Does Education Cause Participation in Politics?

Mikael Persson

UNIVERSITY OF GOTHENBURG
DEPT OF POLITICAL SCIENCE

Distribution:
Mikael Persson
Department of Political Science
University of Gothenburg
Box 711
SE 405 30 Gothenburg
Sweden
mikael.persson@pol.gu.se

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Dedicated to Anna, Viktor and William

Acknowledgments

Many coincidences led up to the writing of this dissertation. Having grown up outside Gothenburg, I left to study philosophy after I had finished school. I had no intention of returning and neither did I have any plan to try to become a researcher; I just wanted to study something interesting somewhere else. As it turned out I was more suited for political science than philosophy and when I had finished my undergraduate courses I still wanted to continue studying political science. However the PhD-program in Lund, where I studied, was temporarily closed, and for a while I turned to my plan B; to become a teacher. While I studied at the teacher college in Malmö, I wrote a paper in which I argued the there was a need to empirically test whether education actually had all the positive effects that some of the theoretical course literature suggested (including effects on political participation). At this time, it was also an opportunity to apply to the PhD-program in political science in Gothenburg. I didn't have much time to prepare the application so I sent a revised version of the paper I had just written at the teacher college as an 'idea for a dissertation'. As fate would have it I was accepted in Gothenburg and I moved home. What started as a course paper at the teacher college grew larger and became this dissertation.

Academically, moving to Gothenburg turned out to be a good decision. I was fortunate to get Peter Esaiasson as my primary supervisor and Henrik Oscarsson as my secondary supervisor. In addition, and without any official duties, Mikael Gilljam supported me a lot and became my mentor in the academic world. Most of what I know about political science research I've probably learned from Peter, Henrik and Mikael. They generously shared their knowledge and supported me far more than they were required.

I don't dare to think about the terrible mess of a dissertation—if it had been any dissertation at all—that I would have written without the influence of Peter. I'm in great debt to him for pushing me early on to write quantitative papers in English and try to publish in academic journals. Additionally, he continuously encouraged me to raise the bar throughout the entire process, expecting nothing less than the most out of me. Peter's enthusiasm is unprecedented and he kept providing me with his right-on-the-spot comments until the last version was finished.

Henrik introduced me to the world of election studies and statistics. He was also my first academic co-author. At that time the main task for someone

working with me was to delete all the crappy text I produced. Today I certainly wouldn't like to work with the person I was then. But Henrik managed, with great patience, to tech me about the craftsmanship of paper writing and statistical analyses.

Mikael has been extremely supportive since my first day in the department. Among his many strengths, he has a great ability to write in an extremely clear, accessible and entertaining way. I'm grateful to him for doing his best to try to teach me this art, although I rarely, if ever, succeed. These years would also have been much less entertaining without all the fun times I've had with Mikael – from shaking Danes on planes to almost getting killed in waters slides. I'm also indebted to him for helping me to get funding to continue to do research in new exciting fields.

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for giving me the opportunity to be able to continue to develop research on education and participation together with them.

During my journey in the educational system some teachers have been particularly important to me. Most importantly, my outstanding social science teacher in the gymnasium, Claes-Göran Hedlund, gave me a solid ground and provided me with the encouragement I needed to continue to study. At Lund University Maria Hedlund, Mats Sjölin and Wlodek Rabinowicz carefully supervised me and encouraged me to apply to the PhD-program.

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Gothenburg, October 2013

Mikael Persson

Introduction

What affects who participates in politics? Many studies point out that education is of central importance. In most studies of political behavior it is found that individuals with higher education participate to a larger extent in political activities than individuals with less education (see e.g., Wolfinger and Rosenstone 1980; Rosenstone and Hansen 1993; Verba, Schlozman and Brady 1995; Franklin 2004; Schlozman, Verba and Brady 2012). In a classic text, Converse pointed out that education "is everywhere the universal solvent, and the relationship is always in the same direction. The higher the education, the greater the 'good' values of the variable. The educated citizen is attentive, knowledgeable and participatory, and the uneducated citizen is not" (1972, 324). The idea that education has a causal impact on participation is widely held in political behavior research. Indeed, the relationship between education and political participation is probably the single most well established relationship in the participation literature.

Why do highly educated persons participate more in political activities? In their seminal work, Verba, Schlozman and Brady (1995, 305) explain that: "Education enhances participation more or less directly by developing skills that are relevant to politics—the ability to speak and write, the knowledge of how to cope in an organizational setting". Lewis-Beck et al. (2008, 102) point out that: "With more formal education comes a stronger interest in politics, a greater concern with elections, greater confidence in playing one's role as a citizen, and a deeper commitment to the norm of being a good citizen". Hence, education increases skills and knowledge which might also affect political interest and efficacy; factors that all in turn trigger participation.

But is this conventional view correct? Does education actually cause people to participate in politics? In this dissertation, I will present empirical evidence that questions this conventional view on how education is related to political participation. While education and political participation are undoubtedly correlated, I suggest that these factors are not causally related in the way that is usually assumed. As the slogan goes: correlation is not causation. Contrary to conventional wisdom, I argue that the relationship between education and participation is misinterpreted in most political behavior research.

This study is not the first to make this claim. In the last decade a number of studies have started dealing with the question of whether education

is a direct *cause* for political participation or merely a *proxy* for other factors (Nie, Junn and Stehlik-Barry 1996; Dee 2004; Milligan, Moretti and Oreopoulos 2004; Hillygus 2005; Kam and Palmer 2008; Burden 2009; Campbell 2009; Highton 2009; Sondheimer and Green 2010; Berinsky and Lenz 2011). The papers in this dissertation contribute to this debate by providing a set of analyses on how education is related to political participation.

Knowing who participates in politics is a central issue in political behavior research. Finding out which model can correctly explain the relationship between education and participation has important implications. If education has no direct causal effect, then the relationship between education and participation found in most political behavior research is misinterpreted. If we do not even know how we should explain the most frequently occurring relationship in participation research, our understanding of who participates in politics must be regarded as shallow. Hence, getting a better understanding of the relationship between education and participation is crucially important for the improvement of knowledge about the causes of political participation. As I will argue, this is not only of importance for political behavior research but it also has important policy implications and consequences for the functioning of democratic systems.

This introductory chapter will proceed as follows: The next section provides a theoretical overview, followed by a summary of the most important empirical analyses in the field. Thereafter, the five papers are summarized in brief and their collected contribution to the field is explained. Finally, the theoretical implications and the policy implications are discussed and the conclusions from the studies are summarized.

Theoretical overview

This study focuses on the driving forces on political participation. I follow the standard definition of political participation provided by Verba, Schlozman and Brady (1995, 38) according to which political participation refers to activity "that has the intent or effect of influencing government action—either directly by affecting the making or implementation of public policy or indirectly by influencing the selection of people who make those policies." This includes acts such as voting, activities in political parties, attending political meetings, demonstrations and contact with politicians. It can also refer to acts such as political consumerism or online participation but it excludes acts such as political discussion. In this study, the focus is primarily

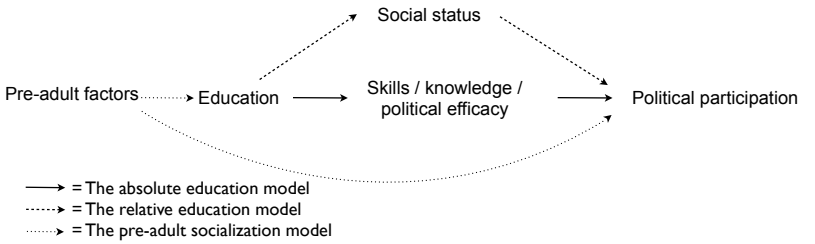
on traditional forms of participation, in particular voter turnout and party engagement.[1]

With education I refer to formal education, from compulsory schooling to higher education at universities. The different studies in this dissertation look at different aspects of education, such as years of education, college education and type of education at the upper secondary level. This dissertation does not focus on informal education, training courses, etc.[2]

Having established the definitions of the main independent and dependent variables, what can we learn from previous research about how education is related to political participation? In the literature, three models dominate the discussion on the links between education and political participation. Following previous research, I will refer to these models as "the absolute education model", "the pre-adult socialization model", and "the relative education model".

