Education Does not Cause Political Participation: Evidence From the 1970 British Cohort Study

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Abstract: According to the 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 but only a proxy for other factors that are not directly related to the educational experience, such as pre-adult factors. This paper engages in a current controversy 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 one week in the UK in 1970. The data includes a rich set of variables measuring factors in childhood and adolescence such as cognitive ability, activities and family socio-economic status. Results show that after matching there is no significant effect of education on political participation.
INTRODUCTION

It is a widely held view that education has a direct causal effect on political participation. Education supposedly increases relevant skills needed to understand and participate in politics, as well as increases political interest, sense of civic duty, and concern for the importance of political participation (e.g., Lewis-Beck et al. 2008; Verba, Schlozman, and Brady 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 not directly related to the educational experience, such as pre-adult factors or social network position (Berinsky and Lenz 2011, Campbell 2010, Kam and Palmer 2008; 2011, Nie, Junn and Stehlik-Barry 1996, Tenn 2007). Given that the literature in the field ascribes central importance to education as a predictor for political participation, it should be a major concern for political behavior research to find out how education is related to political participation.

Recently, 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 cause” or “the education as proxy view”. Results are mixed and there is a need for further replications on data with better information on pre-adult factors as well as a more hospitable ratio of treated to non-treated that could facilitate balanced matches.

This paper brings the following contributions to this debate. It sets out to test
the relationship using genetic matching on British data, and thus extends the geographical scope of the debate and tests the wider generalizibility of the previous findings. It uses data from a British cohort study that follows everyone born in the UK during one 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 SES. This data gives the opportunity to match on a number of important variables that are not included in the US datasets used in previous studies in the field. The results show that after matching, the relationship between education and political participation turns insignificant, i.e., supporting the education as a proxy view.

This paper will proceed as follows. The next section provides the theoretical framework. Thereafter data and techniques of analyses are presented. We then move to results and conclude by discussing the implications of the results.

THEORY

Years of education is a central predictor for political participation (e.g., Converse 1972; Verba, Schlozman and Brady 1995; Wolfinger and Rosenstone 1980). Recently, research has increasingly questioned whether years of education is a direct cause for political participation or merely works as a proxy for other factors (e.g., Berinsky and Lenz 2011; Burden 2009; Campbell 2009; Highton 2009; Kam and Palmer 2008; Sondheimer and Green 2009; Nie, Junn and Stehlik-Barry 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 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 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, Schlozman and Brady 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 cognitive ability and the political socialization process early in life (e.g., Sears and Funk 1999; Searing, Wright and Rabinowitz 1976; Jennings and Niemi 1974; Langton and Jennings 1968). Factors such as family socio-economic status, parents’ level of political participation, the discussion climate at home, and parents’ political orientations are the key 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, 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, Schlozman and Brady 1995; Wolfinger and Rosenstone 1980).
The problem is that the bulk of this research draws on regression analysis of cross-sectional data, which cannot be used to disentangle correlation from causation.

However, 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), randomized field experiments (e.g., Sondheimer and Green 2009), and matching analyses on panel data (e.g., Kam and Palmer, Tenn 2007).

Several of these studies have shown support for the education as a proxy view. Tenn (2007) uses panel data to isolate the marginal effect of years of education. The results show that there is very little impact of years of education on voter turnout. Berinsky and Lenz (2011) arrive at a similar 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. Highton (2009) uses the four-wave panel of the American Youth-Parent Socialization Study to estimate effects of education on political sophistication. He arrives at the conclusion that differences in political sophistication related to education were in place already before education was acquired. Pelkonen (2012) use reforms in the Scandinavian educational systems to gauge the effects of increased length of education on political participation, and show 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 (2009), who employ an experimental approach in which educational attainment is
altered exogenously. Strong support is found for the education as a cause view. However, vouchers for educational attainment were randomly assigned to groups dominated by low-SES citizens, and it is thus an open question whether the results are generalizable to the entire population. Likewise, Milligan, Moretti, and Oreopoulos (2004) use an instrumental variable approach to both American and British data with variation in 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, and 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. They 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 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 obs-
vations in the non-college attendance group are matched to a large number of college attenders, and that the results do not hold when conducting sensitivity tests. To obtain a better balance, both Henderson and Chatfield (2011) and Mayer (2012) use genetic matching. However, even with this more sophisticated technique, neither set of authors obtain balance robust enough to estimate the average treatment effect of the treated. Instead they estimate the average treatment effect of the controls, for which they achieve better 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 effect of education on political participation and confirm their original conclusion in support of the education as proxy view.

