On the Non-Pecuniary Returns to Education: A More Trusting Society, but not Necessarily a More Civic One

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Master's Thesis in International Economics

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ABSTRACT

In this paper, the potential causal effect of education on institutional trust and civic engagement is studied. Leveraging 4 European schooling reforms in regression discontinuity designs, robust evidence is found that educational attainment positively impacts trust in national institutions – both of political nature, and not. Additional schooling seems to prevent people from becoming very distrusting of national institutions, and also shift them to the relatively trusting segment in their country. Further investigation of the causal channels that underly education's impact on political trust indicates that this effect operates through schooling sorting people into more privileged societal positions, making them more politically sophisticated, and causing them to be less receptive to anti-establishment messaging of populist political parties. With regards to education's potential impact on civic engagement, no evidence is found of a general causal link. Overall, the empirical findings suggest the existence of an important non-pecuniary externality of education: a more stable society. This highlights the potential of raising educational attainment as an effective policy lever to combat harmful polarization, an issue which is especially relevant in today's world.

Keywords: Institutional trust; Civic engagement; Education; Schooling reforms; Europe

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Date final version: 5th of October, 2023 | Disclaimer: the views stated in this thesis are those of the author and not necessarily those of the supervisor, second assessor, Erasmus School of Economics or Erasmus University Rotterdam.

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1. Introduction

Perhaps one of the longest-standing and most convincing arguments for the government's extensive involvement in education is that this domain is characterized by the existence of substantial positive externalities; a medical education does not only improve the life of the nurse or doctor, it also improves the lives of the people in their community; an engineering education does not only improve the life of those that are mathematically gifted, their inventions have the potential to improve the lives of millions.

Another externality thought to exist is that education makes people better citizens, and this results in a more well-functioning society. The importance of this belief is highlighted by school systems explicitly stating that a goal of their educational policy is to create an informed, civic-minded, and democratically-oriented citizenry (Glaeser et al., 2007).

This hypothesized effect of education shaping people's attitudes and behavior to be more societally beneficial, however, has received considerably less academic attention than for example investigational efforts of education's effect on pecuniary and labor market outcomes have. Moreover, the majority of the work that has been done on this topic are analyses that do not adequately deal with the endogeneity concerns related to studying the impact of education; there are indeed many socio-economic factors that plausibly correlate with both educational attainment and a person's views and behavior.

The aim of this thesis is to contribute to this field of research, by analyzing the impact of education on trust in national institutions and civic engagement in (and across) Denmark, France, Great Britain, and The Netherlands.

In doing this, this thesis adds value to the existing literature in the following ways. Firstly, this paper studies a pooled sample of a limited number of countries and, furthermore, causally investigates each individual country. Most of the these (Denmark, France, and The Netherlands) have only been studied in large pooled samples of 10 countries or more; thereby, this paper deals with the concern that the aggregate effects of these previous studies mask considerable heterogeneities across countries. Secondly, this thesis examines a considerably more extensive set of ways of civic engagement than previous work, where, furthermore, multiple of these have not been causally studied in the context of the countries of interest in this thesis. Thirdly, this thesis, to the knowledge of the author, is only the second one that comprehensively studies education's impact on national institutions.[1] Moreover, this paper studies education's impact on trust in national institutions for a time period of substantially more polarization than previous work, with the data spanning the time period of the Great Recession, the Eurozone Crisis, and the rise of populism. As will be argued later, these phenomena have possibly created a setting where the causal link between education and institutional trust is especially strong.

[1] The first paper being the one by Yang (2019), who, among other things, studies the impact of education on institutional trust in Great Britain. Additionally, there are two other papers (Milligan et al., 2004) (Österman, 2021) that very briefly investigate education's impact on institutional trust. All papers are discussed in the literature review section.

Thus, by studying this topic for a time period where European society was more under pressure, this thesis also adds value in this manner.

The last, and perhaps most important, way in which this thesis adds to the existing literature, is that it is the first to provide robust evidence that education has a positive impact on trust in a variety of national institutions – both of political nature, and not; it also puts forth valuable insights as to what mechanisms are driving the causal effect on trust in political institutions.

The rest of this paper continues in the following sequential manner. In Section 2, the existing literature on the effect of education on both institutional trust and civic engagement is reviewed, where the focus is primarily on previous work that tries to estimate causal effects. Section 3 elaborates on the potential channels through which education could impact trust in national institutions and civic engagement. Section 4 describes the research design. Section 5 presents the main results. In Section 6, the findings are placed under further scrutiny by robustness testing. Section 7 investigates which causal channels are driving education's impact on political trust. Section 8 discusses the main findings. Section 9 concludes.

2. Literature review

Academic research on the relationship between education and institutional trust consistently has found a positive correlation between the two (Hooghe et al., 2012) (Schoon et al., 2010) (Schoon & Cheng, 2011) (Sønderskov & Dinesen, 2014). Also for civic engagement, multiple papers have found a positive association (Dee, 2004) (Campbell, 2009). However, it is known that educational attainment, because it correlates heavily with ability and one's socio-economic background, is a very endogenous variable. This begs the question if the findings of these papers provide any evidence of causal relationships.

Additionally, a small body of work has analyzed the relationship between education and civic engagement by using matching techniques, and find mixed results (Kam & Palmer, 2008) (Mayer, 2011) (Persson, 2014) (Schnittker & Behrman, 2012). It should be said, however, that although these empirical strategies probably deal better with omitted variable bias than those in the previously mentioned papers, it would still be ignorant to interpret these results as causal.

In the last 20 years, there also have been several academic contributions that use schooling reforms or some other plausibly exogeneous phenomenon to try to get causal insights into education's impact on institutional trust and civic engagement. Some are quite convincing, some others less so. The rest of this section will focus on these works, since they are the most closely related to this thesis.

One of the first causal investigations related to the topics of this thesis is the paper by Milligan et al. (2004). The authors, studying the United States and the United Kingdom, use compulsory schooling laws to analyze the impact of education on citizenship. They find that, both in the US and the UK, education affects various measures of political interest and involvement, but only for the US they find

evidence of an impact on voter participation. The authors, for the US, also briefly study the effect of education on trust in the federal government, and find a significant positive impact; they however do not dive deeper into, or scrutinize, this result. The paper by Dee (2004), using child labor laws and the availability of junior- and community colleges for identification, reinforces Milligan et al.'s (2004) voting result by also finding that education increases US voter participation. Later academic work on the topic of US voter participation finds mixed results; Berinsky & Lenz (2010), using the Vietnam draft to try to estimate a causal effect, do not find an impact of education, while Chevalier & Doyle (2012), leveraging compulsory schooling laws, do find a positive impact. With regards to voter participation outside the US, research leveraging compulsory schooling laws in large pooled samples of European countries generally finds no impact of education (Chevalier & Doyle, 2012) (Le & Nguyen, 2021).

Having a more broad view, Siedler (2010), studying Germany, uses staggered state-level compulsory schooling reforms to analyze the impact of education on various types of civic engagement: being active in a citizen group, signing petitions, participating in demonstrations, and voting. He find no statistically significant effect of education on any of these outcomes. However, it should be noted that these results could also be driven by the large standard errors of the IV estimates, which in turn are partially caused by the modest size of his sample. Another potential reason for these null effects, as laid out by Siedler (2010) himself, is the unique post-WW2 state of German society around the time that these compulsory schooling reforms were passed. On a related note, the work by Pelkonen (2010) uses a staggered compulsory schooling reform in Norway to study the same outcomes, and only finds a (positive) significant impact for signing petitions.

Another relevant paper is that by Borgonovi (2010).[2] This author, studying a pooled sample of countries, uses compulsory schooling reforms to, among other things, study the impact of education on formal volunteering and participation in groups and associations, and voter participation. In her analysis she does not find significant estimates for these outcomes. This thesis would argue, however, that the fact that she studies a pooled sample of 21 European countries, which furthermore are very different[3] from each other along many dimensions, strongly raises the question if these aggregate null effects are masking nonzero effects for individual- or smaller groups of countries within this sample.

A key piece on the causal relationship between education and institutional trust is Kunst et al. (2019). These authors leverage schooling reforms in a fuzzy regression discontinuity design (RDD) to both individually, and in a pooled manner, study the effect of education on trust in the European Parliament in Denmark, Great Britain, The Netherlands, and Sweden. Both for the individual and the pooled analysis, they find that education causes people to be more trusting of the European Parliament. This result, however, is not robust to further testing (i.e., other bandwidths).

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[2] This paper is not available, so this thesis has to infer its contents by using an OECD report (Borgonovi & Miyamoto, 2010) from the same author, that publishes and illustrates its results.

[3] To illustrate, Borgonovi's (2010) sample consists both of countries like Belgium, Luxembourg, and the United Kingdom, but also countries like Finland, the Slovak Republic, and Hungary.

Another paper that investigates institutional trust is Yang (2019). The author uses British Social Attitudes (BSA) Survey data from 1986-2013 and a 1972 British compulsory schooling reform to, via fuzzy RDD, analyze the effect of education on trust in the UK government. What makes his paper unique to other research that leverages compulsory schooling reforms is the time period of the analysis, allowing him to estimate the impact on affected cohorts when they were relatively young. Another interesting element is that the survey questions Yang (2019) uses to study institutional trust are quite unorthodox.[4] The author finds statistically insignificant effects for education's impact on trust in UK politicians and the national government as a whole; he mentions, however, that these results could also be because of the relatively low size of his sample.

Another notable paper is the one by Österman (2021). In his paper, he uses compulsory schooling reforms that have been passed in the 10 countries of his pooled sample to try to study interpersonal (also called social) trust. Additionally, he runs one regression where he estimates the pooled effect of the passed compulsory schooling reforms on an index of institutional trust measures. He does not find a statistically significant estimate. This thesis would argue, however, that this very well could be because of a multitude of other reasons than there actually not being a real impact. Firstly, as also stated for Borgonovi's (2010) paper, the fact that Österman (2021) analyzes a pooled sample of 10 countries raises the serious concern that the aggregate null effect he finds masks a nonzero effect for individual- or smaller groups of countries. Secondly, Österman (2021) estimates the impact of the compulsory schooling reforms directly, but as Cavaille & Marshall (2019) show, most of the reforms Österman (2021) leverages do not actually (statistically significantly) increase schooling. There is thus a strong possibility that Österman's (2021) insignificant estimate is not caused by there not being an actual effect, but because most of the reforms he utilizes do not even (considerably) shift the amount of schooling received. Lastly, Österman (2021) only studies the impact on an additive index of institutional trust measures. It is possible that by just analyzing this index, he has missed to identify significant effects for individual elements of it.[5]

In summary, the causal research done so far on education's impact on institutional trust and civic engagement provides mixed results. What also makes this body of research far from conclusive is that, as elaborated on above, there are multiple methodological shortcomings that could have been at the root of the various estimated null effects.

Furthermore, an important facet of this body of research is considerably limited, and that is the study of the potential causal relationship between education and trust in national institutions; so far, just one

[4] These survey questions are quite unorthodox in that they do not ask the respondent if they *"in general"* trust a certain institution. To illustrate, the survey question used by Yang (2019) to measure trust in UK politicians is the following: "*How much do you trust politicians of any party to tell the truth when they are in a tight corner?"*.

[5] To illustrate, Österman's (2021) index includes trust measures of the police and the legal system; it is possible that education has a null effect on these two trust measures. Then, by analyzing just this index, it could be that significant effects for other individual components (e.g., trust in politicians) are missed out on.

piece of academic work (Yang, 2019) has comprehensively studied this, and only for one single country.

Additionally, on these topics, there are multiple other avenues of study that are still unexplored. For example, no causal research has been done on the impact of education on a type of civic engagement that today is very prevalent: boycotting.[6] Furthermore, most of Europe's countries, which include the countries of interest in this thesis, have not yet been individually analyzed, having only been studied in large pooled samples. It is thus the case that most of Europe's countries have not been studied properly, since these pooled studies raise the concern that aggregate null effects hide nonzero effects for individual- or smaller groups of countries. Another important avenue of exploration that has not yet been researched convincingly, is education's impact on trust in national institutions during the more polarized and unstable times of recent history, like the Great Recession, The Eurozone Crisis, and the rise of populism. It is important to study this topic for these periods, because (as argued later) these phenomena could have created a setting where the causal link between education and institutional trust is especially strong.

