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Notes on recent elections

Education and voter turnout revisited: Evidence from a Swedish twin sample with validated turnout data

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A B S T R A C T

The association between education and voter turnout is well-established in almost a century of research. The causal status of this correlation, however, is still subject to debate. Results in the previous literature differ substantially, and this may reflect both methodological differences and heterogeneous effects across populations or types of elections. This study addresses the question using a discordant twin design and variance decomposition methods with validated turnout data for both first- and second-order elections in a large sample of Swedish twins, paired with population-wide sibling data. Results show that education does not have an effect on national electoral turnout, but does have an effect on turnout in the European elections. Furthermore, the association between education and turnout is shown to be affected by substantial genetic confounding, which leaves a non-trivial amount of bias even in sibling based designs. This underscores the importance of taking genetic confounding seriously in observational research.

1. Introduction

A vast range of research in political science has documented the various ways in which political participation is unevenly distributed across social strata and covaries with different types of individual resources. One such resource is education.

The empirical association between education and voter turnout in democracies is well-established in almost a century of literature (Merriam and Gosnell, 1924; Lewis-Beck et al., 2008). Education may produce several goods for the individual that can serve to increase political participation in general, and electoral turnout in particular. It has previously been argued that one can fruitfully distinguish between absolute or relative education effects (Persson, 2013a), which differ in what these goods are and how they are connected to turnout. First of all, education may endow a person with skills (Condon, 2015; Galston, 2001) and beliefs (Jackson, 1995) conducive to absorbing political information, participating in political discussions and subsequently voting in elections. These are absolute education effects since they can shift the distribution in participation in a population when manipulated, and likely applies along the entire education gradient. Second, education can give access to social networks where (a) the social norms of voting are strong (Hansen and Tyner, 2019) and (b) where political knowledge is more widely shared and accessible (Klofstad, 2010). These are relative mechanisms in the sense that they do not necessarily imply shifts, but rather reallocations between networks. They also likely more strongly connected to post-secondary education.

While these mechanisms may be intuitively compelling, and the correlation between education and turnout appears almost universal, there has been substantial debate over whether this really reflects a *causal effect* of education. An alternative view is that education may simply be a proxy for other causal factors (Persson, 2013a). First, people with different levels of education also differ on other social factors that may not be causally intermediary (Kam and Palmer, 2008). Second, individual factors such as personality traits or cognitive capacity may influence education and participation simultaneously (Mondak and Halperin, 2008; Gerber et al., 2011). Third, there may be common genetic factors that influence both, i.e. genetic confounding (e.g. Dawes et al., 2014, 2015).

In the last decade, several studies have addressed these issues using different types of causal or quasi-causal designs. Sondheimer and Green (2010) present the results from three policy experiments among lower SES groups in the US and find that education has a strong impact on voter turnout. Similarly, Lindgren et al. (2019) use a Swedish educational reform as exogenous variation and find effects among the most disadvantaged groups, but no average effects. Others have found more consistent negative results. Examining the impact of exogenous variation in education induced by the Vietnam draft lottery in the United States, Berinsky and Lenz (2011) argue that there was no discernible effect on voter turnout. Persson (2014), further, uses longitudinal data and matching techniques in a British cohort and finds no effects.

Two studies are especially relevant for the design utilized in this paper. Dinesen et al. (2016) investigate a broader variety of participation measures (but not turnout) using a discordant twin design, and find that the association holds in Denmark and the United States but not in Sweden. Further, Gidengil et al. (2019) use a discordant sibling

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design with validated turnout data from Finland and find evidence for considerable familial confounding, but also remaining effects when within-family estimates are used. While the former can adequately control for genetic confounding, it does not use validated turnout data. The latter, on the other hand, *does* have turnout data, but due to the sibling design cannot rule out genetic confounding.

