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ORIGINAL PAPER

Testing the Relationship Between Education and Political Participation Using the 1970 British Cohort Study

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Abstract According to conventional wisdom in political behavior research, education has a direct causal effect on political participation. However, a number of recent studies have questioned this established view by arguing that education is not a direct cause of political participation but only a proxy for other factors that are not directly related to the educational experience. This paper engages in a current debate regarding the application of matching techniques to assess whether there is a direct causal effect of education on political participation. It uses data from a British cohort study that follows everyone born during 1 week in the UK in 1970. The data includes a rich set of variables measuring factors through childhood and adolescence such as cognitive ability and family socioeconomic status. This data provides the opportunity to match on a number of important variables that are not included in the US datasets used by previous studies in the field. Results show that after matching there are no significant effects of education on political participation.

Keywords Political participation · Education · Matching · Voting

Introduction

It is a widely held view that education has a direct causal effect on political participation. Education supposedly develops the relevant skills needed to understand and participate in politics, as well as increases political interest, sense

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of civic duty and concern for the importance of political participation (e.g., Lewis-Beck et al. 2008; Verba et al. 1995; Wolfinger and Rosenstone 1980).

However, a number of studies question the established view by arguing that education is not a direct cause for participation but only a proxy for other factors that are not directly related to the educational experience, such as pre-adult factors like childhood cognitive ability or social network position (Berinsky and Lenz 2011; Campbell 2009; Jackson 1995; Kam and Palmer 2008, 2011; Nie et al. 1996; Tenn 2007). These contradictory studies are a concern for political behavior research because the literature in the field ascribes central importance to education as a predictor for political participation. Hence, it should be a major concern for political behavior research to find out how education is related to political participation.

The main difficulty in this field is to estimate the causal effect of education on participation. In the absence of full-scale randomized experiments, a few studies have used matching techniques to assess whether there is a causal effect of education on political participation (Kam and Palmer 2008, 2011; Henderson and Chatfield 2011; Mayer 2011). This has resulted in a controversy regarding whether two US datasets, after matching, show support for "the education as a cause" or "the education as a proxy" view. In order to estimate the causal effect in a matching design, the selection into education must be adequately controlled for with extensive early life variables. Previous matching studies have had difficulty achieving balance in matched datasets because their measures of pre-adult factors were too limited. Hence, results of these studies are mixed and there is need for further tests on data with expanded information on pre-adult factors, such as cognitive ability.

This paper brings the following contributions to this debate. It sets out to test the relationship between education and political participation by matching on British data, and thus it extends the geographical scope of the debate and tests the wider generalizability of the previous findings. The paper uses genetic matching, which is a matching technique that employs a search algorithm that optimizes the Mahalanobis distance (a relative distance measure) between the treated (in this case those with college education) and non-treated (in this case those without college education). It uses data from a British cohort study that follows everyone born in the UK during 1 week in 1970. The data includes a rich set of variables measuring factors in childhood and early adolescence such as cognitive ability, activities, and family socioeconomic status (SES). This data allows matching on a number of important variables that are not included in the US datasets used in the previous studies. The results show that after matching, the relationship between education and political participation is insignificant, i.e., the education as a proxy view is supported.

This paper will proceed as follows. The next section provides the theoretical framework. Thereafter data and techniques of the analyses are presented. Finally, the results are presented and the conclusion discusses the implications of these results.

Theory

"Years of education" is a central predictor for political participation (e.g., Converse 1972; Verba et al. 1995; Wolfinger and Rosenstone 1980). Recently, research has increasingly questioned whether years of education is a direct cause for political participation or merely a proxy for other factors (e.g., Berinsky and Lenz 2011; Burden 2009; Campbell 2009; Highton 2009; Kam and Palmer 2008; Sondheimer and Green 2010; Nie et al. 1996; Tenn 2005, 2007). There are two possible explanations for the relationship between education and political participation. First, higher education might cause greater participation (the education as a cause view). Or, second, the relationship could occur due to self-selection, i.e., that the kinds of people who seek higher education are also more likely to participate in politics regardless of their level of education (the education as a proxy view).

According to the education as a cause view, education has a strong positive impact on individuals' civic skills and cognitive capacity, which in turn increases political participation (e.g., Verba et al. 1995; see Campbell 2006 for a literature review). The education as a proxy view, on the other hand, states that education takes credit for other factors related to educational choice, such as childhood cognitive ability and the socialization process early in life (e.g., Sears and Funk 1999; Searing et al. 1976; Jennings and Niemi 1974; Langton and Jennings 1968). Factors such as family SES, parents' level of political participation, the discussion climate at home, and parents' political orientations are key factors in the early socialization process (Achen 2002; Andolina et al. 2003; Westholm 1999). According to the education as a proxy view, these factors not only affect political participation, they also determine the level of education.

The education as a proxy view was supported already in Langton and Jennings's (1968) seminal study, which showed the effects of civic education courses on political participation to be non-existing. However, this study focused on the content of education rather than the length of education. But "years of education" has repeatedly shown a strong impact on participation in cross-sectional studies, which has led scholars to regard education as one of the major factors influencing political participation (Converse 1972; Verba et al. 1995; Wolfinger and Rosenstone 1980). The problem is that the bulk of this research draws on analyses of cross-sectional data, which cannot be used to disentangle correlation from causation.

Isolating the causal effect of education is a hard task. The gold standard for estimating causality is randomized experiments; hence an ideal research design would be to randomly assign some persons to receive higher education and others to receive lower levels of education, or no education at all. However, such a research design would be impossible to implement since it would interfere too much in people's lives. Even if it would be possible to randomly distribute vouchers or scholarships for college it would be hard to make sure that all subjects actually received the treatment, i.e., experienced the educational content and graduated from the education levels they were assigned to. Moreover, it would not be possible to make sure that those who were assigned to the control group (those who do not higher receive education) do not obtain higher education since it is not possible to hinder persons' possibilities to receive higher education. What is left after excluding these experiment options is the estimation of the casual effect from observational studies or quasi-experimental situations.

