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Does education decrease Euroscepticism? A regression discontinuity design using compulsory schooling reforms in four European countries

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Abstract

Previous research shows a strong and consistent relationship between educational attainment and Euroscepticism. As a result, education is considered to be a powerful predictor of attitudes towards European integration. However, these findings are predominantly found using cross-sectional research designs, therefore leaving open the possibility of strong selection effects due to pre-adult experiences and dispositions which both explain educational attainment and political attitudes. To test whether schooling causally reduces Euroscepticism, this article combines data on the compulsory schooling age with seven rounds of pooled European Social Survey data

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(2002–2014). Using compulsory schooling reforms within a 'fuzzy' regression discontinuity design, the results indicate no conclusive effect of education on Euroscepticism, questioning the impact of additional schooling. Consequently, this study provides a novel insight into the much-debated divide in support for European integration between lower and higher educated.

Introduction

On 23 June 2016, in a historic ballot about the future of the United Kingdom in the European Union (EU), a small majority of 51.9% voted to leave the EU. Analyses of the Brexit referendum reveal that the outcome was strongly polarised along educational lines: a significant portion of those with a lower level of education voted to leave the EU, while citizens with the highest educational credentials voted predominantly to remain (Hobolt, 2016). This divide with regards to Euroscepticism is not only visible in the United Kingdom. Across Europe, those with less education are consistently found to be more Eurosceptic than those with higher education (Hakhverdian et al., 2013; Hooghe and Marks, 2005; Lubbers and Scheepers, 2010). Moreover, this gap has also significantly widened over time (Hakhverdian et al., 2013; Lubbers and Jaspers, 2011).

Within the literature on Euroscepticism, a number of theories are put forward which (causally) link education with support for European integration (for an overview, see Hakhverdian et al., 2013). The literature on Euroscepticism argues that the higher educated are less Eurosceptic because they have acquired cognitive skills, have been socialised longer into cosmopolitan values at school, fostering an open and cosmopolitan outlook on society, and face less competition and insecurity on the international labour market. Whatever the mechanism might entail, all these explanations share the implicit assumption that more education leads to a change in attitudes towards the EU.

The association between education and Euroscepticism is most often estimated within a cross-sectional research design. This is problematic, however, since it is possible that the relationship between education and political attitudes, such as Euroscepticism, is confounded by other factors that explain both the cause and the consequence (Persson, 2015). If so, education could potentially be a proxy for factors such as family's socio-economic status, the political socialisation at home and individual differences in cognitive ability and personality traits, rather than a cause of political outcomes, such as Euroscepticism (Kam and Palmer, 2008; Persson et al., 2016). Therefore, although it is evident that there is a strong association between education and Euroscepticism, whether we can also speak of a causal relation is still unclear (Hooghe and Marks, 2018).

This article moves beyond the well-established association between education and Euroscepticism and tests the implicit assumption that more years of schooling lead to a change in attitudes towards Europe. To overcome the above-described problem of endogeneity, this study uses a quasi-experimental 'fuzzy' regression discontinuity (RD) design in combination with five compulsory schooling reforms in four European countries which raised the minimum secondary school leaving age. By combining compulsory schooling reforms as an exogenous instrument with seven rounds of pooled data from the European Social Survey (ESS) (2002–2014), it is possible to observe whether additional years of education result in a decline in Euroscepticism. This specific set-up has already proven to be successful in the studies by d'Hombres and Nunziata (2016) and Cavaillé and Marshall (2019) who find that additional years of schooling reduce hostility towards immigrants. On the other hand, using a compulsory schooling reform in Norway, Finseraas et al. (2018) find no effect of additional years of education for anti-immigrant sentiments.

The results of this examination indicate no conclusive effect of additional years of schooling for both our measures of Euroscepticism. Consequently, our results challenge the predominant view that education in itself has an impact on Euroscepticism. Contrary to the robust effects this particular set-up yields for anti-immigration attitudes (Cavaillé and Marshall, 2019; d'Hombres and Nunziata, 2016), Euroscepticism is not significantly affected by an increase in years of schooling. Therefore, this article calls for more research on the mechanisms linking education to Euroscepticism.

Theoretical framework

Education and Euroscepticism

Explaining Euroscepticism has already received much scholarly attention (for an overview, see Hobolt and de Vries, 2016). One of the most consistent findings within this literature is that Euroscepticism is stratified along educational lines (Hakhverdian et al., 2013). More specifically, lower educated are systematically found to be more Eurosceptic than higher educated. By using the Mannheim Eurobarometer trend file (1970–2002) and appending it with the most recent waves of the Eurobarometer, Figure 1 shows the percentage of Eurosceptics among the lower, middle and higher educated in 12 European countries between 1973 and 2018 (Schmitt et al., 2008). In this case, Euroscepticism is measured by asking respondents whether they believe their country's membership of the EU is good, bad or neither good nor bad. Lubbers and Scheepers (2005) label this as instrumental Euroscepticism. As shown in Figure 1, already since the beginning of the 1970s, lower educated are consistently more Eurosceptic than higher educated citizens. Moreover, as the trend lines illustrate, the existing differences between lower and higher educated for Euroscepticism have only been further exacerbated over time (Hakhverdian et al., 2013).

