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Does university make you more liberal? Estimating the within-individual effects of higher education on political values

Ralph Scott

Department of Politics, School of Social Sciences, Arthur Lewis Building, University of Manchester, Manchester, M13 9PL, UK

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ABSTRACT

An individual's level of education is increasingly significant in explaining their political attitudes and behaviour, with higher education proposed as a new political cleavage. However, there is limited evidence on the causal effect of university on political attitudes, due to self-selection into educational pathways. Addressing this gap, this article estimates the change in political values that occurs within individuals who graduate from university by applying longitudinal modelling techniques to data from the 1970 British Cohort Study, overcoming the selection problem by accounting for time-invariant confounding. It provides the first causal estimate of higher education specifically, finding that achieving a degree reduces authoritarianism and racial prejudice and increases economic right-wing attitudes. This has important implications for the study of politics: as populations become more highly educated on average, we should expect continuing aggregate value change towards lower levels of authoritarianism and racial prejudice, with significant consequences for political behaviour.

1. Introduction

Within established democracies, an individual's level of education is increasingly significant in explaining their political attitudes and behaviour (Norris and Inglehart, 2019), with some suggesting that higher education in particular could represent a new political cleavage in Western Europe (Ford and Jennings, 2020). One potential explanation for this observed effect of higher education is through changes to political values. Studies have shown that these values are valid consistent constructs that vary by education level (Evans et al., 1996) and are more often influential on party identification than the other way around (Evans and Neundorf, 2018). This analysis focuses on three such values: racial prejudice, understood as hostility towards racial out-groups (Kinder and Kam, 2010, p. 8); authoritarianism, or support for social order over individual liberty (for example, the death penalty or harsh sentences for criminals); and economic Left-Right values which captures views on inequality and the role of the state in the economy (Evans et al., 1996).

Competing explanations are given for the particular effect of higher education on these different values, whether it is the graduate premium leading to higher earnings, and so lower support for redistribution (Surridge, 2016); the liberalising influence of faculty and university culture (Dey, 1996); the impact of peer socialisation in what remain relatively elite institutions (Mendelberg et al., 2017); or the effects of increased cognitive sophistication (Gelepithis and Giani, 2022). However, given self-selection into educational pathways (Persson, 2015), an important first step is to determine whether this difference is causally attributable to university. Previous studies have attempted to address this selection problem by leveraging exogeneous changes in educational participation attributable to variation in policy regimes (Bullock, 2021; Cavaille and Marshall, 2019; Marshall, 2016) or randomised encouragement designs (Sondheimer and Green, 2010). However, these well-identified studies do not estimate the specific effect of higher education, instead tending to focus on the effect of increased years of secondary education.

It is this gap that this article addresses, adopting a longitudinal approach to estimate the change in values that occurs within individuals who graduate from university. Specifically, it applies two-way fixed effects and linear mixed effects models to data from the 1970 British Cohort Study to account for all time-invariant confounding, finding that achieving a university degree reduces an individual's authoritarianism and racial prejudice and makes an individual more economically right-wing. In so doing, it makes three main contributions. Firstly, it provides the first causal estimate of the effect of university specifically on these values. Second, it does so for a validated multi-item scale for authoritarianism for the first time, which matters given the growing significance of this 'second' dimension for contemporary politics. Third, it extends the previous causal literature in this area by providing an

E-mail address: ralph.scott@manchester.ac.uk.

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estimate of the effect of higher education among a more nationally representative sample, therefore allowing us to be more confident in drawing general conclusions. The findings have important implications for the study of public opinion and political attitudes, as it suggests that as the population becomes more highly educated on average, we should expect current trends of aggregate value change towards lower levels of authoritarianism and racial prejudice to continue, with significant consequences for political behaviour.

2. The significance of political values

From an initial position of scepticism about the existence of underlying, consistent attitudes among much of the voting public (Converse, 2006), there is now broad acceptance within political science that many individuals hold essentially consistent underlying positions in terms of racial prejudice, left-right economic and liberal-authoritarian social values throughout their adult lives (Evans et al., 1996; Sears and Levy, 2013). In part, this change in perspective can be attributed to the difference between searching for stability in individual survey items, which do demonstrate inconsistency partly attributable to measurement error, and stability in the latent constructs which selections of these items are designed to estimate (Ansolabehere et al., 2008).

Existing empirical evidence on political socialisation suggests that attitudinal orientations are developed early on in life but are subject to change based on early adult and maturation experiences, becoming relatively stable from that point on. Longitudinal studies, such as the Political Socialisation Project, find considerable consistency within individuals over the life course across issues including prejudice against other races and the role of trade unions, with values particularly stable after early adulthood (Stoker and Jennings, 2008). Similarly, analysis of the General Social Survey in the US finds that, after the age of 30, individuals demonstrate a high degree of consistency over time even at the item-level (Kiley and Vaisey, 2020), although there are exceptions due to period effects (for example, on the issue of gay marriage). Importantly, in the British case at least, research designed to disentangle the relationship between these 'core values' and party identification through cross-lagged modelling suggests that the causal direction is from values to party identification, with values possessing twice the amount of stability (Evans and Neundorf, 2018).

To discuss the specific values sets under study here, racial prejudice describes a general predisposition to favour perceived racial in-groups and oppose out-groups, measured through scales addressing out-group stereotypes, for example, how close an individual feels to people of different ethnicities (Kinder and Kam, 2010). So measured, the construct demonstrates a high degree of within-individual validity and stability over a lifespan. As described by Fleishman (1988), the other two attitude sets of interest were derived after a single dimension of political values proved insufficient in explaining variation among the general population. While the survey items used varies, in the British case the economic Left-Right scale measures an individual's degree of support for free enterprise or state involvement in the economy, based on statements such as "big business benefits owners at the expense of workers", while the social liberalism-authoritarianism scale measures attitudes towards liberty and individual rights, such as "people who break the law should be given stiffer sentences" (Evans et al., 1996). Factor analysis models have validated this structure of political values (Fleishman, 1988; Heath et al., 1994), and position on each of these scales is also shown to be stable in individuals over time. While some treat racial prejudice and authoritarianism as part of the same 'cultural values' construct (Fleishman, 1988), following Sobolewska and Ford (2020) I treat it separately, and justify this through Confirmatory Factor Analysis, presented in appendix 6 in the supplementary material.

