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Article

Educational Attainment Has a Causal Effect on Economic, But Not Social Ideology: Evidence from Discordant Twins

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Abstract

In this article, we examine the nature of the relationship between educational attainment and ideology. Some scholars have argued that the effect of education on political variables like ideology is inflated due to unaccounted-for family factors, such as genetic predispositions and parental socialization. Using the discordant twin design and data from a large sample of Danish twins, we find that after accounting for confounders rooted in the family, education has a (quasi)-causal effect on economic ideology, but not social ideology. We also examine whether the relationship between education and economic ideology is moderated by levels of economic hardship in the local context where individuals reside. We find that the (quasi)-causal effect of education on economic ideology increases in economically challenged areas.

Keywords

education, causality, political ideology

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A growing and methodologically sophisticated literature has questioned the time-honored assumption that education is one of the most important causal factors influencing a host of political behaviors such as political participation, political interest, political sophistication, and social trust (Berinsky and Lenz, 2011; Dee, 2004; Dinesen et al., 2016; Highton, 2009; Kam and Palmer, 2008; Oskarsson et al., 2017; Rasmussen, 2015; Shani, 2009; Sondheimer and Green, 2010; Weinschenk and Dawes, 2019). The shared point of criticism in this burgeoning literature is that nobody enters an educational institution as a blank slate, and that—due to problems of self-selection—the quasi-causal effect of education is inflated. Education may be nothing more than a proxy for predispositions that are influenced by genetics and other pre-adult factors (Gidengil et al., 2019; Kam and Palmer, 2008). So far, no one has critically examined the effect of education on *political ideology* while taking genetic and pre-adult social influences into account. This article undertakes such an analysis.

Few of the previous studies arguing that the education effect is inflated due to unobserved confounding have distinguished between the *socialization effects* and *resource effects* of education (cf., however, Rasmussen and Nørgaard, 2017). By failing to distinguish clearly between these different effects of education, we risk misunderstanding when and why education is confounded by unobserved variables. Furthermore, no previous study has examined the differences between socialization and resource effects using twin analyses, which has the obvious advantage that family factors can be directly modeled.

We argue that social and economic ideology are ideal cases if one wants to examine *which* educational effects are more likely to be confounded by family factors. Although economic and social ideology are likely to be affected by both types of effects, the impact of education on economic ideology is likely more strongly related to resources, whereas the impact of education on social ideology is likely more strongly related to socialization effects. We elaborate on these ideas in more detail below.

To examine *whether* and *how* education has a quasi-causal effect on social and economic ideology, we examine twins who are discordant on educational attainment. The research design utilizes the fact that monozygotic (MZ) twins share all of their genes, whereas dizygotic (DZ) twins share, on average, half their separating genes (like other biological siblings) and that both MZ and DZ twins reared together share their upbringing. Therefore, unobserved confounders related to family factors are also shared, and within-pair differences in education that are associated with differences in ideology are less prone to (unobserved) confounding (McGue et al., 2010). Previous discordant twin studies have examined whether differences in educational attainment between MZ twins lead to differences in social trust, political participation, and political knowledge (Dinesen et al., 2016; Oskarsson et al., 2017; Weinschenk and Dawes, 2019). In these studies, the effect of education was substantially confounded by family factors, mainly genetic influences.

In the section that follows, we briefly examine the literature on the relationship between education and ideology and argue that this relationship is likely confounded by family factors, although more so for social ideology, in which socialization effects are dominant. We then present two discordant twin studies that take family factors into account and allow us to more precisely estimate the effect of education on ideology and the sources of confounding. Study 1 shows that both genetic and environmental pre-adult influences confound the educational effect on social, but not economic, ideology. While

the educational effect on economic ideology is more robust, we show in Study 2 that this effect is moderated by the degree of local economic hardship where individuals reside.

Before proceeding, it is worth pointing out that results of this study should be of interest to scholars working across a wide range of subfields in political science. Indeed, questions about the nature of the relationship between educational attainment and political variables have been studied across a number of areas in political science (e.g. comparative politics, American politics), and the underpinnings of ideology are of interest to scholars of American politics, comparative politics, and international relations (Krishna, 2002; Marquart-Pyatt and Paxton, 2007; Weldon, 2006). In addition, we note that the results from Study 2 should be of interest to researchers who are concerned with the interaction between individual-level variables and contextual factors.

Differential Educational Effects

Two primary explanations have been given for why education should affect political attitudes.[1] First, education can affect civic skills and the attainment of attitudes, such as social attitudes, through the educational experience itself—what has been termed the *socialization effect* of education (Dunn, 2011; Gaasholt and Togeby, 1995; Galston, 2001; Hyman and Wright, 1979; Phelan et al., 1995; Stubager, 2007). Many different, partly overlapping mechanisms for students' internalization of attitudes have been suggested. Examples include the actual teaching of values in the classroom by teachers, a broadening of cognitive understanding leading to an attitude change, and informal interactions between the students themselves (Dunn, 2011; Gaasholt and Togeby, 1995; Hyman and Wright, 1979; Phelan et al., 1995; Stubager, 2008). Common among these explanations is that it is what happens *in* the educational system that affects the attainment of attitudes.

