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REPLICATION STUDY

It Matters What and Where We Measure: Education and Ideology in a Swedish Twin Design

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Abstract

Research on the link between education and political ideology is likely affected by biases common in conventional observational methods. A study by Rasmussen et al. (2021) addresses this problem by examining social and economic ideology in a Danish discordant twin design, finding that education shows positive causal effects on economic, but not social, conservatism. In this paper, I provide a set of replications of these results using a dataset of genotyped Swedish twins. I complement this by using random variation within fraternal twin pairs in a polygenic index of education. Results differ markedly from the original study, but are also shown to be sensitive to precise definitions of the ideological dimensions and which sub-dimensions or items are included. Overall, more care may be warranted when empirically defining ideology. Additionally, educational effects on ideology are likely to be sensitive to particular characteristics of the educational experience across time and space.

Keywords: twin design; polygenic index; economic ideology; social ideology; education

Introduction

The relationship between educational attainment and ideology has received considerable attention in research on political preference formation (Fong, 2001; van der Waal et al., 2007). Following the growing realization that traditional correlational methods commonly used across the social sciences often suffer from a substantial amount of bias (see e.g., Angrist and Pischke, 2010) and that political preferences are no exceptions (Ahlskog and Oskarsson, 2023), these questions are now increasingly being scrutinized with methods that have better causal validity.

This article has earned badges for transparent research practices: Open Data and Open Materials. For details see the Data Availability Statement.

In this vein, Rasmussen et al. (2021) investigate the issue in a Danish setting using discordant twin models. In addition to the more rigorous causal test, they also make the crucial distinction between social and economic ideology: whereas economic ideology captures traditional distributional issues like taxation and redistribution, social ideology captures moral and cultural issues that are often orthogonal to economics (Feldman and Johnston, 2014). The main findings in the study are that education is *not* causally related to social ideology, but indeed to economic ideology, such that education increases the degree of economic conservatism (though the effect size is a modest 0.09).

In this study, I attempt to replicate the main results from Study 1 in Rasmussen et al. (2021) in an older Swedish cohort. This replication therefore concerns the *main effect of education on measures of social and economic ideology, using a discordant twin design*. The original study authors also proceed to decompose the confounding using bivariate ACE models and investigate heterogeneity of the effect on economic ideology across levels of competition for resources in Study 2. These analyses are not replicated here.

I will attempt to provide a test that is as close as possible given the available data (the "closer" replication). However, apart from the country setting and the age of the sample, the specific survey items used do differ somewhat. In addition to the closer replication, I also perform conceptual replications using (a) alternative measures of social and economic ideology that are arguably more precise given the available data and (b) polygenic indices (PGIs) of education within fraternal twin pairs, capturing the causal effect of education-linked genetic factors. While the discordant twin design is a large step up from traditional methods, it is still liable to be biased by confounding factors in the twins' unique environment. This is not true of the PGI design, since genetic differences between fraternal twins are random, giving it more robust causal characteristics.

The results in this older Swedish cohort differ from the original Danish study. First, the closer replication shows that education is not significantly related to either social or economic ideology. However, using the alternative measures gives much clearer results: education is significantly negatively related to social conservatism but *not* to economic conservatism. These results are reproduced with the PGI method, providing an even stronger case for a causal interpretation. Further analysis shows that results are dependent on which items or subdimensions are included in the measures of social and economic ideology. Rather than verifying or falsifying the original study, I argue that (a) we should pay more attention to precise conceptual nuances involved in operationally defining ideological constructs and (b) educational effects on ideology are likely to be dependent on differences across time and space in what the educational experience actually entails.

Data and methods

The data in this study consist of a large sample of twins from the Swedish Twin Registry (Zagai et al., 2019). Sample sizes range between 1,682 and 3,182 depending on method and outcome, which provides high statistical power (see online appendix) (Ahlskog, 2023).

I use two complementary methods as well as two different ways of measuring social/economic ideology. The methods are, first, the discordant twin model, which relies on differences in actual education between identical (MZ) twins (Vitaro et al., 2009), and second, models using PGIs, which rely on differences in direct genetic measures of education[1] between fraternal (DZ) twins. Whereas the first method can rule out all confounding that is shared between identical twins (i.e., shared environmental factors and genetics), it can still be confounded by unique environmental factors. The within-family PGI method, on the other hand, can plausibly obtain causal estimates of the effect of education-linked genetics, but these effects may also be transmitted by other mechanisms. More information about these methods and how they are implemented can be found in the online appendix.

The measures of ideology in this replication study were constructed using the SALTY survey from the Swedish Twin Register. The SALTY survey was fielded in 2009–2010 to a set of twins born between 1954 and 1958, making the sample older than the one used by Rasmussen et al. (2021), which was fielded at about the same time but to twins born between 1970 and 1989. Apart from possible generational differences between the samples that are theoretically relevant (these are discussed in more detail under Discussion), the fact that the Swedish sample is older at the time of measurement may also matter methodologically: in light of the generally observed trend for heritability to increase over the lifespan (e.g., Hatemi et al., 2009), the PGI models may be more powerful in an older sample than in a younger sample. The age also makes sure that possible education effects have had ample time to manifest (see Discussion). The survey contains a battery of 34 political preference items (1–5 Likert items on to what extent the respondent agrees with a certain political proposal), which is used to construct the measures of economic and social ideology.