This validity and within-person stability makes understanding the role of higher education in the formation of these values important in its own right: if an effect is present once accounting for selection, then university attendance plays an important role in an individual's political socialisation, with implications for values formation in the aggregate given increasing participation rates. However, additional motivation is provided by the importance of these values for understanding contemporary political behaviour. Analysis of the British Election Study finds that party competition is increasingly taking place more along the social liberal-authoritarian and racially prejudicial axes rather than the traditionally dominant economic Left-Right axis (Fieldhouse et al., 2020), with the electoral 'shock' of Brexit accelerating a pattern of realignment across these values axes. Furthermore, decompositional analysis suggests that the relationship between education and vote choice is primarily mediated by its effect on cultural values (Simon, 2021).

This underlines the importance of understanding whether higher education is truly driving the apparent differences in these values, or whether the observed relationship is a result of selection. If education itself plays a role, then the increasing rate of participation in higher education will have significant implications for both political values and behaviour in the future; if the effect is attributable to selection, then we must look to other social divides to explain the changing basis of party competition.

3. Theoretical perspective and hypotheses

3.1. Education effects

The important influence of early adulthood socialisation on these values raises the question of the role that education might play in their formation. This section discusses the evidence for the relationship between education and each value set in turn, drawing on this to formulate hypotheses to be tested in the analysis.

3.1.1. Racial prejudice

In general, those with higher levels of education demonstrate lower levels of racial prejudice. This is attributed by Kinder and Kam to the "knowledge, principles, and experiences that together act as a counterweight to the 'natural' inclination toward prejudice" (Kinder and Kam, 2010, p. 35). Cross-cultural analysis of the World and European Values Surveys by Drazanová (2017) indicates this appears to be the case across contexts, finding a strong effect of education on social tolerance (defined in opposition to prejudice), although this does depend on the strength of the liberal-democratic tradition in the country.

Research designs aimed at estimating a causal effect also tend to draw these conclusions, while investigating racial outgroup prejudice and related constructs such as opposition to immigration. A series of studies applying fuzzy regression discontinuity designs (RDD) to European Social Survey data, where policy changes to the compulsory education age are used to instrument for the effect of education, reinforce these findings. Focusing on racial prejudice, Gelepithis and Giani (2021) find significant reductions in prejudice among the ethnic majority as a result of the extension to the secondary-school leaving age. Looking instead at anti-immigration attitudes, Cavaille and Marshall (2019) find that increased years of schooling reduce overall anti-immigrant sentiment, and Nunziata and d'Hombres (2016) draw similar findings from an equivalent research design across a broader range of European countries. In line with the conclusions of Hainmueller and Hopkins (2014) both analyses conclude that this effect is not purely attributable to the economic benefits of higher levels of education (placing the more highly educated at less risk of labour market competition from migrants), but also due to changes in cultural outlook.

However, other studies contradict these findings. Using a similar RDD research design, but with a more focused case study of Norway, Finseraas et al. (2018) find no significant effect of increased years of education on attitudes towards immigration. Lancee and Sarrasin (2015) also dispute a causal role for education in reducing opposition to immigration through a longitudinal panel design, finding little variation within individuals over time, even during educational transitions (ie

progressing to university), suggesting instead that the observed differences are attributable to selection effects. In addition, while well-identified, it is worth noting that the RDD studies discussed here are limited by variable and relatively weak first stage results, with the extension of the compulsory school age by one year generally leading to an increase in school participation of around four months on average. This suggests the treatment effects identified by these studies may be quite local to those who would otherwise have left education at a young age.

With all of this in mind, we can formulate the following hypothesis to be tested in the analysis:

H1. achieving a university degree leads to a reduction in racial prejudice.

3.1.2. Authoritarianism

The observed relationship between education and authoritarianism can be traced back at least as far as Lipset, who in seeking to understand high levels of authoritarianism among members of the working-class, argued that this sprung from "greater suggestibility, absence of a sense of past and future, inability to take a complex view, difficulty in abstracting from concrete experience, and lack of imagination" Lipset (1959, p. 492) resulting both from lower levels of education and a more precarious economic position.

Scholars since have reinforced this finding of a negative relationship between education and authoritarianism. For example, Surridge (2016) accounts for numerous possible selection effects in analysis of the 1970 British Cohort Study, and finds a large reduction in authoritarianism among graduates. In addition, given that the liberalising effect of higher education differs by the degree subject of study (with humanities and social science students the most liberal), she concludes that the change can be attributed to education. Through a similar design (albeit adjusting for fewer confounders) but also analysing data from the prior 1958 birth cohort study, Paterson (2009) draws the same conclusions.

Despite this precedent, there are relatively few analyses designed to produce a causal estimate of the effect of education specifically on authoritarianism, with those that do exist focused more on related issues. An early example is provided by Dee (2004), where the effect of more years of schooling is instrumented through the introduction of restrictions on child labour and the proximity of two-year colleges, drawing the conclusion that more education leads to greater support for freedom of speech. In their study, Nunziata and d'Hombres (2016) find that those who received more years of secondary education were more likely to express liberal attitudes on sexuality. Campbell and Horowitz (2016) account for the effects of family background by comparing sibling pairs where one is a university graduate and the other is not through Study of American Families data, finding that graduates are significantly more supportive of civil liberties and gender equality.