A second explanation stresses that education grants *resources* and access in the social structure. The first comprehensive treatment of education's resource effect in the field of political behavior is the important work by Nie et al. (1996) They argue that education, in addition to the (absolute) socialization effect described above, also has a relative effect: Educational attainment affects the resources that individuals are awarded, such as access, influence, and status. An important implication of this conceptualization is that the effect of education is relative, since resources are competitive: There is a finite amount of status and access available at any given point in time. For instance, Nie et al. and others have demonstrated that whenever the effect of education on political behaviors is driven by education granting resources, the effect of education should decrease whenever the competition over these resources increases, for example, when others in one's immediate surroundings are also highly educated or otherwise well-resourced (Campbell, 2009; Nie et al., 1996; Persson, 2011; Tenn, 2005; for a contrary position, see, for example, Helliwell and Putnam, 2007).

To adjudicate between education as a granter of values versus education as a granter of resources, we focus on the effect of education on two important dimensions of political ideology: social and economic attitudes (Feldman and Johnston, 2014). The effect of education on social attitudes primarily reflects socialization, whereas the effect of education on economic attitudes primarily reflects the granting of resources (cf. below). By comparing these two effects of education, we expect, based on the outlined theoretical framework, that the primary difference is in fact the difference between socialization and resources.[2]

Education and Ideology

Although ideology is often conceptualized as unidimensional, scholars have increasingly come to see it as multidimensional. Feldman and Johnston (2014), for example, argue that ideology consists of social *and* economic dimensions (see also Caughey et al., 2019; Johnston and Ollerenshaw, 2020; Malka et al., 2017, for multidimensional treatments of ideology). They also note that "unidimensional treatments of ideology obscure important (and interesting) complexities in the antecedents of political orientations" (Feldman and Johnston, 2014: 341), which is directly relevant to our ideas about how education may be related to different dimensions of political ideology.

Indeed, the existing literature suggests that education may influence political ideology and attitudes in different ways (Converse, 1972; Nie et al., 1996; Persson, 2015). On one hand, the educational experience may influence one's motivations, values, and attitudes, because education entails socialization by teachers or the influence of peers and norms to hold certain political attitudes (Mendelberg et al., 2017). In this case, there is a socialization effect of the educational experience. This type of developmental process has primarily been linked to the effect of education on social ideology (i.e. education makes you more socially leftist because of what happens in the educational system itself) (Phelan et al., 1995; Stubager, 2008).

On the other hand, education could also have an effect on ideology because education influences the resources available to citizens, the networks they have access to, and the opportunities they have to acquire status and income (i.e. a resource effect). Numerous studies have found a strong association between economic attitudes and individuals' socio-economic position; those who are more educated are more economically rightist (Alesina and La Ferrara, 2005; Fong, 2001; van der Waal et al., 2007). Resources and status are inherently relative. For instance, Newman et al. (2015) find that local inequality and hardship for the poor activate "latent opinions" informed by economic self-interest and increase the difference in the belief in meritocracy between low- and high-income earners (cf. also Kelly and Enns, 2010; Weatherford, 1978). Following this reasoning, economic hardship may increase the saliency of differences in educational status so that the effect of education on economic ideology increases in economically challenged areas where competition over resources is more intense. In this case, the effect of education on economic ideology is thus "relative" because it may depend on the context in which individuals live (Jackman and Muha, 1984; Lipset, 1960; Nie et al., 1996; Phelan et al., 1995).

Thus, competition could lead the more educated to become more economically conservative. If I am in favor of inequality and economic competition, these attitudes might sharpen or increase if levels of unemployment are 50%, and I am myself highly educated and thus less likely to be in the unemployment line or to have accumulated enough wealth to not experience the economic competition as strongly. This would suggest that economic ideology has an important contextual component. Conversely, whether one opposes homosexuality (i.e. the socialization effect of education on social ideology; Alford et al. (2005) have demonstrated a significant role for shared environmental influences) is likely not affected by the degree of (economic) competitiveness. We do not mean to imply that economic attitudes are solely resource-driven or that the effect of education on economic attitudes is only resource-driven. We simply argue that the effect of education on economic ideology, compared with its effect on social ideology, is more strongly related to resources, which are competitive.

Confounding by Family Factors?

The literature on the causal effect of education described above argues that college students are not a random sample of the general population. The motivations, values, and resources that make individuals pursue a college education may also influence their political attitudes. Parents transmit values to their children through socialization and by setting an example (Jennings and Niemi, 1968), and numerous studies have shown ideological orientation to be partially heritable, which suggests that part of this socialization effect is genetically confounded (Alford et al., 2005; Dawes and Weinschenk, 2020; Eaves et al., 1997; Hatemi et al., 2014; Kandler et al., 2012, 2016a, 2016b; Martin et al., 1986)—although many studies have not explicitly distinguished between economic and social ideology. The few genetic studies on political ideology that distinguish between economic and social ideology tend to find that economic ideology is less heritable than social ideology (Friesen and Ksiazkiewicz, 2015; Ksiazkiewicz et al., 2020).

If much of the effect of education on social attitudes is indeed based on a socialization effect, we would expect the relationship to be confounded by both shared family experiences and genetics. Conversely, if the effect of education on economic attitudes is based more on a resource effect that does not happen *in* the educational system but is an effect of it (Marshall, 2016), we would expect variation in economic ideology to be less confounded by both genetics and shared family experiences. As discussed above, few studies have directly argued that the effect of education on economic attitudes is due to a socialization effect, which is more likely confounded by family upbringing. The type of socialization effect we focus on in this investigation is the socialization effect in the family while growing up. This leads to our first hypothesis:

- Hypothesis 1: The effect of education on social ideology is more confounded by family factors compared with the effect of education on economic ideology.