It is thus the humble opinion of the author of this thesis that further research on these topics is needed. The rest of this paper aims to do exactly that, methodologically improving on previous work, and filling the above-mentioned gaps in the literature.

3. Theoretical channels of causality

This section elaborates on the hypothetical channels through which education could impact trust in national institutions and civic engagement. What follows is divided in three subsections; the first one lays out the potential channels through which education impacts (national) political trust; the second one does the same for trust in the legal system and police; the last one expands on the theoretical channels through which education could impact how civically engaged people are.

3.1. Education and political trust

This thesis identifies three potential channels through which education is causally interconnected with political trust.

Firstly, education may impact political trust through its "sorting" effect. The idea is that by being more educated, people have better access to the privileged positions in society; this, in turn, makes them more likely to be trusting of the political system that governs said society (Hooghe et al., 2012). The potential existence of this effect is supported by the finding of Schoon et al. (2010) that social status is positively associated with political trust.

This thesis would argue that this first channel, if it exists, is especially powerful in the context of the countries and time period of this paper's analysis. This is because the survey data used encompasses the

[6] If you only consider previous studies that use data on Denmark, France, Great Britain, and The Netherlands, two other types of civic engagement have not been investigated before: signing petitions and demonstrating.

tumultuous time period in Europe of the Great Recession and the Eurozone Crisis. Economic crises like these have been shown to foster political distrust (Algan et al., 2017). If these crises more strongly have impacted people with less schooling – holding positions more susceptible to the cyclicality of the economy – the "sorting" effect of education may be especially relevant in this research context.

The second channel is the "political sophistication" effect, and it is proposed to work in the following way. Education – through its cognitive effects and the direct knowledge it provides – enables and causes people to have a deeper understanding of the national political apparatus and become more politically competent; this all, in turn, results in those with a higher level of education being more supporting and trusting of the political system (Hooghe et al., 2014) (Scheidegger & Staerklé, 2011).

The last identified channel, more specific to the research context of this thesis, is the "populism" effect. The hypothesized mechanism through which this effect works is the following. The share of the European population that likely views deindustrialization, automation, globalization, and immigration, most negatively, are lesser-educated people; this could both be because (**a**) people with less schooling are not as educated about these topics, thus being more prone to fear-mongering views regarding it, and/or (**b**) they are the group actually most negatively impacted by these developments. Because these developments are viewed most negatively by the people with the least amount of schooling and they have happened under the governance of the current political class, this group becomes more in favor of anti-establishment political parties. These populist parties arguably sow distrust in the population about the current political establishment. Since those with less education are more receptive to this anti-establishment messaging, another potential positive causal channel between schooling and political trust exists.

3.2. Education and trust in the legal system and police

Although it is possibly the case for the legal system that through an improved understanding of it education also fosters trust in this institution, this thesis hypothesizes that there is a causal effect of education on trust in the legal system and police because of one major channel. This channel is the "crime-reducing" effect, and its existence is highlighted by the work of Machin et al. (2011).

As Machin et al. (2011) show, increasing educational attainment has the ability to lower crime. This implies that those with less education, compared to those with more, at a higher rate have negative interactions with the legal system and police (i.e., get arrested and sentenced for a crime). These negative interactions likely cause people to view these two institutional actors in more of a bad light. This, in turn, also plausibly causes people to be less trusting of the legal system and police. Because people with varying levels of education are impacted by this effect in a different degree, a potential causal channel exists between educational attainment and trust in the legal system and police.

3.3. Education and civic engagement

Although the proposed theoretical channels regarding the causal relationship between education and institutional trust point toward a positive one, those regarding the relationship between education and civic engagement paint a more ambiguous picture.

The first hypothetical causal channel works through an "opportunity cost" effect. Economic theory predicts that well-educated people, because they are more likely to earn more, generally have a higher opportunity cost of time than those with less schooling. Via this channel, more education is thus expected to cause people to be less civically engaged. The potential importance of this channel is highlighted by the finding of Verba et al. (1995, p. 128-130) that the primary reason for political inactivity, which is closely related to civic engagement, is a lack of time.

The second potential channel through which education impacts civic engagement is the "information acquisition" effect. Education, Borgonovi et al. (2010) show, causes people to acquire more information about political and current affairs (as measured by the time spent reading newspapers). Since civic engagement is motivated by political and current affairs, and because education makes people more interested and informed regarding them, another positive causal channel could exist between the two.

The third hypothesized channel is the "civic ability" effect. Going through more schooling likely results in people having better analytical, communication, and organizational skills (Siedler, 2010). Furthermore, education potentially enhances one's civic skills (Verba et al., 1995). All this, in turn, could make the act of civic engagement easier, resulting in those better-educated participating more in it.

The last identified channel through which education could impact civic engagement is the "democratic values" effect. Attaining more years of schooling arguably fosters an improved appreciation of the importance of democracy (Siedler, 2010). Furthermore, schooling may confer upon people norms encouraging one to be civically engaged (Campbell, 2006). All this, in turn, potentially causes those with a higher level of education to be more likely to exercise the democratic act of civic engagement.

4. Research Design

Around the 1960s and 70s, Denmark (**1**), France (**2**), Great Britain (**3**), and The Netherlands (**4**) passed schooling reforms, which either mandated municipalities to provide an additional year of schooling (**1**), extended the length of education programs (**4**), or raised the minimum age at which a person is legally allowed to drop out of school (**2**-**4**). Table 1 lists, and lays out, the individual reforms.

These schooling reforms have created a situation where being just young enough, or just too old, to be affected by one is plausibly independent of someone's ability, socio-economic characteristics, and other potential confounding factors, but heavily correlated with educational attainment. In other words, these policies have arguably created a quasi-experimental setting, where cohorts of people just young

enough to be affected by one minimally differ from cohorts of people just too old, except in how much schooling they have received (on average).

This setting can thus be leveraged to circumvent the endogeneity issues related to studying the impact of education. The empirical analysis of this thesis does exactly that, using these reforms in regression discontinuity (RD) designs, to identify the impact of education on trust in national institutions and civic engagement.

Table 1: Overview of the schooling reforms leveraged in this thesis

Country/Region	Year(s) of reform	Description reform	Year-of-birth cohort first affected
Denmark	1958	Mandated all municipalities to provide an 8th year of schooling, effectively raising the minimum school leaving age from 14 to 15	1944
France	1959	Raised the compulsory schooling age from 14 to 16 for students who reached the age of 6 on, or after, the year 1959	1953
Great Britain	1972	Raised the compulsory schooling age from 15 to 16	1957
The Netherlands	1973-1975	Extended the length of basic vocational (1973) and lower-tier general (1975) secondary schooling programs from 3 to 4 years, and raised the compulsory schooling age from 15 to 16 (1975)	1959*

Notes: Information on the nature and timing of the reforms is derived from Cavaille & Marshall (2019) and Oosterbeek & Webbink (2007). *The Dutch 1973-1975 reform has multiple elements to it, which makes it so that identifying the year-of-birth cohort that was first affected by the reform is not so clear-cut. Appendix Section A.1. describes the Dutch reform in more detail, and elaborates on why this thesis has chosen the 1959 cohort as the cut-off point.

4.1. Data

The data source used by this thesis is the European Social Survey (ESS). The ESS is an academically driven cross-national survey that measures the attitudes, beliefs, and behavior of European citizens. It is conducted every two years, in the form of face-to-face interviews, with newly-selected, cross-sectional samples. Furthermore, each participating country is sampled individually, so that the data is nationally representative.

The empirical analysis uses British, Danish, Dutch, and French ESS data. This data is used to examine each country[7] individually – to study heterogeneous effects – and also pooled together – which is necessary to analyze the direct impact of education on institutional trust and civic engagement.

[7] For ease, this thesis refers to Great Britain as a country, although the author realizes that it is only a part of one.

The reason for studying this particular set of country-reform pairs is that, out of the many schooling policies implemented across Europe, these 4 countries are among the only ones that have had reforms passed that actually significantly increased schooling.[8] Another relevant aspect is that all these countries have seen the rise of popular anti-establishment parties. This, as previously argued (in Section 3.1), has potentially created another causal channel through which education impacts institutional trust.

Lastly, it is important to note that since the schooling reforms in the countries of interest occurred around the 1960s and 70s, and the ESS has been conducted from 2002 on, that the empirical analysis estimates the long-term impact of education on the outcomes.

4.2. Dependent variables of interest

Table 2 displays the exact survey questions used to measure trust in 5 national institutions and participation in 7 types of civic engagement. The table also lists the response options for each question.

Table 2: Survey questions used to measure institutional trust and civic engagement.

Question	Wording	Response options
1	Please tell me on a score of 0-10 how much you trust each of the institutions I read out. 0 means you do not trust an institution at all, and 10 means you have complete trust. Firstly… …politicians?	0-10 scale
2	" .. ". Firstly… …political parties?	0-10 scale
3	" .. ". Firstly… …[country]'s parliament?	0-10 scale
4	" .. ". Firstly… …the legal system?	0-10 scale
5	" .. ". Firstly… …the police?	0-10 scale
6	There are different ways of trying to improve things in [country] or help prevent things from going wrong. During the last 12 months, have you done any of the following? Have you… …contacted a politician, government or local government official?	Yes/no
7	" .. ". Have you… …worked in a political party or action group?	Yes/no
8	" .. ". Have you… …worked in another organisation or association?	Yes/no
9	" .. ". Have you… …signed a petition?	Yes/no
10	" .. ". Have you… …taken part in a lawful public demonstration?	Yes/no
11	" .. ". Have you… …boycotted certain products?	Yes/no
12	Some people don't vote nowadays for one reason or another. Did you vote in the last [country] national election in [month/year]?	Yes/no/not eligible to vote

[8] Cavaille & Marshall (2019), in their study of the effect of schooling reforms occurring across 14 European countries around the 1960s and 70s, only find statistically significant effects for 6 reforms in 5 countries: Denmark, France, Great Britain (a 1944 reform and the 1972 reform that this thesis leverages), The Netherlands, and Sweden.

4.2.1. Transforming the survey response variables

Leaving the survey response variables regarding trust in the various national institutions unchanged would lead to multiple (potential) problems. Firstly, the ordinal nature of the response options force a linear relationship to exist between different levels of trust, which possibly does not hold. Secondly, the interpretation value of the survey response variables is low; for example, what does it mean if one year of additional education increases trust by 1 on a 0-10 scale? Additionally, it is possible that the same value of a survey response variable signals different degrees of trust across the 4 countries of interest. This, when studying the pooled sample, further complicates the interpretation of the estimates.

To deal with these issues, the survey response variables with a 0-10 scale are recoded to binary institutional trust variables. These binary variables are anchored around the country-specific median values of the survey response variables.

For the survey response variables related to trust in political actors – politicians, political parties, and the national parliament – the binary variables are coded as 1 if the trust score is higher than the country-specific median. For example, if a French respondent provides a trust score for political parties higher than the median of the French ESS data, the binary variable is coded as 1. For the survey response variables related to trust in non-political actors – the legal system and police – the binary variables are coded as 1 if the trust score is equal to, or higher, than the country-specific median. To illustrate, if a Dutch respondent provides a trust score for the legal system equal to, or higher, than the median of the Dutch ESS data, the binary variable is coded as 1.

The reason why, for trust in non-political actors, a score <u>on</u> the country-specific median is also coded as 1, is because a large amount of the respondent answers bunch there. Such a substantial amount, in fact, that if a score on the median would be coded as 0, only a relatively small share of respondents would be coded as 1, such that the binary variables would actually be closer to capturing being above the upper-quartile level of trust in a particular institution. With regards to the effect of education on institutional trust, preliminary research by this thesis indicates that in this segment of the trust-distribution there is barely any movement. The more insightful, and consequently preferable way, to code the binary variables relating to trust in non-political actors is thus in the manner described in the previous paragraph.

To investigate if education shifts people from the very low end of their country's trust-distribution to a higher segment, the main analysis additionally studies a specification of the binary trust variables that is anchored around the country-specific lower quartile values of the survey response variables. This anchoring is done exactly in the same way as described for the median specification.