The divide in Euroscepticism between lower and higher educated ties into a wider, education-based divide in Western Europe (Bovens and Wille, 2017;
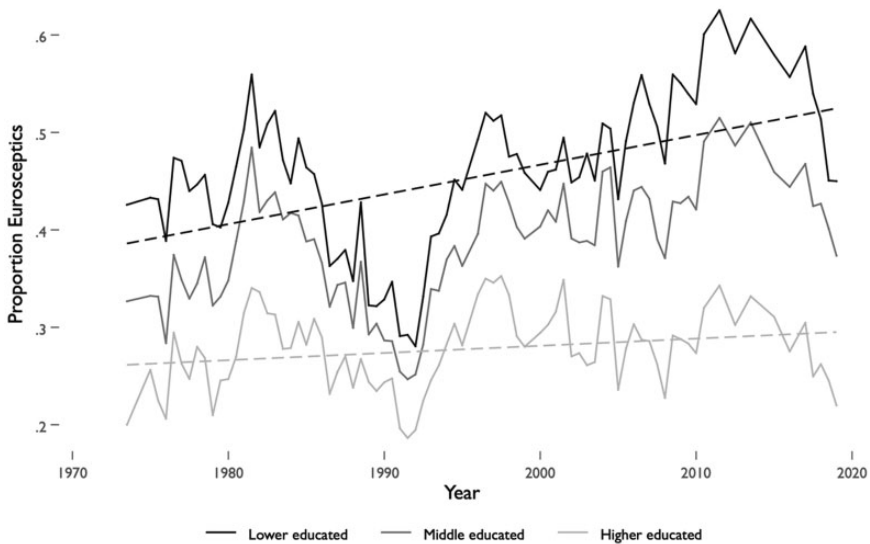
Figure 1. Trend in Euroscepticism by education level between 1973 and 2018.
Note: Euroscepticism measured as whether respondent believes country's membership of EU is bad or neither good nor bad (1) versus good (0). Data consist of the Mannheim Trend File (1970–2002), plus Eurobarometer waves 58.1, 59.1, 60.1, 61.0, 62.0, 62.2, 63.4, 64.2, 65.1, 65.2, 66.1, 67.2, 68.1, 69.2, 70.1, 71.1, 71.3, 72.4, 73.4, 75.3, 77.4, 79.5, 82.4, 84.1, 86.1, 87.1, 88.1, 89.2 and 90.1. Countries included: France, Belgium, The Netherlands, Germany, Italy, Luxembourg, Denmark, Ireland, Great Britain, Greece, Spain and Portugal.

Hooghe and Marks, 2018). Globalisation is argued to polarise societies into groups of 'winners' and 'losers' who have diametrical attitudes towards opening national borders to increase political, economic and cultural interactions (Kriesi et al., 2008). In general, the 'losers' are considered to be those with less education, who feel strongly attached to the nation-state and see their security and social status reduced. In contrast, the 'winners' are the well-educated, who hold cosmopolitan values and see benefits in the opportunities offered by globalisation. In the literature, a variety of concepts is used to name this divide: Green/Alternative/Libertarian versus Tradition/Authority/National (Hooghe and Marks, 2018), cosmopolitans versus communitarians (Teney et al., 2014), cosmopolitan versus parochial (De Vries, 2017) or demarcation versus integration (Kriesi et al., 2006, 2008). The common denominator among these definitions is the observation that higher and lower educated are strongly polarised on issues related to immigration and European integration (Van der Brug and Van Spanje, 2009).

The association between education and Euroscepticism. Although it is well established that the lower and higher educated differ significantly in their views on the EU, can we say that this association also implies causality? Put differently, is education a

causal force that brings about change to people's views on European integration? Based on the current state of the literature on Euroscepticism, we would expect this to be the case. Research on the sources of Euroscepticism usually lists three mechanisms through which additional education affects attitudes towards the EU: improved cognitive skills, the socialisation into cosmopolitan values taught at school and utilitarian cost-benefit calculations (Hakhverdian et al., 2013; Hooghe and Marks, 2005; Lubbers and Jaspers, 2011).