For the same reasons, we also believe that the educational effect on economic ideology is moderated by economic hardship in the local context in which individuals live, since this effect is theorized to be affected by the resources and networks that having a higher education grants access to (Study 2). This leads to the second hypothesis:

- Hypothesis 2: The effect of education on economic ideology is moderated by economic hardship.

As we will discuss in the concluding section, this may suggest more broadly that the absolute effects of education on political behaviors are prone to confounding by family factors, whereas relative effects are less so. We investigate the relative effects by examining whether education is moderated by the level of economic hardship at the municipal level; if the effect of education is indeed relative, we would expect education to matter more when there is economic hardship and vice versa.

Discordant Twin Design

The intuition behind the discordant twin design is fairly straightforward (McGue et al., 2010) and has been used in economics as well as political science (e.g. Dinesen et al., 2016; Isacsson, 1999; Oskarsson et al., 2017; Sandewall et al., 2014). Because MZ twins

share all their genes and their rearing environment, that is, family factors, the discordant design assumes that differences in ideological orientation between twins who have completed different levels of education are caused by this discordance in educational attainment. Unobserved confounders that have either genetic sources or shared environmental sources are accounted for because both twins share them. By contrast, unobserved confounders that are associated with the individual twin's own experience (unique environment), such as a particularly encouraging teacher or peer influences, are not taken into account. The quasi-causal effect can thus be estimated by a simple fixed-effect model (i.e. investigating the within-twin pair effect of educational differences on within-twin pair differences on political ideology; this fixed-effect model has been used in other studies on political attitudes (Dinesen et al., 2016; Oskarsson et al., 2017). Here, we are interested in investigating the *sources* of confounding (i.e. whether we are dealing with genetic confounding or confounding by the rearing environment). By including DZ twins, we are able to estimate the sources of confounding. If MZ twins have more similar ideological orientations than DZ twins for similar levels of discordance in education, it is more likely that familial confounding is due to genetic dispositions (McGue et al., 2010). Conversely, when the correlation in DZ ideological orientations approaches the MZ correlation when holding discordance in educational attainment constant, it is more likely that confounding is a result of shared upbringing and environmental influences. In general, the more that within-twin pair differences in educational attainment correlate with differences in ideology, the less confounding by family factors. The discordant twin design is therefore not only a powerful tool to get a more precise estimate of the educational effect on ideological orientation (by taking confounding associated with family factors into account), but also provides estimates of the *sources* of confounding. More formally, we implement a series of Cholesky decompositions (Neale and Cardon, 1992; Turkheimer and Harden, 2014). We describe the methods used in more detail in the sections that follow.

It is important to stress that the exposure variable—education—is *not* randomly assigned (Frisell et al., 2012). Thus, non-shared confounding is always a possibility and is potentially a problem for twin research (Rasmussen et al., 2019). Confounding can come in many guises, however, and not all are equally problematic. Frisell et al. (2012) have analytically demonstrated that using the within-twin pair estimate of a quasi-causal effect is problematic when the exposure (education in our case) is more shared than the sum of all confounders (i.e. familial upbringing and genetic predispositions). One likely confounder is the rearing environment, since we know that political attitudes are transmitted through family influences (Jennings and Niemi, 1968; Jennings et al., 2009). An example of a genetic confounder could be intelligence, since intelligence is related to both levels of education (Deary et al., 2010) and economic ideology (Onraet et al., 2015). Although there are also non-shared differences in intelligence (Sandewall et al., 2014), within-twin pair correlations of intelligence and many other potential confounders, such as personality traits and rearing environment, are highly shared within families, as captured by the common environment and genetic dispositions. We find it highly unlikely, therefore, that education is more highly shared than these potential confounders. In a sense, this would correspond to saying that the within-twin pair difference is higher (among MZ twins) for education than it is for intelligence. However, studies have shown that heritability is higher for intelligence compared with education (Branigan et al., 2013; Neisser et al., 1996). Still, it may be argued that our estimates of the degree of confounding are conservative since non-shared confounding is always a possibility and confounding unrelated to family factors is still possible.

Data and Model Estimation

Participants were drawn from the Danish Twin Registry's younger cohort of twins born in the years 1970–1989. In this study and the next, we draw on two waves of data on political attitudes collected via questionnaires. The first wave was collected in 2009 and the second wave in 2012. To increase power, the two waves are merged such that those who are new in the second wave are added to participants in the first wave. If data are available for both surveys, the newest data are retained as more respondents will have finished their education, and the variance in levels of education is therefore greater with time. With regard to educational achievement and political attitudes, the Danish Twin Registry is remarkably similar to the general population (Christensen et al., 2006; Klemmensen et al., 2012). We also demonstrate in Supplemental Appendix 3 (Tables 4 and 5) that the effect of education on political ideology in this sample is not significantly different from a nationally representative sample using the same measures of ideology.

As was done in a recent biometrical study of the determinants of education (Krapohl et al., 2014), all variables have been age and sex regressed (McGue and Bouchard, 1984) and standardized to have a variance of 1 and a mean of 0. All estimations are performed in Mplus version 7 using bootstrapped standard errors from 5000 repetitions because normal theory-based standard errors are often not applicable for parameters reaching their boundary, which is often the case in twin models with many parameters.[3] Missing data for dependent variables are handled using full information maximum likelihood (Schafer and Graham, 2002; Yuan et al., 2012), which is the Mplus default.[4]

Study 1: Is there a Quasi-Causal Effect of Education on Economic or Social Ideology?