Recently a number of studies have begun to use more sophisticated methods to gauge the causal effects of education. This literature investigates the relationship using techniques designed to estimate causality, such as instrumental variables (e.g., Berinsky and Lenz 2011), field experiments (e.g., Sondheimer and Green 2010), and matching analyses on panel data (e.g., Kam and Palmer 2008; Tenn 2007). None of these studies, however, come close to the ideal experimental research design (although the study of Sondheimer and Green comes close to it). Given the conflicting set of results from previous studies, further analysis is needed to clarify whether a participation difference is consistently observed across different inferential assumptions and in new data. In the following I will discuss these studies.

Several of these studies have shown support for the education as a proxy view. Berinsky and Lenz (2011) arrive at this conclusion by using the natural experiment of the Vietnam-era draft to compare participation levels among males who attended college with those who did not. This study has the advantage of containing a randomly assigned exogenous chock that affects educational attainment and in turn makes the results valuable to the discussion. Highton (2009) uses US panel data to estimate effects of education on political sophistication. He arrives at the conclusion that differences in political sophistication, related to education, are already in place before the education is acquired. However, this study does not look at political participation but sophistication, and it is not possible to test if differences in all forms of participation are in place before education is acquired since acts such as voting can only be performed by adult citizens. Pelkonen (2012) uses reforms in the Norwegian educational systems to gauge the effects of increased length of education on political participation, and shows null results.

However, there are also a number of recent studies that show support for the education as a cause view. The most interesting example is Sondheimer and Green (2010), who employ an experimental approach in which educational attainment is altered exogenously by different interventions affecting educational attainment such as smaller classes, extra mentoring, preschool activities, etc. This study exhibits strong support for the education as a cause view but it was conducted primarily among low SES students and it is hence not possible to generalize the results to the entire population. It might be the case that the effect of education is stronger for low SES students than the average treatment effect in the population. Milligan et al. (2004) use an instrumental variable approach to both American and British data with variation in the length of schooling over time. They show that education has a positive impact on voting in the US, but not in the UK. However, after taking registration requirements into account, the effect of education in the US is considerably reduced. Moreover, Dee (2004) uses geographical distance to a higher education institution and the adoption of child labor laws as instrumental variables to gauge the causal effects of education. Using this study design Dee also concludes that education causes higher levels of political participation.

Kam and Palmer (2008) were the first to evaluate the problem using matching techniques. If successfully conducted, matching could be used to controlling for the selection into higher education and mimicking an experimental design. Kam and Palmer apply propensity score matching to two datasets: the Political Socialization Study and the High School and Beyond Study. After matching on a large number of covariates, they find no significant differences in the levels of political participation between those who had attended college and those who had not. Hence, education seems to be a proxy rather than a cause.

In two independent responses to Kam and Palmer's article, two sets of authors criticize their study. Both Henderson and Chatfield (2011) and Mayer (2011) argue that Kam and Palmer's propensity score model applied to the Political Socialization Study is not robust. The main problems are that the matching method does not provide balance between the treated and the untreated, i.e., a few influential observations in the non-college attendance group are matched to a large number of college attenders, and the results do not hold when conducting sensitivity tests. To obtain greater balance, both Henderson and Chatfield (2011) and Mayer (2011) use genetic matching. However, even with this more sophisticated technique, neither set of authors obtains balance robust enough to estimate the average treatment effect of the treated (ATT) (i.e., the causal effect for those who experienced the treatment). Instead they estimate the average treatment effect of the controls (this result corresponds to the causal effect, as if everyone in the control group had experienced the treatment), which improves balance. Henderson and Chatfield (2011, 647) conclude that "selection may be so problematic as to make it practically impossible to recover unbiased causal estimates using even the most sophisticated matching methods as yet available." Mayer (2011, 644) concludes that his analysis shows "evidence that postsecondary educational advancement has a positive and substantively important causal effect on political participation." In sum, both studies reject the null result found in Kam and Palmer's original study (2008).

In a response to this critique, Kam and Palmer (2011, 661) acknowledge the methodological advancement in these responses but also claim that "the problems that both Henderson and Chatfield and Mayer find are inherent in the specific dataset they chose to analyze or in the specific estimate they chose." To confirm this argument, Kam and Palmer reanalyze the more hospitable High School and Beyond Study with genetic matching and find balance for a vast majority of the covariates. The post-matching tests show no significant effects of education on political participation and confirm their original conclusion in support of the education as a proxy view.

This research review has focused largely on the impact of length of education. A widely ignored question is the impact of the education's content. It is possible that even among people with the same amount of education some kinds of content (such as civics classes) could make a difference in promoting greater political participation (cf. Persson and Oscarsson 2010; Persson 2012). Although this is indeed an important question that deserves more attention, it is beyond the scope of this paper. However, it should be noted that even though analyses may show that length of education does not have any effect on political participation, other dimensions of education might still have effects. Hence, an analysis of the education length effects does not completely rule out all kinds of education effects.[1]

[1] An alternative theoretical model, which also considers education to be a proxy, is presented by the sorting model of education effects (Nie et al. 1996; Campbell 2009; Persson 2011, 2013). According to this model, education has no value per se but rather serves as a proxy for social network position.

To sum up, the conventional wisdom in support of the education as a cause view draws mostly on cross-sectional data, which is not appropriate to use to draw conclusions about causal relationships. Recent research using sophisticated methods appropriate to evaluate causality shows contradictory results. There is no agreement on whether education is a direct cause or a proxy for political participation and the debate remains unsettled.