The latter of these two models regard education as a proxy for other factors not directly related to education while the first regards the educational experience as a direct cause. Figure 1 illustrates the three theoretical models.

Figure 1. Theoretical models of the relationship between education and political participation



The different models disagree on which, if any, causal mechanisms that trigger the effect of education on political participation.[3] Previous research seldom discusses these three competing explanations together and there is surprisingly little communication between researchers studying the pre-adult socialization model and the relative education model. To date, there exists no study that tests all three models simultaneously. In the absence of one single source of data that could be used for a simultaneous evaluation of the three models, each of the papers in this dissertation test different aspects of the models. Taken together they clarify our understanding of the relationship between education and political participation.

The conventional view: the absolute education model

According to the absolute education model, illustrated by the solid line in Figure 1, education has a causal effect on political participation and for that reason this model is sometimes synonymously referred to as the "education as a cause view". Education increases civic skills and political knowledge which function as the causal mechanisms triggering participation. This is also sometimes referred to as the "cognitive pathway", i.e., what individuals learn at school has positive effects on their cognitive ability, which in turn affects participation. Most important for political participation is the increased verbal and cognitive proficiency that comes with higher education. This is because language is crucial to understanding and communicating about politics. Persons with high verbal and cognitive proficiency can more easily understand political messages and in turn articulate their own political views. In addition, education is supposed to increase political knowledge and provide a better understanding of the political system. According to this model, people who understand how the political system works will also understand the importance of participating in the system.

In addition to skills and knowledge, it has also been argued that education triggers political efficacy. Jackson (1995, 280) explains this idea: "Schooling enhances both the belief that the potential voter can influence what the government does (external efficacy) and the belief that the potential voter has the competence to understand and participate in politics (internal efficacy)". Hence, education supposedly increases citizens' beliefs that they can effectively play a role in the political process.

According to this conventional view, the more education individuals have, the more likely they will be to participate in politics. The model is referred to as the absolute education model since the effects of education are not dependent on the level of education in the environment. This model regards education effects as an individual level cognitive process.

Numerous studies of political participation in Western democracies confirm this view, however, most of these studies draw on cross-sectional data and the alleged causal mechanisms—mainly knowledge and skills—are seldom directly tested. Even those that stick to the view that education is a direct cause have seldom presented evidence on exactly how and through which mechanisms education influences participation. Rather, when it comes to explaining effects of education it is common to describe the mechanisms at work as "remaining hidden" or as an "un-deciphered black box" (cf. Ichilov 2003; Niemi and Junn 1998).[4]

While the literature in the field focuses primarily on skills and knowledge as the causal mechanisms it is not hard to think of other factors that might be affected by education and that in turn might trigger participation, i.e., factors such as income, wealth or health. However, an obvious problem is that these factors are likely to be strongly correlated with education and it might be difficult to test exactly to what extent each of these factors mediates the relationship. Hence, while skills and knowledge are the causal mechanisms that are most often emphasized in the literature, the relationship might flow through alternative causal pathways as well.[5]

It should also be noted that there is no consensus on whether the effect of education is linear or whether it tapers off at some point. While many researchers simply test the effects of "years of education", others argue that it is in fact only higher education (college or equivalent) that is of major importance for participation. To make it even more complicated, studies on the impact of college education disagree on whether it is college *attendance* or college *completion* that is the relevant variable to study (cf. Kam and Palmer 2011; Henderson and Chatfield 2011).

The pre-adult socialization model

The extreme alternative to the absolute education model is the pre-adult socialization model, illustrated by the dotted lines in Figure 1. It suggests that the relationship could be explained with reference to self-selection effects; pre-adult factors affect both educational choice as well as political participation in adulthood. Education works as a proxy for factors such as family socio-economic status, the political socialization in the home environment and personal characteristics such as cognitive ability (e.g., Jennings and Niemi 1974; Langton and Jennings 1968; Kam and Palmer 2008). Hence, some refer to this as the "education as a proxy view". Other researchers argue that factors such as intelligence (Luskin 1990), genetic factors (Alford, Funk and Hibbing 2005) or personality types affect political participation in adulthood (Mondak and Halperin 2008), and these factors could also affect educational choice. It is factors like these, rather than education that affect participation according to the pre-adult socialization model. This idea is supported by research that argues that political attitudes and behavior are formed early and change little after the "impressionable years" (cf. Sears and Funk 1999).

According to the education as a proxy view, the same pre-adult factors that encourage political participation also determine the choice of education. The problem is that the measurement of such pre-adult factors is often omitted in surveys. The pre-adult socialization model suggests that when pre-

adult factors are not included in statistical models of the causes of participation, education will take credit for these unmeasured pre-adult covariates. Hence, the significant coefficients of education are often misinterpreted as a direct effect while they are only a proxy for other factors.

The education as a proxy view goes back to Langton and Jennings's (1968) seminal study, which showed null results regarding the impact of civic education courses on political participation. However, since education repeatedly showed a strong impact on participation in cross-sectional studies, scholars regarded education as a major influence on political participation (Converse 1972).

The implications of the pre-adult socialization model are drastic. If correct, results from most studies on political behavior that include education as a main independent variable are misinterpreted since the content and length of education is irrelevant. It is not the skills and knowledge gained through education that matter but rather unmeasured pre-adult factors that produce the effects. In addition, there is no room for state intervention in encouraging citizens to participate in politics if this model is correct.

The relative education model

Now, lets turn to the third model. The relative education model, which is synonymously referred to as "the sorting model", offers a revisionist view. It takes a different causal path than the two other models, as illustrated with the dashed arrows in Figure 1. According to the sorting model, there is an indirect effect of education on political participation via social status (Nie, Junn and Stehlik-Barry 1996). Within this literature, high social status is defined as having a central social network position in society.

The relative education model has been presented as a potential solution to one of the major puzzles in political behavior research: the paradoxical relationship between education and participation at the micro- and macro-levels. On one hand, many studies claim that education has a positive impact on participation at the individual level; but on the other hand, increased levels of education at the macro-level do not seem to increase aggregate levels of political participation (Brody 1978; Schlozman, Brady and Verba 2012). Therefore, while many studies have shown that at any given time people with higher education participate to a larger extent in political activities, it does not appear that an increasing level of education in the population as a whole, leads to an aggregate increase in political participation. Delli Carpini notes that: "researchers have noted this paradox but have largely addressed it by assuming that other societal changes (the weakening of political parties, the

erosion of civil society, the increased complexity of politics, the declining quality of education, the growing dominance of television as a source of political information, and so forth) have worked to cancel out the positive effects of education" (1997, 972).

Instead of referring to other factors that might be canceling out the positive effects of education, Nie, Junn and Stehlik-Barry try to solve the paradox by providing an alternative way to understand how education is related to participation. While this model was first applied to education effects on political participation in their 1996 book *Education and Democratic Citizenship in America*, similar theories have been used in, for example, labor market studies. In this context, the argument was made by Fred Hirsch, who argues that as educational levels rise, "the effect will be to push competition by hitherto qualified applicants down the hierarchy of jobs" (1978, 50). According to Nie, Junn and Stehlik-Barry, it is the social status and not the educational content received that increases participation. This means that education should be seen as a "positional good", i.e., something that is "valuable to some people only on condition that others do not have it" (Hollis 1982, 236).[6]

According to the relative education model, individuals with a high social status are exposed to networks that encourage participation and they are also more likely to be recruited into political activities. Conversely, individuals with lower levels of education are outside recruitment networks (cf. Verba, Schlozman and Brady 1995). Franklin's discussion of the costs and benefits of voting illustrates this idea: "People in social networks would also incur costs of nonvoting because other members of their group care whether they vote or not ... So, the benefits of voting and the costs of nonvoting are higher for socially connected people" (Franklin 2004, 51). Obviously, it is not only the "social status" of the networks that matter but also which particular people who make up the networks and to what extent these people might trigger participation.

An important implication of the sorting model is that the same amount of education at the individual level has a different impact on political participation depending on the level of education in the environment. In a low education environment, less education at the individual level is needed in order to gain a central social network position. Conversely, in places with a lot of highly educated persons, higher levels of education are needed to get high social status. This could also be illustrated by the trends over time; for example, as more people obtain higher education, the social status of a college

diploma is reduced in relative terms. Hence the impact of education on political participation is hypothesized to be relative rather than absolute.