Results in the literature thus vary from zero to strongly positive. Berinsky and Lenz (2011) argue that this may simply reflect methodological differences. However, the mechanisms invoked above give us several reasons for why the stark differences are also plausibly due to heterogeneous effects across groups, countries, or types of elections. I will specifically highlight the latter: the differences between first- and second-order elections (Reif and Schmitt, 1980).

This study makes use of newly available validated Swedish turnout data for the national elections in 2010 (where turnout was high) and the European elections in 2009 (where turnout was much lower). These are typical instances of first- and second-order elections.[1] The above mentioned mechanisms imply that we should expect the education effect to differ between these two types of elections in the following ways.

First, the voting norm for Swedish national elections is generally considered strong, which is reflected in consistently high turnout. In contrast, a strong norm for voting in the European elections has yet to form. If education has the effect of placing an individual in a social network with strong voting norms, the downstream effect of this should crucially depend on the strength of the voting norm *outside* such networks. If the general voting norm is stronger for the national elections, we should expect this mechanism to be substantially less powerful for national elections than for European elections, and therefore that the effect of education should be weaker.

Second, differences in the informational environment lead to expectations in the same direction: there is a relative lack of information about European politics due to the distance to the political assembly in question (De Vreese, 2001). If education places individuals in networks where information is more easily accessible, this should produce a stronger turnout effect in European elections than in national elections, for which debate is highly prevalent in all domestic media throughout the electoral cycle. This difference is also consistent with for example Lefevere and Van Aelst (2014), who argue that campaign information had larger effects in Dutch second-order elections due to the information-sparse media environment.

Third: if education leads to individual resources, such as verbal skills, that change how people process, interact with or absorb political information (i.e. Condon, 2015), a sparser informational environment may favor people with higher education even absent the proposed relative effects.[2]

In summary, we should expect the effect to be more likely to materialize in the second-order election to the European parliament than to the national parliament.

This study is the first to (1) adequately control the relationship between education and turnout for genetic confounding, and (2) document the relative magnitude of genetic and environmental confounding. It is also, to my knowledge, the first to show to what extent this varies between types of elections. I achieve this using a two-pronged methodological approach. First, a discordant identical (henceforth MZ, monozygotic) twin design is used to test whether the association holds when controlling for genetic and common familial factors. I then proceed to present results from bivariate variance decomposition models, which allows me to document the sources of confounding in the raw association. The design is also similar to the discordant sibling design used by Gidengil et al. (2019), but has the added advantage of completely ruling out genetic confounding, where sibling designs do not. To investigate whether differences visavi Gidengil et al. (2019) are due to the difference in methodological approach, or to the different country context, robustness checks also include population-wide discordant sibling models and within-DZ models.

The unique contribution of this paper is thus three-fold: first, I test the effect of education on voting in a large sample of identical twins with *validated* turnout data. Second, I also test whether this relationship differs between first- and second-order elections. Finally, I am able to document the magnitude of genetic confounding in these relationships.

2. Methods and data

Observational designs always run the risk of capturing non-causal relationships. Even with proper precautions, such as controlling for measurable confounders, results hinge on a number of strong assumptions. An issue of particular salience that has been highlighted in recent studies in the field of political behavior (eg. Oskarsson et al., 2017) is genetic confounding: correlations between measured variables may to some extent reflect a shared genetic etiology. This problem has no straight-forward solution unless genetically informative data are used.

The discordant twin design employed here controls for all confounding shared by MZ twins – that is, all genetic effects as well as shared environmental effects – by restricting the analyzed variation to that *within* MZ twin pairs. This is the approach used by Dinesen et al. (2016), but it has also been used to investigate, for example, the relationship between education and trust (Oskarsson et al., 2017), and education and political knowledge (Weinschenk and Dawes, 2018). Intuitively, if a twin with higher education also has a higher propensity to vote, it at least cannot be attributed to the factors shared with the other twin. The approach has two important differences compared to other within-family designs, such as those using siblings. First, a sibling design will reduce genetic confounding by half, while an MZ twin design will remove it completely. Second, an MZ twin design will automatically rule out effects of time, such as sibling order or cohort effects. Technically speaking, the design essentially entails using MZ twin data with twin pair fixed effects and clustered standard errors at the twin pair level. In this study, furthermore, all models are estimated as linear probability models, since all voting-concordant twin pairs would necessarily be dropped in within-pair logit models.