Methods

In the absence of full-scale randomized experiments where higher education is randomly assigned, matching can be used to artificially mimic such a situation. The basic idea behind matching is simple: to match untreated observations, that are similar on the relevant covariates, with treated observations (Rubin 1973, 1974). If this is done successfully, comparing individuals who are similar across all relevant covariates, except for the treatment variable, is, at least logically, equivalent to comparing individuals randomly assigned to different treatments in an experiment (cf. Dehejia and Wahba 2002). The relevant covariates here refer to every necessary covariate that satisfies the assumption of conditional exchangeability, i.e., there should be no further confounding factors.

In observational studies, causal inferences draw on the selection on observables assumption (SOA), which means that all correct covariates have been matched on. This is a strong assumption, but it is widely held in observational studies. Matching estimators will be biased if some covariates that influence educational attainment and participation are excluded. Hence, causal inference in matching studies is conditioned on the requirement that SOA is satisfied. In that case, the treatment assignment probabilities are identical for any matched units based on stratification on all the covariates.

A problem with previous studies using matching to estimate the effects of education on participation is that they lack important early-life variables that affect selection into education, most importantly cognitive ability. The lack of such variables in previous studies casts doubt about whether all necessary covariates that would be needed to satisfy SOA are included. What motivates the importance of this paper is using a more extensive set of early-in-life measures that predict later non-random choices to get higher education. Including these variables is important since they may predict some unobserved factors that are otherwise difficult to balance. In other words, inclusion of cognition measures is likely needed to satisfy the SOA.

Indeed there are plenty of studies suggesting that educational achievement and college attainment are affected by cognitive ability (e.g. Bartels et al. 2002; Belley and Lochner 2007; Deary et al. 2007; Sternberg et al. 2001) and in this field there is broad agreement that the relationship is "moderate to strong" (Deary et al. 2007, 13). Persons with higher cognitive ability are more likely to be successful in the educational system. In addition, studies have shown that cognitive ability affects political behavior and political orientations (Deary et al. 2008; Denny and Doyle 2008; Rindermann et al. 2012; Luskin 1990). The suggested mechanism linking cognitive ability and political behavior is that intelligent people think more rationally, form more reasonable worldviews and tend to act in accordance with these norms. Highly intelligent people are more likely to acknowledge the value of participating in political activities. Given the importance of early-life cognitive ability for educational attainment and political participation later in life, it is an important contribution to be able to add such covariates when performing matching.

Why is it necessary to conduct matching and not sufficient to just control for the relevant covariates that are related to education in a regression model? The reason is that when treatment and control groups are unbalanced and do not overlap, a simple regression model will not produce a valid estimate of the average causal treatment effect. When there is limited overlap, the estimates will not capture the effect of the treatment in non-overlap segments of the data (cf. Gelman and Hill 2007). For example, if the dataset lacks individuals with a low SES family background who gain higher education and individuals from high SES family backgrounds without higher education, the dataset lacks overlap. Hence, we cannot draw inferences about the effects of education for the entire population, i.e., individuals ranging from low to high SES. If a dataset is heavily skewed and completely lacks overlap, no matching procedure can correct it. Hence, to perform matching successfully, one needs some overlap to be able to match non-treated with treated observations.

To obtain a robust matched dataset, one must identify the covariates predicting the treatment variables, and that also influence the participation outcomes, with little and unsystematically distributed error remaining. The key criteria to judge the quality of a matching procedure is balance, i.e., whether the distribution of the covariates differs significantly between the treated and the untreated after matching. If matching is done successfully, covariates should be balanced and no significant difference with respect to the covariate distribution should remain post matching.

The field offers a cacophony of different matching methods. Rosenbaum and Rubin (1983) proposed the use of propensity score matching. Following this method, a logistic regression model, which includes all relevant covariates, is first used to predict the probability of the treatment. The resulting propensity score is then used for matching treated observations with untreated. The downside of propensity score matching is that it requires both knowledge of the correct propensity score that predicts the treatment and large datasets to find matches.

Recent advancements in matching methods involve genetic matching (Sekhon 2011), full and optimal matching (Hansen 2004) and coarsened exact matching (Iacus et al. 2012). Following Henderson and Chatfield (2011), Mayer (2011), and Kam and Palmer (2011) this paper utilizes genetic matching. The main benefit with genetic matching is that it employs a search algorithm that iteratively checks the balance and improves it automatically (Diamond and Sekhon 2012). Genetic matching estimates a weight for every covariate that minimizes the *p*-values in order to test the difference between the treated and the control's marginal covariate distributions. Simulation studies have shown that genetic matching generally provides greater balance than, for example, propensity score matching.[2]

[2] When analyzing the data used in this paper with propensity score matching results show significantly worse balance than for genetic matching. Moreover, coarsened exact matching leaves too many treated observations unmatched.

Matching does not by itself constitute a test of causality; it is only a way of preprocessing the data. However, it allows the researcher to, post matching, estimate causal effects using the Neyman–Rubin–Holland framework (Holland 1986). Within this framework, causal inferences can be evaluated based on observational non-experimental data. In this framework, y_{i1} represents the outcome of the individual if treated while y_{i0} represents the outcome if not treated. The causal effect is thus $y_{i1}-y_{i0}$, but naturally both of these states cannot be observed for each individual. We thus need to compare the observed state with a substitute for the counterfactual state. As Morgan and Winship (2007, 5) put it, "The key assumption of the counterfactual framework is that each individual in the population of interest has a potential outcome under each treatment state, even though each individual can be observed in only one treatment state at any point in time." This means that when we, for example, evaluate the effect of higher education on political participation, those who have attended higher education have theoretical what-if levels of political participation for a counterfactual state where they did not receive higher education. The difference between the actual and counterfactual state can be considered an estimate of the causal effect.