Importantly, constructing the binary institutional trust variables in these ways results in the following interpretation of the main estimates: *the probability of a person being relatively trusting of the particular*

institution, where *relatively* means 'compared to the median/lower-quartile level of trust in the person's country'.

The survey response variables regarding civic engagement will not be recoded. However, for the analysis of the impact of education on voter participation, respondents that are not eligible to vote are dropped from the estimation sample.

4.3. Identification strategy

To identify the causal effect of education on institutional trust and civic engagement, this thesis leverages the fact that cohorts of people that were just young enough to have been affected by a certain schooling reform likely differ minimally from the cohorts of people that were just too old, except in that the former group (on average) received substantially more schooling.

In practical terms, this entails that the empirical analysis uses the schooling reforms in a regression discontinuity design to get estimates on the effects of interest. More specifically, both reduced form and fuzzy RD designs are used. The reduced form RD design allows for identification of the impact of the reforms themselves on the dependent variables of interest. The fuzzy RD design allows this thesis to study the effect of education on the outcomes directly.

4.3.1. Reduced form RD design

In the reduced form RD design, this thesis defines the treatment indicator variable – being in a cohort (j) affected by a country's (c) schooling reform or not – in the following way:

$$Reform_{jc} = \begin{cases} 1 \; if \; year\; of\; birth - year\; of\; birth\; first\; cohort\; affected\; by\; reform \geq 0 \\ 0 \; if \; year\; of\; birth - year\; of\; birth\; first\; cohort\; affected\; by\; reform < 0 \end{cases}$$

This treatment indicator variable is used to estimate the following regression:

$$y_{ijc} = \beta_0 + \beta_1 Reform_{jc} + \beta X_{ic} + \mu_c + \lambda_t + \varepsilon_{ijc} \quad (\mathbf{1})$$

Where y_{ijc} is a binary outcome variable that either measures, for individual i, some form of institutional trust, or participation in a certain type of civic engagement; the coefficient β_1 captures the local average treatment effect of the schooling reforms on the specific outcome variable, for cohorts of people just young enough, or just too old, to be affected; μ_c is a set of country dummies; λ_t is a set of ESS round dummies.

X_{ic} consists of a set of additional covariates relating to demographics, area of residence, and parental education. Furthermore, to control for potential unobservable, time-varying, confounding factors that correlate with both the reforms and the outcome variables, country-specific quadratic birth cohort trends are also included.

Importantly, equation (**1**) is estimated on restricted samples of people just too old, or just young enough, to be affected by the schooling reforms. More specifically, the estimation samples comprise of a bandwidth of at most +- 5 year-of-birth cohorts around the cohort first affected. This ensures that the regression estimates of interest are minimally plagued by possible omitted variable bias.

4.3.2. Fuzzy RD design

What takes away from the insightfulness of the reduced form RD design is that many people in the cohorts affected by the schooling reforms were not shifted in their educational attainment; they were going to get all the schooling in the segments of education that were shifted by the reforms, independent of the policy being implemented or not. The same also holds conversely: even with the schooling reforms passed, people undoubtedly were still dropping out too early. All this makes it so that the reduced form RD estimates, although arguably unbiased, capture hard-to-dissect effects that do not give a straightforward insight into how *x* amount of additional schooling impacts the dependent variables of interest.

Thus, in order to investigate the direct impact of education on the outcome variables, this thesis additionally employs a fuzzy RD design. This design leverages the fact that the schooling reforms, for a certain subset of the student population, created a discontinuity in the probability of attaining more schooling, where for the cohorts just too old to have been affected by the reform this probability is substantially lower relative to the ones just young enough.

More precisely, the following regression specification is used:

$$y_{ijc} = \alpha_0 + \alpha_1 \widehat{Schooling}_{ijc} + \alpha X_{ic} + \mu_c + \lambda_t + \varepsilon_{ijc} \quad (\mathbf{2})$$

Where the explanatory variable of interest, $Schooling_{ijc}$, is instrumented by the following first-stage regression:

$$Schooling_{ijc} = \gamma_0 + \gamma_1 Reform_{jc} + \gamma X_{ic} + \mu_c + \lambda_t + u_{ijc} \quad (\mathbf{3})$$

It is important to note that the estimate of interest α_1 in (**2**) is even more local than the reduced form RD estimate β_1 in (**1**). Aside from α_1 only capturing the effect of an additional year of schooling on the outcome variable for cohorts around the cut-off, this regression coefficient furthermore only captures the impact for the compliers: the people that <u>because of</u> the schooling reforms attained additional education.

4.3.3. Continuity in density at the cut-off point

In regression discontinuity designs it is commonplace to test if there is a change in density at the cut-off point of the running variable. In the context of this thesis, this is *x = year of birth – year of birth first cohort affected by reform = 0*.

Although tests like these are mainly used in the literature to investigate if manipulation around the cut-off has occurred, this thesis would argue that this is not a concern in this research context. This is because, for manipulation around the cut-off to have occurred, soon-to-be-parents would had to have foreseen far into the future that a schooling reform was going to take place and of which nature this reform was going to be. Furthermore, this information would have needed to significantly incentivize couples to adjust their baby-planning in accordance with (the timing of) this reform. The likelihood that all of these things have occurred, this thesis humbly argues, is less than minimal.

However, a change in density at the cut-off point could still signal concern. It could, for example, be an indication that cohorts on one side of the cut-off are systematically more likely to participate in surveys. That would potentially be problematic for the validity of the research design.

For this reason, Appendix Section A.2. investigates, for both the pooled and individual-country samples, if there is a change in density at the cut-off point of the running variable. More specifically, two types of density tests are performed: the one by McCrary (2008), and the one by Frandsen (2017). These tests provide no evidence of a discontinuity in density at the cut-off.

4.3.4. Validity of the instrumental variable identifying assumptions

Underlying the fuzzy RD research design are 4 identifying assumptions that need to hold for the regression coefficients of interest to yield causal estimates. This subsection elaborates on the validity of each of these assumptions in the context of this study.

Firstly, the instrumental variable $Reform_{jc}$ in (**3**) needs to be uncorrelated with ε_{ijc} in (**2**). The balancing test conducted in Appendix Section A.3. gives insight into this. Although most of the examined background variables are found to be exogeneous, some minor endogeneity exists for two. Still, by controlling for these two variables in the regressions, this thesis would argue that the exogeneity condition is (at the minimum) very close to be satisfied.

Secondly, the instrumental variable $Reform_{jc}$ in (**3**) needs to be strongly correlated with $Schooling_{ijc}$ – i.e., there needs to be a meaningful first stage. The F-statistics in the results section indicate that this identifying assumption holds.

The third identifying assumption is that $Reform_{jc}$ only affects the dependent variables through its impact on $Schooling_{ijc}$ – i.e., the exclusion restriction condition needs to hold. Given how specific the nature of each leveraged schooling reform is, it seems very unlikely that there is another variable through which $Reform_{jc}$ impacts the outcomes.

The last assumption needed for identification is that the schooling reforms did not cause some students to attain less education – i.e., the monotonicity condition needs to hold. This thesis would argue that this is very likely, and that one can comfortably assume that this condition is satisfied.

Overall, given that the identifying assumptions plausibly hold (or in the case of the exogeneity assumption, are (at the minimum) very close to holding), this thesis thus argues that the estimates in Section 5 – the main results – can be given a causal interpretation.

4.4. Constructing the estimation sample

4.4.1. Restricting the analysis to ESS rounds 1-7

As of 2023, 10 rounds of the ESS have been conducted, spanning the time period of 2002-2022. The decision has been made, however, to restrict the estimation sample to data from rounds 1-7, spanning 2002-2015. This is to prevent that the relevant respondents – those part of the estimation bandwidth of +- 5 year-of-birth cohorts around the cut-off – were very old at the time they were surveyed. This would be a danger to the validity of the research design for two reasons.

Firstly, if the relevant respondents are in fact so old that cohorts unaffected by the schooling reforms are substantially more immobile compared to those affected, and this immobility impacts how civically engaged people are, the reform treatment variable would be confounded, biasing the estimates.

Secondly, if the relevant respondents are really old it is plausible that cohorts unaffected by the schooling reforms (on average) are of a poorer health than those affected. This is because at an older age the average health of people worsens more rapidly. In the likely scenario that health status has an impact on one's probability of participating in the ESS, and furthermore health status is correlated with socio-economic status, the reform treatment variable would thus be confounded, biasing the estimates.

To illustrate, if you compare cohorts of people aged 60-65 that were affected by a schooling reform with cohorts of people aged 65-70 that were unaffected, it is more plausible that these two groups are similar in terms of mobility, health, and socio-economic status than if you were to compare them when they are 70-75 and 75-80.

4.4.2. Excluding 4 country-round subsamples

When using all the British, Danish, Dutch, and French data from rounds 1-7, not a strong enough relationship (i.e., a first-stage F-statistic > 10) exists between the schooling reforms and educational attainment. To solve this issue, country-round subsamples for which a relatively weak relationship exists are dropped from the estimation sample.[9] More precisely, for each country, the weakest subsample is dropped: ESS round 3 for Denmark; ESS round 6 for France; ESS round 1 for Great Britain; ESS round 5 for the Netherlands.

[9] The likely reason why there is a difference in the strength of the relationship between a country's schooling reform and educational attainment over the ESS rounds is the following. Each country-round subsample is a newly-selected group of people, and the size of these subsamples is quite small – ranging from +- 1500 to +- 2000. Because of this, even with strict random probability sampling methods, the country-round subsamples are going to differ.

Although suboptimal, making these adjustments should not be problematic. This is because the remaining sample still consists of 24 country-round subsamples that are each individually valid – i.e., each country-round group of data is a newly-selected sample that is nationally representative of a specific country at a certain point in time.

To support the argument that these adjustments are unproblematic, this thesis demonstrates in the robustness section (**6.3.**) that the main insight – that education positively impacts institutional trust – still holds if the 4 excluded country-round subsamples are included back into the estimation sample.

5. Results

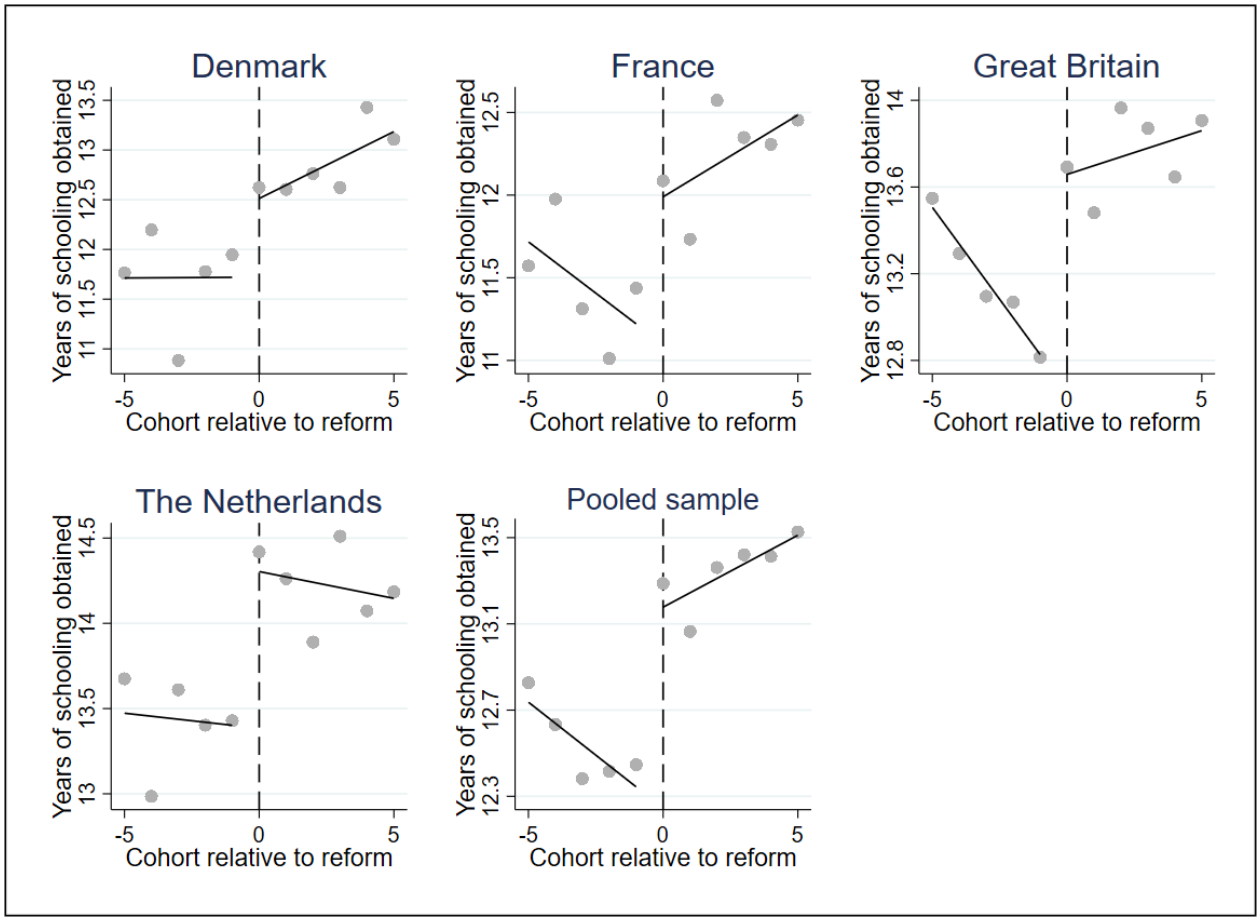
5.1. The impact of the schooling reforms on educational attainment

Before the main analysis commences, this thesis first verifies that the schooling reforms have caused substantial differences in educational attainment between affected and unaffected cohorts.