Other research has investigated the effect of education on related values through panel designs. For example, through analysis of the Citizenship Education Longitudinal Study, Janmaat (2018) finds that those who pursue an academic route for post-16 education develop higher levels of support for liberal democratic principles (including freedom of speech and tolerance of others). In their study of changes in moral dispositions using panel data from the National Study of Youth and Religion, Broćić and Miles (2021) find that graduates in the humanities and social sciences become more morally progressive during the course of their studies, for example, being more likely to agree that moral principles should be updated based on societal changes, while also finding similar differences in terms of a greater regard for the individual over social order (albeit with a weaker design). Yet the issue of there being no study which provides a causal estimate of the effect on a validated authoritarianism scale remains.

Still, based on this we can propose the following hypothesis:

H2. achieving a university degree leads to a reduction in authoritarianism.

3.1.3. Economic left-right values

Finally, previous research also identifies a relationship between education and economic values, with graduates tending to be more economically right-wing. Weakliem (2002) demonstrates this association from analysis of World Values Survey data, concluding that the effect of education is to make a person more individualistic, rendering them opposed to higher taxation and a bigger role for the state in the economy, but more supportive of an individual's right to choose in social matters, as discussed above. Yet this analysis does not address the selection problem, not least as in most jurisdictions we would expect the wealthier to be both more likely to hold economically right-wing views and to have the opportunity to continue in education.

Surridge (2016) finds that this relationship persists while adjusting for confounders including an individual's family background and their pre-university values. She also suggests that this difference between graduates and non-graduates is in large part attributable to the socio-economic position enabled by higher education, as once the mediators of income and occupation are accounted for the relationship is substantially weaker. In contrast, drawing on the European Social Survey to provide a more comparative analysis, Gelepithis and Giani (2022) emphasise the importance of ideas in explaining the effect of education on economic values, as they find the effect remains even when accounting for income and labour market position. They identify this effect as being specifically attributable to higher education, and echo Weakliem in concluding that: "university education fosters norms of inclusion, while eroding norms of solidarity" (2022, p. 2).

A series of studies using research designs appropriate for determining causal effects reinforce this finding. Using legal changes in compulsory schooling in the US as an instrument, Bullock (2021) finds that more years of secondary schooling makes an individual more economically right-wing. Marshall (2016) employs a fuzzy RDD design on British Election Study data to estimate that each additional year of schooling leads to an increase of 12 per cent in probability of voting Conservative, which, as it appears to be mediated by increases in income, can be considered a good proxy for right-wing economic values. Finally, a study of US college students using a panel design with matching (Mendelberg et al., 2017), finds that those attending colleges with a critical mass of affluent students became more economically right-wing during their time there (addressing numerous other potential explanatory factors), an effect they attribute to norm socialisation.

We can therefore propose the following hypothesis:

H3. achieving a university degree makes an individual more economically right-wing.

3.2. Selection effects

Despite this foregoing evidence for education effects, an alternative argument is that the observed differences are more attributable to selection (Lancee and Sarrasin, 2015). Drawing on the approach of Surridge (2016) and other literature we can hypothesise selection effects that may be confounding the relationship between education and values, such as differences in demographics, family background or cognitive skill. For example, the difference in values could be explained by compositional differences by gender and ethnicity in who attends university; parental occupation, income and education; or indeed the cognitive selectivity of higher education, as some have argued that: "when considering social, moral, and political situations, those with greater cognitive skill are able to form more individualistic and open-minded (i.e. anti-authoritarian) attitudes than those of lesser cognitive ability" (McCourt et al., 1999, p. 987). It is therefore important to ensure that the effect of education identified in this analysis is robust to these influences, by accounting for them where appropriate in the estimation.

4. Methodology

4.1. Estimand

Lundberg et al. (2021) argue that every quantitative study should explicitly state its estimand, that is, the target quantity of the research. Doing so provides a clear link from the theory through to the observed data and estimation strategy, which enables identification of this quantity under a set of assumptions. In this spirit, let it be clear that the theoretical estimand of this study is the causal effect of graduating from university on each of authoritarianism, racial prejudice and economic Left-Right values, as set out in the hypotheses.

As the focus is on the within-individual change among those who achieve a degree, this treatment effect can be considered the Average Treatment Effect on the Treated (ATT), which within the potential outcomes framework assumes that "the average level of the outcome under the control for those in the treatment is equal, on average, to the average level of the outcome under the control for those in the control group" (Morgan and Winship, 2015, p. 159). When applied to causal analysis of panel data, as in the Difference-in-Difference (DiD) approach to estimation, this is known as the parallel or common trends assumption (Angrist and Pischke, 2008, p. 171). This assumes the trajectory of those who receive treatment would have otherwise been similar to those in the control condition, with the difference in these trajectories being the estimate of the causal effect. A relaxation of this assumption is the conditional parallel trends assumption (Callaway and Sant'Anna, 2021), which suggests that if the two groups appear to follow similar trends once conditioned on pre-treatment confounding variables, there is no threat to causal inference.

As will become clearer in the discussion of the data and methods, the empirical estimand of this study is the difference in the change in these values (the 'within-individual' effect) between those who do and don't graduate from university, conceptualised as the treatment. This estimation will account for the effects of selection, in addition to adjusting for the influence of pre-treatment confounders in some specifications. This will be the total, unmediated effect of holding a university degree, which could be attributable to any number of the explanations provided by previous research on the topic, whether social and economic position post-treatment, norm socialisation during study, or the effect of learning on values. However, crucially, the contribution of this analysis is to robustly separate out the effect of selection from that of the treatment itself.

4.2. Estimation strategy

In a review of the evidence on the effect of education on political participation, Persson (2015) makes plain that the difficulty of randomly assigning individuals to different educational paths often entails the use of sophisticated methods for causal inference with observational data to produce a less biased estimate of the effect of education (although see Sondheimer and Green (2010) for examples of an experimental approach). In an attempt to do so through a longitudinal mixed model using the Swiss Household Panel, Lancee and Sarrasin (2015) dispute a causal role for education in reducing opposition to immigration, finding little variation within individuals over time, even during educational transitions (ie progressing to university), suggesting instead that the observed differences are attributable to selection effects. This is a significant finding, as if the effect of higher education is reducible to selection, then we would not expect increased participation in university to affect political attitudes in the aggregate. Yet the study explores a range of educational transitions at once, with only 7 per cent of cases attending university, meaning that these estimates may lack sufficient power to determine a precise null effect.