The Neyman–Rubin–Holland framework is used to investigate whether receiving higher education had any causal effect on political participation for those who received this treatment. When estimating the ATT, we condition the comparison on the distribution of the covariates among the treated individuals. The ATT essentially captures what the debate on the effects of education on political participation is all about, whether education has a causal impact on political participation among those who received it.[3]

Data

In order to evaluate the effect of education on political participation, data from the 1970 British Cohort Study is used.[4] This study follows all 17,278 children born in the UK from April 5 to April 11, 1970. The first surveys were conducted with the babies' parents. Follow-up surveys with those born in 1970 were conducted in 1975, 1980, 1986, 1996, 2000, 2004, 2008 and 2012. The political participation items are taken from the 2004 survey when the respondents were 34 years old. The political participation items measure reported voting in the 2001 election and whether the respondent had signed a petition, contacted a member of parliament (MP), attended a public meeting or rally, and/or participated in a demonstration during the last

[3] An alternative causal estimate is the average treatment effect for the controls (ATC). The main difference between the ATT and the ATC is that when estimating the ATC, the comparison is conditioned on the covariate distribution among the *untreated*. Hence, rather than evaluating whether education has a causal effect on those who received it (the ATT measure), the ATC evaluates a hypothetical counterfactual state in which untreated individuals would receive higher education. I follow Kam and Palmer (2008, 2011) in their interpretation of the literature as I am primarily concerned with whether education has a causal effect among those who actually received it, rather than what the effect would be if higher education were given to a random person not receiving it. Hence, if we are interested in whether education had any causal effect among those receiving it, the ATT is the primary relevant estimate.

[4] Information about the 1970 British Cohort Study is available at http://www.cls.ioe.ac.uk/.

12 months.[5] Since previous research has shown that different explanatory factors affect different forms of participation (Verba et al. 1995), the participation items are analyzed separately.

The treatment variable is also measured in the 2004 survey. In this case, higher education is coded as having received a bachelor's degree or higher.[6] Some recent studies have used college attendance as the treatment variable (cf. Henderson and Chatfield 2011) rather than college graduation, but it is unclear why only attending college should have positive effects on participation. If education causes higher participation by increasing individuals' skills and knowledge it is reasonable to expect that a completed college education should be what matters, since those who attend but do not finish do not fully experience the treatment and it is not reasonable to expect them to have developed their knowledge and skills to the same extent as those who do graduate. Hence, we should expect stronger effects from college graduation than from attendance. However, college attendance might be harder to manipulate in an experiment than college graduation since it might be difficult to control who will put in the effort required to graduate. In a hypothetical, ideal experimental situation one has to make efforts to incentivize subjects not only to attend but also to graduate from college. In such an ideal experiment, college attendance would correspond to the intent-to-treat effect (leaving problems of missing data and non-compliers aside) while college graduation would serve as the treatment effect.

The literature points at several main factors affecting educational choice that are covered by the matching covariates (e.g., Akerhielm et al. 1998; Klasik 2012). First, family background is a main factor influencing educational choice (and political participation). Children who grew up in intellectually stimulating environments, in which they are encouraged to study, are more likely to attend college. This might not solely be a socialization effect but could also be due to genetic transmission. Items measuring parents' education, family activities and the number of siblings pick up these factors.

Family income is also important for college attendance since students who are able to get financial support are more likely to attend college (e.g., Chevalier and Lanot 2002). Growing up in a surrounding with other school-motivated peers might also increase the likelihood of attending college. We include measures detailing the

[5] Some persons might over-report voting; this tendency might increase over time after the election and might be more severe for people who are generally more likely to vote (Bernstein et al. 2001; Granberg and Holmberg 1991).

[6] As for the treatment variable some dichotomization is necessary because of the restrictions of the available matching algorithms and software. Moreover, college education is the level of education that is largely considered to be a figurative step in people's lives. Research on educational attainment describes college education as a "key transition" that has more explanatory power than, for example, cumulative years of education (cf. Kam and Palmer 2008). Additional analyses on data from the 1970 British Cohort Study confirm that college education is the most important educational level in relation to political participation. Participation levels are always significantly higher for individuals with college education than for individuals with all lower educational levels. Further, in regards to contact with politicians, and attending rallies and demonstrations, there is no significant difference between persons with no education and persons who have completed high school/secondary school; the significant difference is between those with higher education and those with lower levels of education.

frequency of cultural activity experiences because these variables are likely to say something about the kind of environment the child grew up in. Most importantly, educational enrollment is affected by cognitive ability (e.g. Belley and Lochner 2007). Individuals with high cognitive ability are more likely to achieve higher education and might also be more likely to participate in politics. The cognitive test scores from ages 5 and 10 provide a rare opportunity to account for cognitive ability in the models.

However, it should be emphasized that the aim of this paper is not to analyze the relative weight of which factors affect college attendance; the aim is only to best predict college attendance in order to achieve balanced samples of persons with and without higher education.

Does this list of covariates represent all the factors that education is a proxy for if the "education as proxy"-view is correct? In other words, does the set of covariates satisfy the SOA? In my view, these covariates cover the most important factors pointed out by previous research on the influences on college attendance. They also represent one of the best sets of covariates available in existing panel studies that could be used to estimate the causal effect of education on participation using matching. At this point it should be remembered that interpreting the resulting estimates as causal inferences requires that the SOA be satisfied, i.e., that the list of covariates is exhaustive. Consequently, any excluded covariates result in bias. At the same time, we know that balancing on the cognition covariates accounts for much of the variation in educational choices, and probably also many unobserved things that arise in the interim before college but after age 5 or age 10 when the tests were conducted. Hence, it is reasonable to assume that any bias that remains is likely to be small.