Figure 1 displays, for each individual country and these pooled together, how the *average years of total schooling obtained* differs for cohorts just too old, or just young enough, to be affected by the reforms. For all individual countries, the graphs clearly show that the schooling reforms have created a quasi-experimental setting, where cohorts that are considerably close to each other in terms of year of birth – and thereby are arguably very similar in terms of endogenous characteristics – differ substantially in how educated they are.

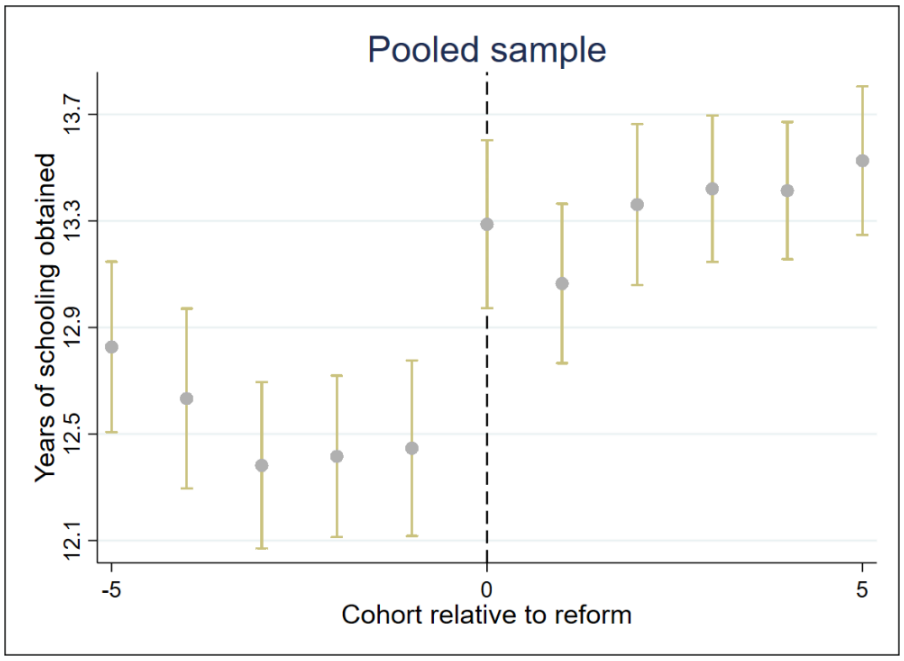
Then, looking at all the countries of interest pooled together, again a visually clear-cut difference in educational attainment between affected and unaffected cohorts is found. Furthermore, a considerable jump in obtained years of total schooling is found at the cut-off (0.84 years), which creates a statistically significant discontinuity between the cohort closest to the cut-off and the one on it (see Figure 2).

Figure 1: Average years of total schooling obtained for cohorts just too old, or just young enough, to be affected by the schooling reforms, for the individual countries and the pooled sample



Note: The average years of schooling obtained for the cohorts either side of the cut-off are fitted with a linear polynomial.

Figure 2: Average years of total schooling obtained for cohorts just too old, or just young enough, to be affected by the schooling reforms, for the pooled sample



Note: For each cohort, 95% confidence intervals are displayed.

5.1.1. Have the schooling reforms increased the attainment of post-secondary education?

Since all of the 4 schooling reforms implemented a policy change with regards to secondary education, it is obvious that people have been shifted in this segment. However, what potentially also could have happened is that these reforms have increased the attainment of post-secondary schooling.

It could for example be that people, by being forced, or incentivized, to stay longer in secondary school, were more likely to find a subject they were passionate about, resulting in more people pursuing tertiary education. Another possibility is that the reforms changed the (perceived) cost of attaining post-secondary education for students and/or their parents. For example, if a certain attractive (e.g., well-paying) degree previously took 5 years of non-mandatory schooling but post-reform now takes only 3 (because the students had to stay in school anyway), it could be that the students are more strongly incentivized to attain that degree (and the parents might also be more likely to push their children to pursue it), resulting in more tertiary education attainment.

This thesis thus investigates if the schooling reforms have increased the attainment of tertiary education, defined as the respondent having obtained (at least) a bachelor degree or equivalent. The used bandwidth in all the regressions is 5 – the maximum bandwidth used in the main analysis.

And indeed, as Table A3 (in Appendix Section A.4.) shows, the estimates indicate that the reforms have increased the attainment of post-secondary schooling: both for the French and the pooled sample, statistically significant evidence is found that the schooling reforms have increased the probability that someone has completed tertiary education, by 7.8% and 3.6%, respectively.[10]

5.2. Fuzzy RD results

In this section, for the pooled sample, the impact of education on the dependent variables of interest is investigated directly. To achieve this, a fuzzy RD design is used, where being just young enough, or just too old, to be affected by the schooling reforms is used as an instrument for educational attainment. The specification of educational attainment that is instrumented for is *total years of schooling obtained.*

Importantly, the main bandwidth used in the fuzzy RD estimations is 5. There are, however, two exceptions to this. Firstly, for the regression on trust in a country's national parliament, a bandwidth of 4 is used. The reason for this is that the estimate is statistically significant at this bandwidth and not at a greater one. Secondly, for the regression on trust in political parties, a bandwidth of 4 is used for the Danish data but a bandwidth of 5 is used for the other countries. This adjustment was necessary because using a bandwidth of 5 for all the countries results in a first-stage F-stat < 10.[11]

[10] A logical explanation why the French reform has had an especially substantial impact is because it was quite extreme compared to the other reforms, raising the mandatory school leaving age by 2 years.

[11] The Danish data is the only option for making this adjustment, since narrowing the bandwidth of the data for one of the other 3 countries causes the first-stage F-stat to decrease.

5.2.1. The impact of additional education on institutional trust

This subsection investigates the impact of additional education on institutional trust in two distinct ways.

Firstly, it investigates if additional education has caused people to be shifted to the relatively trusting segment of their country with regards to a particular institution. More specifically, estimations are done to measure how one additional year of education impacts the probability that a complier has a level of trust in a specific national institution that is above (for non-political institutions, including on) the country-specific median level. Secondly, this subsection also studies if additional education has prevented people from having very low levels of trust in a particular institution, relative to others in their country. More precisely, estimations are done to measure how one additional year of education impacts the probability that a complier has a level of trust in a specific national institution that is above (for non-political institutions, including on) the country-specific lower-quartile level.

The results are shown in Table 3, where the estimates regarding the first and second approach can be found in the '*median'* and '*lower-Q'* columns, respectively.

In line with the theoretical causal channels laid out in Section 3.1 and 3.2, this paper finds considerable, statistically significant, positive estimates of the effect of additional education on trust in a country's national institutions.

Interestingly, with regards to the national parliament and the legal system, education seems to shift people along the middle of their country's trust distribution, causing them to have relatively positive views of these institutions. More specifically, one additional year of education increases the probability of a complier having an above-median level of trust in the national parliament by 7.8%; for the legal system, one additional year of education increases the likelihood of having a trust level on, or above, the country-specific median by 6.5%.

On the other hand, with regards to politicians and political parties, education seems to shift people out of the very low end of the trust distribution, preventing them from holding views of these institutions that, compared to their countryfolk, are very negative. More precisely, one additional year of education increases the probability of a complier being above the lower-quartile level of trust in politicians by 6.9%; for political parties, one additional year of education increases the probability of a complier being above the lower-quartile level of trust by 9.9%.

With regards to the police, the estimates indicate that education both shifts people from the very low end of their country's trust distribution, and also makes them relative trusting of this institution, where the effect is stronger for the latter: one year of additional education increases the likelihood of a complier being on, or above, the lower-quartile and median-level of trust in the police by 6.1% and 7.8%, respectively.

Table 3: Fuzzy RD results for trust in national institutions

	(**1**) Trust in politicians		(**2**) Trust in political parties		(**3**) Trust in national parliament		(**4**) Trust in the legal system		(**5**) Trust in the police	
	Median	*Lower-Q*	*Median*	*Lower-Q*	*Median*	*Lower-Q*	*Median*	*Lower-Q*	*Median*	*Lower-Q*
Years of schooling	0.060	0.069*	0.053	0.099**	0.078**	0.051	0.065*	0.048	0.078*	0.061*
	(0.038)	(0.037)	(0.039)	(0.045)	(0.039)	(0.036)	(0.036)	(0.030)	(0.040)	(0.033)
Bandwidth	5		5 (4 for Denmark)		4		5		5	
Observations	7,259		6,042		5,931		7,247		7,271	
1st stage F-stat	11.7		10.1		10.6		12.4		11.5	

Notes: Controls included are: gender, age (both linear and squared terms), dummies indicating if the respondent lives in an urban area, belongs to an ethnic minority group in the currently residing country, is a citizen in the currently residing country, was born in the currently residing country, his/her parents were born in the (respondent's) currently residing country (1 dummy for each parent), his/her parents having completed (at least) upper secondary education (1 dummy for each parent). ESS round & country dummies are also included, as well as country-specific quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

5.2.2. The impact of additional education on civic engagement

Table 4 displays the estimated impact of education on how civically engaged people are. More specifically, for all outcomes except voting, the coefficients measure how one additional year of education impacts the probability that a complier has participated in a certain type of civic engagement in the last 12 months; for voting, the respective coefficient measures how one additional year of education impacts the probability that a complier has voted in the last national election.

The signs of the estimates indicate, overall, an ambiguous effect of education on how civically engaged respondents are. This is not entirely surprising, given that the hypothetical causal channels laid out in Section 3.3 are both negative and positive. The magnitude of the coefficients, however, show that the estimated effect is very minor for most examined types of civic engagement. The exceptions are for the acts of voting and signing a petition: one additional year of education is estimated to increase the probability of voting by 3.0%, and decrease the probability of signing a petition by 4.5%. However, given the standard errors, the strength of these two effects is not sufficient to reach statistical significance for either.

All in all, the fuzzy RD results thus provide no evidence of a causal link between education and civic engagement.