To discuss the models in more detail, a common approach to DiD estimation is to apply the two-way fixed effects model (TWFE), formalised as follows:

$$y_{it} = \alpha_i + \gamma_t + \beta x_{it} + \varepsilon_{it} \quad (1)$$

Here the subscripts i and t signify individuals and timepoints respectively, while y is the dependent variable. As described by Imai and Kim, the average within-individual change attributable to a time-variant treatment (represented by βx_{it}) is estimated by including fixed effects for both the unit (α_i) and time (γ_t) and so the model "accounts for both unit-specific (but time-invariant) and time-specific (but unit-invariant) unobserved confounders in a flexible manner" Imai and Kim (2020, p. 2). The result is that the estimated effect of the time-varying treatment is not subject to bias attributable to unobserved confounding, enabling greater confidence that is what is observed is a causal effect.

However, a downside of this approach is that it provides no information on the effect of time-invariant covariates, including the 'between' effect (ie the average differences between those who are treated and those who are not) (Bell et al., 2019). It also assumes a homogeneous treatment effect, which is liable to produce biased estimates if the effect varies across units (de Chaisemartin and D'Haultfœuille, 2020). An alternative is to adopt the hybrid approach of Lancee and Sarrasin (2015), and combine fixed and random effects in a panel design. Such a model, a version of which was first formalised by Mundlak (1978) and further developed by Bell et al. (2019) as the Random Effects Within-Between model (REWB), can be expressed as:

$$y_{it} = \mu + \gamma_t + \beta_{1W}(x_{it} - \bar{x}_i) + \beta_{2B}\bar{x}_i + \beta_3 z_i + v_{i0} + v_{i1}(x_{it} - \bar{x}_i) + \varepsilon_{it0} \quad (2)$$

The key parameter here is β_{1W}, the within-individual effect, measured as the time-specific demeaned treatment status. This estimates the change in the outcome within the individual attributable to a change in treatment condition, accounting for the effects of time (γ_t). Wooldridge (2021) has demonstrated the equivalence of this 'within-effect' and the TWFE estimator. β_{2B} is the between effect, parameterised as the individual-level grand mean of treatment, which estimates the overall difference between individuals who are and are not treated at the point of treatment, and therefore the effects of selection. The effects of additional time-invariant covariates (which are themselves between-effects) can be incorporated as fixed effects as represented by β_3. Finally, the model estimates two random effects: first, a random intercept for each individual (v_{i0} , while μ expresses the mean intercept value) and second, a random slope for the effect of treatment ($v_{i1}x_{it}$). This latter allows heterogeneous treatment effects and incorporates this variation into the estimation of the standard errors.

As shown, both models account for all unobserved time-invariant confounding and therefore selection effects. The causal identification is thereby strengthened by focusing on the change within individuals across the two treatment groups over time (less the effects of time itself). While the strength of the TWFE is its simplicity, the REWB enables two further extensions to test the robustness of the findings. First it explicitly accounts for pre-treatment differences in the outcome and observed covariates between individuals at the point of treatment. This supports the partial relaxation of the parallel trends assumption to a conditional parallel trends assumption, conditioning on those pre-treatment covariates expected to confound the relationship between higher education and the outcomes. A further test of this is presented in appendix 4 of the supplementary material, where the models are fitted on entropy-balanced data (Hainmueller, 2012), weighted so that the treated and control groups have the same values on the pre-treatment outcome and covariates, producing broadly similar results.

Second, the use of random effects parameters in the REWB does not simply model out the variance attributable to the grouping variables as in the TWFE approach, but instead models this assuming the variance follows a normal distribution (Bell et al., 2019). In the approach taken for this analysis, the mixed effects models include random intercepts for the outcome (factoring in each individual's unique starting point) and random slopes for the treatment, allowing for variation in the effect of treatment, thereby reducing bias (Long, 2011, chap. 5). In addition, the

estimation of these parameters is carried out with 'partial pooling', whereby the estimates (for example, the intercept for a given individual) recognise the dependence within the data and shrink the more extreme estimates towards the mean (Gelman and Hill, 2007).

For this reason, the results section will present results estimated through both TWFE and REWB modelling. The TWFE models are fitted using the *lfe* package for R (Gaure, 2013), while the REWB models are fitted using the *lmer* package (Bates et al., 2015b), with the parameters estimated using restricted maximum likelihood estimation with the *nloptwrap* optimiser using the BOBYQA algorithm. On fitting the models, the estimated random slope for the within-effect parameter for racial prejudice was singular, and so following various attempted modifications to the model and optimiser, this was dropped from the final model following the recommendations of Bates et al. (2015a). The standard error for this parameter estimate should therefore be interpreted with greater caution – although not overly so, given the minimal difference with the standard error for the within-effect in the entropy-balanced model which contains a random slope (in appendix 4). Further details on the models and diagnostics are available in appendix 3.

5. Data

The 1970 British Cohort Study (BCS) is a birth cohort study following approximately 18,377 people born in England, Scotland and Wales in one week in April 1970 (TNS BMRB, 2013) – the target population is adjusted at each wave due to changes attributable to migration and mortality. Since then there have been eleven waves of data collection, with data collected from the cohort members themselves, as well as their parents, midwives and schools at appropriate stages of their development. The study initially focused on childbirth and health but expanded to include a broader range of topics including political values and voting behaviour in later waves. This makes the dataset incredibly rich for longitudinal analysis, as we can observe individual change over time, with data prior to a life event not reliant on respondent recall.

The dataset also has limitations, primarily due to its nature as a longitudinal study. The BCS suffers from attrition over time, partly from natural attrition such as mortality and emigration, and partly due to non-response (Plewis et al., 2004). For example, of an expected sample of 18,377, 17,196 cohort members participated in the birth survey, 11,615 participated at the age of 16 and 9841 at 42. As can be seen in Fig. 1, due to additional effort made by the survey team on particular waves, the attrition is not monotonic. Survey elements within each wave are also prone to missingness, as not all respondents provide a valid response to each question, an issue which particularly affects the attitudes questions at 16 (as these were included on a self-completion survey). More information on the item-level missingness is presented in appendix 2.