An underdeveloped area in this field is *how* social networks mediate the relationship between education and participation. Nie, Junn and Stehlik-Barry employ a narrow definition of social network position since it only takes into account relations to people active in politics and media. However, in research on social networks, the size and composition of the social networks are seen as central. More specifically, the range of people (e.g., Mutz 2002; McClurg 2003; Siegel 2009) and size of the networks (e.g., Kotler-Berkowitz 2005) matter for participation. Drawing on this literature this dissertation will test the refined hypothesis that having strong ties to a large and wide network of high-status persons mediates the relationship between education and participation.

Summary of the theoretical models

To sum up the theoretical overview, the reason why previous research has had problems determining whether a causal link between education and political participation actually exists is due to the difficulties in testing the three potential explanations empirically. The central question concerns whether the relationship is causal or merely a correlation. The three models can be summarized as follows: a) the correlation exists due to self-selection processes and education is only a proxy for pre-adult factors, b) education actually has a causal effect on political participation primarily via the causal mechanisms skills and knowledge, or c) social status gained by relative education affects participation. In other words, the relationship can be explained with reference to a) self selection processes *before* education is acquired, b) skills and knowledge gained *while* education is being acquired, or c) the social network position gained *after* education is acquired. All three competing explanations are seldom discussed together in previous research and there is surprisingly little communication between researchers studying the pre-adult socialization model and the relative education model.

Literature review: empirical results

Methodologically, it is a difficult task to estimate the causal effect of education. In one-shot cross-sectional observational studies the causal effect of education is hard to isolate due to confounding factors, i.e., variables possibly related to both education and participation. In studies of causal relationships, randomized experiments are the gold standard for estimating causality (Gerber and Green 2012). Hence, in this case an ideal research design would randomly assign persons to receive different levels of education. However, such

a research design could obviously never be implemented. Even if it would be possible to randomly distribute scholarships for higher education it would be hard to ensure that everyone participated in such an experiment. Likewise, it would not be possible to hinder those who were assigned to the control group from receiving education. In addition, such an experiment would raise ethical questions and would probably be impossible to justify morally.

What is left is the possibility of estimating the effect from observational studies and quasi-experimental situations. Even though we lack fully randomized experiments we can still get some answers by following persons over time in panel studies, exploiting natural experiments like educational reforms, using statistical tools designed to estimate causal relationships from observational studies and by exploiting regional differences and differences over time. Such studies can provide us with valuable knowledge on how education is related to political participation. Here follows a review of the most important empirical contributions to the debate.

Education as a cause vs. education as a proxy

Recently some studies have begun to use the aforementioned techniques to gauge whether education is a direct cause (the absolute education model) or a proxy (the pre-adult socialization model) of political participation. These studies include applications using techniques such as instrumental variable approaches (e.g., Berinsky and Lenz 2011), field experiments (e.g., Sondheimer and Green 2009), and matching analyses on panel data (e.g., Kam and Palmer 2008; Tenn 2007).

The education as a proxy view has been supported in a number of these studies. Pelkonen (2012) uses the natural experiment of an education reform in Norway, initiated in 1959, that increased the length of compulsory education to gauge the causal effects of education on political participation. This reform was implemented quasi-randomly at different times and in different areas of Norway. Pelkonen uses both individual level data and data at the municipality level, both of which show no effects of education on different participatory acts such as voting, contacting political representatives and demonstrations (with the exception of a significant effect on signing petitions). Given the solid research design—the quasi-experiment is a strong identification strategy—the results should be regarded as strong evidence in favor of the pre-adult socialization model.

Berinsky and Lenz (2011) arrive at a similar conclusion by using the natural experiment of the Vietnam-era draft in which young males were randomly assigned to the military by draft lotteries. It was possible however,

to bypass the system since those who went to college could defer military service; hence the draft lottery functioned as an exogenous shock on educational attainment. Berinsky and Lenz find little evidence that increased educational attainment positively affected political participation. Results from this study weigh heavily since it has the advantage of a randomly assigned exogenous shock that affects educational attainment. A problem with this study however, is that there might be a bias in the distribution of the treatment towards (male) persons who wanted to avoid military service; the treatment is not distributed equally across the population.

Kam and Palmer provided the first study that used matching techniques to evaluate this question. Matching can be used to control for the selection into education and thereby mirroring an experimental design (Rubin 1973; 1974). When using this method, persons with higher education, which are as similar as possible on all relevant covariates, are matched with less educated persons. Kam and Palmer applied propensity score matching to two studies from the United States. They did not find any significant differences in participation between college attendees and non-attendees after matching.

Kam and Palmer were criticized in two independent works. Henderson and Chatfield (2011) as well as Mayer (2011) argue that a main problem in Kam and Palmer's analyses is that the groups of college and non-college persons remained very different even after matching. To obtain better balance between the "control group" and "treatment group", Henderson and Chatfield as well as Mayer use genetic matching which is a superior technique. 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) is more positive regarding the possibility of obtaining causal estimates from matching and concludes that his analysis shows "evidence that postsecondary educational advancement has a positive and substantively important causal effect on political participation". The debate continued when Kam and Palmer (2011, 661), in a response, reanalyzed one of their datasets that confirmed their initial results as well as showed that when using genetic matching, balance could be achieved.

However, there are also a number of recent studies showing evidence indicating that education actually has a direct causal effect. Sondheimer and Green (2009) exploit three field experiments in which different interventions affecting educational attainment were randomly assigned to different students (i.e., smaller classes, extra mentoring, and pre-school activities). Students who experienced these treatments had a higher probability of graduating from high

school. In this study, which particularly focuses on voting, strong support is found for the education as a cause view. A shortcoming of the study is that randomization was not made among the entire population, or a representative sub-sample, but directed primarily to students with a low socio-economic status. The generalizability of the results is therefore unclear; it might be the case that the effect of education is stronger for low socio-economic status students than among the population in general.

Milligan, Moretti and Oreopoulos (2004) use compulsory schooling laws as instrumental variables. They show that completion of high school has a positive impact on voting in the United States, while it has no effect in Britain. However, after controlling for registration requirements, the effect of education in the United States is considerably reduced. The reason why this study comes to different conclusions in the United States and Britain remains unclear.

The study by Dee (2004) also uses the adoption of school leaving laws as an instrumental variable to gauge the causal effects of education in the United States. In addition, he also uses geographical distance to colleges as an instrument for education. Dee's analyses indicate that education has a positive impact on voting and also increases the support for free speech and civic knowledge. However, both instrumental variable approaches used are problematic. As for distance to college, it could reasonably be suspected that the place of residence is correlated with other unmeasured factors influencing participation. Regarding the child labor laws it is unlikely that changing child labor laws provide an exogenous shock on educational attainment that is proportionally spread among the population. It is reasonable to expect that changes in child labor laws primarily affect students from homes with a low socio-economic status, so it does not correspond to a treatment that would be distributed equally throughout the population.

While the studies of Sondheimer and Green, Milligan Moretti and Oreopoulos and Dee use solid research designs exploiting exogenous shocks on educational attainment, they are still far from the ideal experimental design. It must also be pointed out that none of these studies say anything about *how* (i.e., through which causal mechanism) education affects participation. They do not confirm that the effect runs along the hypothesized cognitive pathway.

This survey of the field shows that studies using sophisticated designs to trace causality are not in any agreement on whether education causes political participation. Rather, this is an unsettled issue in which different studies show contradictory results. On both sides there exist studies with solid research designs showing support for the different models.

The fourth paper in this dissertation will contribute to this discussion by presenting a matching analysis that aims to mimic an experimental test of the causal effect of college education. This analysis will use a richer source of data with more comprehensive pre-adult measures than used by previous studies.

The impact of type of education

The literature on whether education functions as a cause or proxy for political participation has largely focused on the effects of length of education. However, there are other relevant dimensions of education as well. One important field of research has looked particularly at the effects of civics courses. The panel study by Langton and Jennings (1968) is the most important contribution to this field. They found that civic education courses did not affect civic outcomes. However, Niemi and Junn (1998) later challenged this conclusion by showing that civic education actually had a positive impact on civic outcomes, in particular civic knowledge (see Denver and Hands (1990) for a similar argument using data from Britain). Recent evidence from Kenya provides further evidence of the positive effects of civic education courses on political participation (Finkel, Horowitz and Rojo-Mendoza 2012).