Table 4: Fuzzy RD results for the civic engagement types

	(1) Voting	(2) Contacted politician or gov't official	(3) Worked in a political party or action group	(4) Worked in other civic organisation	(5) Signed a petition	(6) Taken part in a demonstration	(7) Boycotting
Years of schooling	0.030	-0.013	0.006	-0.003	-0.045	-0.016	-0.007
	(0.026)	(0.028)	(0.015)	(0.029)	(0.037)	(0.019)	(0.031)
Bandwidth	5	5	5	5	5	5	5
Observations	7,129	7,288	7,289	7,287	7,270	7,288	7,274
First stage F-stat	10.5	11.7	11.8	11.6	11.1	11.7	11.6

Notes: Controls included are: gender, age (both linear and squared terms), dummies indicating if the respondent lives in an urban area, belongs to an ethnic minority group in the currently residing country, is a citizen in the currently residing country, was born in the currently residing country, his/her parents were born in the (respondent's) currently residing country (1 dummy for each parent), his/her parents having completed (at least) upper secondary education (1 dummy for each parent). ESS round & country dummies are also included, as well as country-specific quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

5.3. Reduced form RD results

Although the fuzzy RD design arguably leads to more insightful estimates, it does have one limitation. Because of the relevant first stage condition, the fuzzy RD analysis is unable to validly investigate the impact of additional education for the individual countries, thereby missing out on the insight of which countries are driving the pooled results.

The reduced form RD analysis, discussed in this section, which estimates the impact of the schooling reforms themselves on the outcome variables of interest, is able to shine light on this. This results in the fuzzy and reduced form RD analyses, together, forming a comprehensive investigation.

For the sake of consistency and inter-comparability, the bandwidth used for each outcome variable in the reduced form RD analysis is the same as the corresponding fuzzy RD estimation. Importantly, the relevant reduced form RD regression tables also display the values of the country-specific median and lower-quartile trust scores of the survey response variables (SRV) which the trust outcome variables are based on.

5.3.1. The impact of the schooling reforms on institutional trust

Table A4 & A5 (in Appendix Section A.5.) display the estimated impact of the schooling reforms, both for the individual countries and them pooled together, on trust in a country's various national institutions; Table A4 displays the results for the median specification of the trust outcome variables, and Table A5 for the lower-quartile specification. The estimates in the former table thus capture the impact of the reforms on the probability of someone, compared to others in his/her country, being relatively trusting of the respective national institution; the estimates in the latter table capture the impact of the reforms on the probability of not belonging to the most distrusting segment of one's country, thereby capturing if these policies have been able to prevent people from having (in relative terms) very negative views of national institutions.

The signs of most of the estimates indicate that the schooling reforms, and thus increased educational attainment, have fostered institutional trust, which again is in line with the theoretical mechanisms proposed in Section 3.1 & 3.2.

Studying the pooled sample for the median specification of the outcome variables, the reduced form RD results indicate that the reforms, and thus increased educational attainment, have increased institutional trust. For nearly all examined institutions – politicians, the national parliament, the legal system, and the police, statistically significant effects are found, which indicate that the reforms have made people relatively trusting compared to their countryfolk. These findings are largely in line with the corresponding fuzzy RD results, although the reduced form RD results also show a statistically significant effect for trust in politicians. The reduced form RD estimates for the individual countries highlight that the pooled results related to trust in political institutions are driven by the schooling reforms in France, Great Britain, and The Netherlands, where especially the French reform has had a substantial impact. The pooled result for trust in the legal system, on the other hand, is driven by the British and Dutch schooling reforms only. The statistically significant result for trust in the police is driven by the schooling reforms of all 4 countries.

Examining the pooled sample for the lower-quartile specification of the outcome variables, the reduced form RD estimates provide evidence that the reforms, and the increase in educational attainment they brought, have prevented people from having very negative views of national institutions compared to others in their country. For 4 out of the 5 examined institutions – politicians, political parties, the legal system, and the police – evidence is found that the reforms have shifted people away from the most distrusting segment of their country's trust distribution. These results are again very much in line with the corresponding fuzzy RD results, albeit that the reduced form RD results additionally indicate a statistically significant effect for trust in the legal system. Like with the median specification of the outcome variables, the pooled results for trust in political institutions are driven by the schooling reforms in France, Great Britain, and The Netherlands, with the main driver being the French reform. The pooled

result for the lower-quartile specification of trust in the legal system, however, is this time primarily driven by the British and Danish reforms. The statistically significant finding that the schooling reforms have prevented people from being very distrusting of the police is driven by all 4 countries.

Overall, the reduced form RD results further solidify the evidence from the fuzzy RD regressions that education has a positive causal effect on institutional trust.

5.3.2. The impact of the schooling reforms on civic engagement

Table A6 (in Appendix Section A.5.) displays the estimated impact of the schooling reforms, both individual and pooled, on the civic engagement of respondents.

Like the corresponding fuzzy RD results, the signs of the estimates paint an ambiguous picture about the effect of the reforms, and thus increased educational attainment, on how civically engaged respondents are. Most of these estimates are of a small magnitude, however, and nearly all are furthermore statistically insignificant, suggesting that there generally is no causal link between education and civic engagement.

Nonetheless, for Great Britain, there is evidence that the schooling reform, and the increase in educational attainment it brought, has affected one's propensity to demonstrate, decreasing the likelihood of participating in this type of civic engagement by 3.7%.

6. Robustness of findings

In this section the findings of the main analysis are placed under further scrutiny. More precisely, this thesis checks if the following results are robust to different bandwidths: the finding that education fosters trust in political institutions – i.e., politicians, political parties, and a country's national parliament; the finding that education fosters trust in non-political institutions – i.e., the legal system and the police.

Importantly, this section also verifies that our findings are not driven by the exclusion of the 4 country-round subsamples, namely ESS round 3 for Denmark, ESS round 6 for France, ESS round 1 for Great Britain, and ESS round 5 for the Netherlands.

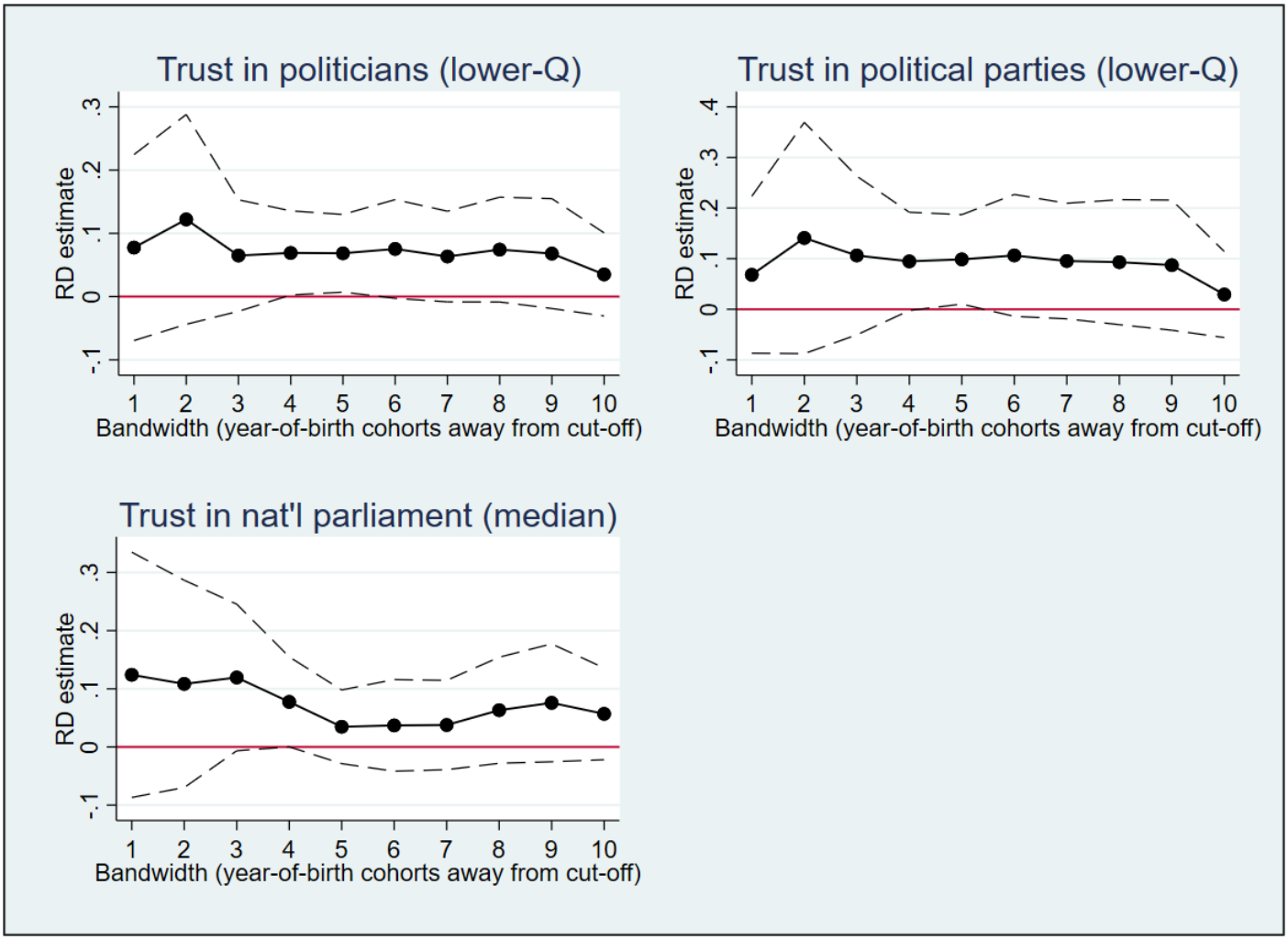
6.1. Robustness test of education's effect on political trust

Figure 3 displays how the fuzzy RD estimates relating to education's effect on trust in political institutions change over the range of a bandwidth from 1-10. Interestingly, for trust in politicians and a country's national parliament, restricting the estimations to smaller bandwidths only makes the magnitude of the coefficients more substantial. This supports the argument that the significant effects this thesis finds in the main analysis indeed capture the causal effect of education on institutional trust, and not some potential confounding factor(s) active around the implementation of the schooling reforms.

Looking at education's impact on trust in political parties over the bandwidth range, we can observe that, as the bandwidth narrows, the coefficient generally increases in magnitude. Importantly, however, the magnitude of the estimate dips in a meaningful way at a bandwidth of 1. Nevertheless, the estimate at this bandwidth is still of considerable size (RD estimate = 0.068). This, together with the fact that the RD estimate reaches its highest magnitude at the small bandwidth of 2, leads this thesis to conclude that this specific effect is quite robust.

Overall, these robustness graphs thus further solidify the finding that education has a positive causal effect on trust in national political institutions.