Methods

The twin methodology decomposes the variance of a trait (T) into three components: additive genetic factors (A), common environmental factors (C), and non-shared factors (E). The total variance can be expressed as

$$cov\left(T_1, T_2\right) = a^2 + c^2 + e^2$$

where a^2, c^2, and e^2 refer to the proportion of additive genetic, shared environmental, and non-shared variance out of the total variance, respectively. The covariance between MZ and DZ twin pairs can be described as follows, where e^2 is zero, since this is, by definition, not shared within a twin pair

$$cov\left(T_1, T_2\right)_{MZ} = a^2 + c^2$$

$$cov\left(T_1, T_2\right)_{DZ} = \frac{1}{2}a_2 + c^2$$

To examine whether the effect of education on political attitudes is causal or whether it is confounded by familial influences, we investigate the covariance between education and political ideology using the within-twin pair estimate. This can be thought of as a simple

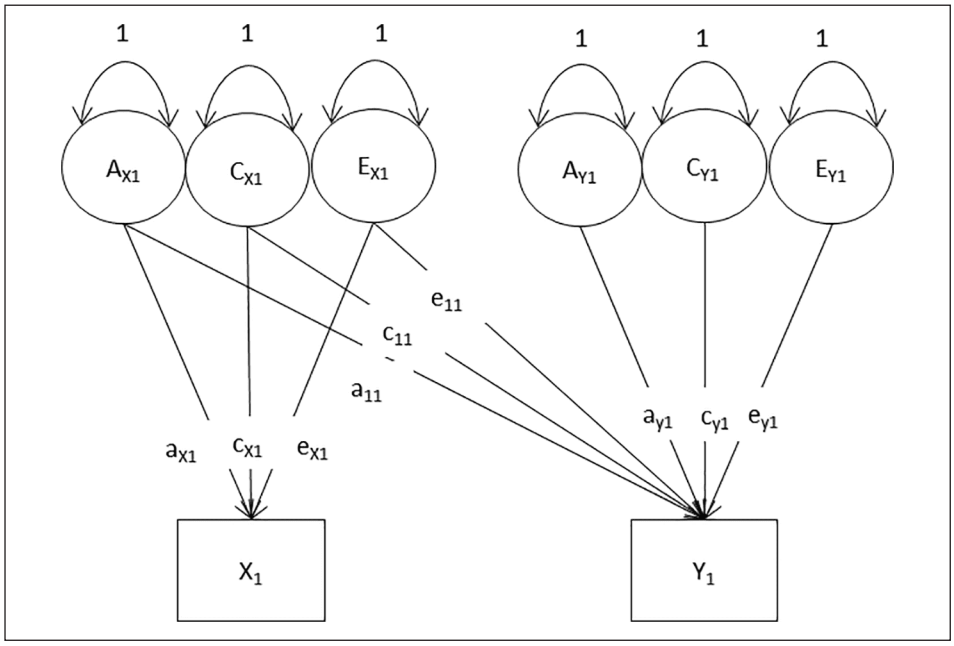

Figure 1. Bivariate Cholesky Decomposition.
*Only shown for one twin.

regression of the A, C, and E variance components on political ideology (Turkheimer and Harden, 2014). So, for instance, we can use the typical regression formula for regressing $\times$ (education) on y (political ideology), that is, $cov(x, y) / var_x$ for the effect of additive genetic influences of education on political ideology. Thus, the equation is as follows

$$a_{11} = \frac{cov(A_x, y)}{var_{Ax}}$$

The same applies for the common environmental influences c_{11} and for the quasi-causal effect estimate e_{11}; these are simply unstandardized regression coefficients and can be interpreted as such if we have not standardized the variables—for one more year of education, how much does political ideology increase? Figure 1 illustrates the model.

Although each of the paths a_{11}, c_{11}, and e_{11} represent regression coefficients, they represent different quantities: If the genetic variance in education and ideology is shared, we have genetic confounding (Path a11), and if the variance in familial upbringing is shared, we have confounding by familial socialization (Path c11). Supplemental Appendix 4 outlines the model in more formal detail, but the intuition is quite simple: If the within-twin effect of education on political ideology is stronger among DZ twins compared with MZ twins, this suggests genetic confounding (i.e. a stronger a_{11} path), and if the MZ and DZ within-twin effect is smaller compared with a cross-sectional estimate, this suggests confounding by familial background (i.e. Path c_{11}) (McGue et al., 2010; Turkheimer and Harden, 2014).

The non-shared environment, Path e11, provides an estimate of the effect of education on ideological orientations that is *not* confounded by familial socialization or genetics

Table 1. The Effect of Education on Ideological Orientations.[11]

	Social ideology	Economic ideology
a11	0.335	0.003
	[0.112 to 0.598]	[–0.252 to 0.230]
c11	0.297	0.108
	[0.026 to 0.559]	[–0.204 to 0.417]
e11	0.008	0.092
	[–0.058 to 0.073]	[0.030 to 0.155]
Twin pairs (MZ/DZ)	875/986	875/991

MZ: monozygotic; DZ: dizygotic.
95% confidence intervals in brackets.

(i.e. a quasi-causal estimate of the effect of education) (Oskarsson et al., 2017; Turkheimer and Harden, 2014). This is because the non-shared environment represents the within-pair differences in education and ideology for MZ twins who are genetically identical and share their family upbringing (Carlin et al., 2005; McGue et al., 2010; Turkheimer and Harden, 2014).