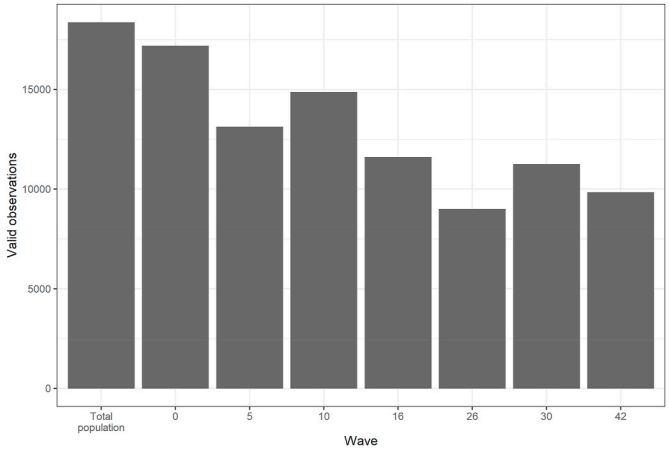

Fig. 1. Number of valid responses to the 1970 BCS at each wave.

Missing data is addressed in this study firstly by performing the analysis on a complete case dataset across the sweeps included, comprising the respondents who provided a response to all of the variables included in the analysis from birth to the age of 42, which reduces the total sample to 1520. In addition, to address potential bias attributable to systematic missingness, analysis was also performed on 75 multiply imputed datasets (N = 15,874), with the imputation carried out according to the procedure described for the British Cohort Studies by Silverwood et al. (2020). The results using the imputed data are presented alongside the main results, while more detail on the imputation approach is included in appendix 2. This multiply imputed sample is smaller than that of the original population and the sample who responded to the birth survey due to natural attrition, with cohort members who are known to have died or emigrated permanently not imputed, following the recommendation of Silverwood et al. (2020).

There are further design issues in the composition of the sample which affect interpretation: while efforts were made to incorporate new migrants to Britain during their childhood (contacting them through their school), it was not possible to do this since, meaning that the ethnic diversity of the sample is not fully representative of the target population (Elliott and Shepherd, 2006). Finally, a further restriction on generalising from these findings are the historical and geographic specificity of the sample, meaning that we can only be confident that the results relate to the specific target population (ie this generation in Britain) in the first instance.

5.1. Treatment variable

As this analysis is focused on the effect of graduating from university on political values, a binary measure of holding a university degree or higher qualification is coded as the treatment variable. In each adult wave of the BCS, cohort members are asked their highest qualification, whether academic or vocational. Those who reported having a degree-level or higher qualification (according to the ISCED-2011 classification (UNESCO, n.d.)) at each age point are included in the treatment condition, with all others placed in the control condition. As it refers to graduate status, the treatment is considered irreversible, so that once someone has reported holding a university degree they remain in the treatment condition until the end of the panel. Table 1 presents how the ISCED classification maps onto qualifications reported in the survey, while Fig. 2 presents the proportions in the complete case sample at each age point.

As we might expect, the proportion of those in the treatment group grows over time, as cohort members gain qualifications over their lifetime. This varying composition of the treatment and control groups has implications for the empirical estimand. The treatment effect as estimated is essentially a weighted average of the effects of these different levels of education on the outcomes, with the effect of achieving no or some school-leaver qualifications (and sub-degree vocational qualifications) grouped together. The potential impact of this can be inferred from Fig. 3, where a standalone OLS regression analysis enables comparison of mean outcomes (with uncertainty) across ISCED levels at the

Table 1
ISCED-2011 classification of British qualifications.

ISCED	Example qualifications
1	No qualifications, left school at 12 or younger
2	Fewer than 5 good GCSEs, CSEs or O-levels; fewer than 5 good Ordinary or Standard grades (in Scotland); NVQ level 1
3	5 or more good GCSEs, CSEs or O-levels, or any A/AS-levels; 5 or more good Ordinary or Standard, or any Higher or Advanced Higher grades (in Scotland); NVQ level 2 or 3
4	Higher National Certificate, NVQ level 4
5	Diploma of Higher Education, Higher National Diploma, NVQ level 5
6	Bachelor's degree
7+	Master's degree, postgraduate diploma or certificate (eg PGCE), PhD

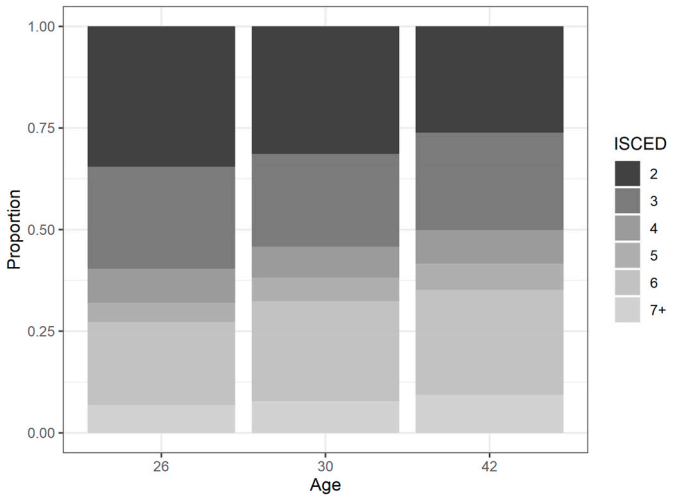

Fig. 2. Proportion achieving each ISCED-2011 qualification level by age.