The outcomes are defined in two different ways. First, I use measures that are intended to be as close as possible to the original study given the available data. Second, I construct measures that are arguably more precise given other items that are available in the Swedish sample and using a more flexible way of aggregating these items. The precise considerations as well as details on scale construction are available in the online appendix.

Rasmussen et al. (2021) use a survey-based measure of education, whereas I have access to educational attainment reported in validated register data from the LISA database from Statistics Sweden. LISA contains education data according to the Swedish Educational Nomenclature, but has here been transformed to years of education. In general, we should expect register-based measures to have less noise and therefore provide better precision.

Descriptive statistics for all variables are available in the appendix. In all analyses, all variables except sex are standardized (mean zero, standard deviation one) to make direct comparisons of coefficients with the original study possible. Main analyses are done in Stata 15.1, whereas Bayes factors are calculated using the R package BayesFactor with an "ultra-wide" prior of $\sim \mathcal{N}(0, \sqrt{2}/2)$. All tests are two-tailed, and results are considered statistically significant at $p < 0.05$, but

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[1]The PGI I use is a single-trait index of education-linked alleles from the Polygenic Index Repository Project (Becker et al., 2021).

Table 1. Discordant twin models, MZ twins

Variables	(1) Orig. EI	(2) New EI	(3) Orig. SI	(4) New SI
Education years	−0.0295	−0.0117	−0.0671	−0.137***
	(0.0520)	(0.0545)	(0.0483)	(0.0415)
Constant	0.0533***	0.0521***	0.0469***	0.0438***
	(0.00468)	(0.00587)	(0.00434)	(0.00402)
Observations	1,852	1,682	1,908	1,824
R^2	0.696	0.731	0.737	0.794
Twin pair FE	Yes	Yes	Yes	Yes

Note: EI = economic ideology, SI = social ideology. Robust standard errors in parentheses. All variables are standardized. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Bonferroni-adjusted for eight main tests (i.e., $p < 0.00625$): four discordant MZ models and four PGI models.

Results

The closer replication results can be found in Table 1, columns 1 and 3. The original study estimated the effect of education on economic ideology to 0.092 (i.e., a positive effect on the degree of economic conservatism). Here, the coefficient in column 1 is instead negative, at −0.03, but not statistically significant ($p = 0.570$) and with the evidence strongly favoring the null ($BF_{10} = 0.045$). In the Swedish data, we therefore get null results with regards to economic ideology. When looking at social ideology, the original study did not find a significant relationship, with a point estimate of 0.008. Looking at the Swedish results in column 2, we see a nonsignificant coefficient of −0.067 ($p = 0.165$), with moderate evidence favoring the null ($BF_{10} = 0.119$).

Moving to the new ideology measures aimed at more precisely capturing the latent constructs, we can see in columns 2 and 4, Table 1, that the situation is different. Here, we instead see a robust negative coefficient for social conservatism at −0.137 ($p = 0.001$, significant after Bonferroni correction), and with strong evidence favoring H_1 ($BF_{10} = 16.1$). The result for economic ideology is still a nonsignificant −0.0117 ($p = 0.829$), with strong evidence favoring the null ($BF_{10} = 0.04$).

There are thus two surprises: using the "original" measure of economic ideology does not show any results, whereas the new measure of social ideology does. This warrants digging into the individual items for these two scales in Table 2. By comparing the magnitude of these coefficients, we can get a rough picture of which items are driving the (non)results. What stands out in the first case is that the taxation and economic inequality items have opposite signs, whereas the remaining coefficients are all close to zero. Lower education thus, if anything, appears to be associated with wanting to cut taxes, but also wanting less income inequality. The

Table 2. Discordant twin models, MZ twins, individual items

Approximation of original items on economic ideology		
Decrease the public sector	−0.0349	(0.0562)
Decrease social welfare	0.0219	(0.0516)
Give companies more freedom	−0.0242	(0.0508)
Taxes should be cut	−0.1078**	(0.0479)
Decrease economic inequality (reversed)	0.0887	(0.0617)
New measure items on social ideology		
Give school grades at younger age	−0.0191	(0.0472)
Ban pornography	0.0421	(0.0448)
Limit access to abortion	−0.0304	(0.0448)
Introduce much harder punishment for criminals	−0.0798*	(0.0444)
Instate a language test for Swedish citizenship	−0.0829*	(0.0485)
Admit fewer refugees	−0.1236***	(0.0404)
Allow more skilled immigration (reversed)	−0.1187***	(0.0444)
Increase assistance for immigrants' culture (reversed)	−0.1035**	(0.0482)
Decrease defense spending (reversed)	0.0611	(0.0486)

Note: All variables are standardized. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

lack of an observed effect for the complete scale could therefore be a consequence of these two items cancelling each other out. Moving instead to the new social ideology items, it appears that the results for this measure are mainly driven by the items on multiculturalism and migration.