Figure 3: Robustness graphs of education's impact on political trust



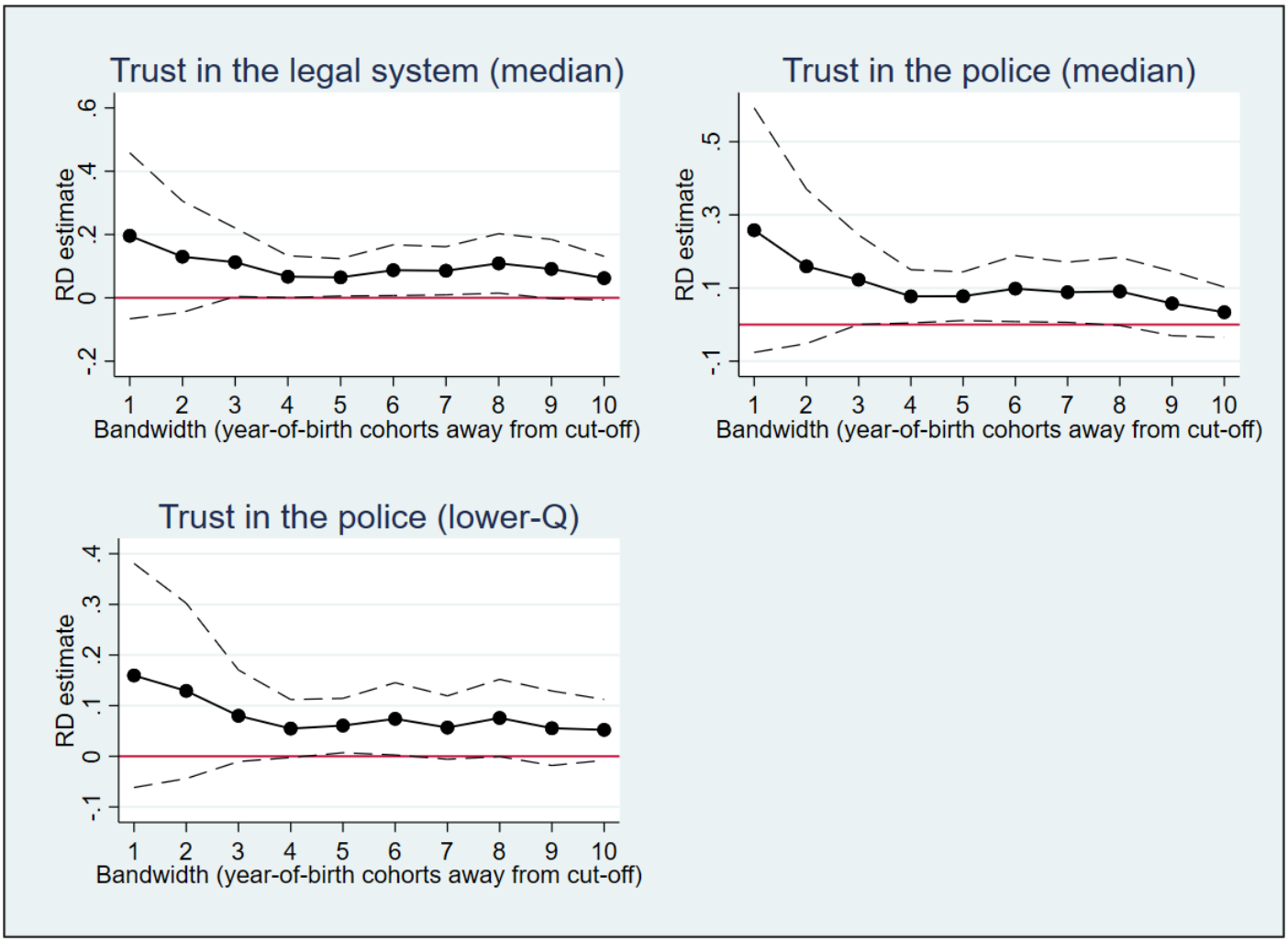
Notes: For education's impact of trust in politicians, 90% confidence interval bands are displayed (in dashed lines); for education's impact on trust in political parties and a country's national parliament, 95% confidence interval bands are displayed (in dashed lines).

6.2. Robustness test of education's effect on trust in non-political institutions

Figure 4 displays the fuzzy RD estimates relating to education's impact on trust in non-political institutions over a 1-10 bandwidth range. For all three outcome variables – being on, or above, the median/lower-quartile trust level in the legal system/police – the graphs show that the magnitude of the RD estimates substantially increases as the bandwidth gets more narrow, with the strength of the effects peaking at the minimum bandwidth of 1.

Overall, these graphs thus provide strong evidence that the positive, statistically significant effects, found in the main analysis indeed capture the causal effect of education on trust in non-political institutions, and not some confounding factor(s).

Figure 4: Robustness graphs of education's impact on non-political trust



Note: For all graphs, the dashed lines display 90% confidence interval bands.

6.3. Adding the excluded country-round subsamples

To have a first-stage relationship between the schooling reforms and educational attainment that is above the academically-accepted level of strength needed for instrumental variables estimation, this thesis has excluded 4 country-round subsamples from the estimation sample. This is argued (in subsection 4.4.2.) to not be problematic, since every country-round subsample remaining in the estimation sample should be individually valid, and thus also as a whole.

Still, to be diligent, this thesis investigates if the finding that education fosters institutional trust is driven by the exclusion of these 4 country-round subsamples.

To confirm that excluding this data does not drive the main finding that education fosters institutional trust, reduced form RD regressions on the trust outcome variables are re-run for a sample with these 4 country-round subsamples included back in. Table A7 (in Appendix Section A.6.) shows these results, which provide strong evidence that the schooling reforms, and thus increased educational attainment, have positively impacted institutional trust. This supports the argument that excluding the 4 country-round subsamples is not problematic, which in turn increases the validity of the findings of the main analysis.

7. Why does education foster political trust?

Having established that education positively impacts trust in political institutions, this section now investigates which mechanisms underly this effect.[12] This investigation is guided by the theoretical causal channels outlined in Section 3.1.

To test if the causality operates through a "sorting" effect, this thesis investigates if education has caused people to end up in more privileged societal positions; in practical terms, this means that the effect of additional education on the probability of working (if unemployed: having worked) in a low-skilled job is estimated.[13]

To test if the causality operates through a "political sophistication" effect, this thesis examines if education enables and causes people to have a deeper understanding of the national political system and become more politically competent. To investigate if this has occurred, the impact of additional education on one's self-reported ability to comprehend politics is estimated. More precisely, this ability

[12] Unfortunately, this thesis is unable to do the same for the established causal relationship between education and trust in non-political institutions – the legal system and the police. The reason for this is that the content of the ESS does not allow for the existence of education's "crime-reducing" effect to be tested.

[13] The following occupations are coded as low-skilled: clerks; service workers, agricultural and fishery workers; trade workers; machine operators; what the ESS classifies as "elementary occupations". Conversely, the following occupations are not coded as low-skilled: armed forces; legislators, senior officials, and managers; professionals (e.g., engineers, accountants, and medical doctors); associate professionals (e.g., engineering technicans, finance and sales associate professionals, nursing associate professionals).

is measured by one's response to the statement "politics is too complicated to understand", where the answer options are "never", "seldom", "occasionally", "regularly", and "frequently". The dependent variable where the effect is estimated on is a dummy variable which is either anchored around the country-specific median or lower-quartile degree of self-reported ability – i.e., equalling 1 if the respondent voices an understanding of politics that is above the thresholds of the response options "occasionally" and "regularly", respectively.

To test if the causality operates through a "populism" effect, this thesis analyzes if education causes people to be less receptive to the messaging of anti-establishment parties. To study this, a person's self-reported closeness to anti-establishment political parties is used. More precisely, the effect of education on both closeness to left- and right-wing populist parties is estimated, where the dependent variable is a dummy that equals 1 if the respondent reports feeling closest to one of the populist political parties listed in Appendix Section A.7., and 0 otherwise (= feeling closest to a non-populist party or not close to any). Since a left-wing populist party only was an answer option for respondents from The Netherlands, the estimation of education's effect on closeness to a left-wing populist party is restricted to the Dutch data.

As Table 5 shows, the estimation results provide evidence that the causal effect of education on political trust operates through all three of the identified causal channels.[14] Firstly, additional education statistically significantly decreases the likelihood of ending up in a low-skilled job, thus highlighting the existence of a "sorting" effect. Secondly, additional education significantly improves one's ability to comprehend politics, which underscores the existence of a "political sophistication" effect. Thirdly, additional schooling significantly decreases the probability of reporting feeling closest to a right-wing populist party, indicating that education's effect on political trust also operates through a "populism" effect. Interestingly, however, no statistically significant effect of the Dutch schooling reform, and thus increased educational attainment, on closeness to a left-wing populist party is found. The estimates in column (**3**) – showing that education decreases anti-immigrant sentiment – provides a possible explanation for this discrepancy.[15] It suggests that through a stronger aversion to immigrants the lesser-educated feel more closely to anti-establishment parties. Fierce opposition to immigration as a core ideological position is an important distinguishing characteristic between right- and left-wing populism, and if this particular position is the primary driver of what makes the lesser-educated feel more closely to anti-establishment parties, that would explain the absence of a significant effect of schooling on closeness to a left-wing populist party.

[14] The estimates in columns (**1**), (**2**), (**3**), and (**5**) are derived using fuzzy RD regression. On the other hand, the estimate in column (**4**) is derived using reduced form RD regression.

[15] Anti-immigrant sentiment is measured by a respondent's answer to the statement "immigrants make the country a worse or better place to live", where the answer options range from 1 to 10, the values of 1 and 10 meaning "worse place to live" and "better place to live", respectively. The variable capturing the responses to this statement is converted to binary indicator variables that are anchored around the country-specific median and lower-quartile values, where a score below these thresholds is coded as 1.

Table 5: Investigation of the channels of causality between education and political trust

	(**1**) Working in low-skilled job	(**2**) Ability to comprehend politics		(**3**) Anti-immigration attitude		(**4**) Feel closest to left-wing populist party	(**5**) Feel closest to right-wing populist party
		Median	*Lower-Q*	*Median*	*Lower-Q*	*Dutch data only*	
Years of schooling / Reform (for (**4**))	-0.100***	0.094**	0.070**	-0.135***	-0.075**	-0.004	-0.034**
	(0.038)	(0.038)	(0.034)	(0.048)	(0.032)	(0.025)	(0.017)
Bandwidth	5	5		5		5	5
Observations	7,125	4,435		6,881		2,184	7,216
1st stage F-stat	10.5	13.0		11.2		-	10.9

Notes: Controls included are: gender, age (both linear and squared terms), dummies indicating if the respondent lives in an urban area, belongs to an ethnic minority group in the currently residing country, is a citizen in the currently residing country, was born in the currently residing country, his/her parents were born in the (respondent's) currently residing country (1 dummy for each parent), his/her parents having completed (at least) upper secondary education (1 dummy for each parent). ESS round & country dummies are also included, as well as country-specific quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

8. Discussion: education as a shaper of attitudes but not behavior?

Clearly, having trust or distrust in national institutions and participating in activities to address (political and non-political) issues of public concern are two conceptually very different things. The fact that this thesis finds a robust positive effect of education for the former and no evidence of an effect for the latter can thus not be considered an odd outcome.

Still, one could interpret these findings as highlighting a striking dichotomy. On the one hand, education influences how favorable people are towards the institutions that govern, regulate, and enforce within society. On the other hand, education does not seem to affect if people undertake actions to change or improve the functioning of this same society.

Assuming that this dichotomy exists, what could explain it? This thesis argues that the most plausible explanation is highlighted by the "opportunity cost" effect (described in Section 3.3). This hypothesized causal channel proposes that education decreases civic engagement through increasing a person's opportunity cost of time. It could thus be that any positive effect that education has on civicness is counteracted by this increased opportunity cost, resulting in the positive and negative causal channels

cancelling each other out. This, together with the absence of an opportunity cost effect for having trust or distrust in national institutions, would explain the dichotomy.

Another explanation for this possible dichotomy, put forth by the seminal work of LaPiere (1934), is that there exists a general gap between attitudes and actions. Applying this view to the context of this thesis, a possible reason why education affects institutional trust and not civic engagement is because changing one's behavior requires substantially more commitment and effort than changing one's views.

However, concluding from this apparent dichotomy that education is only able to shape positive attitudes, but not foster societally beneficial behavior, would be a mistake. There are indeed many forms of societally beneficial behavior other than civic engagement that could be affected by education, both through its effect on institutional trust and via other ways. Through increased trust in political institutions, for instance, education arguably improves compliance with political decisions and government regulation (Hakhverdian & Mayne, 2012) (Marien & Hooghe, 2011). This thesis would also argue that education, through increasing political trust, could substantially improve the effectiveness of governmental campaigns such as those related to public health. Lastly, through increased trust in non-political institutions such as the legal system and the police, education also plausibly improves cooperation with these actors (Hough et al., 2013).

Thus, if one looks at these findings and on the basis of them posits that education only shapes attitudes but not behavior, the author of this thesis would say that, if anything, these findings should give the reader a sense of optimism about education's ability to instil in people a way of conduct that benefits society. Indeed, the finding that education is able to build institutional trust emphasizes the potential of schooling as a means of fostering a wide variety of societally beneficial behaviors, albeit not civic engagement.

9. Concluding remarks

This thesis presents robust evidence that education positively impacts one's trust in national institutions, both political – i.e., politicians, political parties, and a country's parliament – and non-political – the legal system and the police. Valid causal inference is achieved by comparing cohorts of survey respondents that were just young enough, or just too old, to be affected by one of 4 European schooling reforms which substantially raised educational attainment; both fuzzy and reduced form RD designs are used in the analysis. With regards to how institutional trust is affected, additional education seems to be able to prevent people from becoming very distrusting of a particular institution, and also shift them to the relatively trusting segment in their respective country.