The first two mechanisms point to the idea that education fosters a cosmopolitan outlook on the world, which results in more support for further European integration. Previous work demonstrates that cultural intolerance and feelings of an exclusive national identity are strong predictors of Euroscepticism (De Vreese and Boomgaarden, 2005; Hooghe and Marks, 2005; Luedtke, 2005; McLaren, 2002). Additional schooling is linked with ascribing to more cosmopolitan attitudes and values. People with more years of education are consistently found to be more open and accepting of people with different ethnic and cultural backgrounds, and they hold a less exclusive national identity. The cognitive skills argument posits that education has a strong 'liberalizing' effect on students by improving their cognitive abilities and broadening their horizon (Bobo and Licari, 1989; Hyman and Wright, 1979; Jackman, 1978). These improved cognitive skills are linked with endorsing more cosmopolitan political attitudes, thus reducing nationalist sentiments and cultural intolerance.

Related to this argument, education is argued to not only 'enlighten' students but also socialise them into a set of political values (Van de Werfhorst and De Graaf, 2004). Accordingly, education brings about an inner cultural shift towards cosmopolitan values. In school, students are taught ideas such as pluralism, democracy and individual freedom which help foster a more open and tolerant outlook on the world. Moreover, since the 1970s, there has been an increasing emphasis on promoting European values and more recently even European citizenship (Busemeyer and Trampusch, 2011; Keating, 2009).

Besides enhancing cognitive skills and socialising students into a specific set of values, education also provides them with a stronger position on the labour market. The utilitarian cost-benefit calculations argument points to Euroscepticism as a consequence of self-interest, or more specifically, whether people profit (economically) from European integration or not (Anderson and Reichert, 1995; Gabel, 1998; Gabel and Palmer, 1995). Due to their skills and mobility, higher educated are more supportive of European integration, because they reap the benefits of more cooperation on the European level. In sum, we would expect additional years of education to reduce Euroscepticism if (a) cognitive skills are successfully improved, (b) cosmopolitan values are being transmitted or (c) labour market position is strengthened.

Disputing the causal link? Although previous studies have demonstrated that there is a strong association between education and Euroscepticism, and different mechanisms have been proposed, the predominant use of cross-sectional research designs makes it hard to immediately equate association with causation. It is possible that

the relationship between education and political attitudes, such as Euroscepticism, is confounded by other factors that explain both the cause and consequence (Persson, 2015). Consequently, rather than a cause of Euroscepticism, education could turn out to be a proxy for other factors such as cognitive ability, political socialisation at home and the family's socio-economic status. Since such factors are often missing in analyses, previous studies have most likely overestimated the effect of education on Euroscepticism.

This view of education-as-proxy is already influential in the literature on political participation (Persson, 2015). Previous work on the link between education and political acts, such as voting or joining a political party, finds that once using more sophisticated statistical or quasi-experimental methods to control for omitted variable bias, the relationship between the two variables disappears (Kam and Palmer, 2008; Persson et al., 2016). These methods are also increasingly used in the literature on the link between education and anti-immigration attitudes, with mixed results. Although d'Hombres and Nunziata (2016) and Cavaillé and Marshall (2019) show that additional years of schooling causally reduce hostile feelings towards immigrants, Lancee and Sarassin (2015) and Finseraas et al. (2018) find no such effect of additional schooling.

Also, in the case of Euroscepticism, there is evidence that pre-adult factors are important in explaining differences in Euroscepticism. For example, Kuhn et al. (2018) find that students in Switzerland do not change in their Euroscepticism as they pass through education. They find that especially parental socialisation is important for youngsters' opinion towards the EU. Furthermore, also in the case of Brexit, Fox et al. (2019) identify the family as a key source of the values that shape support for European integration, providing evidence of intergenerational transmission of hostility towards the EU from parent to child. Therefore, it is possible that the effect of education on the level of Euroscepticism is overestimated. Thus, to move beyond estimating associations and to approximate causality, we use a quasi-experimental 'fuzzy' RD design. The next section provides a more detailed description of this method, the data used and the operationalisation of variables.

Data, method and operationalisation

Method

In this article, we make use of compulsory schooling reforms within a RD design. Compulsory schooling reforms are laws which change the age up to which students are obliged to stay in secondary education. The idea of the design is that the probability of receiving the treatment (additional years of schooling) is a discontinuous function of the running variable (year of birth) (Lee and Lemieux, 2010). This allows us to estimate the causal effect of schooling by comparing the dependent variable (Euroscepticism) for birth cohorts just below and just above the treatment cut-off. The key identifying assumption of the RD design is that

Euroscepticism is continuous across cohorts in all other variables except years of schooling (Hahn et al., 2001). This means that (a) no discontinuities are visible in other variables at the cut-off except for years of schooling, and (b) students cannot 'sort' themselves into the treatment group by manipulating their year of birth.

