in Section 5.8.

Table 5.5: Robustness Checks - other

	political interest	political participation				
	(1)	(2) discussion	(3) citizen initiative	(4) party	(5) demonstration	(6) petition
Panel A. Baseline						
G8 reform	-0.115**	-0.039	-0.004	-0.032**	-0.007	-0.084
	(0.0526)	(0.0470)	(0.0124)	(0.0140)	(0.0274)	(0.0598)
N	3959	2072	2072	2072	2072	2072
Panel B. Students only						
G8 reform	0.004	-0.118***	-0.133***	-0.104***	0.030	-0.146***
	(0.0836)	(0.0368)	(0.0129)	(0.0076)	(0.0380)	(0.0346)
N	2460	878	878	878	878	878
Panel C. without Double Cohort						
G8 reform	-0.105	-0.125	-0.005	-0.007	-0.080	0.078
	(0.0869)	(0.1176)	(0.0242)	(0.0570)	(0.0748)	(0.0915)
N	3778	1898	1898	1898	1898	1898
Panel D. states without comprehensive school						
G8 reform	-0.150*	0.366**	-0.073	-0.191**	0.512***	0.049
	(0.0649)	(0.1239)	(0.0878)	(0.0706)	(0.1160)	(0.0607)
N	1871	964	964	964	964	964
Panel E. late adopter states						
G8 reform	-0.235**	-0.063	-0.002	-0.037***	0.021	-0.157**
	(0.0800)	(0.0715)	(0.0164)	(0.0085)	(0.0217)	(0.0539)
N	2391	1263	1263	1263	1263	1263

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Controls: age, has graduated, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. OLS estimations on political interest (participation) contain individuals aged 13 to 20 (16 to 20).
Source: AID:A 2009, 2014, 2019, own calculations.

[15] Being part of the double cohort is defined as being born in the year for which both affected and unaffected students exist in a federal state.

Out-of-treatment selection is an important issue to consider but avoiding the exposure to G8 is not easy, as all schools within a federal state are forced by law to implement it at the same time in all higher secondary schools.[16] However, as G8 was only implemented in higher secondary schools, one possibility to avoid the higher schooling intensity is to choose another secondary school track. However, this seems to be unlikely as this is the only way to directly earn a university entrance qualification. Also, some states have comprehensive schools offering to graduate with Abitur. But still, self-selection into other tracks does not seem to be a major concern, as Huebener and Marcus (2017) do not find a reform-induced difference in enrollment rates to higher secondary schools using administrative data. In line with these findings, also Huebener et al. (2017) show, using PISA data, that the probability of attending the Gymnasium is not affected by the G8 reform. Nevertheless, in Panel D. I rerun my estimations including federal states in which comprehensive schools (schools combining all three tracks) are rare or even nonexistent.[17] The rationale behind this is that students in federal states where the supply of these schools is high are therefore facing an alternative to attending higher secondary school. This possibility is ruled out for students in federal states without comprehensive schools. The results show that coefficients are higher in magnitude and partly show opposing signs and diverging levels of significance compared to the baseline. The statistically significant and negative effects of higher schooling intensity on political interest and the probability of joining a party are also significant and negative here, but larger in size (that means more negative). I argue that this is in line with expectations, as those students who are most likely trying to select out of treatment are the ones expecting to be harmed most. Consequently, negative effects should be higher when selection is not possible. In this constellation, also the likelihood of discussing political issues and joining demonstrations is significantly affected, but in a positive way. This seems plausible, as respondents in this sample do not have an easy way to get around G8 and can therefore be expected to try to influence the political discussion and protest most.

The second possibility for selecting out of treatment is that students may attend school in other federal states where the reform was not yet implemented, which is possible due to the gradual implementation process across federal states. As moving to another federal state with the entire family would cause disproportionately high costs, I argue that this is only realistic for students

[16] An exception is the federal state of Hesse, where the implementation phase contains three consecutive years.