A number of key covariates from the first four survey waves are used for the matching procedure. These are theoretically chosen based on the criterion that they should pick up important pre-adult factors related to the treatment variable. For this reason, none of the covariates were collected after the respondents were 16 years old.[7]

Variables measuring respondent gender and parents' education are taken from the first survey round in 1970. The second wave in 1975 included a test of cognitive ability, which consisted of four subtests: the Copying Design Test, the Human Figure Drawing Test, the English Picture Vocabulary Test, and the Profile Test. The Human Figure Drawing Test was an adaption of a test developed by Harris (1963) and scoring was done using a modified version of the Harris–Goodenough scale (Scott 1968). The Copying Design test consisted of making copies of eight designs (Davie et al. 1972). As for the Vocabulary Test it was a modified version of the American Peabody Picture Vocabulary Test (Brimer and Dunn 1968). In the Profile Test, the respondents were asked to complete an incomplete drawing of a head in addition to identifying the different parts. These tests have been subject to rigorous reliability tests in previous research (Deary et al. 2008). I include the test scores from each of these tests in the matching routine.[8]

[7] The full questionnaires including the cognitive ability test can be found at http://www.cls.ioe.ac.uk/shared/get-file.ashx?id=142&itemtype=document.

[8] Previous studies have used the summary scale of these indices as a proxy for IQ (Elliott et al. 1978). Following Deary et al. (2008), I construct a variable consisting of the scores on the first unrotated component and convert it to a traditional IQ scale with a mean of 100 and SD of 15. I use this measure to check for balance, see the online appendix for further details.

From the third wave in 1980 measures of family income and whether the father or mother had gained further higher education since the child was born are used. Moreover, two indices are constructed covering cultural and family activities that are correlated with future educational choice. The cultural activities index includes five items on how frequently the child reads books, goes to a club or organization, goes to a museum or library, and plays a musical instrument. The family activities index covers six items measuring how often the family members go for walks together, go on outings, go on vacations together, go shopping together and chat for at least 5 min. For both these indices, measures are constructed using the scores from the first unrotated factor in a principal component analysis. The variables are then recoded into eight categories. Moreover, the 1980 survey a measured the number of other children in the household (measured on a six-point scale where the highest value is more than five).

The 1980 survey also included a cognitive test that draws on the British Ability Scales, which includes four subtests. The first two subtests measured verbal ability using word recognition tests (word definitions and word similarities), and the second two measured recall of digits (numbers and matrices). These tests have also been subject to rigorous reliability tests in previous research (Deary et al. 2008; Breen and Goldthorpe 2001). I include the test scores from each of these tests in the matching procedure.

As is always the case with longitudinal individual data, panel mortality is a concern. The study started with 17,287 children born in the prescribed week in 1970, and 16,571 (95.9 %) of the families participated in the first wave. The second wave response rate was 79.0 %, the third wave was 88.8 % and the fourth wave was 70.2 %. At the time of the seventh wave in 2004, i.e., when the treatment variable and the dependent variables for this study were collected, the target sample was reduced to 15,289 persons and the response rate was 60.9 % (or 53.9 % of the original sample). Since the composition of respondents who did not respond changed from wave to wave the balanced panel sample (including those who participated in all waves) was 45 % in 2004.[9] Item non-response reduces the sample further and leaves us with 2,837 individuals with full information on all relevant variables.[10] As a robustness check I have used multiple

[9] The problem of attrition out of the panel is difficult to address. Particularly troublesome is that low cognitive ability persons drop out of the panel to a higher degree. If setting the mean on the summary scales of the cognitive ability variables to 100 with a SD of 15 for the entire sample that answered these questions, the mean for those who participate in the 2004 survey are 101.7 (age 10 cognitive ability) and 101.2 (age 5 cognitive ability), a relatively small but statistically significant difference. Since non-graduates are less likely to answer the survey, it might be the case that the non-college attenders in the sample are more educated and have higher cognitive ability than the non-college attenders in the population. The low educated in the dataset may plausibly be participating more than the true population of people without college degrees. This would mean that the differences between college graduates and non-college graduates are underestimated in the dataset.

[10] As for the item non-responses, I have checked among the 45% of the sample that made it to the 2004 survey to determine whether a binary indicator for deleted or not deleted, due to item non-response, is balanced across persons with college degrees and without college degrees (for each covariate used in the matching procedure). *T* test and Kolmogorov–Smirnov (K–S) test *p*-values indicate that most covariates are balanced across educational groups. However, significant differences in the amount of non-responses are found for family income 1986, parents' education 1970, and the cognitive ability scores (except for the profile test). As for family income 1986, the differences in item non-responses between college educated and non-college educated is five percentage points. For the other covariates, differences in item non-responses are not higher than two percentage points.

imputation to assign values to the variables for which item non-responses are unbalanced between college and non-college persons. (I include only those persons who have non-missing values on at least eight of the matching covariates since it does not make sense to impute using only a few weakly related covariates. 15 % of the respondents have item non-responses to more than eight of the variables.) This provides us with 1,270 persons with college education (instead of 569). Estimates of political participation among the treated and non-treated in this dataset are presented in Table 4 in the online appendix. No significant differences at the 95 % confidence level are found, however it should be noted that the difference in voting reaches statistical significance at the 90 % level. Among these individuals, 569 had achieved a bachelor's degree or higher and 2,268 had lower educational qualifications.[11] The large volume of missing data is a serious problem. In the main analyses I use only persons without item non-responses.[12] This means that the inferences presented here are conditional on the attrition and non-response rates observed in this survey. This is a drawback of the study, but a drawback that it shares with most long-term panel studies and with previous matching studies on the effects of education on political participation.