In sum, 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 as similar on all relevant covariates as possible with treated observations (Rubin 1973; 1974). If this is done successfully, comparing individuals similar on 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).

Why is it 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 nonoverlap 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 indi-
viduals from a 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., ranging from the individuals with 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 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 including all relevant covariates is first used to predict the probability of the treatment and 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), coarsened exact matching (Iacus, King and Porro 2012), and the support vector machine classifier (Ratkovic 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 to test the difference between treated and control’s marginal covariate distributions. Simulation studies have shown that genetic matching generally provides better balance than, for example, propensity score matching.\textsuperscript{1}

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 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,\textsuperscript{1}

\textsuperscript{1}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. Hence, coarsened exact matching cannot be used in order to robustly estimate the average treatment effect of the treated.
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. Formally this is referred to as the average treatment effect of the treated (ATT): \( \tau(T = 1) = E[Y_1 - Y_0 \mid T = 1] \). Using this formula, we condition the comparison on the distribution of the covariates among the treated individuals. The average treatment of the treated captures essentially 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.\(^2\)

\(^2\)An alternative causal estimate is the average treatment effect for the controls (ATC): \( \tau(T = 0) = E[Y_1 - Y_0 \mid T = 0] \). 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 had a causal effect on those who received it (as 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 preoccupied primarily 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 estimand. If there is no effect of higher education among those who received it, it is not reasonable to assume that there is a hypothetical education effect for those who do not receive it (cf Morgan and Winship 2007, 43).
DATA

In order to evaluate the effect of education on political participation data from the 1970 British Cohort Study is used. 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, and on some occasions their parents and teachers as well as their own children, 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 an MP, attended a public meeting or rally, and/or participated in a demonstration during the last 12 months. Since previous research has shown that different explanatory factors affect different forms of participation (Verba, Schlozman and Brady 1995), the participation items are analyzed separately. The treatment variable is also measured in the 2004 survey. As for the treatment variable some dichotomization is necessary and in this case higher education is coded as having received a Bachelor’s degree or higher.

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

\footnote{Information about the British Cohort Study 1970 is available at http://www.cls.ioe.ac.uk/. A study with similar design, National child Development Study, was conducted with individuals born in 1958. However, the 1958 cohort study unfortunately lacks enough variables measuring pre-adult factors to obtain balanced matches.}
old.

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, that 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 in making copies of eight designs (Davie, Butler, & Goldstein 1972). As for the Vocabulary test it was a modified version of the American Peabody Picture Vocabulary Test (Brimer & 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). A Principal Component Analysis of the items shows one single item. Following Deary et al (2008) I use the scores on the first unrotated component and convert it to a traditional IQ scale with a mean of 100 and SD of 15. This constructs the indicator of cognitive ability at age 5.

The 1975 survey included a maternal questionnaire with 13 Likert scale items measuring authoritarian rearing. The factor scores from the first component in a PCA covering these 13 items construct the measure of authoritarian rearing.4

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. A four point scale measure on to what extent the father played a role in managing the

4The 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
child is also included. Moreover, two indices are constructed covering cultural and family that predict future educational choice. The cultural activities index includes five items on how frequently the child reads books, goes to a club, or organization, museums, 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 for holidays together, go shopping together and chat for at least five minutes. For both these indices, measures constructed using the scores from the first unrotated factor in a PCA are used. Moreover, from the 1980 survey a measure on the number of children in the household (measured on a six point scale where the highest value is more than five) was collected.

The 1980 survey also included a cognitive test that draws on the British Ability Scales, which includes four subtests (Elliott, Murray and Pearson 1978). 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), and similar to the cognitive ability test at age 5 a variable is constructed that is totaling all four subtests using the scores from the first component in a PCA standardized with a mean of 100 and SD of 15. This item has been considered a good proxy for IQ (Elliott, Murray and Pearson 1978). Finally, from the 1986 survey I collect a variable measuring family income at age 16.

As is always the case with longitudinal individual data, panel mortality is a

\[\text{The full questionnaires including the cognitive ability test can be found at}\ http://www.cls.ioe.ac.uk/page.aspx?\&\text{sitesectionid}=809&\text{sitesectiontitle}=BCS+1980]
concern. The study started with 17,287 children born in 1970, and 16,571 (95.9\%) of the families participated in the first wave of the study. At the second wave response rate was 79.0\%, at the third wave 88.8\% and at the fourth wave 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. Item non-response reduces the sample further and leaving us with 2,837 individuals with full information on all relevant variables. Among these individuals, 569 had achieved a Bachelor’s degree or higher and 2,268 had lower educational qualifications.