[17] In the definition I apply here, which is borrowed from Dahmann and Anger (2018), federal states are considered to typically have no or only few comprehensive schools if the share of students in these schools is below 10% between 2000 and 2013. These are: Baden-Württemberg, Bavaria, Saxony, Saxony–Anhalt, Thuringia, Lower Saxony, and Mecklenburg-West Pomerania (Autorengruppe Bildungsberichterstattung (2006, 2010, 2012, 2014)). For more information see also Table A5.6 in the Appendix.

in states with late reforming direct neighbor states and the possibility to commute. Taking only late adopter states rules out any self-selection by moving or commuting to another federal state that has not implemented the reform. As late adopter states, I define federal states facing the first graduation cohort affected by the reform in 2012 or later.[18] Here, the pattern of the baseline results is mainly preserved. The effect on political interest is higher in magnitude, which means slightly more negative, and the effect on the likelihood of joining a party appears to be unchanged. Other than in the baseline, the negative reform effect on the probability of signing petitions is significant in late adopter states. At least for petitions related to the implementation of G8, this may be due to the possibility to observe what actually happened in other early reforming states and the experience that its consequences did not turn out as negative as initially feared.

Replication of Krekel (2017) - Lastly, I replicate Krekel's (2017) study as close as possible with the AID:A data on a sample restricted to age 17 to 20 to see how my estimates on political interest harmonize with his. As visible in Table A5.7 in the Appendix, my results are very similar to his. Just like Krekel, I can not find a significant effect of the G8 reform on the political interest scale. Splitting up the scale into dummy variables for the distinct categories shows the same depolarization of political interest that Krekel also describes. Effects are significantly negative on the two most extreme categories on both ends of the scale and positive, though not statistically significant, on the categories in between. Krekel (2017, p. 18) interprets this result as an indication of a crowding out of political interest for those reporting to already be interested and a G8-induced encouragement to get politically involved for those who have not been politically interested before.

5.7 Potential channels

As already elaborated on in Section 5.2 on the potential linkings between the G8 reform and political interest and participation, there are two ways in which the reform is directly reflected: prolonging school days and therefore reducing the amount of leisure time activities in the afternoon as well as going through the (largely unchanged) curriculum in a faster pace. The question is now whether the effects identified before are driven by the decrease in free time (the former) or the increased intensity of instruction (the latter). To investigate these potential channels, I check the effect of the G8 reform on the probability of joining a sports club or musical free time activities, representing the leisure time channel. I also use information on receiving extra

[18] These are: Baden-Württemberg, Berlin, Brandenburg, Bremen, Hesse, North Rhine-Westphalia, and Schleswig-Holstein

tutoring in the afternoons, being related to both channels. For the instruction intensity channel, I use some questions on students' school experiences and performance. These include assessments of having too little time for friends because of the high requirements of school as well as the feeling that school tasks are simple and learning in school is easy. For approximating performance levels, I consider all students holding grade 4 or below[19] in mathematics[20] as low-performing. As this kind of information was only collected for students and the fact that students are arguably affected most immediately, I restrict the investigation of potential channels to students only.

Table 5.6: potential channels

	private lessons	freetime activities		school experiences		low performance
	(1)	(2) sports	(3) music	(4) no time for friends	(5) school tasks easy	(6)
G8 reform	0.110	0.017	0.008	-0.001	-0.082	0.248***
	(0.0734)	(0.0435)	(0.0612)	(0.1070)	(0.0983)	(0.0287)
N	2458	2460	2460	946	946	2220

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01. Controls: age, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. All estimation contain individuals aged 13 to 20 who have not graduated so far.
Source: AID:A 2009, 2014, 2019, own calculations.

The corresponding results are depicted in Table 5.6 and show that the G8 reform does have a statistically significant effect on only one of the channels under investigation. Nevertheless, also the other estimates provide some interesting insights. With the implementation of G8, the probability of getting additional private lessons is rising. This finding is in line with suggestive evidence by Dahmann and Anger (2014) who show in their mechanisms Section that the share of students attending tutoring lessons increases by about six percentage points with the G8 implementation. However, their coefficient also fails to reach conventional levels of statistical significance. For Saxony-Anhalt, Meyer and Thomsen (2015) find that boys are spending more time receiving additional tutoring under G8 compared to G9. Cleuvers (2010) is pointing in a similar direction, as the investigation of a survey among tutoring providers shows a rise in the demand for private lessons that is attributed to the G8 reform.

Also, the free time activities channel cannot be demonstrated to be at work here, as both

[19] The German grading system consists of grades from 1 to 6 with grade 1 being the best and grade 6 being the worst.