Results

In the original unmatched data, we find, as expected, that individuals who have achieved a bachelor's degree or higher participate in politics to a higher extent than those with lower educational qualifications. Table 1 presents the mean levels, the difference in means, and the associated *p*-values for the political participation items among those with a higher education degree and those with no degree. While the forms of participation differ starkly in frequency, e.g., voting was performed by a majority of the individuals whereas only 2 % attended a public demonstration, the levels of participation differ significantly between the higher and lower educated for all items. Moreover, the differences are of substantial size. For voting, the difference is about 15.5 % points and for petition signing it is 8.5 % points. The differences in absolute terms are smaller (only a few percentage points) for demonstrations,

[11] A potential problem is that education is correlated with panel attrition. Among people who had graduated from college and responded to the survey in 2000, 85 % participated in 2004. The corresponding number was 79 % for the non-graduates. In 2008, 81 % of those with a college degree in the year 2000 participated in the survey, while only 70 % of the year 2000 non-graduates.

[12] A comparison of the means for the matching covariates in the full 2004 dataset, with only the respondents in the balanced dataset, containing only those who participated in all previous waves, show small differences of means. All differences of means are less than 1/10 of a standard deviation. This is also the case within the sub-groups of those with and without higher education. For more information on the non-responses see, McDonald and others (2010). Non-responses were not missing at random: "Response was lower for cohort members who were men, having a mother who was younger at the birth, a mother who did not attempt to breastfeed, a lower birth weight baby, in a family with two or more children, born of non-married parents, a manual father and living in London" (McDonald and others 2010, 26). However, interest in politics was not found to be associated with non-response. This means that the population that this sample represents differs slightly from the total population and is biased towards the groups with high response rates mentioned above. Still, it should be acknowledged that the panel attrition rate is high and this should be taken into account when interpreting the results.

contacts with MPs, and attending public meetings. However, taking into account the low overall participation levels in these activities, the differences imply that the higher educated are about twice as likely to participate in demonstrations, contact MPs, and attend public meetings than those without higher education.

Table 2 presents the difference, after matching, between those with higher and lower educational qualifications in terms of the ATT. More specifically, the matching has been carried out using genetic matching 1:1 with replacement.[13] After matching, the differences are considerably reduced and no *p*-values signal statistical significance. In other words, we cannot detect any effect of education after matching, and education should consequently be regarded as a proxy rather than a cause for political participation.

How robust are these estimates? There is not one single generally accepted way to determine whether balance is achieved. Rather, methodological research advises combining several balance tests (cf. Sekhon 2011) such as *t* tests, Kolmogorov–Smirnov (K–S) tests, Quantile–Quantile (Q–Q) plots, placebo tests and balance simulations.

Table 3 presents *t* test *p*-values for all covariates and K–S *p*-values for the continuous variables from tests of the covariate balance before and after matching. Before matching, the covariate distribution was significantly unbalanced for nearly all covariates. However, after the genetic matching procedure was applied, all of the covariates show *p*-values that indicate balance between the treated and the controls at the 95 % confidence interval level.[14]

Although widely used, the procedure to conduct *t* tests to check for balance has been criticized both for being dependent on the sample sizes and for not being informative enough (Imai et al. 2008). One alternative way to examine balance after matching is to evaluate the standardized bias (cf. Mayer 2011). Thus, the standardized bias before and after matching is calculated for all covariates (the difference in means between the control and treatment groups divided by the pooled standard deviation). Some standardized bias would occur even in a true randomized experiment due to random variations. Following Mayer (2011), I use simulations to assess whether bias in the matched data is significantly different from what would be the case in a randomized experiment. To do this, each individual in the dataset is randomly assigned to one of two randomly created groups and standardized bias is calculated for each covariate. I use a group size of 569 which corresponds to the number of treated individuals in the dataset. This process is repeated 1,000 times and thereafter bootstrapped confidence bounds, containing 95 % of the simulated standardized bias estimates, are calculated. Hence, if the standardized bias estimates fall within the confidence bounds, bias in the matched dataset is not significantly worse than the bias that would exist in a randomized experiment. Figure 1 presents the standardized bias measures before and after matching as well as the 95 % confidence bounds. Before matching nearly all covariates fall outside the confidence

[13] The matching has been carried out with the GenMatch package in R.

[14] Table 1 in the online appendix presents results from the balance tests of the items that are used to construct the summary scales used for the matching procedure, as well as, the summary scales for cognitive ability at ages 5 and 10. While most covariates are unbalanced before matching, balance is achieved after matching (all K–S *p*-values indicate non-significant differences).

Table 1 Political participation among individuals with and without higher education, unmatched data

	Unmatched			
	No college degree	College degree	Difference	*p*-value
Voted in 2001 election	0.620	0.775	0.155	0.000
Demonstration	0.019	0.040	0.021	0.002
Signed a petition	0.218	0.302	0.085	0.000
Contacted MP	0.034	0.070	0.037	0.000
Attended public meeting or rally	0.037	0.067	0.029	0.002

Table 2 Effects of higher education on political participation

	Matched - ATT			
	No college degree	College degree	Difference	*p*-value
Voted in 2001 election	0.736	0.775	0.039	0.199
Demonstration	0.032	0.040	0.008	0.592
Signed a petition	0.264	0.302	0.038	0.264
Contacted MP	0.060	0.070	0.010	0.615
Attended public meeting or rally	0.060	0.067	0.007	0.739

Genetic matching (1:1) with replacement

bounds, while after matching all bias measures are within the simulated confidence bounds.[15]

Another alternative to test bias is to use empirical Q–Q plots. Figures 2 and 3 show the distribution of summary scales for cognitive ability at ages 5 and 10 for the treated and control groups before and after matching (the other Q–Q plots are not shown due to space constraints). If balance is achieved, the distribution should be the same in the treated and the control groups and the points in the plot should be placed on the 45° line (Sekhon 2011). As we can see, the distance to the 45° line is considerably reduced after matching, suggesting that the balance has thus increased.[16]

[15] Figure 1 in online appendix presents standardized bias before and after matching for all items that are utilized to construct the summary scales used for the matching procedure, as well as, the summary scales for cognitive ability at ages 5 and 10. While most covariates are unbalanced before matching, only two covariates fall outside the confidence bounds after matching.