RESULTS

In the original unmatched data, we find, as expected, that individuals who have achieved higher education participate in politics to a higher extent than those with lower educational qualifications. Table 1 presents the mean levels, the difference

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6A 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 subgroups of those with and without higher education. For more information on the non-responses see, McDonald et al. (2010). Non-responses were not missing at random; rather, they conclude that “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 2 or more children, born of non-married parents, a manual father and living in London” (McDonald et al. 2010, 26). However, interest in politics was not found to be associated with non-response.
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 level 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 percentage points and for petition signing it is 8.5 percentage points. The differences in absolute terms are smaller (only a few percentage points) for demonstration, contacts with MP, and attending of public meetings. However, taking into account the low overall participation levels in these acts, the differences imply that the higher educated are about twice as likely to participate in demonstrations, contact MPs, and attend public meetings then those without higher education.

Table 2 presents the difference between those with higher and lower educational qualifications in terms of the average treatment effect of the treated after the data has been preprocessed with matching. More specifically, the matching has been carried out using genetic matching 1:1 with replacement. 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.

How robust are these estimates? We begin by looking at the balance in the matched data. Table 3 presents p-values from tests of the covariate balance before and after matching. Before matching, the covariate distribution was significantly

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7The matching has been carried out with the GenMatch package in R.
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 controls. An alternative measure for continuous and multinomial variables is the Kolmogorov-Smirnov test. However, neither the naive nor the bootstrapped (10,000 draws) Kolmogorov-Smirnov p-values indicate significant imbalance at the 95% confidence level.

Although widely used, the procedure to conduct a t-test 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). Thus empirical QQ plots can be used as a more illustrative way to inspect balance. Figures 1 and 2 show the distribution of cognitive ability at age 5 and 10 for the treated and controls before and after matching (the other QQ plots are not shown due to space constraints). If balance is achieved, the distribution should be the same in the treated and the control group and the points in the plot should be placed on the 45 degree line (Sekhon 2011). As we can see, the distance to the 45 degree line is considerably reduced after matching, suggesting that the balance has thus increased.

So, the matched data seems to be balanced with regard to the covariates used in the matching procedure. However, the results might still not be robust if the estimates are biased by unobserved factors after matching. The Rosenbaum test is a sensitivity test used to evaluate such bias. It tests how much the probability of receiving the treatment needs to change in order to alter the p-value of the estimate (in this case the ATT). The probability of an underlying factor affecting the treatment is set at different levels of \( \Gamma \). If the significance of the estimate remains up to a specific
level of $\Gamma$, we can conclude that the probability of an underlying factor biasing the result is not a concern up to that specific level. Previous research disagrees upon the relevant levels of $\Gamma$ in social science research. Rosenbaum (2002) suggest values up to 6 or 7, while Keele (2010) argues that few results in the social sciences are robust to that level and instead suggests levels between 1 and 2.\(^8\) Rosenbaum sensitivity tests of the matched data from the British Cohort Study show that all ATT estimates reported in Table 3 remain insignificant, and become even more insignificant, when $\Gamma$ increases above 1.\(^9\) Hence, both balance tests and sensitivity tests indicate that results are robust.

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 4 and 5 report ATT estimates from genetic matching 1:2 and 1:3 with replacement and 1:1, 1:2, and 1:3 without replacement. Except for voting, for which the estimate gains significance at the 95 significance level when matching 1:2, the ATT estimates remain insignificant.

**CONCLUSION**

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 analyzes of this kind. In particular previous studies have struggled with obtaining balance after matching. This study brings the following contributions

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\(^8\)In previous research on education and political participation, Mayer suggests 1.3 and Henderson and Chatfield suggest 2 and 3.

\(^9\)Rosenbaum sensitivity tests are conducted using the rbounds package in R.
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. In particular the data facilitates matching on a number of important pre-adult factors that previous research has lacked measures on, most importantly, 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 no significant effect of education on political participation. Thereby, this study supports for Kam and Palmer’s (2008) finding that education does not cause political participation but rather works as a proxy.

The effect of education on political participation, which is considered conventional wisdom in political behavior research, clearly 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 is a control for. If we were sure that it, for example, measures skills we might not be as concerned about whether or not skills are a causal effect of education. But education can capture several different aspects of pre-adult socialization, for example a family tradition of participation, social status, social network centrality, political efficacy, etc. If education is used as a control variable and captures effects of other variables correlated with the main variables of interest in the analyses, the interpretation of the estimates might be problematic.
This study has focused on whether higher education has any causal effect, 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 robust 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 affecting childhood socialization.