It is important to keep in mind that although bias due to common genetic and environmental factors is removed, within twin-pair estimates may still be biased for several reasons. On the one hand, since twins in the same pair may exert influence on *each other*, statistical assumptions of independence may be violated (i.e. the SUTVA assumption). Such a violation could bias estimates in either way: if twins tend to mimic each other and thus become more alike, it could bias coefficients toward zero. If, on the other hand, twins tend to segregate into behavioral "niches" within families and thus become *less* alike, this could bias coefficients away from zero.

On the other hand, *unique* environmental factors are still uncontrolled, as long as they happen before the age at which one finishes one's education. For example, if twins are put in separate classes, and the social environment imposed by their school class influences both later educational attainment and turnout, this may leave residual confounding. Such confounders may also bias the results in either direction. Simple robustness checks for adult height and cognitive capacity, that may capture differences in certain developmental processes, are included in Appendix.

[1] Another possible second-order election to investigate would be the regional (county and municipal) elections, but these are held at the same time as the national elections in Sweden, leading to almost identical participation rates.

[2] One may also note that this follows the law of dispersion (Tingsten, 1937), which states that inequalities in turnout across any social strata, descriptively speaking, will decrease as the general level of turnout increases. This a direct mathematical function of the decrease in variation that accompanies an increase in turnout, but also has recent quasi-experimental support in the Swedish context from Persson (2013b).

Table 1
Descriptive statistics.

VARIABLE	Mean	SD	Min	Max	N
Nat, MZ	.96	.2	0	1	5428
Nat, DZ	.96	.2	0	1	4002
EU, MZ	.62	.48	0	1	5420
EU, DZ	.63	.48	0	1	3967
Education, MZ	12.39	2.64	7	19	5601
Education, DZ	11.93	2.76	7	19	4134
Birthyear, MZ	1955.66	12.28	1922	1979	5618
Birthyear, DZ	1951.58	12.11	1921	1979	4138
Nat, pop	.82	.39	0	1	5,776,821
EU, pop	.47	.50	0	1	5,607,660
Education, pop	11.56	2.82	7	19	5,885,351
Birthyear, pop	1954.10	15.77	1904	1979	5,885,351

Note: Nat=national election participation, EU=European election participation, MZ=monozygotic twins, DZ=dizygotic twins (same-sex pairs), pop=population data.

To get a sense of the external validity of the results, regular (naive) models are also included for the full Swedish population for the same elections. This allows for a direct comparison between the naive effects in population-wide data and the twin sample used. Furthermore, to investigate whether any differences in results between the present study and the related study by Gidengil et al. (2019) are merely due to the different country studied, or if they are due to the methodological differences, sibling fixed effects are also added to the population models.[3]

In order to disentangle the amount of trait correlation that can be attributed to genetic correlation (overlapping genetic influences), I also leverage the variation from same-sex DZ (dizygotic, or fraternal) twin pairs and apply a bivariate ACE decomposition. Variance decomposition models are structural equation models designed to estimate the amount of variance in a given trait (and in this case, the covariance between multiple traits) that can be attributed to different sources. The variance components typically used in behavior genetic studies are an additive genetic component (A), common environmental factors shared within the twin pair (C), and a unique environmental factor (E). Intuitively, if the correlation between education in one twin and turnout in the other is higher among identical twins (who share 100% of their segregating DNA) than among fraternal twins (who share on average 50%), then education and electoral turnout must share some degree of genetic etiology. The models reported here were estimated using the R package *umx* (Bates et al., 2019).