[20] AID:A also contains grades in German. According to Malouff and Thorsteinsson (2016) anonymity, as a potential way of reducing bias in grading, is more important when grading is more subjective, as in assessing essays, and less so in objective grading, for example for evaluating responses to multiple-choice or true-false questions. As math may be interpreted as belonging to the more objective subjects, math grades are supposed to be less influenced by the interpretation and evaluation of teachers, I focus on math instead of German. When taking into account grades in German, no significant effect on low performance in German can be found.

participating in sports and music are not significantly influenced by G8 in my sample. This may indeed not come as a huge surprise, as the literature on the effects on leisure time is mixed. For example, Hübner et al. (2017) show that students in the G8 system in Baden-Württemberg spend more time on doing sports and no difference in musical activities, whereas Meyer and Thomsen (2015) find no difference for both between G8 and G9 students in Saxony-Anhalt.

Regarding students' school experiences, the reform did not have any effect on students reporting to have only little time for friends as school demands are high. This appears to be quite plausible, as it can be suspected that most of the friends adolescents have may be their classmates. When more time is spent in school, these friends are also around. Additionally, as many schools also started to provide lunch in the course of the implementation of G8, the lunch break can also be used to spend time with friends, chat, and play. A small negative but not statistically significant effect is visible on experiencing school tasks and learning at school as being easy. Lacking significance may be due to the small sample size and the respective loss of power. Nevertheless, this rather suggestive finding is interesting as it corresponds to the literature on students' perceived stress and the increased demands of schooling. The negative effect here means that tasks at school and learning are experienced to be less easy, or – more strictly speaking – to be harder. This is in line with the theoretical expectation but also with existing empirical evidence (see for example Hübner et al. (2017); Marcus et al. (2020); Quis (2018)) and the increase in attending private lessons that I discussed above.

Considering the performance channel shows that the G8 reform has a significant and positive effect on the probability of being low-performing in math, which is defined as receiving a grade 4 or worse. This finding is in line with Büttner and Thomsen (2015) showing that final grades in math are negatively influenced by the increased schooling intensity, especially for boys, whereas this is not the case for grades in German literature. Looking at final grade point averages (GPAs) and using national administrative data, Huebener et al. (2017) show that the G8 reform had a moderate negative effect on GPAs of about 4,4 to 6,9% of a standard deviation.

5.8 Timing of G8 Effects and potential spillover Effects

As outlined before, one of the advantages of using AID:A data is that a broad range of age groups is available for analysis. In Section 5.6, I showed that my effects vanish as soon as the double cohort is excluded from the analyses and I conclude from that that the effects that I find in the main analyses seem to be transition effects which are only significantly impacting student

cohorts right on the edge between the 9-year and the 8-year system. To further check the validity of this conclusion, I rerun my baseline analyses on individuals from the double cohort only to get an impression of the most immediate effects on a sample of individuals with a maximum of similarities.

In a second step, I take an older sample, only containing people who already left the educational system but who are at the same time young enough to be affected by G8 to cover long-term effects of schooling intensity. The oldest age group containing affected and unaffected individuals in my sample is 30 years old and constitutes the upper bound of the older sample. To make sure that schooling and the following transitions do not impact on my results, I chose age 25 as lower bound.

Table 5.7: Timing of Effects

	political interest	political participation				
	(1)	(2) discussion	(3) citizen initiative	(4) party	(5) demonstration	(6) petition
Panel A. only Double Cohort						
G8 reform	-0.090	0.088*	0.045	-0.055*	0.092*	-0.222***
	(0.0584)	(0.0413)	(0.0291)	(0.0249)	(0.0410)	(0.0361)
N	181	174	174	174	174	174
Panel B. long term effect, sample aged 25 to 30						
G8 reform	0.093	0.016	-0.011	0.003	0.046	-0.079
	(0.1378)	(0.0345)	(0.0133)	(0.0282)	(0.0561)	(0.0560)
N	2492	2492	2492	2492	2492	2492

Notes: Clustered standard errors in parentheses; * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Controls: age, has graduated, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. In Panel A. the reference category contains only individuals in the last G9 classes within the double cohort. Estimations in Panel B. contain individuals aged 25 to 30.
Source: AID:A 2009, 2014, 2019, own calculations.

The results for the double cohort, which are depicted in Panel A. of Table 5.7, show that individuals within the same birth cohort behave significantly different when considering whether or not they have been schooled in a system with higher or lower intensity. Treated individuals show lower levels of political interest, although this difference is not significant in statistical terms. Also, students in the G8 system are more likely to discuss political issues and join demonstrations. At the same time, they show a significantly lower probability to engage in a political party and to sign a petition compared to their G9 peers. Compared to the main results in Table 5.2, restricting the analysis to the double cohort reveals more significant effects that are also bigger in magnitude, even though the sample size gets very small.