In this case, we make use of a 'fuzzy' RD design. Within a 'fuzzy' set-up, the probability of staying longer in school changes at the cut-off but does not involve a 0–1 change. We list the birth cohorts which were first affected by the reforms, as they receive an incentive to stay in secondary school (see the Online appendix). The treatment in our case is not deterministically assigned on the basis of year of birth, since these reforms could not force people to stay in school. Obviously prior to the reforms, some individuals left school at an age older than the reform required, but also after the reforms, still some individuals left school prematurely. Moreover, since we assign treatment on the basis of year of birth[1] and not month of birth, it is uncertain whether everyone in the specific first-affected birth cohorts also experienced the reforms. It is therefore necessary to utilise a 'fuzzy' RD design, because only the probability of receiving an additional year of education changes for those specific birth cohorts on the cut-off and later (Lee and Lemieux, 2010).

Using the 'fuzzy' set-up, we can estimate the treatment effect, conditional on the running variable equaling the cut-off and only for compliers, i.e. students who are induced to remain longer in school due to the reforms (Angrist and Imbens, 1995; Lee and Lemieux, 2010). This is called the local average treatment effect (LATE). To estimate the LATE, we need not only adhere to the key RD assumption laid out above but also to the assumption of monotonicity (the reforms do not lead some students to shorten their education, i.e. no defiers) and the exclusion restriction (the reforms only have an effect on Euroscepticism through years of schooling) (Angrist and Imbens, 1995; Lee and Lemieux, 2010).

Our analytical strategy is as follows: we first test which reforms actually have the intended effect of raising the average years of education. We then pool the effective reforms together and discuss the assumption of continuity at the cut-off, monotonicity and the exclusion restriction. Finally, we use the pooled sample to estimate 'fuzzy' RDs to observe the effect of an additional year of education for Euroscepticism for compliers on the cut-off (Lee and Lemieux, 2010). As a result, the generalisability of our findings is limited. Thus, we have to be careful not to overinterpret our results as we can only generalise them to a small subsample. This being said, the 'fuzzy' set-up allows us to move a step beyond estimating association between education and Euroscepticism.

Data

We use seven rounds of pooled data from the ESS between 2002 and 2014. The ESS is a high-quality biennial cross-national survey of attitudes and behaviour, using probability samples of individuals of 15 years and older which are representative of the population in each participating country. For each survey

round, around 1000 respondents per participating country are surveyed. In this study, the focus is on the western member states of the EU. We limit our sample to respondents who are older than 30 and were born after 1915. As noted above, we use the ESS in combination with compulsory schooling reform data, which is obtained from previous studies which use these reforms as well to estimate the effects of additional education (Cavaillé and Marshall, 2019).[2]

Measurements

The main dependent variable is Euroscepticism. Most studies traditionally operationalise Euroscepticism by using a single-item measure. For example, this entails asking respondents about whether they believe their countries' membership of the EU is a good thing, or whether they believe European integration should go further or not. However, studies show that Euroscepticism is not a single-, but a multidimensional construct (Boomgaarden et al., 2011; Lubbers and Scheepers, 2005; Stoeckel, 2013).[3] To take this into account, we use two different measures of Euroscepticism. First, we look at people's stance towards European integration by asking whether respondents believe that European integration has gone too far ($=0$) or should go further ($=10$). In the literature, this is argued to tap into people's policy evaluations of the EU, i.e. how people assess the content of collective decisions made on the European level (De Vries, 2018). Second, we look at people's trust in the European Parliament (EP) by asking respondents how much trust they have in the EP, running from no trust at all ($=0$) to complete trust ($=10$). This is argued to tap into people's regime evaluations of the EU, i.e. how people assess the way in which democratic procedures and rules operate and work out on the European level (De Vries, 2018). To ensure comparability, both dependent variables are standardised.

Empirical analysis

Which reforms were effective in raising years of schooling? We first determine which reforms have the intended effect of raising the average years of schooling to rule out the possibility that our results might be spurious (Cavaillé and Marshall, 2019). It is possible that some of these reforms merely reaffirm the status quo, considering that students already stayed longer in school beyond the compulsory schooling age in some contexts. In such cases, the change in the law does not impact their years of education. In total, we list 13 different compulsory schooling reforms in 12 countries (two reforms in Great Britain). The earliest took place in 1947 in Great Britain, while the most recent was in Belgium in 1983. The indicator for whether a respondent is in the treatment or control group is our treatment variable (T):

$T = 1$ if *Year of Birth – Year of Birth First Cohort Affected* ≥ 0
$T = 0$ if *Year of Birth – Year of Birth First Cohort Affected* < 0

We estimate the impact of the reforms for years of education with the following equation

$$S_{ijc} = \eta + \beta T_{jc} + f(R_{jc}) + \varepsilon_{ijc} \tag{1}$$

where the subscripts i, j and c refer to individuals, cohorts and country-reform. S_{ijc} denotes the dependent variable, namely years of schooling (with an upper limit of 13 years).[4] T_{jc} is a binary indicator that takes on value 1 if the cohort is exposed to a compulsory schooling reform. Finally, $f(R_{jc})$ denotes a first-order polynomial function of the running variable (R), which is specified here as: *Year of birth – Year of birth first affected*. This ensures that we control for cohort trends around the cut-off. In addition, we use an optimal bandwidth and a triangular kernel (Calonico et al., 2017).