Measures

Education is measured using two questions on educational attainment that are merged into one interval-level variable ranging from those with only primary education to those with advanced degrees (e.g. doctor, lawyer, economist).[5] We note that this measure is not based on registry data, but if we had an even better measure of educational attainment (i.e. one with less measurement error than a self-reported measure), we would likely find even stronger effects than those reported here.[6]

Social and economic ideology are measured using the scales from several previous studies (Klemmensen et al., 2012; Ksiazkiewicz et al., 2020). Economic ideology is measured with five items, such as "Competition is good" and "People with high incomes pay too little in taxes." The Cronbach's alpha is 0.73 for this measure. Social ideology is measured using seven items, including "In Denmark we need to protect our national cultural norms" and "Violent crimes should be punished more harshly than they are today." The Cronbach's alpha is 0.72 for this measure. We note that the two ideology measures are correlated at $r = 0.30$, which indicates a moderate relationship. Full question wordings and descriptive statistics can be found in Supplemental Appendices 1–2. Both of our ideology constructs rely on items used by the Danish Election Study.

Results

The results from two bivariate Cholesky decompositions are shown in Table 1. In terms of the quasi-causal effect estimate, Path e11, we find that there is *no* significant effect of education on social ideology 0.008 (95% CI = [–0.058 to 0.073]), but an effect of education on economic ideology at 0.092 (95% CI = [0.030 to 0.155]). Since all constructs are standardized and we are estimating a path coefficients model, the estimate of the education effect on economic ideology can be interpreted as a usual regression coefficient (Turkheimer and Harden, 2014). The effect is rather small, but it is statistically significant.

It is worth noting that familial upbringing, Path c11, and genetic dispositions, Path a11, are both statistically significant and thus help explain why there is no effect of education on social ideology. Thus, whereas Oskarsson et al. (2017) found that the association between education and social trust was primarily accounted for by genetic confounding in the case of social ideology studied here, shared environmental confounding is of roughly equal importance to genetic confounding.

Study 2: Why an Effect of Education on Economic Ideology?

So far, we have shown that there is a quasi-casual effect of education on economic ideology, but not social ideology due to confounding by family factors. This suggests that the relationship between education and economic ideology is not driven by absolute socialization effects, but by relative resource effects. We therefore examine whether the quasi-causal educational effect on economic ideology is moderated by the degree of local economic hardship where individuals reside.[7]

As we noted above, we expect that economic hardship and competition over scarce resources will increase economic self-interest and, by implication, the saliency of educationally informed status differences (Books and Prysby, 1999; cf. also Johnston et al., 2000). For instance, when unemployment is high, those who are less educated and face a higher risk of unemployment will be more interested in economic redistribution and adequate social welfare (Hibbs, 1977; Hibbs et al., 1982; OECD, 2011; Weatherford, 1978). We expect the education effect on economic ideology to increase (decrease) when local economic hardship increases (decreases) and use four different measures to gauge the level of local economic hardship.

Methods

To investigate whether the quasi-causal effect of education on economic ideology is moderated by economic hardship in an individual's local context, we estimate a gene–environment interaction model as outlined by Purcell (2002). The idea is that the effect of education on economic attitudes can depend on the environment. The idea is quite similar to a moderation effect in an ordinary least squares (OLS) model, where X is the independent variable (in our case, education) affecting Y (political ideology) and M is the moderator (competitiveness in our case) (Brambor et al., 2006)

$$Y = \beta_0 + \beta_1 X + \beta_2 M + \beta_3 XM + \epsilon$$

If this were an OLS model, we would simply test whether the coefficient on our interaction term (β_3) was statistically significant or not. If it were, we would typically graph the relationship by letting the moderator vary from high to low. We could then calculate the effect of education on political ideology by calculating the marginal effect (i.e. partial derivative)

$$Marginal\,effect = \beta_1 + \beta_3 M + \epsilon$$

If, for instance, our competitiveness variable was scaled to range from 1 to 5, β_1 would represent the effect of education when competitiveness is zero and the maximum of our effect would be calculated as $\beta_1 + \beta_3 \cdot 5$. This is very similar to what we have done here. The easiest way to think of this is again to remember that we are simply estimating

regression coefficients when we estimate the effect of education on political ideology in a twin setting. The *A*, *C*, and *E* components are scaled in the same way that we have scaled our variables, so if our education variable is scaled to range from 1 to 4, our *A*, *C*, and *E* components will also have a scale of 1–4. In this analysis, we are presenting standardized coefficients, so the e_{11} simply represents the standardized effect of education on political ideology after accounting for confounding and family upbringing as discussed above. Our interaction model for the e_{11} path is thus almost identical to a traditional OLS interaction model:

$$Marginal\ effect = e_{11} + \beta_{Me} M$$

If our moderator is scaled from 1 to 5, e_{11} corresponds to the effect of education on political ideology where our moderator is zero. The maximum effect can be calculated as $e_{11} + \beta_{Me} \cdot 5$ (exactly as in an OLS regression). We therefore examine whether the covariance between education and political attitudes is a function of economic resources, as illustrated in Figure 2.