Turning to the older sample, depicted in Panel B of Table 5.7, shows that being affected by higher

schooling intensity through G8 does not have an impact on any of my political participation and interest outcomes when measured at age 25 to 30.

The findings from both panels strengthen the point of view that the G8 effects on political outcomes seem to be temporary transition effects that do not persist in the long run.

Another interesting extension is taking an intergenerational perspective. Here, the question is if intensified schooling also has effects on the parents of affected children. Unfortunately, parental information on some of their political outcomes is only available in wave 2014 of AID:A, so these analyses can only be run for a smaller sample (N=1556). Similar to the investigation of children, I take into account political interest (similarly measured) for parents, too. Additionally, AID:A provides a variable covering seeking politically relevant information from parents. More precisely, it is asked how often parents get informed about political issues for example in the newspapers, on TV, or the Internet.[21] Basically, I rerun the same models including parental outcomes and basic demographics of the reporting parent (sex[22] and age). I also keep information on the child's birth year and if it already graduated as well as my family-level controls.

Table 5.8: Spillover effects on parents

	(1) political interest	(2) political information
G8 reform	-0.390***	0.123*
	(0.0459)	(0.0660)
N	1556	1556

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01. Controls: age of reporting parent, child has graduated, sex of reporting parent, migration background, living in east Germany, living in small town, parental education, own employment status, partners employment status, single parent, only child, federal state of living and childs birth year fixed effects. All estimation contain parents of individuals aged 13 to 20.
Source: AID:A 2014, own calculations.

Table 5.8 shows that there are spillover effects on parents of children affected by a higher schooling intensity. Just like their children, parents show significantly lower levels of political interest if their children face intensified schooling. According to Table A5.8 in the Appendix, this effect does not emerge because of a depolarization (as for their children) but shows a very clear pattern of negative effects on the two values of the scale indicating higher interest in politics and positive effects on the three other values indicating a medium or little interest in political matters.

In the case of seeking political information on the media, an overall positive effect is visible. That means that parents of children attending higher secondary school under the G8 system reach out

[21] Possible answers are (1) every day, (2) more than once a week, (3) 1-2 times per week, (4) 1-2 times per month, (5) seldom, or (6) never. I inverted the scale to gain a more intuitive interpretability of higher values standing for a higher frequency of seeking information.

[22] Mind that, as in many other surveys, parental information in AID:A is predominantly provided by mothers.

for political information more often. Having a closer look at the effects on the single categories of the response scale (see Table A5.9 in the Appendix) reveals that the pattern of effects is not as clear as for political interest. Here, significant negative effects on seeking information "1-2 times per month'" and "every day" as well as positive effects on the categories "seldom", "1-2 times per week", and "more than once per week" are apparent.

5.9 Conclusion

In a nutshell, this study indicates that the educational system may indeed play a role in shaping political interest and participation in politically motivated activities of adolescents (and their parents) in the short run. Impacting on these issues can come along with changes in the organization of education as a rather unintended effect. The present study sheds light on the effect of increased schooling intensity implemented through the G8 reform in Germany on the so far largely neglected issues of political interest and active political behavior. I contribute to the related strand of literature, as I expand the age range under investigation as well as the variety of political outcome measures, by including variables covering active participation in politically motivated activities and individuals starting at age 13.

My study provides several important findings. I show that there are some negative effects, but also that most of the outcomes under investigation were not significantly affected. More precisely, the level of self-rated political interest and the likelihood of joining a political party significantly decreased with the introduction of intensified schooling. In terms of political interest, I was able to replicate Krekel's (2017) conclusions with another data set. Additionally, I find that these effects are -at best- marginally heterogeneous according to different dividing lines like gender, parental education, living with a single parent, or living in east Germany. My main findings appear to be robust to varying specifications with respect to federal state specific implementation schemes as well as sample composition and selection issues. Searching for potential mechanisms shows some suggestive evidence, but only student performance levels reach conventional levels of statistical significance. Considering students only shows higher significantly negative effects on political participation. Together with all significances vanishing as soon as the double cohort is excluded as well as more pronounced effects when restricting the analysis to the double cohort, it can be suspected that the effects are limited to students exposed to the transition phase between the old and the new system.