Further investigating the underlying causal channels of education's impact on political trust, the results indicate that this effect operates through schooling sorting people into more privileged societal positions,

making them more politically sophisticated, and causing them to be less receptive to anti-establishment messaging of populist political parties.

The potential impact of education on how civically engaged people are is also studied. No evidence is found of a general causal link between the two. It has to be said, however, that this result could also be driven by the fact that the respondents over which an effect is estimated are quite old; an interesting avenue to explore in future research is thus to study if a causal link exists when people are younger, for example in their 20s or 30s. The result of no causal link could also be driven by the fact that the leveraged schooling reforms primarily shift secondary education, and additional schooling in this specific segment has no impact. However, it could be that additional schooling in other education segments has an impact; for example, it could be that tertiary education, taught, and occurring, in an arguably more activist environment, does affect civic engagement. For future research on the causal impact of education on civic engagement, it could thus be a fruitful endeavour to leverage phenomena that (primarily) shift post-secondary schooling.

Importantly, this paper's empirical findings suggest the existence of an important externality of education: a more stable society. This highlights the potential of raising educational attainment as a policy instrument to combat harmful polarization, a pressing issue in today's world.

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Appendix

A.1. The Dutch 1973-1975 schooling reform

The Dutch education system in the early 1970s was characterized by an early educational tracking system, where after 6 years of primary education, at the age of 12 years, pupils were tracked into secondary education programs by ability. These tracks can be divided in vocational programs and general programs, with a duration of 3-4 years and 3-6 years, respectively.

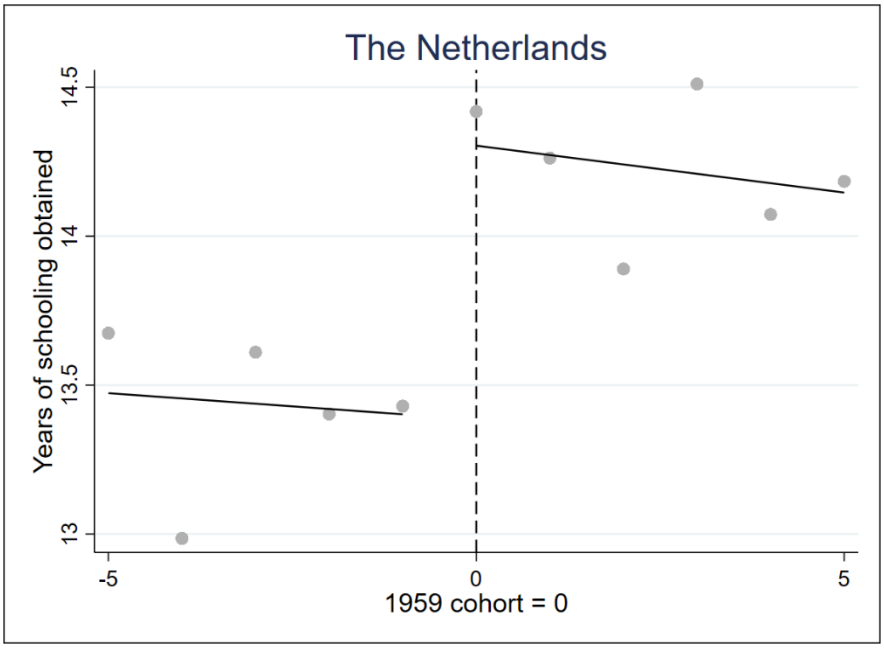
The first change implemented by the Dutch schooling reform was with regards to vocational programs, where from 1973 onwards all 3-year programs were extended to 4 years (Oosterbeek & Webbink, 2007). However, an exception was made for cohorts who started a basic vocational program in 1971: they could still graduate in 3 years if they wished so.

In 1975, a similar thing happened with 3-year general programs, where from that point on they all were extended to 4 years. This 1975 change also coincided with the compulsory school leaving age being raised from 15 to 16.

It is thus the case that the first cohort that was *forced* to stay longer in school because of the Dutch reform was the 1960 cohort that entered secondary education in 1972, having to complete a 4 year program instead of 3. Importantly, however, it is likely that a cohort earlier than the 1960 one was also affected by the Dutch reform. It is namely so that all 3-year basic vocational programs had already been extended to 4 years in 1973, thereby creating an incentive for the 1958 and 1959 cohorts to stay in secondary school for 4 years instead of 3.

Figure A1, showing the average years of total schooling obtained by Dutch cohorts, investigates this possibility. And indeed, as the figure shows, there is strong evidence of a discontinuity at the 1959 cohort, indicating that this group was also substantially shifted in its educational attainment by the Dutch reform. Because of this, this thesis treats the 1959 Dutch cohort as the first one affected by the Dutch 1973-1975 schooling reform. Importantly, this decision is very much consistent with the previous RD literature, as all the academic research (known to the author of this thesis) leveraging the Dutch reform also treats the 1959 cohort as the cut-off point (Brunello et al., 2009) (Cavaille & Marshall, 2019) (Kunst et al., 2019).

Figure A1: Average educational attainment for Dutch cohorts (year-of-birth 1954-1964)



Note: The average years of schooling obtained for the cohorts either side of the cut-off are fitted with a linear polynomial.

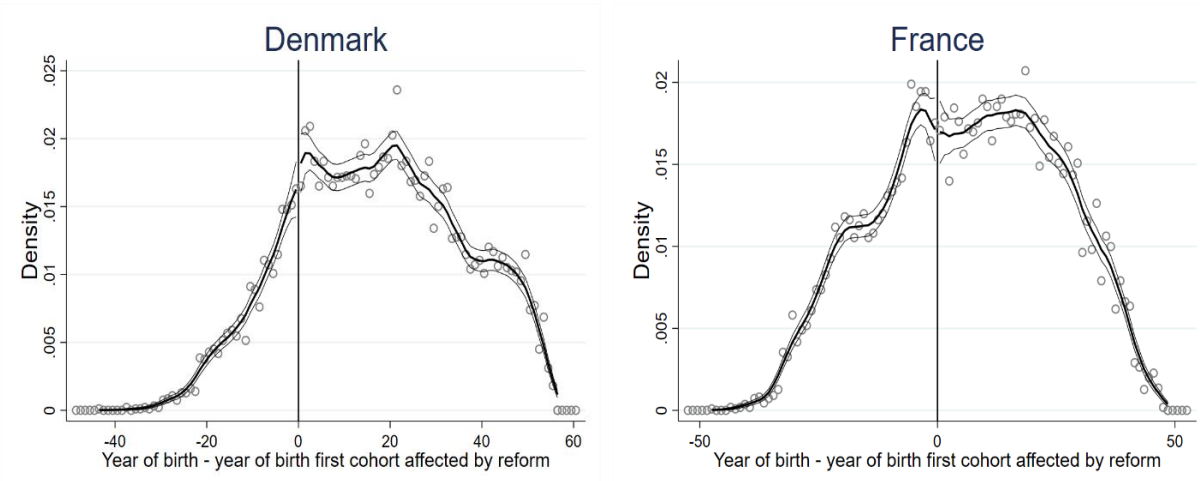
A.2. Testing for discontinuity in density at the cut-off

This section examines if there is a change in density at the cut-off point *x = year of birth – year of birth first cohort affected by reform = 0*. More specifically, two types of density tests are performed: the one by McCrary (2008), and the one by Frandsen (2017). The primary reason for performing these tests is to investigate the possibility that cohorts on one side of the cut-off were systematically more likely to participate in the ESS.

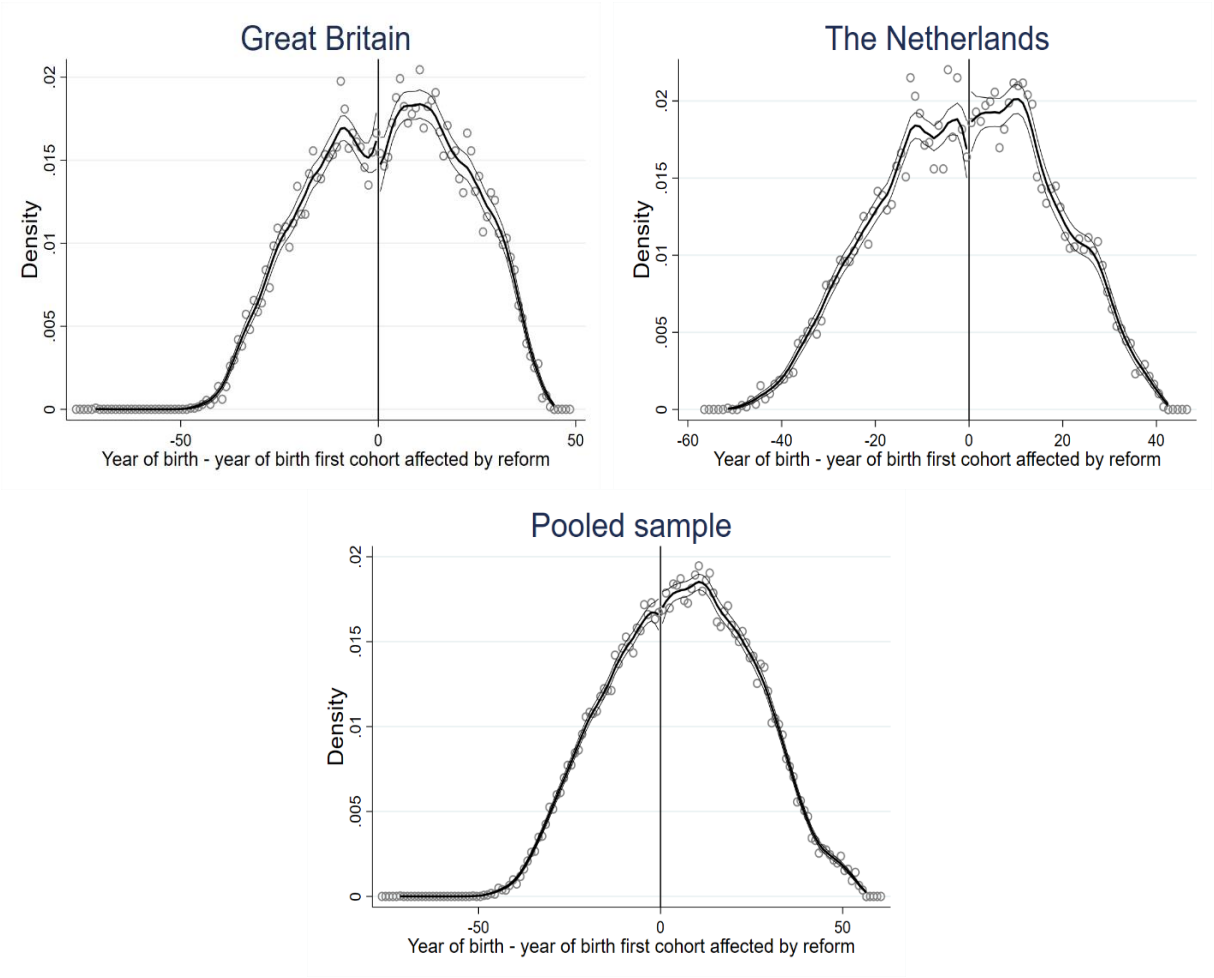
A.2.1. Density tests

The McCrary density graphs for the individual, and pooled, country samples are displayed below. As the graphs show, for all the samples, the number of respondents varies smoothly across the cut-off point *x = 0*. Furthermore, for each sample, the McCrary test fails to reject the null hypothesis that the density does not change at the cut-off point.[16]

Figure A2: McCrary density graphs



[16] The used bandwidth for the McCrary tests is 5.



Additionally, for robustness, Frandsen's (2017) test for this same null hypothesis is performed for all samples; an arguably superior test, since it was developed for the case of a discrete running variable like the one in this thesis. This test, as Table A1 shows, for each sample, also fails to reject the null hypothesis of no change in density at the cut-off.