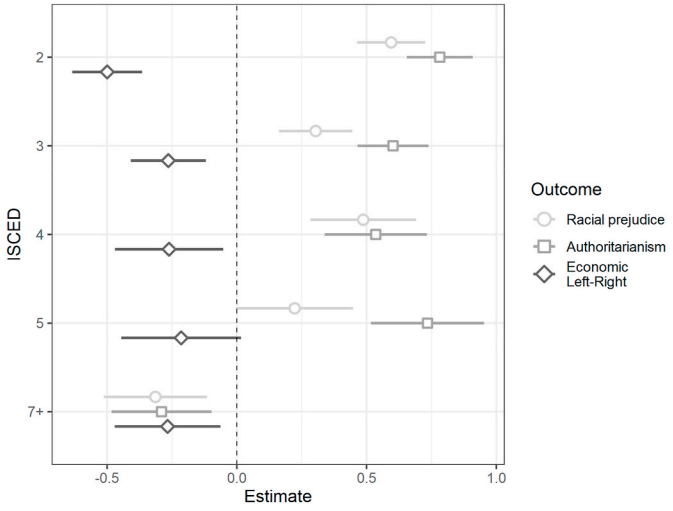

Fig. 3. Political values by ISCED level of qualification aged 30.
N = 1520.

age of 30, where those holding first degrees (level 6) are the reference category. In addition, an alternative specification where the control group excludes those without good school-leaver qualifications (ISCED level 2) are presented as a robustness check in appendix 5.

Here and throughout, a higher value on the outcome indicates higher levels of racial prejudice and authoritarianism, and more economically right-wing values, and the error bars indicate the 95% confidence interval. As can be seen, across the outcomes, the differences between graduates and non-graduates are generally the most significant. This difference tends to be strongest when compared with those achieving levels 2 and 3, the most populous of the non-graduate groups, and less so with those achieving level 5 qualifications. However, there are notable differences between those achieving some good school-leaving qualifications (level 3) and those who do not (level 2), which does mean the effects of achieving a degree on both racial prejudice and economic values are attenuated if those without these qualifications are excluded from the control group (see appendix 5 for more).

In addition, postgraduates are significantly different in their values from graduates, and although this is generally in the expected direction, it is not for economic values, where postgraduates are more left-wing and therefore closer to non-graduates. We should note that this may mute the estimated effect of higher education on economic values – yet as the intended estimand is the total effect of gaining a degree, including pursuing postgraduate education, this group should remain in the treatment condition. The decision to focus the analysis on this binary difference in qualification status is not just informed by the underlying data but also by the previously discussed research interest in the differences between graduates and non-graduates, and with a view to preserving statistical power by not disaggregating the treatment across different levels of qualification. But it is important to bear in mind that it may obscure patterns that exist underneath these larger groupings.

5.2. *Pre-treatment covariates*

To address the conditional parallel trends assumption, we must first identify the pre-treatment covariates that might confound the relationship between university attendance and adult political values. The choice of variables is informed by the theoretical discussion of selection effects in section 3.2. These, plus their age point of measurement, are presented alongside the type of selection they represent in the model in Table 2. Through their inclusion as fixed effects (ie time-invariant effects) in the REWB modelling, the causally prior nature of these variables will reduce any bias attributable to these. Note that ethnicity was not reported prior to the age of 30, but is assumed to be pre-treatment as a relatively static demographic variable.

5.3. *Outcome variables*

The three values scales of interest are measured at four age points with varying items in the 1970 BCS. Standardised scales across time are produced by calculating the arithmetic mean of the constituent survey items, then scaling this value to have a mean of zero and a standard deviation of one at each age point. As summarised in Table 3, the scales over time demonstrate a good degree of internal reliability – with Cronbach's alphas above the conventional threshold of 0.7 (Cheng et al., 2012; Cronbach, 1951). An alternative approach, producing scales through inverse covariance weighting is presented as a robustness check in appendix 5 of the supplementary material.

However, there are a few issues of note. First, there is no measure of racial prejudice at the age of 26, so this timepoint is excluded from that model. Second, the internal reliability of the authoritarianism measure is weak at the age 42 – for this reason a separate analysis is performed on the consistently included death penalty item as a robustness check, demonstrating similar relationships and included in appendix 5. Finally, while there is some consistency over time in the items, there are divergences, particularly in the items measuring Left-Right economic values in the age 16 and 26 surveys. The internal reliability of these scales over time provides some reassurance in this regard. Nonetheless, measurement error attributable to these differences is addressed primarily by the scaling at each age point – meaning that any changes over time reflect differences in relative position, rather than absolute value. As the estimand of interest is the difference in the within-individual effect between graduates and non-graduates and not the levels themselves, and the analysis is performed on a balanced panel (making the relative changes meaningful), these measurement differences are not considered a threat to inference.

In addition, the fixed effects for age (and time, as the two are colinear

Table 2
Pre-treatment covariates included as fixed effects.

Category	Covariates	Age measured
Demographic selection	Gender	Birth
	Ethnicity	30
Parental selection	Parental social class	Birth
	Parental education	5
	Mother's authoritarianism	5
Ability selection	Maths ability	10
	Reading ability	10

Table 3
Outcome scales example items and across-time reliability.

Outcome	Example statement	Std. Alpha
Racial prejudice (higher score = more prejudiced)	- I would not mind if my child went to a school where half the children were of another race	0.81
Authoritarianism (higher score = more authoritarian)	- For some crimes the death penalty is the most appropriate sentence	0.82
Economic Left-Right (higher score = more right-wing)	- Government should redistribute income from the better off to those who are less well off	0.79

in cohort data) will capture variance attributable to differing approaches to measurement over time, which also means they cannot be simply interpreted as age or period effects. Finally, the estimation of random intercepts and slopes in the REWB estimation allows individual-level variance in the starting point and effect of treatment to be modelled, meaning that variance between individuals attributable to these differences in measurement are accounted for in the standard errors and therefore the significance testing. Further details on the constituent items and analysis of the measurement properties of these scales including Confirmatory Factor Analysis are available in appendix 6.