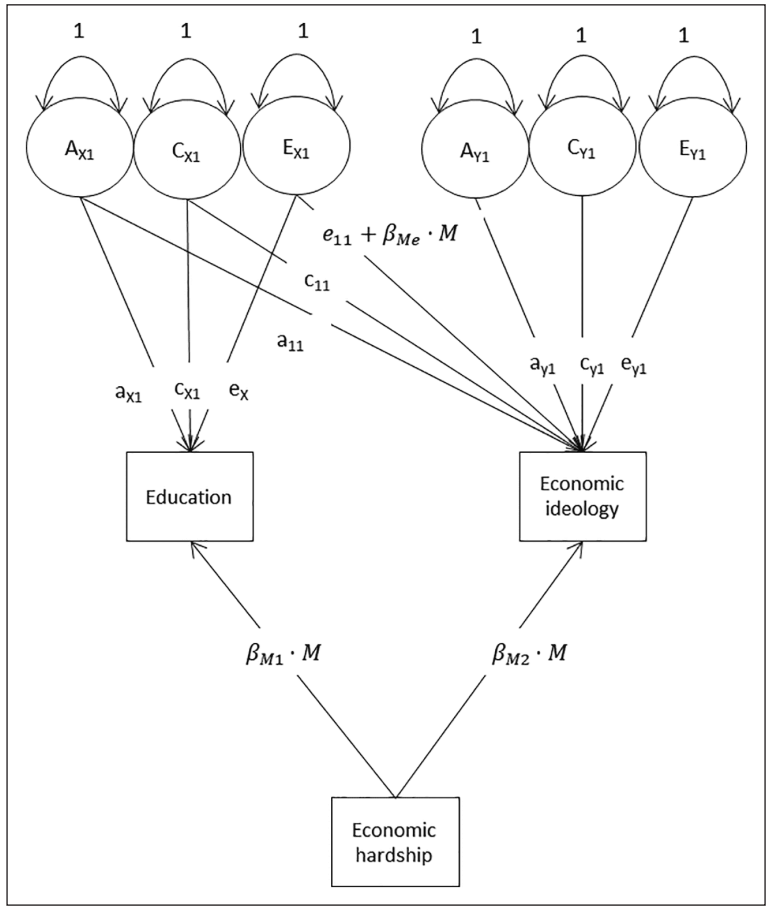

Figure 2. The Effect of Education on Economic Attitudes as a Function of Local Economic Hardship.
*Only shown for one twin.

Just as in an OLS regression, it is important to include all constitutive terms when we estimate the interaction model (Brambor et al., 2006; Purcell, 2002). This means that when we calculate the moderation effect, we also include the regression of the moderator on both education and political ideology ($\beta_{M1} \cdot M$ and $\beta_{M2} \cdot M$ in Figure 2). If we do not do this, we risk running a misspecified model. In the vocabulary of behavior genetics, we might risk confusing a gene–environment *correlation*[8] with a gene–environment *interaction*. If we include the paths $\beta_{M1} \cdot M$ and $\beta_{M2} \cdot M$, we are not conflating these two important concepts. Or, as put by Purcell (2002: 563) after including these paths, "Any interactions detected will not be due to r_{GE} [gene–environment correlation], but rather will be interactions between the moderator and variance components specific to the trait." Obviously, this does not rule out the possibly that respondents can self-select into their environments in general, which is a potential source of bias in our estimation. More to the point, competitiveness is *not* exogenously imposed, which potentially limits our ability to draw strong causal inferences regarding this moderating relationship. Previous studies have demonstrated that genes associated with educational attainment are also associated with various economic measures such as social mobility (Belsky et al., 2018; McGue et al., 2020), labor earnings (Papageorge and Thom, 2020), inequality (Barth et al., 2020), and geographical sorting (Laidley et al., 2019). Ultimately, we believe it will be valuable for future studies to replicate the interactions examined here in other samples and contexts in order to make sure that the interaction effects are robust.

Apart from the discussion of how to model interactions using twin data, there is also the causal issue of *confounding*, which we discussed above but which has some additional dimensions here. Above, we noted that when the exposure (here, education) is more shared than the sum of all confounders (i.e. familial upbringing and genetic predispositions), the within-twin pair estimate of the quasi-causal effect is less biased than a cross-sectional estimate. Non-shared confounding *for our moderator* is still a potential problem since we want to investigate whether the within-twin pair effect of education on economic ideology is *moderated* by municipal competitiveness. To minimize the potential pitfalls of non-shared confounding for our moderator (Frisell et al., 2012; Rasmussen et al., 2019), we only include twin pairs who reside in the same municipality. If we do not, we are comparing apples to oranges.[9]

Measures

Our measures of local economic hardship are based on registry data from the Ministry of Social Affairs and the Interior from the year 2009 and measured at the municipal level.[10] Most constructs are self-explanatory: The higher the land value and income tax revenue, the less economic hardship, while high unemployment indicates economic hardship. The socio-economic index represents a municipality's social expenditure and is a function of the number of psychiatric patients, people on welfare benefits, single-breadwinner families, and so on. A higher score is an indication of economic hardship. Full definitions can be found in Supplemental Appendix 1. The measures are obviously related, but represent independent aspects of economic hardship, as illustrated in Table 2.

Results

The results from the interaction models using the four different registry-based measures of economic hardship are shown in Table 3. We find that educational effects on economic ideology are moderated by context.

Table 2. Correlations Between Measures of Economic Hardship.

	(1)	(2)	(3)	(4)
Land value (1)	1.0000			
Socio-economic index (2)	–0.1614	1.0000		
	(0.000)	(0.000)		
Unemployment rate (3)	–0.5267	0.6035	1.0000	
	(0.000)	(0.000)		
Revenue from income taxes (4)	0.8029	–0.2688	–0.5963	1.0000
	(0.000)	(0.000)	(0.000)	

*p-values in parentheses.

Table 3. Results from Gene–Environment Interaction Model.[12]

	Land value	Revenue from income taxes	Socio-economic index	Unemployment rate
e11	0.071	0.075	–0.094*	–0.060
	(0.046)	(0.046)	(0.045)	(0.044)
e11 × environment	–0.046#	–0.054*	0.082*	0.078**
	(0.026)	(0.025)	(0.036)	(0.029)
Twin pairs (MZ/DZ)	456/482	456/482	456/482	456/482

Standard errors in parentheses. #$p < 0.1$; *$p < 0.05$; **$p < 0.01$.