Looking more broadly at the impact of type of education, it is a frequently occurring argument that different educational tracks lead to different patterns of political behavior and attitudes. In the Swedish case, several studies have found significantly higher levels of participation among individuals from theoretical gymnasium tracks (academic upper-secondary tracks aiming to prepare students for further studies at universities) compared to students with education from vocational gymnasium tracks (Ekman 2007; Öhrvall 2009). In addition, Westholm et al. (1990) showed that students from vocational tracks had significantly lower levels of political knowledge than students from the theoretical tracks. A similar pattern is occurring in several other countries such as Norway (Lauglo and Øia 2006), Italy (Losito and D'Apice 2004) and Belgium (Quintelier 2008). In addition, a cross-national study by Van de Werfhorst (2007), which covers 17 countries, shows that students from vocational tracks were less politically active than students with theoretical educations. Yet, all this evidence comes from observational studies that cannot say whether the relationship is causal.

The Persson and Oscarsson (2010) study used the natural experiment of the Swedish reform of the Swedish gymnasium (upper-secondary school) to analyze the effect of type of education on political participation. In the mid-1990s an extensive reform of the Swedish educational system was initiated to

create a "school for everyone" which intended to function like a "social equalizer". The new unified gymnasium initiated longer vocational educational tracks with an extended curriculum of social science courses. Given the importance ascribed to social science courses in previous research, this could be hypothesized to have positive effects on political participation. Trends in participation among students before and after the reform can provide some evidence on the potential effects of the reform. However, the reform of the Swedish gymnasium did not produce the hypothesized positive effects on political participation. Significant differences in political participation between students from different tracks remained after the reform.

Other studies focusing more broadly on type of education also find significant correlations with political participation. Using British longitudinal data, Paterson (2009) finds positive relationships between political participation and taking social science courses at universities. The best study on the impact of type of college education in the United States (Hillygus 2005, 38) finds that "students who concentrated their studies in biology, chemistry, engineering and the like appear less inclined to participate politically, while those in the social sciences and humanities are more likely to vote and participate in other forms of political activity." This study is especially interesting since it suggests a causal mechanism; curriculums that develop civic skills are those which have the strongest impact on participation (Hillygus 2005; also see Nie and Hillygus 2001). A similar pattern has also been found in later studies. Niemi and Hanmer's (2010, 319) study of voter turnout among American college students showed that those who study mathematics, science and engineering voted less often than those who study subjects such as social science and humanities. Hence, it is fairly well established that there is a correlation between social science courses and political participation. But there is a lack of studies using solid research designs to estimate whether the type of education has causal effects on participation. Indeed many studies point in the direction that social science education, or academic tracks in general, might have positive effects on political participation. But the results from most studies discussed in this section however, could be consequences of self-selection processes.

The fifth paper in the dissertation aims to bring some new knowledge to this sub-field by testing whether different tracks in the Swedish gymnasium (students aged 16 to 19) affect intentions to participate, using a panel study design.

It should also be mentioned that there are other dimensions of education that could possibly affect future political participation, such as the quality of

education, teaching styles, etc. However, such studies are very rare. Hillygus (2005) shows that the quality of the educational institution has insignificant effects on participation. When it comes to the impact of teaching styles, Campbell (2008) shows that American students who are experiencing an open classroom climate show higher levels on several civic outcomes, including that they are more likely to vote in the future. In a Swedish study Andersson (2012) shows that "deliberative teaching" has beneficial effects, especially on students with low socio-economic status, on a range of outcomes including readiness for political participation. The papers in this dissertation do not bring any new evidence regarding the effects of factors such as quality of education, teaching styles, etc. Persson (forthcoming) however, replicates the ideas from Campbell's (2008) study in the Swedish case with a panel study design and confirms the beneficial effects of an open classroom climate on political knowledge. Additionally, Esaiasson and Persson (forthcoming) present evidence on the civic outcomes of political science education.

Previous research on the relative education model

The relative education model proposed by Nie, Junn and Stehlik-Barry (1996) suggests that social status is the causal mechanism connecting education and political participation. According to them, the impact of education is relative rather than absolute. This means that the value of education depends on how many others possess it. The provocative implication of the model is that, when it comes to its relation to political participation, education is only a proxy for social status; it is not the skills or knowledge gained through education that matter. Using American data Nie, Junn and Stehlik-Barry empirically test the relative education model and find support for it on a broad range of participation types. They find that the aggregate level effect of education is negative and discounts the positive effect of education at the individual level, which could possibly explain the paradox of participation. Educational inflation is thus hypothesized to be the reason why higher aggregate levels of education have not resulted in higher aggregate levels of participation: "More education does not change the nature of the hierarchy; rather, it simply shifts the baseline upward" (Nie, Junn and Stehlik-Barry 1996, 106).

A handful of studies, all concentrated on the US case, have pushed this sub-field forward and developed the relative education model (see Campbell 2013 for a review). Several aspects of the model have become contested. First and foremost, studies in the wake of Nie, Junn and Stehlik-Barry disagree on the scope of the model. Nie, Junn and Stehlik-Barry claim that all types of participation are affected by education in a relative rather than an absolute

way. The reason for this is that they consider participation to be a zero-sum game: "The instrumental behaviors and cognitions of political engagement can be seen as more of a zero-sum game, bounded by finite resources and conflict, where one's gain will necessarily be another's loss" (Nie, Junn and Stehlik-Barry 1996, 101).

Campbell (2009), by contrast, suggests that some forms of participation are not competitive in character. Hence, it is unclear why the relative education model should be relevant to all forms of political participation and Campbell argues that the model is only valid for forms of political participation that are actually competitive and social in character.

There is also disagreement on how to test the relative education model. The disagreement concerns how to define the "educational environment". In the original work of Nie, Junn and Stehlik-Barry, each person's educational level is compared with the mean national levels among individuals aged 25–50 when the respondent was 25. A serious problem with this model specification is that it is not possible to separate the impact of the educational environment from the impact of age and year of birth (cf. Tenn 2005). Additionally, it does not take geographical variation into account. Helliwell and Putnam (2007) try to overcome these problems in their study by using narrow geographic units. More precisely, Helliwell and Putnam (2007, 3) compare each respondent's education to "all other living adults, both older and younger" within the same geographical unit. Helliwell and Putnam focus only on social capital and find no support for the relative education model, however, since Helliwell and Putnam's relative education measure is correlated with geographic region, it is impossible to control for state-level variations. In the study by Tenn (2005), intra-birth cohort measures of the educational environment are used to test the relative education model on voter turnout in the United States. Tenn defines relative education as each individual's education compared to the mean level of education of everyone born in the same year (throughout the United States), but again, a problem with this kind of measure is that it does not consider geographic differences. However, the results from Tenn's study provide strong support for the relative education model. Campbell's (2009) study tries to overcome the problems associated with the previous studies by narrowly defining the educational environment as for *both* age and place. Campbell claims that since social status is formed in relation to one's personal contacts, the local geographical context needs to be taken into account. Campbell finds support for the relative education model on competitive forms of political participation, including "electoral activities".

In sum, a number of studies have found empirical support for the idea that the effect of education is relative rather than absolute. However, there is no agreement on the scope of the model nor how the educational environment should be operationalized. Further studies would benefit from implementing better ways to measure the "educational environment" and thereby separating the effects of relative and absolute education.

Summary of the literature review

Taken together, what can we learn from the state of the field? It is a frustrating fact that studies with equally strong research designs point in different directions. Studies using randomized field experiments, instrumental variable approaches and matching techniques show support for the idea that education is a direct cause for participation (Dee 2004; Milligan, Moretti and Oreopoulos 2004; Sondheimer and Green 2010; Mayer 2011). However, other studies using natural experiments in the form of education reforms, randomized shocks such as the Vietnam-era draft and matching analyses show support for the education as a proxy view (Kam and Palmer 2008; Berinsky and Lenz 2010; Pelkonen 2012). Hence, it is not the case that studies using strong research designs show support only for one side rather than the other. The literature provides a frustrating, divided picture and we are left without a clear answer as to whether education causes political participation. In addition, a number of studies complicate the discussion further by arguing that the effect of education is relative rather than absolute.