Nevertheless, this is only a first glimpse and more research on the question of whether G8 reform

effects are permanent or temporary is needed. For this, panel data following individuals for a longer time span also after leaving the educational system is needed to see if deficits caused by a higher schooling intensity may be compensated for later. Also, the underlying mechanisms mostly remain unclear and constitute a gap that should be filled in the future. When interpreting political participation as a leisure time activity it would be interesting to find out, why various kinds of these activities are affected quite differently by the implementation of G8. Another possibility for future research would be further enriching the range of political outcome measures within the G8 literature. Especially investigating political knowledge and political efficacy could provide valuable insights, as both are essential for the prevention of political apathy. As some federal states are planning or already implementing another reform that partly reverses the G8 reform, it is necessary that further research is taking up the possibility to collect rich data and investigate the effects this new reform may have on multiple fields of adolescents' lives.

Appendix

Table A5.1: Item wording in AID:A

Political interest	Wie stark interessierst (interessieren) Du (Sie) Dich (sich) für Politik? Sehr stark, stark, mittel, wenig oder überhaupt nicht	How interested are you in politics? very much, much, medium, little, or not at all
Participation in political activities in the past 12 months	Was hast (haben) Du (Sie) in den letzten 12 Monaten gemacht, um in politischer Hinsicht Deinen (Ihren) Standpunkt zur Geltung zu bringen beziehungsweise Einfluss zu nehmen?	What have you done in the past 12 months to show your point of view concerning politics or to influence politics?
	(1) Dich (sich) an Wahlen beteiligt	(1) Voted
	(2) Dich (sich) in Versammlungen an öffentlichen Diskussionen beteiligt	(2) participated in public discussions
	(3) Mitarbeit in einer Bürgerinitiative	(3) worked in citizens' initiative
	(4) in einer Partei aktiv mitgearbeitet	(4) worked in a party
	(5) Teilnahme an einer Demonstration	(5) demonstrated
	(6) Beteiligung an einer Unterschriftensammlung	(6) signed a petition
	(7) Aus politischen, ethiscchen, oder Umweltgründen Waren boykottiert oder gekauft	(7) bought or boycotted products out of political, ethical or invironmental reasons
	(8) Dich (sich) an einer Online- Protestaktion beteiligt	(8) protested online

Notes: Authors translation, as AID:A does not provide translations.
Sources: Deutsches Jugendinstitut (2014, 2019); Infas Institut für angewandte Sozialwissenschaft (2009a,b)

Table A5.2: systematic of G8 implementation across federal states

federal state	year of implementation (double cohort)	Leading party (year of last election)	change after last election	voter turnout federal election	GDP per capita (in year of implementation)
early reforming states					
Saarland	2001 (2009)	CDU (1999)	yes	84,8% (1998)	24.459
Hamburg	2002 (2010)	CDU (2001)	no	79,6% (2002)	49.416
Saxony-Anhalt	2003 (2007)	CDU (2002)	yes	68,8% (2002)	17.818
main implementation year 2004					
Mecklenburg-West Pomerania	2004 (2008)	SPD (2002)	no	70,6% (2002)	17.826
Lower Saxony	2004 (2011)	CDU (2003)	yes	81,0% (2002)	24.351
Bavaria	2004 (2011)	CSU (2003)	no	81,5% (2002)	31.702
Bremen	2004 (2012)	SPD (2003)	no	78,8% (2002)	37.211
Baden-Württemberg	2004 (2012)	CDU (2001)	no	81,1% (2002)	31.705
Hesse	2004 (2012-14)	CDU (2003)	no	80,1% (2002)	34.383
late reforming states					
North Rhine-Westphalia	2005 (2013)	CDU (2005)	yes	78,3% (2005)	28.090
Brandenburg	2006 (2012)	SPD (2004)	no	74,9% (2005)	20.145
Berlin	2006 (2012)	SPD (2006)	no	77,4% (2005)	27.781
Rhinelan-Palatinate	2008 (2016)	SPD (2006)	no	78,7% (2005)	27.172
Schleswig-Holstein	2008 (2016)	CDU (2005)	yes	79,1% (2005)	26.128
non-reforming states					
Thuringia	/	CDU (2003)	no	74,8% (2002)	18.324 (2004)
Saxony	/	CDU (2004)	no	73,7% (2002)	19.901 (2004)

Notes: For the non-reforming states I report the respective information for matters of comparison for the year 2004, because this is the main implementation year among the reforming states.
Source: Bundeswahlleiter (2022), Statistisches Bundesamt (2021), Camarero Garcia (2022) online Appendix, own illustration.