Finally, the alternative method of getting at causality by using random variation in genetic factors within DZ twin pairs is presented in the PGI models in Table 3. As before, columns 1 and 3 contain results using the closer replication measures, whereas columns 2 and 4 contain results using the new measures. When using the polygenic index instead of the trait measure of education, all results now align with the discordant MZ twin results using the new measure: no effects on economic ideology ($p = 0.537/p = 0.896$ for the original/new measure, with strong evidence favoring the null: $BF_{10} = 0.033/BF_{10} = 0.031$) and negative effects on the degree of social conservatism. The size of the effect on social ideology is somewhat larger with the new measure (-0.118, $p = 0.0003$ with the original measure vs. -0.140, $p = 0.00004$ with the new measure, both significant after Bonferroni correction) and with either strong or "extreme" evidence in favor of H_1 ($BF_{10} = 25.7/BF_{10} = 351.2$).

Discussion

The first set of replication results was intended to be as close as possible to the original study, and turned out similar (i.e., nonsignificant) results for the social

Table 3. PGI models, DZ twins

Variables	(1) Orig. EI	(2) New EI	(3) Orig. SI	(4) New SI
PGI EA	0.0196	0.00449	−0.118***	−0.140***
	(0.0318)	(0.0342)	(0.0327)	(0.0338)
Sex of twin	−0.166***	−0.0998**	−0.132***	0.0232
	(0.0454)	(0.0480)	(0.0451)	(0.0450)
Constant	0.0521**	−0.000906	0.0321	−0.0472**
	(0.0242)	(0.0254)	(0.0244)	(0.0240)
Observations	3,022	2,674	3,182	3,032
R^2	0.634	0.637	0.629	0.637
Twin pair FE	Yes	Yes	Yes	Yes

Note: EI = economic ideology, SI = social ideology, PGI EA = polygenic index of educational attainment. All variables except sex are standardized. Robust standard errors in parentheses. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

ideological dimension, but also did not find any results for the economic dimension, with strong or moderate evidence for the nulls. Upon close inspection, the lack of results for economic ideology may stem from respondents with lower education both being more in favor of cutting taxes (without being more in favor of cutting spending), while also being more opposed to income inequality. Speculating on this, it might be that more education is simply associated with a better understanding of the connection between taxation and public spending/redistribution.

When instead using measures of ideology based on a wider selection of survey items and a more careful aggregation, the picture is slightly different: still no results for economic ideology, but a negative coefficient for social conservatism. These results for social ideology were furthermore mostly driven by items related to multiculturalism and migration. This raises the question (as noted by an anonymous referee) about the underlying factor structure and whether these issues should be treated as a dimension of its own.[2]

Results using the "new" measures were also confirmed in the PGI analysis, providing reasonable grounds for claiming that these effects are, in fact, causal. Furthermore, results using the original and new measures converge in the PGI models, suggesting that some factor in the unique environment may suppress the relationship between education and social ideology in the MZ design. If nothing else, it underscores the utility of the within-family PGI method as a complement to existing observational methods in strengthening the case for causal interpretation.

In summary, positive effects on economic conservatism in the original Danish sample are replaced by negative effects on social conservatism in the Swedish one. One can only speculate on why. The two countries are generally thought to be very

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[2]Indeed, a three-factor structure provides somewhat better goodness of fit. Ultimately, this comes down to deciding squarely on a theoretical definition of social ideology, which is outside the scope of this paper.

similar in terms of political and economic structure, being strong Scandinavian welfare states with similar histories. A few differences are that Swedes may be more in favor of increased economic redistribution than Danes (Dinesen et al., 2020), while also being much less hostile to immigration (Czaika and Di Lillo, 2018).

I argue that the best explanation is simply the difference in birth cohorts: 1943–1958 here vs. 1970–1989 in the Danish cohort. The hypothesized mechanism for social ideology in Rasmussen et al. (2021) is the socializing effect of education. A substantial portion of the Swedish sample would have gone through their higher education at the apex of the "student revolts" of the late 1960s/early 1970s (Östberg, 2008) with high exposure to radical left-wing ideology. Additionally, in a setting with such widespread pro-immigration sentiment as the Swedish case, this could arguably magnify the socialization effects of education. The Swedish case can thus be viewed as a most-likely case for finding education effects on social ideology.

To understand why this appears to have not affected economic ideology, we can consider the main proposed mechanism from Rasmussen et al. (2021), namely resources: education brings income. Resource effects would therefore work in the opposite direction. The socializing effect of education would have made the Swedish sample both more economically and socially left-wing, but the resource effect would counteract and "neutralize" the impact on economic ideology. The age of the sample also implies that there has been ample time for these resource effects to manifest, as the twins advance in their careers.

Ultimately, whether the differences between this study and Rasmussen et al. (2021) are due to country, age or cohort characteristics, or merely due to differences in survey items, it does appear that the original findings do not generalize very far. It is also evident that results hinge on precisely which policy preferences are used to operationalize the ideological dimensions. The first main takeaway is therefore that we need to be careful in how we define ideological dimensions and which subdimensions we choose to include. In addition to this, educational effects ideology is likely dependent on the characteristics of the educational experience itself.

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