ACE models come with a set of assumptions of their own – additivity of genetic factors, no gene–gene or gene–environment interaction, no assortative mating and, perhaps most importantly, no systematic differences in within-pair environmental exposures between identical and fraternal twins (the so called Equal Environments Assumption). While each assumption on its own can be legitimately called into question, the final estimates appear to be fairly robust (see e.g. Conley et al., 2013; Polderman et al., 2015).

Data come from registry sources. The sample consists of genotyped twin pairs in the Swedish Twin Registry, which delivered the zygosity (fraternal or identical) of the twins. The independent variable, years of education, is taken from the 2009 LISA registry. Naive twin models and population models also include controls for age and sex; sibling fixed effects also require family identifiers (connection to a common mother and father). These are all available in the multigenerational registry.

Validated turnout data from the national elections of 2010 and the European elections of 2009 are used. The reliance on actual turnout data rather than self-reported turnout has two major advantages. First, some selection bias is avoided. Second, self-reported turnout is known to be notoriously unreliable both due to social desirability bias and misremembering (Karp and Brockington, 2005), which is bound to decrease statistical precision.[4] In this case relying on registry data instead of survey data also increases the sample size substantially, from roughly 2200 in Dinesen et al. (2016) to more than 5400 twins (and even more when including DZ twins in the decomposition models).

Both twin and population samples are restricted to individuals born before 1980 (who have consequently reached the age of 30 during the election years) to ensure that they will have finished their formal education, and to decrease the risk of reverse causation.

Descriptive statistics are presented in Table 1. As is evident, the turnout for the national elections is very high by international standards, and even higher in this sample of twins than in the general population.[5] By comparison, the turnout for the European election is substantially lower.[6] The levels of education are comparable but marginally higher among the twins — with an average that implies a distribution of education that covers variation in both secondary and post-secondary education.

3. Results

Table 2 presents OLS models for the twin sample (naive and within-twin pair models). Here, effects are significant in both of the naive models. The effect is substantially larger for the European elections than for the national elections (.0492 vs. .0069 when controlling for age and sex, i.e. roughly five percentage points increased likelihood of voting per added year of education, vs. 2/3 of a percentage point increase). These estimates suggest that descriptively speaking, there is indeed an education gradient in voter turnout even when turnout is exceptionally high.

This can be compared to the results for the general population in Table 3: without sibling fixed effects, the estimate for the EU election is just slightly higher (5.9 percentage points per year of education), whereas the estimate for the national election is three times higher (2.1 percentage points). The reason why the effect size differs by such a large magnitude between the twin sample and the general population for the national election is most likely that the aggregate turnout is even higher in the twin sample than in the general population, leaving less variation to be explained.

The two main models of this paper are models 3 and 6 in Table 2, where variation is restricted to within twin pairs. Here, both estimates are substantially depressed. The effect for the European election

[3] Full siblings only. Also note that all individuals for which either maternal or paternal connections were missing were dropped, since family ID's could not be constructed. This accounts for the difference in sample size between the table of descriptives and the models estimated.

[4] When matching the validated turnout data for the 2009 European election to a survey question regarding the same election (the SALTY survey from the Swedish Twin Registry), the fraction of individuals who self-report in accordance with their registry based turnout is 82% – consequently, there appears to be a substantial amount of error in self-reported turnout measures in this sample.

[5] The higher turnout among the twins is most likely due to self-selection into the sample of people with stronger civic norms. Since the sample is composed of twins who have agreed to participate in genotyping, and this action in itself can be interpreted as a prosocial act, the twin sample is going to be slightly skewed in this direction. The easiest way to see this is to compare the twin sample to the subsample of twins identified in the general population data (i.e. siblings born in the same month). Among these, the participation rates are 88.2 and 46.8% in the national and European elections; that is, identical or just slightly higher than the general population. The overrepresentation does not seem to be driven by parental education (10.08/9.58 years for fathers/mothers in the twin sample vs. 10.01/9.56 in the population sample), but the ages of the parents when giving birth was slightly higher on average in the twin sample (33/30.3 years of age vs. 231.7/28.4 in the population sample).