The papers in brief

The overarching research question of this dissertation is how education is related to political participation. Unfortunately the theoretical models presented are not possible to test in one single study since one single dataset does not exist that makes possible a simultaneous evaluation of the three models. For that reason, the papers in this dissertation test different aspects of the three models. For each research questions presented I try to use the best data available and the most suitable research design and statistical analyses. Hence, the papers draw on a variety of data sources: the Swedish National Election Studies, the Swedish SOM surveys, the Comparative Study of Electoral Systems, the European Social Surveys, the 1970 British Cohort Study and original data collected by the author. Additionally, in order to use the most suitable statistical analyses for each research question, the papers employ techniques such as regression analyses, multilevel modeling, structural

equitation modeling, panel data analyses and genetic matching. Each of the papers brings parts of evidence to the puzzle. Taken together they clarify our understanding of the relationship between education and political participation.

The first three papers test different aspects of the relative education model. The first paper tests the relative education model in Sweden, a more egalitarian and homogenous context than the United States that constitutes a hard test case for the model. The first paper deals with the research question: Is the relative education model supported in the Swedish context?

The second paper deals directly with the causal mechanism: Does social network position mediate the effect of education on political participation? The second paper is the first to present a solid test of the causal mechanism using structural equation modeling.

The third paper brings the discussion on the relative education model further by providing it with the first country comparative test using data from 37 countries. Hence, it deals with the research question: Is the effect of education on political participation absolute or relative in a comparative perspective? These three papers mainly present evidence in favor of the relative education model over the pre-adult socialization model.

The last two papers deal with the pre-adult model versus the absolute education model. The fourth paper uses matching on data from the United Kingdom to mimic an experimental test of the causal effect. It deals with the question: Is college education a cause or a proxy for political participation? This paper brings the important contribution of using a more extensive set of pre-adult covariates than previous studies, including important information on childhood cognitive ability.

The fifth paper moves from studying the length of education to test the impact of the type of education. It presents a panel study following Swedish adolescents over time during their first year in the gymnasium in order to answer the question: Does type of education affect political participation? The findings of both paper four and five point in favor of the pre-adult socialization model; education seems to be a proxy rather than a cause for political participation. Here follows a more detailed summary of the papers.

Paper 1. An empirical test of the relative education model in Sweden

The first paper evaluates the predictions of the relative education model using Swedish data and it therefore provides the first in depth evaluation of the relative education model outside the US (Persson 2011). Despite the fact that

the paradox of participation and education is valid in most Western countries, the relative education model has previously only been tested on data from the US. This paper examines whether education affects political participation through sorting mechanisms in the European context as well, and if so, where and when sorting processes operate. An examination of the relative education model is provided by an analysis of data from the Swedish National Election Studies (SNES) from 1985 to 2006 in combination with detailed census data from Statistics Sweden.

In this case, the most important difference between Sweden and the United States is the greater amount of equality in Sweden. The level of equality is of interest in relation to the relative education model because it is reasonable to expect that the model will receive less support in societies that have high levels of equality. Nie, Junn and Stelhik-Barry claim that what matters for political participation is the position in the educational hierarchy and they do not provide any discussion as to how the distance between the positions in the hierarchy affects the applicability of the model. I hypothesize that the relative education model should gain more support the more unequal a society is. In a society where there is a large amount of inequality, there are also larger distances between social networks—it is harder for those with low levels of education to access the social networks that are most important for gaining political influence. Low levels of social stratification may make it easier for the disadvantaged to participate in politics. For that reason, I hypothesize that Sweden constitutes a harder test—a less likely case—for the relative education model.

Here another important difference between the contexts should also be emphasized: the Swedish educational system has been explicitly designed to achieve egalitarian values, such as promoting social equality (Rothstein 1996; Meghir and Palme 2005). Since the Swedish educational system is more egalitarian, education may have a weaker impact on social networks, which in turn would imply that the hypothesized pathway between relative education and political participation via social network centrality is less evident in Sweden than in the United States.

Moreover, the pathways of recruitment to political assignments in Sweden are very different compared to those in the United States. As in most countries, the members of the Swedish parliament constitute an elite group with higher education than the electorate (Esaiasson and Holmberg 1996). However, in Sweden there has been a close link between the dominant party—the Social Democratic Party—and the working class movement. The working class movement constitutes an alternative pathway to political participation

that has no counterpart in the United States, which should reasonably dampen the importance of education on participation.

The paper also brings a contribution to the debate on the scope of the model. Remember that Nie, Junn and Stehlik-Barry claim that the relative education model is valid for all forms of political participation while Campbell (2009) argues that there is reason to believe that not all forms of participation are affected by relative education. Since not all forms of political participation are socially based and affected by recruitment via social networks, we only have reason to expect that the relative education model is valid on the social and competitive forms of political participation. Likewise, in an early contribution to the debate Huckfeldt (1979) argued that socially based forms of political participation are strongly affected by contextual factors (such as social network composition), whereas individually based forms of participation are not affected by contextual factors whatsoever.

In this paper, the predictions derived from the relative education model are tested on four indicators of political participation: writing letters to political representatives, voting, political party activities, and party membership. Support is found for the relative education model on voting and activities related to political parties, whereas the model is not supported when it comes to writing letters to political representatives. Hence, the relative education model is, at least partially, supported in the Swedish context as well. Campbell's view is confirmed from the evidence of relative education effects on the socially based forms of participation under study (activities and membership in political parties) and the absence of relative education effects on an individually based form of participation (writing letters to political representatives).

The final contribution of the paper regards how to operationalize relative education. In this paper, three different units of aggregation for the educational environment are evaluated: (a) both age and place are aggregated narrowly; (b) age is aggregated widely and place is aggregated narrowly; (c) age is aggregated narrowly and place is aggregated widely. By using these three definitions we can trace where sorting processes operate. The relative education model is supported for political party activities and party membership when defining the unit of aggregation for educational environment widely with regards to age and narrowly with regards to place. When it comes to voting however, support for the sorting model is found when applying any of the three different units of aggregation for the educational environment.

Paper 2. Social network position mediates the effect of education on political participation

The second paper offers a refined test of the relative education model (Persson 2015). Previous research on this model has focused primarily on testing the observable predictions derived from the model. The hypothesized causal mechanism—social network position—has not been sufficiently tested. Although a small number of studies have re-examined and refined the relative education model following Nie, Junn and Stehlik-Barry, all of them focus on the observable implications derived from the model, i.e. whether the effect of education at the individual level is conditioned on the level of education in the environment (Campbell 2009; Helliwell and Putnam 2007; Tenn 2005).

This paper employs Swedish survey data from the SOM Institute with more comprehensive measures on social connections than has previously been used in prior research. It looks specifically at activities in political parties as the dependent variable. The main reason for the lack of research on the causal mechanism is the absence of high-quality data on social network connections together with measures of the dependent and independent variables. To date, the only study examining the indirect effect of education via social network position is Nie, Junn and Stehlik-Barry's (1996, ch. 4) original study, in which they use data from the 1991 Current Population Survey. They use an additive scale constructed of the number of high-status people that the respondents say they know. Simple regression path models are used to estimate the indirect effects of education via those social network connections. They find that social network position (and verbal cognitive proficiency) explains almost the entire relationship between education and voting and that social network position is the main factor determining participation in "difficult political activities".

However, Nie, Junn and Stehlik-Barry's path analyses are problematic for several reasons. First, they do not perform any significance tests of the indirect effects and thus leave it an open question whether education has a significant indirect effect via social network position. A second problem is that Nie, Junn and Stehlik-Barry's measure of social network position includes only connections with elected officials and persons working with the news media. This problem concerns the causal direction in the model. It is not obvious that these connections are consequences of education (as the sorting model states); they can also be consequences of political participation.

A third problem is that in Nie, Junn and Stehlik-Barry's study, social network position is treated as a manifest variable defined as a simple additive index rather than a latent variable. The more sound approach employed in

this paper is to treat social network position as a latent variable in a structural equation model that allows the different indicators to vary in their contribution to the measure.