For five reforms in four countries, we find a significant increase ($p < 0.1$): Great Britain (1947, 1972), the Netherlands (1974), Denmark (1958) and Sweden (1965). We find that especially in Denmark and Great Britain (1947), the reforms have a far-reaching impact on years of schooling with around 0.5 years. For the other reforms, the effects are somewhat smaller (around 0.3 years). Once we pool the five reforms together and include country-reform dummies, the results indicate that years of education significantly ($p < 0.00$) increase by around 0.3 years in our sample due to the compulsory schooling reforms. This result is robust for different bandwidths (see the Online appendix). Figure 2 presents the trend of 10 cohorts before and after the cut-off, indicating a clear discontinuity at the cut-off (see the Online appendix for full results).[5]

To use these effective reforms and to estimate the LATE of an additional year of education for Euroscepticism in a 'fuzzy' RD set-up, we need to meet the assumptions of continuity at the cut-off, monotonicity and the exclusion restriction.[6] First, we show that the continuity assumption is plausible by showing that (a) the cohorts just below and above the cut-off are highly similar on key background variables and (b) with the help of a McCrary density graph that no 'sorting' is taking place around the cut-off (both tests are reported in the Online appendix).

Second, the assumption of monotonicity entails in this case that the increase of the compulsory schooling age should induce some of the individuals exposed by the reforms to stay longer in school, but it should simultaneously not prompt anyone to shorten their education, i.e. there are no defiers (Angrist and Imbens, 1995; Lee and Lemieux, 2010). This is important because if the reforms increased the years of education for some, and decreased it for others, our estimation would be meaningless. The assumption of monotonicity is plausible in this case, as there are no logical reasons for why being exposed to a reform, that lifts the compulsory schooling age upwards, would lead to a decrease in years of education.

Third, the exclusion restriction entails that the instrument only has an effect on the outcome variable through our main independent variable (Angrist and Imbens, 1995; Lee and Lemieux, 2010). If this is not the case, then our estimates are likely to be biased. Since compulsory schooling reforms are so aligned with the years of schooling, we believe that it is unlikely to expect the instrument to work through
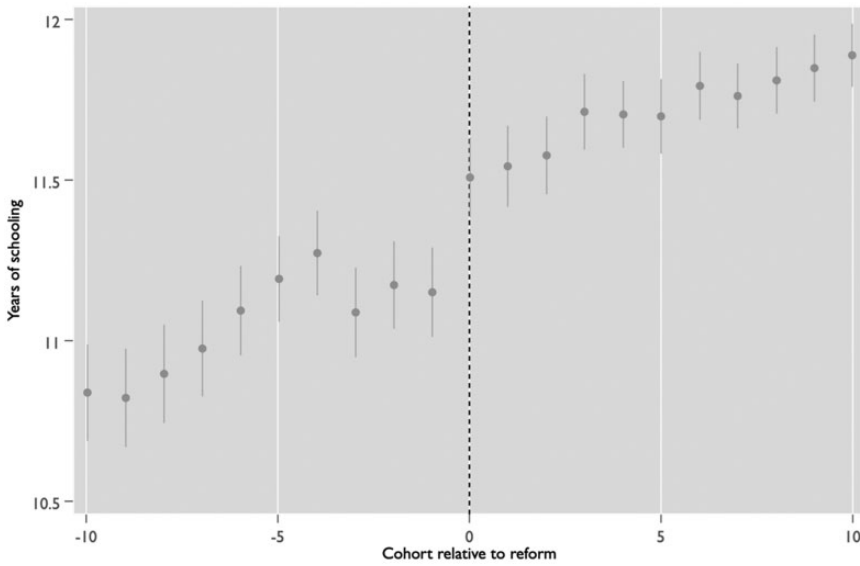
Figure 2. Effect of compulsory schooling reforms on years of schooling.
Note: Pooled from reforms in Great Britain (two), the Netherlands, Sweden and Denmark.
The bars display 95% confidence intervals.

another mechanism. Moreover, we believe that external events, such as the entrance of a country into the European Economic Community, or broad shifts in the political climate which coincide with the reforms and possibly affect EU-attitudes, can be discarded, since it is unlikely to expect that these events affect cohorts just below and just above the cut-off differently.