[16] A further concern could be that the results are an artifact of the specific matching routine applied (1:1 with replacement). Replacement refers to whether a matched observation can be used again for another match. Tables 2 and 3 in online appendix report ATT estimates from genetic matching 1:2 with replacement (each treated matched to two untreated), as well as 1:1 and 1:2, without replacement. The results from 1:2 matching with replacement show no significant differences. However, estimates from matching without replacement show several significant differences in political participation between those with higher education and those without, in particular estimates derived after 1:2 matching. But since it is harder to achieve balance without replacement, and balance decreases as more untreated observations are matched with treated observations, we should not put too much confidence in these results.

Table 3 Covariate balance of higher educated and lower educated

	Unmatched baseline		Matched ATT 1:1	
	p-value	K–S *p*-value	*p*-value	K–S *p*-value
Sex	0.917	–	0.814	–
Father higher education at age 0	0.000	–	0.890	–
Mother higher education at age 0	0.000	–	1	–
Cognitive ability, age 5 (1): the English picture vocabulary test	0.000	0.000	0.628	0.205
Cognitive ability, age 5 (2): the copying design test	0.000	0.000	0.964	0.205
Cognitive ability, age 5 (3): the profile test	0.000	0.001	0.249	0.262
Cognitive ability, age 5 (4): the human figure drawing test	0.000	0.000	0.726	0.138
Cognitive ability, age 10 (1): word definitions test	0.000	0.000	0.693	0.642
Cognitive ability, age 10 (2): recall of numbers test	0.000	0.000	0.563	0.544
Cognitive ability, age 10 (3): recall of matrices test	0.000	0.000	0.438	0.294
Cognitive ability, age 10 (4):	0.000	0.000	0.756	0.367
Family income, age 10	0.000	0.000	0.424	0.909
Father higher education, age 10	0.000	–	0.1	–
Mother higher education, age 10	0.000	–	0.892	–
Cultural activities, age 10	0.000	0.000	0.742	0.067
Family activities, age 10	0.000	0.000	0.170	0.496
Number of children in household, age 10	0.001	0.001	0.846	0.909
Family income, age 16	0.000	0.000	0.440	0.938

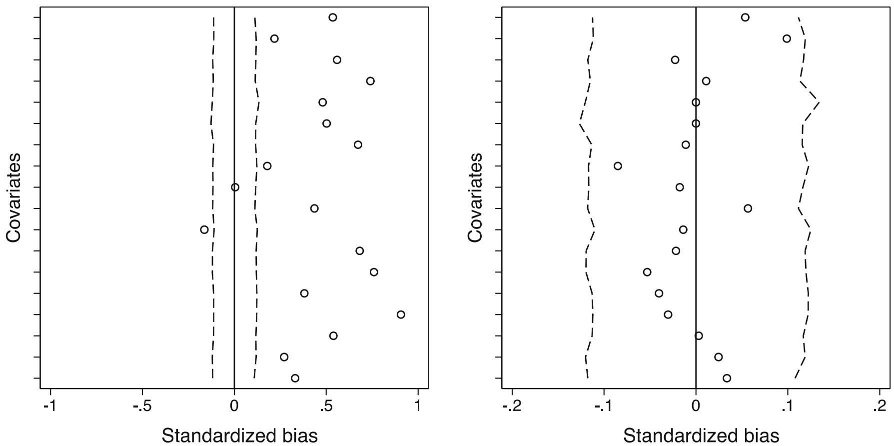

Fig. 1 Standardized bias in the unmatched sample (*left*) and the matched sample (*right*)

Further robustness checks can be conducted using placebo tests. Placebo tests should ideally be used on pre-treatment/prior outcome variables that are not included in the matching routine but are related to the outcome variables. If there