[6] The variation within MZ twin pairs is an average of 1.2 education years, six percentage points in national election participation and 32 percentage points in European election participation. The corresponding variation for DZ pairs is 1.8 years and seven vs. 38 percentage points.

Table 2
OLS results, MZ twins.

	(1) Naive Nat	(2) Naive Nat	(3) Within Nat	(4) Naive EU	(5) Naive EU	(6) Within EU
Education	0.0052***	0.0069***	−0.0017	0.0377***	0.0492***	0.0182*
	(0.0010)	(0.0011)	(0.0036)	(0.0026)	(0.0027)	(0.0079)
Sex		0.0004			−0.0465**	
		(0.0061)			(0.0147)	
Age		0.0010***			0.0063***	
		(0.0003)			(0.0006)	
Constant	0.8939***	0.8165***	0.9800***	0.1548***	−0.3002***	0.3970***
	(0.0139)	(0.0247)	(0.0450)	(0.0343)	(0.0564)	(0.0981)
Observations	5414	5414	5414	5408	5408	5408
R-squared	0.0048	0.0082	0.6129	0.0425	0.0684	0.6703
Twin pair FE	No	No	Yes	No	No	Yes

Robust standard errors in parentheses.
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, + $p < 0.1$.

Table 3
OLS results, population.

VARIABLES	(1) Population Nat	(2) Population Nat	(3) Population Nat	(4) Population EU	(5) Population EU	(6) Population EU
Education	0.0210***	0.0209***	0.0134***	0.0585***	0.0592***	0.0347***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Sex	0.0192***	0.0193***	0.0235***	0.0006	0.0007	0.0146***
	(0.0003)	(0.0003)	(0.0006)	(0.0005)	(0.0005)	(0.0009)
Age	0.0025***	0.0025***	0.0012***	0.0079***	0.0079***	0.0071***
	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)	(0.0001)
Constant	0.5063***	0.5109***	0.6666***	−0.6127***	−0.6199***	−0.2879***
	(0.0013)	(0.0014)	(0.0039)	(0.0017)	(0.0019)	(0.0062)
Observations	4,069,885	3,404,064	3,404,064	4,051,233	3,385,708	3,385,708
R-squared	0.0337	0.0336	0.5334	0.0954	0.0958	0.5641
Sample	All	Sibs	Sibs	All	Sibs	Sibs
Sibling FE	No	No	Yes	No	No	Yes

Robust standard errors in parentheses.
*** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$.

decreases by 63%, compared to model 2, to .0182, i.e. roughly two percentage points per added education year, but remains statistically significant (the reduction between models is itself also significant, $p < .05$).[7] Meanwhile, the effect for the national election disappears completely, even showing the opposite sign ($b = -.0017$, NS).

This can be contrasted to the sibling fixed effects models for the population data (models 3 and 6, Table 3). As expected, effect estimates are decreased in these models as well, but not nearly as much (35 and 42% respectively). They also remain significant for both national and European elections, at magnitudes of 1.5 and 3.5 percentage points per year of education, respectively.[8]

To further tease apart why we observe the effect reductions between the naive and within pair models, we can turn to the bivariate ACE models presented in Table 4. It is evident, first and foremost, that there is a moderate to high degree of heritability for both voting behavior and education years among Swedish twins. The heritability of education is 38%, whereas the heritability of electoral participation is 29% for the European election and 22% for the national election. The remaining variation is mainly captured by unique environment for voting behavior, and roughly equally split between common and unique environment for education.

Education and voter turnout also appear to share some degree of genetic etiology. The bivariate heritabilities are .056 for the national elections and .083 for the European elections. These figures indicate genetic correlations of about .20 and .24, i.e. that the genetic underpinnings of education and turnout overlap by 20–24% depending on the type of election. Furthermore, this translates to proportions of the naive trait correlations that can be attributed to genetic overlap of 79% and 42%, respectively. The larger drop in effect size for the national elections when moving to the within-pair models thus reflects a larger reliance on some endophenotype that is shared with education. It should also be noted, however, that these overlap figures are only significant for the European elections.