Do additional years of schooling reduce Euroscepticism? We continue to examine whether the increase in average years of education also affects Euroscepticism. To estimate the effect of education on Euroscepticism, we use 'fuzzy' RDs with an optimal bandwidth and a triangular kernel (Calonico et al., 2017). In the first stage, years of schooling is instrumented using the following first-stage regression equation

$$S_{ijc} = \phi + \theta T_{jc} + f(R_{jc}) + \alpha_c + \varepsilon_{ijc} \tag{2}$$

where α_c denotes country-reform fixed-effects. We then use the results of the first stage to use in the second-stage regression

$$Y_{ijc} = \lambda + \gamma \hat{S}_{ijc} + f(R_{jc}) + \alpha_c + \varepsilon_{ijc} \tag{3}$$

where Y_{ijc} represents our two measures of Euroscepticism and $\hat{S}_{ijc}$ is the predicted years of education from the first-stage regression. In equation (3), the coefficient γ
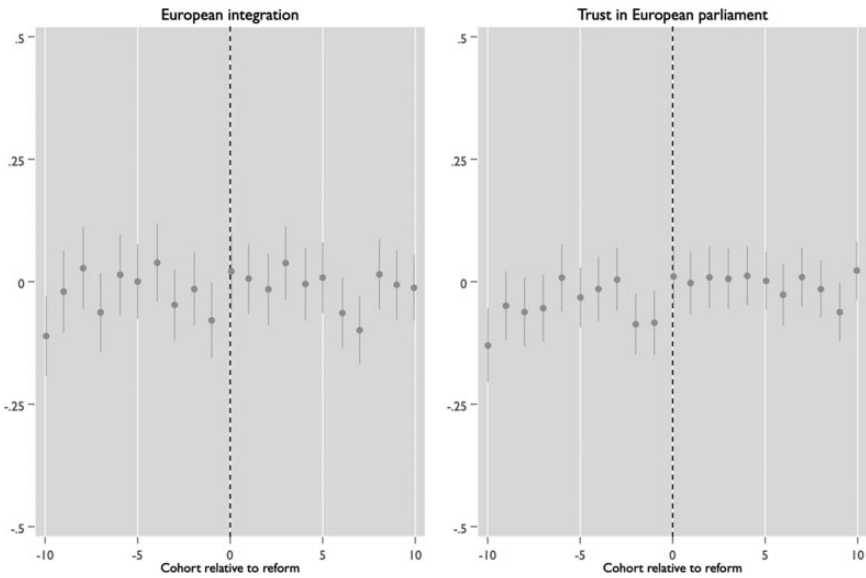
Figure 3. Visualisation of the effect of education on Euroscepticism.
Note: Pooled from reforms in Great Britain (two), the Netherlands, Sweden and Denmark.
The bars display 95% confidence interval.

represents the causal effect of an additional year of education on Euroscepticism
for reform compliers at the cut-off.

We first provide a visual inspection of the results. Figure 3 presents 10 cohorts
before and after the cut-off for both dependent variables. For European integra-
tion, we do not see a clear discontinuity at the cut-off. For trust in the EP, we
observe some evidence for a small discontinuity at the cut-off. Overall, these initial
visual results do not indicate a clear effect of education on attitudes towards
European integration and trust in the EP.

We estimate the effect of education on Euroscepticism using 'fuzzy' RDs.
We run both country-specific models and pooled models. The results for the
pooled models can be found in Table 1. We confirm that we have a strong and
significant first-stage effect of the reforms for years of education. As is clear from
Table 1, the 'fuzzy' RD estimates largely confirm the visual results in Figure 3.
For European integration, our results do not indicate a significant effect of edu-
cation. However, for trust in the EP, we find an initial significant result: at the cut-
off, an additional year of education increases trust in the EP for reform compliers.[7]

We test the robustness of the pooled results by running 'fuzzy' RDs for different
bandwidths, ranging from 4 to 20. The results of these models are shown in
Figure 4. The effect of education on European integration remains consistently
insignificant. For trust in the EP, the evidence is more mixed. For a small limited
number of bandwidths, the result remains borderline significant ($p < 0.05$).

Table 1 'Fuzzy' RD estimates for Euroscepticism.

	European integration	Trust in EP
Estimate	0.164	0.199**
	(0.106)	(0.098)
Optimal bandwidth	8.452	9.324
N	12,016	18,269
First stage	0.388***	0.337***
	(0.088)	(0.068)