As mentioned earlier, previous studies on the relative education model provide little information on *how* social networks mediate the relationship between education and participation and what aspects of social networks are important. Nie, Junn and Stehlik-Barry (1996, 44) define social network centrality as "proximity to governmental incumbents and political actors who make public policy and to those in the mass media who disseminate and interpret issues, events, and activities of people in politics". This is a narrow definition of social network position since it only takes into account friendship relations to two sorts of people.

Research on social networks and political participation can help us refine this part of the model. The size and composition of the social networks are seen as central in determining the effects on political participation. Usually research in this field emphasizes social connections to a wide range of people (e.g., Mutz 2002; McClurg 2003; Siegel 2009). Research has also found that large networks have a strong positive effect on participation: the more people you know, the broader your opportunities for recruitment (e.g., Kotler-Berkowitz 2005). More precisely, we arrive at the refined hypothesis that having strong ties to a large and wide network of high-status persons mediates the relationship between education and political participation.

The data used in this paper measures the social connections of people with 20 different occupations, such as lawyers, members of the national parliament, professors, journalists, etc. These indicators were used to construct the latent variable for social network position in the analysis. Structural equation modeling is used to test the indirect effect of education via social network position on active political party membership. The results indicate that the causal path proposed by Nie, Junn and Stelhik-Barry's relative education model is confirmed. The effect of education is significantly mediated through social network position.

Paper 3. Is the effect of education on voter turnout absolute or relative? A multi-level analysis of 37 countries

The first two papers focus on the Swedish case and from other studies we know that the relative education model has been supported in the United States as well (Persson 2013). However, what about other countries? Nie, Junn and Stehlik-Barry claim that their model is universally applicable but they do not provide empirical evidence that their model resolves the paradox

between education and voter turnout in countries other than the United States. The present paper tests the wider generalizability of the model and goes beyond previous research by using comparative survey data. It combines data from the Comparative Study of Electoral Systems and European Social Survey covering about 275,000 individuals over 173 country-years in 37 countries. This paper looks at voter turnout as the dependent variable, a variable that studies from the United States and Sweden previously have shown is affected by relative education.

The paper presents a refined modeling strategy for relative education, relying on country-comparative intra-birth cohort measures. A problem with the comparative data is the lack of a valid measure of years of education that is equivalent in the 37 countries. In order to model relative education using this data, we need a measure that relies on the categories of education that are reported in the datasets. Thus, a modeling strategy similar to the one applied by Tenn (2005) is used. A measure is calculated of each respondent's percentile rank position, in the education hierarchy, within each respondent's five-year cohort, in each country, at the time of each survey.

Logistic multi-level regression modeling is employed in which the nested three-level structure—individuals (i), within country-years (j), within countries (k)—is explicitly modeled. The results show that when taking the relative education measures into account, the effect of absolute education is reduced considerably. In contrast, the relative education measures have strong and significant effects.

One further issue that previous research on the relative education model has not dealt with is whether the effect of relative education is different in different contexts. According to the so-called "law of dispersion", formulated by Tingsten (1937), the level of equality in political participation is higher when the level of voter turnout is higher. Consequently, political inequality will increase as voter turnout decreases. If this theory holds it would suggest that differences in turnout between citizens with different levels of relative education should be larger when aggregate turnout is lower and that the differences should be smaller when aggregate turnout is higher. The empirical analyses show that, as could be theoretically expected, the difference between individuals with high and low relative education is largest when turnout is low and the difference is smaller when turnout is high, albeit the difference remains statistically significant also at the highest levels of aggregate turnout.

Paper 4. Testing the relationship between education and political participation using the 1970 British Cohort Study

The fourth paper tests the pre-adult socialization model versus the absolute education model (Persson 2014). This paper engages in the current controversy regarding the application of matching techniques to assess whether there is a direct causal effect of education on political participation. Genetic matching is used to test the causal effect of higher education (bachelor's degree or higher) on five forms of political participation (voting, demonstrations, signing petitions, political meetings and contacts with politicians).

The basic idea behind matching is simple in this case: to match persons with low levels of education that are as similar on all relevant covariates as possible with persons with higher education (Rubin 1973; 1974). If this is done successfully, comparing individuals similar on all relevant covariates, except for the treatment variable, is equivalent, at least logically, to comparing individuals randomly assigned to different treatments in an experiment (cf. Dehejia and Wahba 2002). 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).

Matching is superior to standard regression models because 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).

The paper uses data from the British Cohort Study that follows everyone born during one week in April 1970 in the United Kingdom. Turning to the British context is primarily for pragmatic reasons; this is where we can find a high quality panel-study that includes a rich set of variables that measure factors in childhood and adolescence such as cognitive ability, family socio-economic status and cultural activities. This data gives the opportunity to match on a number of important variables that are not included in the US datasets that were used by previous studies in the field. Most important is the data on cognitive ability; individuals with high cognitive ability are more likely to achieve higher education (e.g., Belley and Lochner 2007) and might also be more likely to participate in politics. The data includes test scores from cognitive ability tests at age five and age ten.

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. However, after matching, the differences are considerably reduced and not statistically significant. In other words, we cannot detect any effect of education after matching and thus the results suggest that education should consequently be regarded as a proxy rather than a cause for political participation. Hence the study confirms the pre-adult socialization model, i.e., that education is a proxy rather than a cause.

A series of robustness checks and sensitivity tests are carried out, including bias simulations, placebo tests, balance checks and alternative matching routines, which strengthens the confidence in the results.

Paper 5. Does type of education affect political participation? Results from a panel survey of Swedish adolescents

While the previous papers in the dissertation deal with length of education, this paper shifts focus to type of education (Persson 2012). As mentioned in the literature survey, in several countries there is a gap in political participation related to different types of education at the upper secondary level. In Sweden for example, individuals with theoretical gymnasium education show significantly higher levels of political participation than individuals with vocational education. However, previous studies on this issue draw exclusively on one-shot cross-sectional data. The paper deals with the question whether different educational tracks lead to different levels of participation or if the correlation between type of education and political participation is the result of self-selection.

This paper reports findings from a Swedish one-year panel survey among adolescents, which is an original dataset collected by the author. Approximately 500 Swedish students were followed during their first year in the gymnasium. The first wave of the survey was conducted after the respondents graduated from comprehensive school, wherein they all shared the same curriculum. Hence, the panel study takes advantage of a crucial moment of educational choice that allows us to compare intended political participation even before different types of education were acquired.

The results show that there are already significant differences between students from theoretical and vocational tracks, in regards to political participation intentions, at the first wave of the study. This significant difference remains at the second wave. In other words, these differences appear already as students enter different educational tracks and the type of

education cannot reasonably have had any effect at this time. Moreover, the size of the gap in intentions to participate in political activities between students from theoretical and vocational tracks does not change significantly during the first year of study in the gymnasium. Results show that instead of factors related to education, factors such as the amount of political discussion at home and/or the number of books at home, positively affect participation. The socio-economic status of the family affects both intention to participate in politics and educational choice.

Implications of the findings

Having presented the theoretical foundation and empirical results, it is time to discuss the implications of the findings. In this section, I will first discuss how the papers fit together and the collected contribution to the field. Thereafter the implications for political behavior research are discussed. The next section discusses the implications for the functioning of the democratic system and the policy implications. Finally, prioritized areas for further research are discussed.

How the papers fit together and what we can learn from them

Taken together, what can we learn from these five papers? The first three papers test the relative education model versus the absolute education model and find support for the relative education model. The papers show evidence that the relative education model is valid in a hard test case like Sweden, that social network position works as the causal mechanism and that the model gets support in a comparative perspective as well. The second two papers test the pre-adult socialization model versus the absolute education model and find support for the pre-adult socialization model. The empirical applications suggest that, contrary to what could be expected from previous studies, there is no causal effect of higher education on political participation in the United Kingdom and type of education in the Swedish educational system does not affect intentions to participate. Taken together, the papers provide little evidence that education is a direct cause for participation. Thus, they all challenge the conventional wisdom regarding effects of education on participation.