Table A1: Results Frandsen's (2017) density test

Sample	Statistical significance (with $k = 0$)	Statistical significance (with $k = 0.1$)
Denmark	p = 0.274	p = 0.524
France	p = 0.716	p = 0.871
Great Britain	p = 0.901	p = 0.966
The Netherlands	p = 0.581	p = 0.800
Pooled sample	p = 0.593	p = 0.970

Notes: column 2 displays the results of Frandsen's (2017) density test with bound coefficient k = 0, while column 3 displays the results of this test with k = 0.1. According to Frandsen (2017), using a bound coefficient of k = 0 yields the most powerful test.

A.3. Balancing test

To check if unaffected and affected year-of-birth cohorts around the schooling reforms are statistically indistinguishable (once part of the main regression specification is imposed) along a number of predetermined characteristics in the estimation sample, a balancing test is conducted. More specifically, the following (binary) characteristics are analyzed: being female; the respondent belonging to an ethnic minority group in the country he/she currently resides; the respondent's father being born in the country where the respondent currently resides; the respondent's mother being born in the country where the respondent currently resides; the respondent's father having completed (at least) upper secondary education; the respondent's mother having completed (at least) upper secondary education.

Table A2 displays the results. The used bandwidth in all the respective regressions is 5 – the maximum bandwidth used in the main analysis.

With regards to two of the analyzed predetermined characteristics, there is evidence that unaffected and affected respondents statistically significantly differ. However, it should be noted that these imbalances are somewhat minor. To account for these imbalances, this thesis controls for these predetermined characteristics in the main regressions.

Table A2: Results balancing test

	(**1**) Female	(**2**) Ethnic minority	(**3**) Father born in country	(**4**) Mother born in country	(**5**) Father completed secondary education	(**6**) Mother completed secondary education
Reform	-0.029	0.021**	-0.013	0.003	0.043**	0.026
	(0.024)	(0.010)	(0.015)	(0.015)	(0.022)	(0.018)
Bandwidth	5	5	5	5	5	5
Observations	7,294	7,294	7,294	7,294	7,294	7,294

Notes: Controls included are: age (both linear and squared terms) and a dummy variable indicating if the respondent lives in an urban area. ESS round & country dummies are also included, as well as country-specific quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

A.4. The impact of the schooling reforms on the attainment of tertiary education

Table A3: The impact of the schooling reforms on the attainment of tertiary education

	(1) Denmark	**(2) France**	**(3) Great Britain**	**(4) The Netherlands**	**(5) Pooled sample**
Dependent variable: completed tertiary education					
Reform	0.021	0.078*	0.029	0.023	0.036*
	(0.042)	(0.041)	(0.054)	(0.038)	(0.022)
Bandwidth	5	5	5	5	5
Observations	1,613	1,252	808	2,204	5,877

Notes: Controls included are: gender, age (both linear and squared terms), dummies indicating if the respondent lives in an urban area, belongs to an ethnic minority group in the currently residing country, is a citizen in the currently residing country, was born in the currently residing country, his/her parents were born in the (respondent's) currently residing country (1 dummy for each parent), his/her parents having completed (at least) upper secondary education (1 dummy for each parent). ESS round and (for the pooled sample) country dummies are also included, as well as (country-specific) quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

A.5. Reduced form RD results

Table A4: Reduced form RD results for trust in national institutions (median specification)

	(1) Trust in politicians	(2) Trust in political parties	(3) Trust in the national parliament	(4) Trust in the legal system	(5) Trust in the police
Denmark					
Reform	0.015	0.009	0.033	-0.033	0.041
	(0.049)	(0.060)	(0.054)	(0.049)	(0.047)
SRV median (*1-10 scale*)	5	5	6	8	8
Observations	1,608	1,117	1,349	1,594	1,610
France					
Reform	0.060	0.078	0.112**	-0.005	0.052
	(0.049)	(0.053)	(0.050)	(0.048)	(0.049)
SRV median (*1-10 scale*)	3	3	5	5	6
Observations	1,756	1,513	1,425	1,755	1,756
Great Britain					
Reform	0.019	0.042	0.062	0.093**	0.048
	(0.048)	(0.046)	(0.053)	(0.047)	(0.048)
SRV median (*1-10 scale*)	3	4	4	5	7
Observations	1,734	1,728	1,414	1,736	1,741
The Netherlands					
Reform	0.062	0.037	0.044	0.106**	0.064
	(0.043)	(0.050)	(0.043)	(0.041)	(0.043)
SRV median (*1-10 scale*)	5	5	6	6	7
Observations	2,196	1,710	1,768	2,196	2,200
Pooled sample					
Reform	0.039*	0.041	0.056**	0.044*	0.051**
	(0.024)	(0.025)	(0.025)	(0.023)	(0.023)
Observations	7,294	6,068	5,956	7,281	7,307

Notes: Controls included are: gender, age (both linear and squared terms), dummies indicating if the respondent lives in an urban area, belongs to an ethnic minority group in the currently residing country, is a citizen in the currently residing country, was born in the currently residing country, his/her parents were born in the (respondent's) currently residing country (1 dummy for each parent), his/her parents having completed (at least) upper secondary education (1 dummy for each parent). ESS round and (for the pooled sample) country dummies are also included, as well as (country-specific) quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Table A5: Reduced form RD results for trust in national institutions (lower-Q specification)

	(**1**) Trust in politicians	(**2**) Trust in political parties	(**3**) Trust in the national parliament	(**4**) Trust in the legal system	(**5**) Trust in the police
Denmark					
Reform	-0.013	0.021	0.055	0.044	0.041
	(0.046)	(0.056)	(0.054)	(0.039)	(0.038)
SRV lower-Q (*1-10 scale*)	4	4	5	6	7
Observations	1,608	1,117	1,349	1,594	1,610
France					
Reform	0.078	0.115**	0.070	-0.007	0.056
	(0.048)	(0.052)	(0.052)	(0.038)	(0.042)
SRV lower-Q (*1-10 scale*)	2	2	3	3	5
Observations	1,756	1,513	1,425	1,755	1,756
Great Britain					
Reform	0.061	0.077	0.073	0.068	0.034
	(0.047)	(0.047)	(0.049)	(0.043)	(0.040)
SRV lower-Q (*1-10 scale*)	2	2	2	4	5
Observations	1,734	1,728	1,414	1,736	1,741
The Netherlands					
Reform	0.065	0.069	-0.015	0.029	0.034
	(0.041)	(0.047)	(0.045)	(0.037)	(0.031)
SRV lower-Q (*1-10 scale*)	4	4	4	5	5
Observations	2,196	1,710	1,768	2,196	2,200
Pooled sample					
Reform	0.045**	0.072***	0.035	0.033*	0.039**

	(0.023)	(0.025)	(0.025)	(0.020)	(0.019)
Observations	7,294	6,068	5,956	7,281	7,307

Notes: Controls included are: gender, age (both linear and squared terms), dummies indicating if the respondent lives in an urban area, belongs to an ethnic minority group in the currently residing country, is a citizen in the currently residing country, was born in the currently residing country, his/her parents were born in the (respondent's) currently residing country (1 dummy for each parent), his/her parents having completed (at least) upper secondary education (1 dummy for each parent). ESS round and (for the pooled sample) country dummies are also included, as well as (country-specific) quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

Table A6: Reduced form RD results for the civic engagement types

	(**1**) Voting	(**2**) Contacted politician or gov't official	(**3**) Worked in a political party or action group	(**4**) Worked in other civic organisation	(**5**) Signed a petition	(**6**) Taken part in a demonstration	(**7**) Boycotting
Denmark							
Reform	0.020	-0.024	0.013	0.011	-0.035	-0.009	-0.042
	(0.012)	(0.039)	(0.026)	(0.045)	(0.042)	(0.019)	(0.043)
Bandwidth	5	5	5	5	5	5	5
Observations	1,587	1,613	1,613	1,613	1,607	1,613	1,609
France							
Reform	0.007	0.009	0.013	-0.019	-0.019	0.026	0.027
	(0.036)	(0.039)	(0.023)	(0.039)	(0.047)	(0.038)	(0.047)
Bandwidth	5	5	5	5	5	5	5
Observations	1,677	1,760	1,758	1,760	1,755	1,760	1,758
Great Britain							
Reform	0.018	0.020	-0.025	-0.035	-0.027	-0.037*	-0.007
	(0.042)	(0.039)	(0.016)	(0.029)	(0.047)	(0.020)	(0.043)
Bandwidth	5	5	5	5	5	5	5
Observations	1,726	1,749	1,750	1,746	1,744	1,750	1,744
The Netherlands							
Reform	0.020	-0.031	0.015	0.028	-0.037	-0.019	-0.010
	(0.029)	(0.033)	(0.016)	(0.039)	(0.039)	(0.019)	(0.031)

Bandwidth	5	5	5	5	5	5	5
Observations	2,170	2,202	2,203	2,204	2,200	2,201	2,199
Pooled sample							
Reform	0.020	-0.009	0.004	-0.003	-0.031	-0.011	-0.007
	(0.016)	(0.019)	(0.010)	(0.019)	(0.022)	(0.012)	(0.020)
Bandwidth	5	5	5	5	5	5	5
Observations	7,160	7,324	7,324	7,323	7,306	7,324	7,310

Notes: Controls included are: gender, age (both linear and squared terms), dummies indicating if the respondent lives in an urban area, belongs to an ethnic minority group in the currently residing country, is a citizen in the currently residing country, was born in the currently residing country, his/her parents were born in the (respondent's) currently residing country (1 dummy for each parent), his/her parents having completed (at least) upper secondary education (1 dummy for each parent). ESS round and (for the pooled sample) country dummies are also included, as well as (country-specific) quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

A.6. Robustness main findings to including the 4 excluded country-round subsamples

Table A7: Reduced form RD results for trust in national institutions (with the estimation sample including the previously excluded 4 country-round subsamples)

	(**1**) Trust in politicians		(**2**) Trust in political parties		(**3**) Trust in national parliament		(**4**) Trust in the legal system		(**5**) Trust in the police	
	Median	*Lower-Q*	*Median*	*Lower-Q*	*Median*	*Lower-Q*	*Median*	*Lower-Q*	*Median*	*Lower-Q*
Reform	0.041*	0.044**	0.052**	0.068***	0.054**	0.043*	0.041*	0.035**	0.038*	0.037**
	(0.021)	(0.021)	(0.024)	(0.023)	(0.022)	(0.023)	(0.021)	(0.018)	(0.021)	(0.017)
Bandwidth	5		5 (4 for Denmark)		4		5		5	
Observations	8,525		6,979		6,956		8,511		8,539	

Notes: Controls included are: gender, age (both linear and squared terms), dummies indicating if the respondent lives in an urban area, belongs to an ethnic minority group in the currently residing country, is a citizen in the currently residing country, was born in the currently residing country, his/her parents were born in the (respondent's) currently residing country (1 dummy for each parent), his/her parents having completed (at least) upper secondary education (1 dummy for each parent). ESS round & country dummies are also included, as well as country-specific quadratic birth cohort trends. Robust standard errors are in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, * p < 0.1

A.7. List of populist political parties

Table A8 lists the populist political parties active over the estimation period in the countries of interest.[17] These parties are identified using Van Kessel (2015) and *The Populist* (Rooduijn et al., 2019), the latter being an overview constructed by academics of populist, far right, far left, and Eurosceptic parties in Europe since 1989; *The Populist* is supported by the Amsterdam Institute for Social Science Research, the Amsterdam Centre for European Studies, and the ECPR Standing Group on Extremism and Democracy.

Table A8: Populist parties active over the estimation period

Country	Party	Left-right on the political spectrum
Denmark	Dansk Folkeparti (DF)	Far-right
Denmark	Fremskridtpartiet	Right
France	Front National (FR)	Far-right
Great Britain	British National Party (BNP)	Far-right
Great Britain	UK Independence Party (UKIP)	Far-right
The Netherlands	Leefbaar Nederland (LV)	Right
The Netherlands	Lijst Pim Fortuyn (LPV)	Right
The Netherlands	Partij voor de Vrijheid (PVV)	Far-right
The Netherlands	Socialistische Partij (SP)	Far-left

[17] Aside from those listed, this thesis identified two other populist parties: Debout la république/Debout la France, a far-right party in France; Respect – The Unity Coalition, a far-left party in Great Britain. These political parties have been excluded from the list, however, because they were not asked about in the ESS.