Note: Conventional estimates are used. Standard errors are indicated within parenthesis.
$*p < 0.1$, $**p < 0.05$, $***p < 0.01$. All calculations use local linear regression, with an optimal bandwidth and a triangular kernel. These models include country-reform fixed effects.
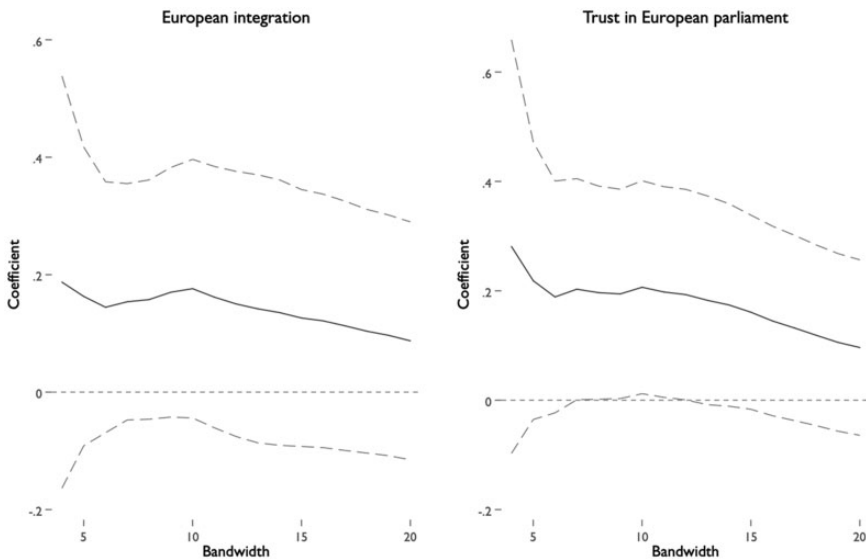
Figure 4. 'Fuzzy' RD estimates for different bandwidths.
Note: Conventional estimates are used. Dotted lines indicate 95% confidence interval. All calculations use local linear regression, with an optimal bandwidth and a triangular kernel. These models include country-reform fixed-effects.

Therefore, it is hard to draw strong conclusions about the effect of an additional year of education on trust in the EP.

Taken together, the image that arises is that additional time spent in secondary education does not lead to clear and conclusive effects on Euroscepticism. For European integration, our results show a consistent insignificant effect, while for trust in the EP, we obtain a non-robust borderline significant effect. In contrast to the clear education effect for Euroscepticism observed in cross-sectional studies, these results point to a mixed picture with less conclusive evidence for a causal link between education and Euroscepticism.[8]

Conclusion and discussion

One of the most consistent findings in the literature on Euroscepticism is that the lower educated are more Eurosceptic than the higher educated (Hakhverdian et al., 2013; Lubbers and Jaspers, 2011). Education is argued to reduce Euroscepticism through the enhancement of cognitive skills, socialisation into cosmopolitan values and improvement of one's position on the (international) labour market. However, the education effect has only been tested using cross-sectional research designs, leaving open the possibility of unobserved confounders. This article moves beyond this well-established association to approximate the causal effect of education by using a 'fuzzy' RD design. Within this design, we analysed the effect of education on two different measurements of Euroscepticism in the Netherlands (1974), Great Britain (1947, 1972), Sweden (1965) and Denmark (1958) using compulsory schooling reforms which were effective in raising the average years of schooling. Since our individual country-reform models lack statistical power, we emphasise the results of our pooled models. Our pooled results indicate insignificant to mixed effects of education on Euroscepticism. An additional year of secondary education did not affect people's opinion towards European integration, while for trust in the EP, we find a borderline significant result which is not robust after additional testing.

Although both European integration and immigration tie into a wider, education-based divide in Western Europe (Bovens and Wille, 2017), the evidence for a direct link between education and these attitudes is mixed. While d'Hombres and Nunziata (2016) and Cavaillé and Marshall (2019) find that additional years of education reduce hostility towards immigration, Lancee and Sarassin (2015) and Finseraas et al. (2018) show no effect of schooling for anti-immigrant sentiments. The results of our study add to the literature on explaining the origins of Euroscepticism. Recent studies using longitudinal data indicate that Euroscepticism is not changing when students pass through education (Kuhn et al., 2018) and that especially parental socialisation is a crucial factor in explaining differences in Euroscepticism (Fox et al., 2019; Kuhn et al., 2018). Although we do not test the parental socialisation mechanism here, our results are in line with the idea that education might be less important than previously thought.

It is important to note that we have to be careful with the generalizability of our results, since the external validity of our study is limited. One crucial limitation is that the 'fuzzy' design only estimates the LATE. Since our estimates only apply to the specific subpopulation of compliers at the cut-off, we cannot exclude the possibility that education has an effect for the average person in the population. However, considering that the association between education and Euroscepticism is such a robust finding in many cross-sectional studies, these results are still surprising.

Some might argue that the first stage of 0.3 years increase in years of education in our sample is too small to see any effects on Euroscepticism. It is indeed true that we introduce measurement error by assigning treatment on the basis of year-of-birth instead of month-of-birth. As a result, this weakens the first-stage effect sizes and consequently impacts our main results. However, both d'Hombres and

Nunziata (2016) and Cavaillé and Marshall (2019) report robust findings for the effect of additional years of schooling on anti-immigration attitudes also using data from the ESS and similar first-stage effect sizes which makes us confident that our non-results are not due to measurement error. However, future research should examine if these results hold up with different data.