Table A5.3: Descriptive statistics

	Sample 13-20 years					*Sample 16-20 years*				
	(1) overall mean (Std. Dev.)	(2) mean treatment group (Std. Dev.)	(3) mean control group (Std. Dev.)	(4) Δ (treatment-control)	(5) normalized difference	(6) overall mean (Std. Dev.)	(7) mean treatment group (Std.) Dev.)	(8) mean control group (Std. Dev.)	(9) Δ (treatment-control	(10) normalized difference
age	16.73	16.30	18.75	-2.45***	-0.87	18.05	17.92	19.10	-1.18***	-0.60
	(2.40)	(2.33)	(1.56)			(1.48)	(1.45)	(1.30)		
has graduated	0.38	0.33	0.60	-0.27***	-0.39	0.58	0.56	0.74	-0.18***	-0.27
	(0.49)	(0.47)	(0.49)			(0.49)	(0.50)	(0.44)		
male	0.48	0.49	0.44	0.05**	0.06	0.48	0.48	0.44	0.04	0.06
	(0.50)	(0.50)	(0.50)			(0.50)	(0.50)	(0.50)		
migration background	0.17	0.17	0.16	0.01	0.02	0.15	0.15	0.15	0.00	0.004
	(0.37)	(0.37)	(0.37)			(0.36)	(0.36)	(0.36)		
living in east Germany	0.13	0.15	0.03	0.12***	0.31	0.12	0.13	0.004	0.13***	0.37
	(0.33)	(0.35)	(0.16)			(0.32)	(0.34)	(0.07)		
living in small town	0.23	0.21	0.32	-0.11***	-0.19	0.20	0.20	0.21	-0.01	-0.02
	(0.42)	(0.40)	(0.47)			(0.40)	(0.40)	(0.40)		
at least one parent	0.38	0.38	0.39	-0.01	-0.00	0.40	0.40	0.37	0.03	0.05
with tertiary degree	(0.49)	(0.49)	(0.49)			(0.49)	(0.49)	(0.48)		
mother employed	0.84	0.83	0.86	-0.03*	-0.05	0.83	0.83	0.83	0.00	-0.003
	(0.37)	(0.37)	(0.35)			(0.38)	(0.38)	(0.38)		
father employed	0.93	0.93	0.93	0.00	-0.01	0.91	0.91	0.91	0.00	0.001
	(0.26)	(0.26)	(0.26)			(0.28)	(0.28)	(0.28)		
single parent	0.14	0.16	0.04	0.12***	0.27	0.16	0.17	0.12	-0.05*	0.09
	(0.34)	(0.36)	(0.21)			(0.37)	(0.37)	(0.33)		
only child	0.12	0.14	0.04	0.10***	0.26	0.15	0.16	0.08	0.08**	0.16
	(0.33)	(0.35)	(0.19)			(0.35)	(0.36)	(0.28)		
N	3959	3258	701			2072	1843	229		

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$; normalized differences, as suggested by Imbens and Wooldridge (2009), with the critical threshold at $|0.25|$.
Source: AID:A 2009, 2014, 2019, own calculations.

Table A5.4: Placebo other school forms

	political interest	political participation				
	(1)	(2) discussion	(3) citizen initiative	(4) party	(5) demonstration	(6) petition
G8 reform	-0.004	0.190	-0.181	-0.082	-0.156	0.233
	(0.1190)	(0.1351)	(0.1274)	(0.0921)	(0.2145)	(0.1556)
N	1871	671	671	671	671	671

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01. Controls: age, has graduated, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. Estimation on political interest (participation) contain individuals aged 13 to 20 (16 to 20).
Source: AID:A 2009, 2014, 2019, own calculations.

Table A5.5: Baseline Results - Political interest (OLS) and participation (average marginal effects from logit regressions)

	political interest	political participation				
	(1)	(2) discussion	(3) citizen initiative	(4) party	(5) demonstration	(6) petition
G8 reform	-0.115**	-0.042	-0.003	-0.019*	-0.012	-0.086
	(0.0526)	(0.0479)	(0.0156)	(0.0103)	(0.0263)	(0.0570)
N	3959	2072	1839	1752	2072	2055

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01. Controls: age, has graduated, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. Estimations on political interest (participation) contain individuals aged 13 to 20 (16 to 20).
Source: AID:A 2009, 2014, 2019, own calculations.