A further potential source of bias in measuring these outcomes is social desirability: as respondents could consider the items sensitive, in particular those addressing racial prejudice, and adjust their responses accordingly. This could be a particular threat to validity if, in line with previous research on some political measures such as voter turnout (Bernstein et al., 2001; Karp and Brockington, 2005), those with higher levels of education are more likely to give the perceived socially desirable response, as this would make it appear as though the effect of university is larger than it actually is. In contrast, if the more educated feel less need to obscure their true feelings, as has been found with some measures of racial prejudice in a political context (Heerwig and McCabe, 2009), then this could mute the apparent effect of achieving a degree.

To address this, the fact that these items are consistently measured within the BCS data through self-completion surveys provides some reassurance, as research into survey modes has found that these methods, where the respondent is not prompted by a researcher, tend to elicit a 'truer' response (Presser and Stinson, 1998). However, it is also worth noting that meta-analytic research seeking to quantify the effect of social desirability bias on reported racial prejudice is sceptical about its influence (Blair et al., 2020): suggesting that those in the ethnic majority who hold prejudiced views tend to be comfortable expressing these in anonymous surveys. Indeed, some argue against considering social desirability effects as measurement error at all, advocating instead for "dual process models of prejudice [which] incorporate motivation to conform to antiprejudice norms directly into our understanding of what constitutes prejudice and what drives prejudiced behaviour" (Blinder et al., 2019). Given the challenges in addressing possible social desirability bias in secondary data, the outcomes, and racial prejudice in particular, should therefore be considered as representing this dual process model.

Having discussed the measurement properties of the outcome scales, we can now observe the relationships between our variables prior to accounting for selection. The mean of each political value by treatment condition over time is presented in Fig. 4. As can be seen, based solely on the mean differences, graduates are starkly less authoritarian and racially prejudiced, although they appear to become more so and therefore closer to non-graduates with age. The picture for economic values is more complex, with graduates appearing more left-wing at 26 (although not significantly so compared with their starting position), and then more right-wing at 30. It is possible this is a real effect, reflecting a lag in the graduate premium to earnings, or an artefact of measurement as discussed above. Yet in any case, these differences are

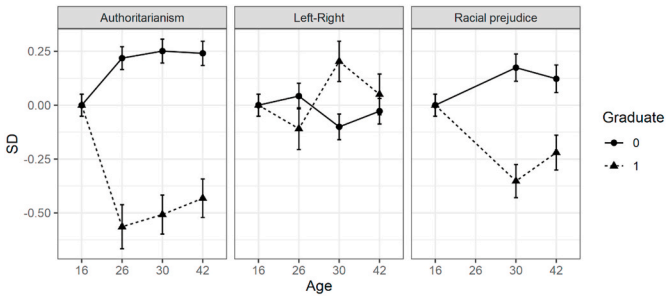

Fig. 4. Mean political values for graduates and non-graduates by age.

not estimating the change within individuals and therefore accounting for time-invariant confounding, as we will now go on to do to provide a more robust estimate of the effect of higher education.

6. Results

As set out in the methodology, the theoretical estimand of interest for this study is the effect of graduating from university on racial prejudice, authoritarianism and Left-Right economic values. This is estimated empirically as the average difference in values before and after achieving a degree within individuals who do so during the panel – and so can be interpreted as the effect of university on values less any time-invariant confounding. Three estimators are employed to give confidence in the results: the standard TWFE estimator; a REWB estimator which accounts for observed pre-treatment confounding and includes random effects parameters to allow for heterogeneous treatment effects; and finally, a REWB estimator applied to 75 multiply imputed datasets to account for non-random attrition in the cohort study data.

Fig. 5 presents these estimates of the effect of university for each of the values under study. As the outcomes have been standardised and the treatment is binary, the x axis can be interpreted as an effect size (measured in standard deviations), while the standard errors used to estimate the 95 per cent confidence intervals represented by the error bars take account of the clustering within individuals. In addition, the multiple imputation estimates have been pooled according to Rubin's rules (Rubin, 1987). The full results, including the random effects parameters for the REWB estimates, are presented in appendix 1, and more detail on the approach to the multiple imputation in appendix 2.

To take the hypotheses in turn and start with racial prejudice: the results indicate that achieving a degree has a moderate and statistically

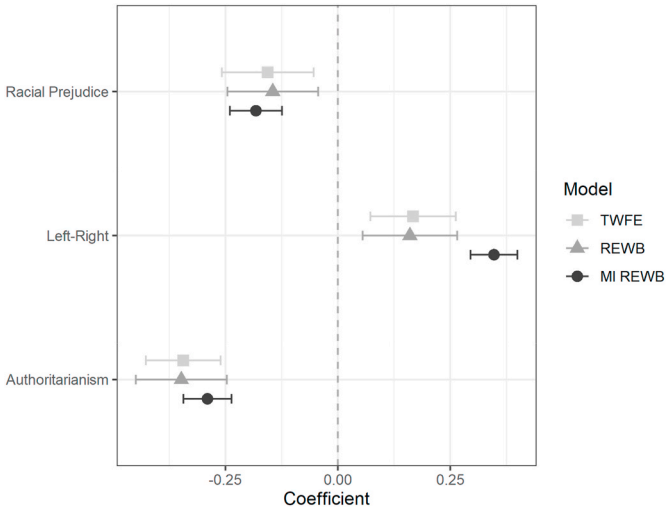

Fig. 5. Estimated treatment effects of university attendance on political values. N (TWFE and REWB) = 1520, N (MI REWB) = 15874, M = 75.

significant negative relationship with this outcome, meaning that graduates become less prejudiced on average, accounting for the effects of selection. The results are remarkably consistent across the three estimators, suggesting that graduating from university results in a small reduction in prejudice, of around 0.15 of a SD. We can therefore accept hypothesis 1. Moving on to review the results for authoritarianism, we can see that higher education demonstrates its strongest effect here. The results show a significant reduction of around 0.3 of a SD among those who achieve a degree. This effect is corroborated with greater precision by the multiple imputation estimates. This provides strong support for hypothesis 2.