The interaction effect is significant at the 0.05 level for three of the four measures of local economic hardship; the interaction model for land value borders significance ($p = 0.082$). The effect of education on economic ideology increases when competition over scarce economic resources increases, such as when the unemployment rate and the expenditures on socio-economic issues increase (as indicated by the positive interaction terms in the last two models). Similarly, the effect of education decreases when the local economy is prosperous, as indexed by land value and income tax revenue (as indicated by the significant negative interaction terms in the first two models). Thus, across four different indicators, the level of economic hardship in the local environment moderates the effect of education on economic ideology. We take this as strong evidence of the fact that it is the relative resource effects of education that influence economic ideology. The full effect over the range of the moderated variables is illustrated in Figure 3.

Discussion and Conclusion

Traditionally, education has been seen as one of the most important causal factors influencing democratic engagement, political ideology, and other political behaviors. A new literature has questioned the ubiquity of the causal effect of education, with the argument being that educational attainment is prone to self-selection and, therefore, that reported educational effects have been inflated due to unaccounted-for familial sources of confounding (Berinsky and Lenz, 2011; Highton, 2009; Kam and Palmer, 2008; Oskarsson et al., 2017; cf., however, Dee, 2004; Milligan et al., 2004; Sondheimer and Green, 2010). We added to this new literature by undertaking, to our knowledge, the first analysis of the causal effect of education on social and economic ideology that employs a discordant

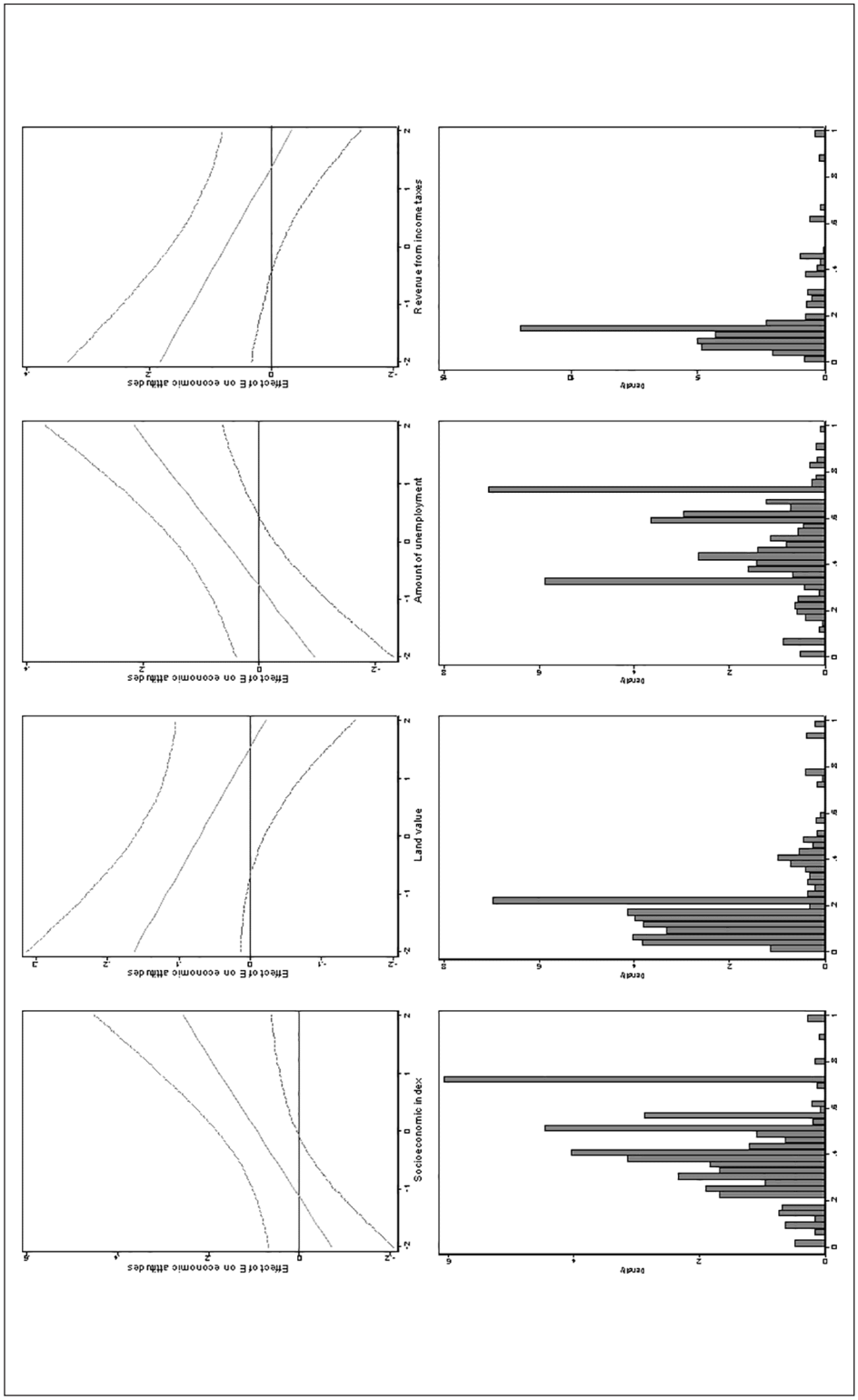

Figure 3. The Effect of Education on Economic Attitudes as a Function of Local Economic Hardship.

twin design. This design enables us to partition the familial sources of confounding into genetic and social influences. In addition, we take into account the two different mechanisms (or models) of educational effects that have been espoused in the literature: the "absolute educational model" and the "relative educational model" (Nie et al., 1996).