Table A5.6: Share of students (in %) attending comprehensive schools in different school years and federal states

	school year				
	2000/2001	2004/2005	2008/2009	2010/2011	2012/2013
federal state					
Baden Württemberg	**0.5**	**0.5**	**0.5**	**0.6**	**0.9**
Bavaria	**0.4**	**0.2**	**0.3**	**0.3**	**0.2**
Berlin	29.6	27.7	27.4	35.3	50.6
Brandenburg	50.3	48.3	16.8	15.6	13.8
Bremen	14.0	17.5	26.8	35.2	66.6
Hamburg	26.3	28.0	31.7	37.9	54.0
Hesse	16.6	16.4	17.9	19.0	21.0
Mecklenburg West-Pomerania	**4.4**	**6.2**	**9.2**	**7.9**	**8.1**
Lower Saxony	**3.9**	**4.3**	**5.1**	**6.4**	**9.1**
North Rhine-Westphalia	15.1	15.9	16.9	17.7	19.6
Rhineland-Palatinate	**4.5**	**5.0**	**5.8**	12.2	13.8
Saarland	15.4	16.6	20.8	21.5	60.2
Saxony	**0.0**	**0.0**	**0.0**	**0.0**	**0.0**
Saxony-Anhalt	**0.7**	**1.8**	**2.6**	**2.9**	**3.2**
Schleswig-Holstein	6.4	6.6	11.4	25.5	38.7
Thuringia	**1.1**	**2.0**	**2.9**	**2.6**	**9.1**

Source: Autorengruppe Bildungsberichterstattung (2006, 2010, 2012, 2014), own illustration.

Table A5.7: Robustness Checks - Replication Krekel (2017)

	scale	dummies				
	(1) political interest	(2) very	(3) fairly	(4) medium	(5) little	(6) not at all
Panel A. OLS Models						
G8 reform	-0.110	-0.044**	-0.002	0.014	0.044	-0.011*
	(0.0721)	(0.0198)	(0.0251)	(0.0278)	(0.0315)	(0.0063)
N	2130	2130	2130	2130	2130	2130
Panel B. Probit models for dummies						
G8 reform	-0.110	-0.043**	0.003	0.014	0.050	-0.013**
	(0.0721)	(0.0181)	(0.0243)	(0.0274)	(0.0322)	(0.0063)
N	2130	2119	2130	2130	2130	1907

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01. Controls: age, has graduated, sex, migration background, living in east Germany, living in small town, parental education, mother employed, father employed, single parent, only child, federal state of living and birth year fixed effects. OLS estimations on political interest and participation contain individuals aged 17 to 20.
Source: AID:A 2009, 2014, 2019, own calculations.

Table A5.8: Spillover effects on parents - political interest

	scale	dummies				
	(1) political interest	(2) very	(3) fairly	(4) medium	(5) little	(6) not at all
G8 reform	-0.390***	-0.048***	-0.188***	0.156***	0.058*	0.023***
	(0.0459)	(0.0138)	(0.0200)	(0.0281)	(0.0300)	(0.0078)
N	1556	1556	1556	1556	1556	1556

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01. Controls: age of reporting parent, child has graduated, sex of reporting parent, migration background, living in east Germany, living in small town, parental education, own employment status, partners employment status, single parent, only child, federal state of living and childs birth year fixed effects. All estimation contain parents of individuals aged 13 to 20.
Source: AID:A 2014, own calculations.

Table A5.9: Spillover effects on parents - Frequency informing

	scale	dummies					
	(1) political information	(2) never	(3) seldom	(4) 1-2 / month	(5) 1-2 / week	(6) < once / week	(7) every day
G8 reform	0.123*	0.001	0.022***	-0.102***	0.040***	0.068***	-0.028***
	(0.0660)	(0.0019)	(0.0018)	(0.0013)	(0.0043)	(0.0078)	(0.0093)
N	1556	1556	1556	1556	1556	1556	1556

Notes: Clustered standard errors in parentheses; * p < 0.10, ** p < 0.05, *** p < 0.01. Controls: age of reporting parent, child has graduated, sex of reporting parent, migration background, living in east Germany, living in small town, parental education, own employment status, partners employment status, single parent, only child, federal state of living and childs birth year fixed effects. All estimation contain parents of individuals aged 13 to 20.
Source: AID:A 2014, own calculations.

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