Finally, the results indicate that achieving a degree renders an individual significantly more economically right-wing, although the estimators differ in the exact magnitude of the effect. Those using the complete case sample estimate a similarly-sized effect to that on racial prejudice, of just over 0.15 SD – yet the REWB model applied to the multiply imputed data suggests a much larger effect, of around 0.35 SD. This larger effect may be attributable to differential attrition affecting those in the control condition, where cohort members from more disadvantaged backgrounds (and who therefore would be less likely to achieve a degree and more likely to hold economically left-wing views) were less likely to continue with the study. This result provides evidence in support of hypothesis 3, and is in line with previous literature, where authors have attributed this change both to peer socialisation and to economic allocation effects.

Additional perspective can be provided on these findings by plotting the estimated marginal means from the TWFE modelling over time to provide a sense of the modelled linear trajectories, as in Fig. 6. This shows the strength of the effect of higher education on authoritarianism, and the weaker but still significant effect on the other two outcomes. Given that these are predicted based solely on change in individuals over time and therefore adjusting for time-invariant confounding, these results provide strong evidence for the effect achieving a degree has on political values, and authoritarianism in particular. However, it also reveals something interesting about the nature of the effect, by suggesting that the effect of university is quick to materialise but then stable, with political values relatively unchanging over the rest of the life course after early adulthood. This lends strength to the idea that higher education plays an important role in political socialisation and values formation.

7. Discussion

This study has presented new analysis of the causal effect of higher education on political values, finding that individuals become less authoritarian, less racially prejudiced and more economically right-wing due to achieving a degree. It makes three main contributions. Its primary contribution is to estimate the causal effect of university specifically. Much of the previous causal literature has focused on additional years of secondary schooling as the treatment, understandable given the potential for natural experiments linked to policy changes. But this new insight on the effect of university specifically is valuable in understanding the increasingly divergent political behaviour of graduates, not least given their growing numbers in much of the world.

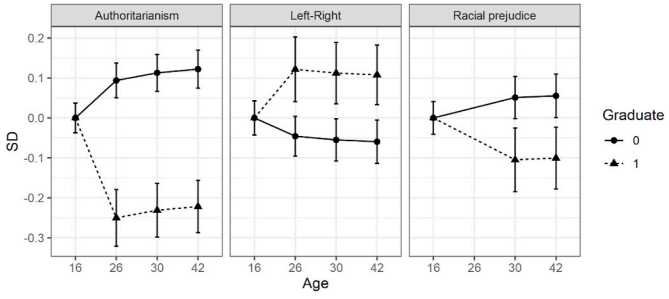

Fig. 6. TWFE-estimated marginal means of political values for graduates and non-graduates by age. N = 1520.

Secondly, it provides the first causal estimate of the effect of education on an established measure of authoritarianism. Despite the well-evidenced cross-sectional relationship between education and authoritarianism, few studies have employed a research design enabling causal identification of these effects. Those that have employed such a design (eg Brońić and Miles, 2021) have looked at adjacent values such as moral relativism and freedom of speech rather than a validated multi-item scale. This matters given the growing significance of this 'second' dimension for contemporary politics. This study addresses that gap, providing good evidence that graduates become less authoritarian as a result of university.

Third and finally, the data used for this analysis extends the previous causal literature in this area by providing an estimate of the effect of higher education among a more nationally representative sample, therefore allowing us to be more confident in drawing general conclusions. As noted above, previous research (Cavaille and Marshall, 2019; d'Hombres and Nunziata, 2016) making use of policy changes in the compulsory participation age to address the selection problem through instrumental variable and regression discontinuity designs are well-identified, but tend to find variable and quite small effects of the policy change itself, suggesting that the education effects they estimate are specific to a less representative subpopulation who are prone to educational disengagement. This study estimates the effect of graduating from university across the whole cohort, irrespective of background or the nature of the higher education experience itself.

There are, however, a few caveats to the interpretation. One is the potential for time-variant confounding to be influencing these results, particularly as the post-treatment outcomes are measured at the age of 26, which may be some years after graduation for some respondents. It's worth noting again that these estimates are the total effect of university of political values, including any downstream effects such as differences in socio-economic position, peer socialisation, geographic mobility and family formation. This should be borne in mind in the interpretation, particularly when considering the effects on economic values, which are likely to be sensitive to changes in occupation and income.

Another is the specificity of the sample. This is British data, justifiably chosen for the quality of the cohort study, but the findings cannot be assumed to be true of other populations where education systems and political values differ. Also, by its nature this analysis is of just one cohort, and so the findings cannot be confidently generalised without being replicated in other generations, as both the nature of education and political values change over time. Therefore, similar analysis with alternative high-quality longitudinal data will enable greater generalisation of these effects, as well as understanding of how they vary across settings and generations.

Yet having estimated the effect of higher education on political values, a question that remains for future study is why education is demonstrating this effect: to unpick the mechanism. Based on the values trajectories of graduates, the effect appears to be more likely one of socialisation: of rapid change during university and then stabilisation during later life. But from the existing literature it is not clear the extent to which this is attributable to a process of peer socialisation, identity formation, changes in socio-economic position, greater cognitive sophistication or some combination of all of these. Yet with the significance of higher education in politics only growing, addressing this question is vital in understanding how it will affect political values and behaviour in the future.

Indeed, these findings indicate that the more polarised politics observed in Britain since the EU referendum may be partly attributable to the values differences from differential higher education participation across the generations. Given the participation rate in higher education continues to grow among under-30s in Britain (now being well in excess of 50 per cent of that population (Department for Education, 2020)), this

suggests that we should expect to see continued long-term aggregate value change towards lower levels of authoritarianism and racial prejudice, as the population becomes more highly educated on average. However, during this transition, it is also likely that values divides will continue to intensify in the short to medium-term, with significant consequences for political behaviour.

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Declaration of competing interest

The author declares none.

Data availability

Replication data and code is available at: https://doi.org/10.17632/kpv6r63gmc.1

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.electstud.2022.102471.

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