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, 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. And most importantly, if cognitive ability is heritable 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.
REFERENCES


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

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<td>Difference</td>
<td>p-value</td>
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<td>.155</td>
<td>.000</td>
<td></td>
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<td>Demonstration</td>
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<td>.040</td>
<td>.021</td>
<td>.002</td>
<td></td>
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<tr>
<td>Signed a petition</td>
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<td>.302</td>
<td>.085</td>
<td>.000</td>
<td></td>
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<tr>
<td>Contacted MP</td>
<td>.034</td>
<td>.070</td>
<td>.037</td>
<td>.000</td>
<td></td>
</tr>
<tr>
<td>Attended public meeting or rally</td>
<td>.037</td>
<td>.067</td>
<td>.029</td>
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Table 2: Effects of higher education on political participation. Genetic matching (1 = 1) with replacement

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<tr>
<td></td>
<td>Estimate</td>
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<td>Voted in 2001 election</td>
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<td>Demonstration</td>
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<td>Attended public meeting or rally</td>
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Table 3: Covariate balance of higher educated and lower educated, t-test p-values

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<tr>
<td>Father higher education at age 0</td>
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<tr>
<td>Mother higher education at age 0</td>
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<td>.317</td>
</tr>
<tr>
<td>Cognitive ability at age 5</td>
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<td>Authoritarian rearing at age 5</td>
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<td>Cognitive ability at age 10</td>
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<td>Mother higher education at age 10</td>
<td>.000</td>
<td>1</td>
</tr>
<tr>
<td>Father plays a role in managing the child at age 10</td>
<td>.711</td>
<td>.158</td>
</tr>
<tr>
<td>Cultural activities at age 10</td>
<td>.000</td>
<td>.143</td>
</tr>
<tr>
<td>Family activities at age 10</td>
<td>.000</td>
<td>.286</td>
</tr>
<tr>
<td>Number of children in household at age 10</td>
<td>.000</td>
<td>.820</td>
</tr>
<tr>
<td>Family income at age 16</td>
<td>.000</td>
<td>.681</td>
</tr>
</tbody>
</table>
Figure 1: Cognitive ability at age 5 before and after matching

Figure 2: Cognitive ability at age 10 before and after matching
Table 4: Robustness checks of the effects of higher education on political participation. Genetic matching (1:2, 1:3) with replacement

<table>
<thead>
<tr>
<th></th>
<th>1:2</th>
<th></th>
<th>1:3</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimate</td>
<td>p-value</td>
<td>Estimate</td>
<td>p-value</td>
</tr>
<tr>
<td>Voted in 2001 election</td>
<td>.081</td>
<td>.010</td>
<td>.054</td>
<td>.091</td>
</tr>
<tr>
<td>Demonstration</td>
<td>.016</td>
<td>.178</td>
<td>.012</td>
<td>.367</td>
</tr>
<tr>
<td>Signed a petition</td>
<td>.035</td>
<td>.279</td>
<td>.0</td>
<td>1</td>
</tr>
<tr>
<td>Contacted MP</td>
<td>.030</td>
<td>.084</td>
<td>.007</td>
<td>.709</td>
</tr>
<tr>
<td>Attended public meeting or rally</td>
<td>.026</td>
<td>.198</td>
<td>.021</td>
<td>.227</td>
</tr>
</tbody>
</table>

Table 5: Robustness checks of the effects of higher education on political participation. Genetic matching (1:1, 1:2, 1:3) without replacement

<table>
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<th>1:1</th>
<th></th>
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<th></th>
<th>1:3</th>
<th></th>
</tr>
</thead>
<tbody>
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<td></td>
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<td>p-value</td>
<td>Estimate</td>
<td>p-value</td>
<td>Estimate</td>
<td>p-value</td>
</tr>
<tr>
<td>Voted in 2001 election</td>
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<td>.053</td>
<td>.105</td>
<td>.001</td>
<td>.049</td>
<td>.117</td>
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<td>.157</td>
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<td>.602</td>
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<td>.033</td>
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<tr>
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<td>.410</td>
<td>.026</td>
<td>.101</td>
<td>.019</td>
<td>.267</td>
</tr>
<tr>
<td>Meeting/rally</td>
<td>-0.005</td>
<td>.790</td>
<td>.021</td>
<td>.201</td>
<td>.012</td>
<td>.467</td>
</tr>
</tbody>
</table>