These figures further illuminate the difference between the naive and within-pair estimates above. For national elections the naive effect disappeared completely, whereas we would expect an 79% reduction purely based on genetic confounding. For European elections, the estimate was reduced by more than half, whereas we would expect a 42% reduction from genetic confounding only. The reductions in the estimates thus appear to be largely driven by genetic confounding.

[7] The Appendix contains two robustness checks to see whether unique environmental influences might still confound this relationship. First, within-pair differences in adult height (Table 5 in the Appendix) is used a proxy for differential developmental influences in a subsample of the data where adult height was available: the TwinGene sample, born before 1958, and from conscription records for male twins born after 1951. Second, within-pair differences in cognitive capacity (Table 6 in the Appendix) as measured at conscription tests, also for male twins born after 1951. Neither of these controls have any effect on the results.

[8] This reduction in the effect size is comparable to fraternal twins (who are genetically siblings), showing effect size reductions with age and sex controls of 40 and 44% for national and European elections, respectively (see Table 7 in the Appendix). If the difference in effect size reductions between the sibling models and the MZ twin models were an artefact of the uniqueness of twins, selection bias, or lack of variation within pairs, we would instead observe effect size reductions for DZ twin models more resembling the MZ twin models than the sibling models.

Table 4
Bivariate ACE models.

	Nat	EU	Education
h^2	.223	.294	.376
	[.109;.337]	[.187;.402]	[.319;.436]
c^2	−.024	.028	.361
	[−.120;.071]	[−.065;.118]	[.304;.414]
e^2	.801	.678	.263
	[.765;.837]	[.645;.711]	[.248;.279]
Biv. h^2	.056	.083	–
	[−.004;.117]	[.025;.142]	
Trait corr.	.071	.198	
	[.051;.091]	[.179;.218]	
$\%r_g$	79%	42%	–

The first and second columns show the bivariate heritabilities and trait correlations between the respective electoral participation and education years.

4. Discussion

The first thing to note is that with the stronger case of within-pair MZ twin comparisons, the association between education and electoral turnout holds in the European elections, but disappears completely in the national elections. An increased level of education does not seem to produce increased domestic turnout, but indeed seems to lead to increased participation in the European elections.

There are several possible reasons for this difference. First and foremost, there is the objection that it may just be a variance artefact: the turnout in the national election, especially among the twins in this sample, is very high, meaning that there may not be enough variation to be explained. This is not the case for the European elections. However, a strong argument against this interpretation is that naive effects are significant even for national elections, and that the reduction in the effect estimate is itself significant. Getting a significant reduction between models would be unlikely if it represents a variance artefact. Furthermore, a simple comparison with within-DZ models, which would suffer from the same variance problem, gives no ground for suspecting that the reduction is a variance artefact. It could also be, as per Lindgren et al. (2019), that the lack of an average effect for the national elections obscures a local effect among disadvantaged groups.[9]

However, it is also possible that the results reflect a genuine difference of a more fundamental nature between the two types of elections. The mechanisms for so-called relative education effects discussed in the introduction shed more light on why. First of all, it was posited that education may transfer individuals to social networks with strong voting norms (Hansen and Tyner, 2019). If high turnout is indicative of a strong *general* social norm to vote, this norm should therefore also dampen the effect of education. Similarly, elections where the general informational landscape is richer should also decrease the effect of being situated in high-education networks where political discussion and information is more commonly shared. The more general implication seems to be that for relative education effects to be present, the secondary resource that this relative effect depends on also needs to be unequally distributed. In one sense, the absence of any clear education effect in the national elections should therefore be somewhat encouraging for democracy scholars: the resources necessary (whether they be skills, norms or information) for effective participation, at least in national elections, are equally distributed enough for education to lose its "participatory edge".