There are a number of possible explanations for these deviating findings. First of all, it might be due to the fact that educational reforms mainly took place between the 1950s and the 1970s. As many of these reforms were introduced long before European integration became a salient issue, those affected by the reforms were already long beyond their impressionable years when it eventually did become politicised (Rekker, 2018). That makes our estimates quite conservative; as younger generations grow up in a more globalised society and have more direct experiences with European integration, they are found to be more positive about the EU than older generations (Rekker, 2018). This is also evident in school curricula, as schools are nowadays more focused on teaching students about European integration and citizenship than in the decades the compulsory schooling reforms were passed (Busemeyer and Trampusch, 2011; Keating, 2009). This is in contrast perhaps to the values which are important for attitudes towards immigrants, such as tolerance towards outgroups and respect for different cultures, and which are likely to have already featured prominently in school curricula during the time when the compulsory schooling reforms were passed.

Second, the exogenous variation in schooling in our study is limited to the secondary school level. Hence, it is possible that in contrast to anti-immigration attitudes, the effects of schooling on Euroscepticism occur mainly at another education level. Most prominently is university education, since especially large differences in Euroscepticism are observed between university and non-university graduates (Bovens and Wille, 2017). Research has suggested that the adoption of cosmopolitan values might actually also take place while students are in university (Hastie, 2007). Moreover, university education is associated with more transnational interactions via exchange programmes, such as ERASMUS (European Action Scheme for the Mobility of University Students), which are argued to strengthen EU support (Fligstein, 2008; Mitchell, 2012, 2015).[9] Therefore, it would be worthwhile to study specifically the consequences of students progressing through university education for Euroscepticism.

In a similar vein, also the economic impact of university education for Euroscepticism should be studied in more detail. The utilitarian cost-benefit calculations argument posits that more education is positively associated with EU support because of the positive economic benefits that additional education brings, such as the skills, mobility and knowledge necessary to compete in a globalised labour market (Kriesi et al., 2008). The positive repercussions of compulsory schooling reforms for the economic returns to education are well-documented (Brunello et al., 2009). However, since we find no significant effects of additional years of secondary education for Euroscepticism, the utilitarian cost-benefit mechanism is not substantiated here. Of course, it could be the case that the important economic gains mentioned in the literature on for the educational effects on EU support (i.e.

the skills, mobility and knowledge necessary to compete in a globalised labour market) are mostly obtained via university education. Ultimately, since we cannot make any far-reaching conclusions about the mechanisms at this point, future research should expand on the links between education and Euroscepticism in more detail. Concluding, this article hopes to stimulate future research to further unravel the underlying causes for the educational divide in Euroscepticism.

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Supplemental Material

Supplemental material for this article is available online.

Notes

1. Ideally, we would have relied on month-of-birth data. However, the ESS does not provide data on the month of birth of the respondents. Therefore, we assign treatment on the basis of year of birth, which has been used in other RD designs as well to estimate the LATE utilising compulsory schooling reforms (e.g. Cavaillé and Marshall, 2019; Marshall, 2016; Oreopoulos, 2006; Parinduri, 2014; Yang, 2019). Moreover, one of the issues of the RD design is that it requires a substantial number of observations around the cut-off. Since we do not have a very large sample, spreading out the respondents over month-of-birth would mean less observations around the cut-off than assigning treatment on the basis of year-of-birth. Therefore, using year of birth as the running variable allows for more statistical power to reject the null hypothesis than assigning the treatment on the basis of month of birth.
2. For an overview of the reforms, see the Online appendix. Cavaillé and Marshall (2019) provide sources for determining the year of each reform and a detailed summary of each its contents.
3. For a contrasting view, see Anderson and Hecht (2018) who argue that Euroscepticism contains one dominant underlying dimension.
4. This roughly captures the end of secondary education (Cavaillé and Marshall, 2019).
5. All graphs use the 'plottig' scheme in Stata by Bischof (2017).

6. We are aware that the assumptions of the 'fuzzy' RD design are to a large extent empirically untestable. Although we provide tests and theoretical arguments for why we believe our design meets these assumptions, we cannot completely rule out that our design violates one of them.

7. The results for the individual country-reforms can be found in the Online appendix. The results largely mirror the pooled findings. We do not report any highly significant effects but see that trust in the EP is higher for cohorts at the cut-off who complied with the reforms in the Netherlands (1974) and Great Britain (1972) at $p < 0.1$. The sizes of both effects are quite large but are also accompanied by very large standard errors. Since our individual country-reform models lack statistical power due to the low number of observations at the cut-off, we emphasise the pooled models.

8. If we would have had more observations around the cut-off, our standard errors would have been smaller, making it more likely to find significant effects. Therefore, as an additional robustness check, we also run reduced-form RDs. Although these models yield smaller standard errors, the results remain similar to our 'fuzzy' models (see the Online appendix).

9. For a skeptical view on the effects of the ERASMUS exchange programme, see Kuhn (2012) and Sigalas (2010).

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