We showed that the educational effect on *social ideology* was fully confounded by family factors and that both genetic disposition and social factors related to the rearing environment were sources of familial confounding. We take this as an indication of the fact that absolute effects of education, perhaps also beyond the case of social ideology, may be confounded by family factors. By contrast, we could not detect significant confounding by family factors for the effect of education on *economic ideology*. With a positive educational effect on economic ideology, we analyzed whether this effect was moderated by local economic hardship. We found substantial support for an interaction effect between education and economic hardship. We note that this finding may be suggestive for future research: when the education effect on political and social behavior is due to relative resource effects, confounding by family factors may be less likely and education effects more robust, but also more context-dependent (cf. Rasmussen and Nørgaard, 2017).

Needless to say, more studies that carefully theorize and test the different effects of education will be of great value, not only to get a better grasp of which educational effects are likely to be confounded by family factors, but also to get a more nuanced understanding of exactly how education causally influences political behavior when the causal effect is robust (cf. Persson, 2015). However, the findings presented here also leave us somewhat in the dark regarding the mechanisms of family influences. It is one thing to show that both social and genetic factors confound the educational effect on social ideology, but this tells us little about how these familial influences occur. Are they due to the transmission of values and socialization from parent to offspring (cf. Eaves et al., 1997; Jennings and Niemi, 1968)?. Are cognitive factors relevant (Oskarsson et al., 2014)? Or are parental affluence and resources the most important (Mendelberg et al., 2017)? Finally, replication elsewhere of the results we have found in a Danish context would also be worthwhile. With few social and economic barriers to education (e.g. no college tuition fees), social sorting effects are less likely and education effects more likely in the Danish case, but educational systems and their effects may well vary across countries.

In the collective endeavor to get more firm evidence on the causal effect of education (and the mechanisms through which it operates) on political behavior, we strongly advocate for the use of the discordant twin design. Although it does not solve all of the challenges associated with getting more reliable causal estimates, the quasi-experiment of twinning is a powerful tool to control for some of the most important, often unobserved, potential confounders that are highlighted in the literature. As this study shows, the quasi-causal effect of education does seem to influence economic but not social ideology, although the educational effect is contingent upon context. Educational effects cannot be taken for granted.

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Supplementary Information

Additional Supplementary Information may be found with the online version of this article.

Appendix 1: Descriptive statistics and phenotypic twin correlations

Appendix 2: Question wordings

Appendix 3: Additional results

Table 1: Remaining set of path coefficients for Figure 1

Table 3: Full set of path coefficients for Figure 3

Table 4: Comparison of the effect of education on economic ideology with a representative sample of the Danish population

Table 5: Comparison of the effect of education on social ideology with a representative sample of the Danish population

Appendix 4

Notes

1. It is common in the literature on the impact of education on political attitudes and behaviors to distinguish between the "absolute education model" and the "relative education model" (Converse, 1972; Nie et al., 1996; Persson, 2015).
2. The correlation between the two ideology constructs is 0.30.
3. https://www.statmodel.com/examples/genetics.shtml. This is unfortunately not possible in the gene–environment interaction model, where we estimate an interaction between an observed variable and a latent variable by employing the approach of Klein and Moosbrugger (Klein and Moosbrugger, 2000; Muthen, 2003), and we therefore use robust standard errors instead (Muthén and Muthén, [1998] 2012).
4. It is not directly possible to do this when estimating interaction if there is missing data for the observed variable, which is why there are fewer observations when estimating the interaction effect models.
5. Full question wording, as well as how the educational variable is recoded, for all variables can be found in the Supplemental Appendix.
6. In a univariate ACE model, the E component also contains measurement error. This is of course still true here, but since we are mainly interested in the covariance between the two E components, this poses less of a problem unless there is some systematic measurement error affecting both education and political ideology at the same time. The impact of measurement error in this context would thus likely be to *attenuate* the relationship found here between education and economic ideology.
7. It is of course also possible to create interaction models where the A and/or C components are moderated. Since we have no hypotheses regarding these, we only estimate models where the shared E path is moderated. This is in line with the classical approach toward estimating moderation effects in political science where only constructs with a strong expectation are included as moderators (Kam and Franzese, 2007).
8. It would go beyond the scope of this article to fully discuss these two concepts, but a brief definition of gene–environment correlation is in order. In this context, gene–environment correlation refers to whether there are "genetic influences common to a trait and some other variable that could be considered the environment in which the trait occurs" (Johnson, 2007: 428). Gene–environment interaction is discussed above.
9. We might of course be worried about whether the moderated effect of education on economic ideology is different if we only focus on municipalities where both twins reside. This is not the case: The quasi-causal effect for this subset of twins is almost identical to the effect estimated in Table 1, with an estimated effect size of 0.113 [0.028–0.199] versus 0.092 [0.030–0.155] in Table 1.

10. http://www.noegletal.dk/. The municipal address is based on registry data from 2009, and some might have moved in the intervening period between 2009 and 2012. If we find the expected results in the face of this measurement error, it is strong evidence that the relationship is in fact present.
11. The remaining set of results are in Supplemental Appendix 3.
12. Full set of results shown in Supplemental Appendix 3.

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