Do the papers contradict each other? Not necessarily, but it is a major drawback that all three models cannot be tested in the same study. However, as indicated previously, such data (that includes measures of pre-adult factors, education variables, social network indicators and measures of political

participation) is not available. At this stage of research we simply do not know what the result would be if all models could be tested simultaneously.

One possible explanation to how the studies fit together is that relative education, as tested in the first three papers, is also a proxy for pre-adult factors such as family socio-economic status, cognitive ability and political socialization in the family environment. These factors might also be strongly related to social network centrality. Persons from families with high socio-economic status, with high cognitive ability, and a stimulating home environment are likely to get relatively high education in relation to those in their surroundings, get high social status manifested by their large social networks and participate more often in political activities. However, their social status may not necessarily be a consequence of their relative education; it can just as likely be a consequence of pre-adult factors. Nie, Junn and Stelhik-Barry argue that relative education is what drives social status but they do not show any results that rule out factors such as socio-economic status of the family in which one grew up, cognitive ability, or any other pre-adult factor as being the true causes to social status. Hence, it could be the case that relative education, social network position and political participation are all driven by pre-adult factors that are unmeasured in studies on relative education.

In the absence of a strict empirical test of all three models, the best explanation at hand is that relative education might be influenced by pre-adult factors as well. It is indeed reasonable to expect that relative education is more strongly influenced by pre-adult factors than absolute education. This is evident if we look at levels of education in the population over time. For example, 50 years ago, graduating from a gymnasium in Sweden or a high school in the United States might bring some social status while today this is more common. On the other hand, university education used to be something for a privileged few that has become far more accessible to larger groups in the population. When looking at education over time and between regions, it is not sensible to say that persons who grew up under favorable circumstances can be predicted to have a *specific* level of education (irrespective of which time and place one lives in). However, persons who grew up under favorable circumstances can reasonably be predicted to have a *relatively* high level of education (in relation to people in their surroundings).

Pre-adult factors, education and social network centrality are interlinked in a complicated nexus and it is hard to isolate the effects of each factor on political participation in empirical analyses. Given the results presented here, the papers in this dissertation answer some questions but also raise a number

of new ones. The important contributions lie in refinements of the relative education model and its ability to be generalized. In addition, the papers provide tests of the effects of college education and gymnasium tracking using panel studies. The major question raised that remains to be dealt with is primarily, how to simultaneously test all three models to get a better understanding of the interlinked effects of pre-adult factors, education and social network centrality on political participation.

Overall the studies in this dissertation undermine our belief in the absolute education model. The papers offer very limited support for the hypothesis that education is a direct cause for political participation. This means that the skills and knowledge gained through the process of education seem to be of little importance for political participation and do not function as the causal mechanisms connecting education and participation.

Does this mean that it is pointless for individuals to get high levels of education? No, it only means that education does not seem to cause political participation. However, it should be noted that education has an impact on other factors. For example, studies using robust estimation techniques have shown that education affects earnings (Angrist and Krueger 1991; Beckert 1993; Öckert 2010) and health (Eide and Showalter 2011). Education has also been found to affect outcomes in the political domain such as civic and political knowledge (see e.g., Green et al. 2011; Finkel and Smith 2011). So while education does not seem to drive participation, it is of course not the case that education might not have effects on other important factors. Additionally, as mentioned previously, it is still possible that specific forms of education, educational content or teaching styles might affect political participation.

Implications for political behavior research

What are the implications for political behavior research? I began this introductory chapter by claiming that finding out which model can correctly explain the relationship between education and participation has important theoretical and societal implications. The studies presented here lend support to a revisionist view, moving beyond the idea of education as a direct cause for participation, and hopefully they provide a more nuanced picture of the role of education in political behavior research. The central implication is that education should not be seen as a simple, individual level cause for participation. In the wake of the Michigan school (Campbell et al. 1954, 1960) political behavior research has long been dominated primarily with analyses of individual level factors. The relative education model moves beyond such

individual level focus by emphasizing the interplay between individual and aggregate levels of education and the importance of social networks. In that sense it brings some restoration to the Columbia school (Lazersfeld et al. 1948; Berelson et al. 1954), which emphasized contextual level effects on political behavior.

However, the results are also a reminder of the perils of correlational analyses and the importance of not drawing conclusions from correlational evidence too quickly (cf. Achen 1977). Education almost always turns out to be a significant coefficient in regression models for political participation, but understanding what this coefficient really means is a hard task. Many researchers are too quick to say that significant effects of education signal direct causal effects of education. From standard regression models using one-shot cross-sectional data, it will continue to be almost impossible to understand exactly what the significant coefficient for education actually means. Additionally, remember that even the studies arguing that education has a direct causal effect on participation say very little about the causal mechanism and seldom show evidence regarding how the relationship can be explained.

Do the results matter for political behavior research that is not pre-occupied with the effects of education and focuses instead on the effects of other variables? I suggest that it does matter. Education is one of the most frequently used control variables in the field; it is one of the "usual suspects" in political behavior research. For that reason 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 caused by education. Say that we study the impact of some other factor on political participation and we just want to control for skills in a regression model, so we control for education and leave the issues about causality aside. Then, it might not be so important if the relationship is causal or due to self-selection. The problem however, is that we are not sure that education is a proxy for skills. Verba, Schlozman and Brady (1995) put forward three factors as central explanations for political participation: resources, motivation and recruitment (see also Bäck, Teorell and Westholm (2011) for a discussion on the causes of political participation). Drawing on the literature on educational effects, education could be related with each of these factors; education might capture social network centrality (possibilities for recruitment), skills (resources), political efficacy (motivation), or other factors. Usually, researchers do not know what they control for when adding education as a control variable. 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 will be problematic. Hence, even if education is used only as a control variable, it is important to understand what the relationship actually means and what education is a control for.

Democratic and policy implications

Why does it matter if people with lower education participate in politics to a lower extent than people with high education? It matters since unequal participation results in unequal political influence. Inequalities in political participation, such as systematic differences related to education, are often considered to be a democratic problem (cf. Lijphart 1997; van der Eijk and Franklin 2009; Schlozman, Verba and Brady 2012). Unequal participation hinders the possibilities of implementing policies in accordance with the will of all people. If people with low education do not express their political preferences it will be difficult for political representatives to implement policies that are in accordance with their preferences. Also if, at the same time, highly educated people make their voices heard to political representatives, public policies will likely be even more biased in favor of the highly educated peoples' preferences. This means that equal voice can be seen as important because it provides equal protection of interests.

In the United States there is strong evidence that unequal voice affects policy making; implemented policies have a strong relationship with the preferences of citizens with high socio-economic status (cf. Bartels 2008; Gilens 2005, 2012). Many commentators find this a troubling fact, but what can be done about it? How can the less educated be stimulated to participate more in political activities?

Some people would perhaps suggest increasing the educational levels among low educated people. This leads us to the policy implications. If education is a cause for political participation, raising the educational levels in a society could help address this problem. This means that the state can actually influence peoples' levels of political participation by reforming the educational system. For example, Lewis-Beck et al. (2008, 102) claim that "effective citizen participation depends on the operation of a nation's educational system". However, if education is a proxy for mainly pre-adult factors, inequalities in participation are not likely to be mitigated by education. Additionally if education is primarily a proxy for social status, more education will not get more people active in politics. The conclusion from this study is that reforming the educational system, in order to increase participation, is not necessarily a successful strategy. Expanding the

educational system might be good for other reasons, but the results presented here do not indicate that it is likely to have any strong effects on political participation. At least not when it comes to the traditional forms of participation analyzed in this study.

The road forward

How should this field move forward? First and foremost, further studies would benefit from trying to better estimate the causal effect of length of education, specific levels of education, as well as different types of education. Until the debate is settled and consensus can be reached regarding the impact of length and specific levels of education, further studies are valuable. Moreover, there is a need for studies focusing on different aspects of education, such as the educational content. While we have some solid evidence on the impact of education length, we know surprisingly little about other aspects of education.

Much focus has been on the impact of higher education (at colleges or universities), but the potential impact of lower levels of education is largely ignored. Recently, Esping-Andersen et al. (2012) have shown that high quality childcare in pre-schools at age three can affect cognitive ability later in life. However, there are no studies on whether high quality preschool education in turn also affects political participation later in life.