The second thing to note is the general depression of effect sizes when accounting for genetic and shared familial confounding — roughly half for the European elections and completely for the national elections. The extended analysis using bivariate variance decomposition showed that this reduction to a large extent appears to reflect shared genetic influences for education and voting. This study cannot shed any light on what genetic effects or underlying traits are driving this, but it is likely that a substantial portion of naive correlations between education and voting behavior can be attributed to genetic correlation. Possible explanations are that the shared genetics reflect endophenotypes like cognitive capacity (Dawes et al., 2014, 2015; Denny and Doyle, 2008) or personality characteristics (Dawes et al., 2014; Gerber et al., 2011; Hakimi et al., 2011; Mondak and Halperin, 2008). Whichever the confounding endophenotype is, it appears that it is also more strongly associated with national than European electoral turnout in this sample. This underscores the importance of taking genetic confounding seriously when studying individual-level associations of this nature.

As noted, there are a few caveats with within-twin pair analyses. One such caveat is unique environmental factors active before the age of finishing one's education, that are in fact not controlled for by this design. While certain developmental processes may be captured by a simple measure of adult height or cognitive capacity (which did not alter the results in this study for the parts of the sample where this information was available), there may still be unique environmental factors that are not. It can be noted, however, that Dinesen et al. (2016) produce robustness checks with a range of pre- and postnatal variables, which is reassuring since their Swedish sample partially overlaps with the one employed here.

One can also compare the results of this study to previous family designs. Dinesen et al. (2016) found that education was not related to political participation in Sweden. The present results for national elections are a partial corroboration of their results. However, results for the European elections are also partially a refutation. This could be either due to the larger sample size employed here, differences between participating in elections and other types of political activity, or due to more substantive differences between domestic and supra-national political arenas discussed above.

Moving to the univariate ACE estimates, it is worth pointing out that the figures are broadly within the range of previous studies. The comprehensive overview by Branigan et al. (2013) finds that the heritability of education varies widely between contexts, but with a grand mean of 40% – not far from the 38% observed here. Previous heritability estimates for political participation in a Scandinavian context are difficult to compare directly since they have relied on either self-reported turnout data (e.g. Dawes et al., 2014) or other types of participation measures (Klemmensen et al., 2012). One salient point to mention is that while Dawes et al. (2014) find a stark difference in the heritability of turnout for national and European elections (41% and 17% respectively), the estimates found here are more closely aligned (22% and 29%) and also more precisely estimated, most likely owing to the larger sample size and the reliance on validated turnout data.

Furthermore, results on the degree of genetic correlation underscore the crucial role of shared genetics and may therefore indicate that estimates relying on sibling comparisons (such as Gidengil et al., 2019) may still contain a non-trivial amount of genetic confounding. The latter conclusion is also corroborated by the population models with sibling fixed effects. When comparing the twin models to the sibling models, it is clear that the effect reduction is substantially larger with twins — for both national and European election turnout. This indicates that the absence of effects for the national elections among the Swedish twins as compared to the Finnish siblings in Gidengil et al. (2019) is not likely to be a consequence of the different country contexts, but is indeed an effect of better control of genetic confounding.

To conclude, a simple, comprehensive answer to the question of whether education has a causal influence on voting behavior should probably not be expected. The effects of education are likely to be attenuated when other resources (whether they be material or psychological)

[9] Unfortunately, the current sample is underpowered to reliably estimate interactions or do subgroup analyses. A cursive test of how the effects differ across parental education can be found in Table 8 in the appendix, but does not show such a tendency.

are present, or when voting norms are strong. Rather than singularly focusing on the if question, future research may want to turn an eye to mapping out the where/who questions — and importantly, do so while taking the problem of genetic confounding seriously.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix

Tables 5–8 can be found in online appendix available at https://doi.org/10.1016/j.electstud.2020.102186.

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