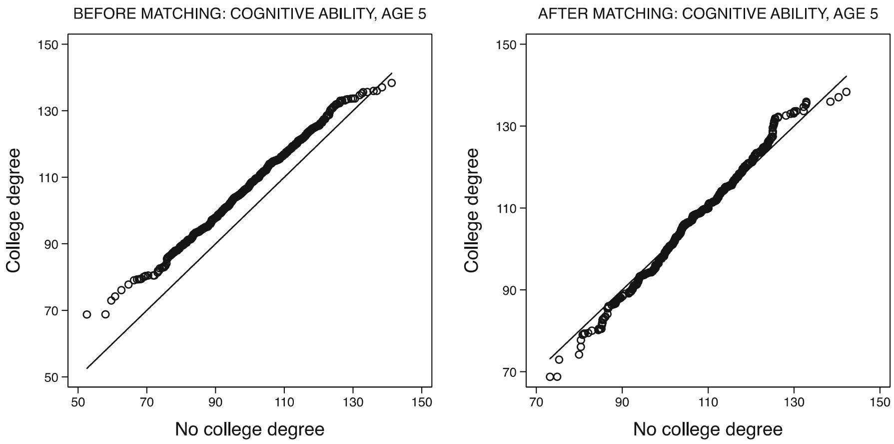

Fig. 2 Cognitive ability at age five before and after matching

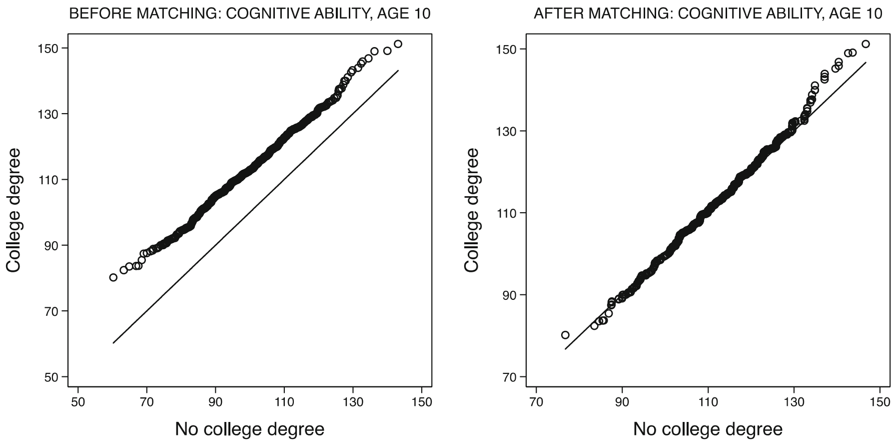

Fig. 3 Cognitive ability at age ten before and after matching

are no significant differences in such variables between the control and treatment groups it strengthens our confidence in the exchangeability of the groups. In other words, such tests can indicate whether there are any remaining confounding factors that might be an issue after matching. There are very few political participation variables (or related variables) in the British Cohort Study waves before 2004 that could be used for placebo tests. However, the 1986 survey (when participants were 16) includes some items measuring different activities. Among these are frequency of attending political meetings and frequency of attending youth clubs (measured on a four-point scale ranging from "rarely/never" to "more than once a week", recoded to vary between 0 and 1). I use these two items as placebo tests.[17]

[17] A drawback of these measures is that this particular survey had an unusually low response rate. Hence, the number of persons with college education and responses on all relevant variables is reduced to 401.

Table 4 Placebo tests, estimates from matched data

	No college degree	College degree	Difference	*p*-value
Means, difference of means and *t* test *p*-values				
Frequency of attending political meetings, age 16	0.034	0.041	0.007	0.608
Frequency of attending youth clubs, age 16	0.234	0.262	0.028	0.404

	Standardized bias	Standardized bias 95 % confidence interval: lower bound	Standardized bias 95 % confidence interval: upper bound
Standardized bias			
Frequency of attending political meetings, age 16	0.080	−0.252	0.241
Frequency of attending youth clubs, age 16	0.047	−0.267	0.266

Table 4 presents means, differences of means, *t* test *p*-values as well as standardized bias measures and confidence intervals for these items post-matching. I find no statistically significant differences in the placebo test items between the treatment and control group after matching, which increases our confidence in the matched data.

Conclusions

This paper uses matching to assess whether education works as a cause or proxy for political participation. Previous research on this issue disagrees on what conclusion to draw from analyses of this kind. In particular, previous studies have struggled with obtaining balance after matching. This study brings the following contributions to the debate: It uses longitudinal data from the UK covering a longer time span than previous studies, making it possible to use information from early childhood for the matching procedure. Specifically, the data facilitates matching on a number of important pre-adult factors that previous research has lacked, measuring cognitive ability early in life. Using this data, genetic matching produces good balance and the results hold after sensitivity checks. Overall, the results support the null hypothesis indicating that there is no significant effect of education on political participation. Thereby, this study supports Kam and Palmer's (2008) conclusion that education does not cause political participation but rather works as a proxy.

However, it is not obvious that these results tell us anything definitive about the impact of education on participation in other countries. Some other studies using research designs appropriate to gauge causality have found that the effects of education on participation are stronger in the US than in Europe (Milligan et al. 2004; Dinesen et al. 2012). This would suggest that the education effect is context dependent and direct causal effects could develop in some contexts but not in others. But it is an open question whether an equivalent study conducted in the US would produce the same results. We should keep in mind that in a comparative perspective Britain is a society in which the link between social class origin and adult position is strong (Blanden et al. 2005). While we can be quite confident about the relationship between education and participation in the UK, we should be cautious to not directly generalize it to other contexts.

The effect of education on political participation, which is considered conventional wisdom in political behavior research, takes credit for factors that are most often unobserved such as cognitive ability and childhood socialization. When taking these factors into account, it is revealed that higher education does not in itself seem to have any causal effect on political participation.

Why does this matter for political participation research in general? Education is one of the most frequently used control variables in the field. Hence, it is important to know what it controls for. If we were sure that it, for example, measures skills we might not be as concerned about the causal relationship between skills and education. But education can be a proxy for several different factors, such as, a family tradition of participation, social status, social network centrality, political efficacy, etc. If education is used as a control variable and it captures the effects of other variables correlated with the main variables of interest in the analyses, the interpretation of the estimates can be problematic.

This study has focused on whether higher education has any causal effect on political participation, but if it does not, which other factors matter instead? Among the covariates used for matching in this article, cognitive ability, cultural activities and parents' education stand out as strong predictors for both educational attainment and political participation. It is not possible to point at one single variable as responsible for being the one and only variable that education is a proxy for, rather, it is likely a nexus of factors.

The results have important policy implications. Systematic inequalities in levels of political participation are often considered to be a democratic problem (cf. Lijphart 1997). If education is a cause for participation, raising the educational levels in a society could help address this problem. Yet instead if education is a proxy for mainly pre-adult factors, inequalities in participation are not likely to be mitigated by education. If governments want to increase political participation it would be reasonable to focus on those pre-adult factors that could influence participation, such as providing opportunities for young people to discuss politics, or even to learn how to participate in political activities early in life. However, if cognitive ability is the main factor driving educational choice and if this factor is inherited and individuals tend to raise their children as they were raised themselves, the education as a proxy view suggests that inequalities in participation might be reproduced from generation to generation.

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