In the absence of large-scale randomized experiments, researchers should continue to take advantage of natural experimental situations such as, reforms of educational systems that have been implemented in a randomized or quasi-randomized way, or employing other techniques to estimate causality such as regression discontinuity designs or instrumental variable approaches.

Moreover, further studies should try to simultaneously test the relative education model, the pre-adult socialization model and the absolute education model. In order to do so, panel studies with comprehensive measures on the variables of interest—from pre-adult factors to adult social status—would be required.

The studies presented in this dissertation have shed some new light on how education is related to political participation, yet we do not have the full picture. Future studies like those outlined above would further improve our understanding of the relationship between education and political participation.

Notes

[1] Further studies should evaluate whether the conclusions from the studies in this dissertation hold when looking at "new" forms of participation, such as online-participation, consumer behavior, etc. The concentration on traditional forms of participation in this study is partly a consequence of a focus on such factors in existing surveys. Despite the importance of new alternative forms of participation, focusing on traditional forms of political participation is relevant given its importance for the functioning and legitimacy of democratic societies.

[2] This does not mean that informal forms of education are unimportant or do not affect political participation, it is merely a constraint on the scope of this dissertation to make the research task manageable. Future research however, would benefit from studying the impact of informal forms of education. For example, in the Swedish case an important educational institution has been "study circles" organized by the working class movement (cf. Jansson 2012; Milner 2002, 2010). Testing the causal effects of that type of education would be of importance for the field, but it falls outside the scope of this dissertation.

[3] The concept "causal mechanisms" requires some further explanation. Theoretically, a causal mechanism can be seen as an explanation of what triggers the effect of the independent variable on the dependent variable (cf. Elster 1989; Hedström and Swedberg 1998). In this study, I discuss civic knowledge, skills and social networks as the possible causal mechanisms that might trigger the effect of education on political participation. Empirically, the factors serving as causal mechanisms can be seen as mediating variables (cf. Imai et al. 2012).

[4] At this point, it should be noted that some studies show evidence that political knowledge is strongly correlated with political participation. For example, Milner (2002) uses data from the International Adult Literacy Survey (IALS) that shows correlations between civic literacy and voter turnout. This data draws on studies from a number of countries in which tests have been conducted on the ability of adult citizens to understand texts such as news articles and official documents. However, this study focuses on correlations at the country level and it is not obvious how to understand the causality, if it exists, in this relationship between factual knowledge and participation (cf. Rothstein 2003). Hence, this study does not show evidence that factual knowledge is a causal effect of education and it does not confirm

the hypothesized causal pathway from education via knowledge to participation.

[5] At this point it should be clarified that the studies in this dissertation will not provide extensive tests of the different causal mechanisms. In the absence of a solid research design that would accomplish this testing, the studies in the dissertation mainly test the observable predictions of the models. However, the third paper does provide a test of whether social network position mediates the relationship, but the studies do not provide any tests of whether alternative factors work as causal mechanisms linking participation and education.

[6] Here it should be emphasized that it is not obvious that the value of education should be decreasing over time; the value of individual education depends on both the supply and demand for education. Put differently, Nie, Junn and Stehlik-Barry focus only on the supply side while ignoring the demand for education. However, recent studies show that as a consequence, different levels of supply and demand of education affect the impact of education on participation; i.e., the effect of education might vary in different institutional and socio-economic contexts (cf. Busemeyer and Goerres 2014).

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Paper 1

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ORIGINAL PAPER

An Empirical Test of the Relative Education Model in Sweden

Mikael Persson

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Abstract Numerous studies show that education has a positive effect on political participation at the individual level. However, the increase in aggregate levels of education in most Western countries over the last decades has not resulted in a corresponding increase in aggregate levels of political participation. Nie et al. (Education and democratic citizenship in America, 1996) propose the relative education model as a possible solution to this paradox. According to this model, it is not the skills promoted by education that have positive effects on political participation. Rather, education influences individuals' social status, which in turn influences political participation. The relative education model expects that the individual-level effect of an additional year of education will decrease as the mean level of education in the environment increases. This article evaluates this theory using Swedish election surveys (1985–2006) and it thus provides the first in depth evaluation of the relative education model outside the US. On voting and political participation related to political parties, support is found for the relative education model.

Keywords Political participation · Voting · Education · Political socialization · The sorting model · Relative education

The Problem

The "paradox of participation" has puzzled political scientists over the last few decades (Abramson and Aldrich 1982; Brody 1978; Leighley and Nagler 1992; McDonald and Popkin 2001; Miller 1992). The paradox is that, on the one hand,

M. Persson (✉)
Department of Political Science, University of Gothenburg, Box 711, 405 30 Gothenburg, Sweden
e-mail: mikael.persson@pol.gu.se
URL: http://www.pol.gu.se/staff/mikaelpersson

numerous studies show that education has a positive effect on political participation at the individual level, but that, on the other hand, increased levels of education at the macro level do not increase aggregate levels of political participation. The most promising solution to the paradox so far is provided by Nie, Junn and Stehlik-Barry (1996) (henceforth NJS),[1] who put forward the so-called relative education model.[2] According to this model, it is not the skills gained through education that have positive effects on political participation, but rather the fact that education influences individuals' social network centrality, which in turn influences political participation. Thus, NJS hypothesize that the effects of education are *relative*; what matters is an individual's position in the educational hierarchy, not one's level of education per se. Education works only as a sorting mechanism placing individuals with higher levels of education into environments that encourage political participation.

This article contributes to an emerging literature that questions the absolute effects of education on political participation (Berinsky and Lenz forthcoming; Burden 2009; Highton 2009; Kam and Palmer 2008; Luskin 1990; Persson and Oscarsson 2010; Tenn 2007). Recently, the relative education model has been a central matter of debate in this discussion (Campbell 2006, 2009; Desjardins 2008; Emler and Frazer 1999; Helliwell and Putnam 2007; Hillygus 2005; Nie and Hillygus 2001; Tenn 2005). The discussion has primarily revolved around three main questions. The first concerns the accuracy of the predictions derived from the model; are the effects of education on political participation relative or absolute? In other words, does education affect political participation through sorting processes, and if so, where and when do sorting processes operate? The second question concerns the scope of the model; can the relative education model be generalized to all forms of political participation? The third question concerns the unit of aggregation that should be used when measuring the educational environment; i.e., how the relative position in the educational hierarchy should be operationalized.

This article contributes to the discussion on the relative education model on each of these three questions. First, the model will be applied in a new context in which it is less likely to be supported. Despite the fact that the "paradox of participation" is valid in most Western countries, the relative education model has been almost exclusively tested on data from the US.[3] This article examines whether education affects political participation through sorting mechanisms in the European context as well, and if so, where and when sorting processes operate. An examination of the relative education model in the European context is provided by an analysis of data from the Swedish National Election Studies (SNES) from 1985 to 2006. Hence, this study further investigates the generalizabilty of the relative education model by providing the first in-depth country-specific analysis of the relative education model in a country other than the US. Since the level of equality and social mobility is

[1] Although it is not referred to as 'the relative education model' the same idea is put forward already by Huckfeldt (1979) and Verba and Orren (1985, Chap. 10).

[2] The model is synonymously referred to as 'the sorting model' in the literature.

[3] Chapter 10 of NJS (1996) provides a brief investigation of effects of raised educational levels on tolerance in Europe. Campbell (2006) presents results in partial support of the sorting model on non country-specific European data.

higher in Sweden than in the US, and because the working class movement constitutes an influential alternative pathway for recruitment to political participation, it is argued that Sweden constitutes a harder test for the model; it is less likely that the relative education model is supported in Sweden than in the US. Despite this fact, the results show support for the relative education model on three of the four political participation indicators under study. The analysis demonstrates that the individual level of education does not have the same impact on political participation in all contexts; instead the individual-level effect of education is conditional on levels of education in the environment.

Second, by discussing the scope of the model the article also contributes to the discussion on how contextual factors—such as social networks—influence political participation (Dyck et al. 2009; Leighley 1990; McClurg 2003). NJS claim that the relative education model is valid for all forms of political participation. However, Campbell (2009) has recently argued that there is reason to believe that not all forms of participation are affected by relative education via social network centrality. Since not all forms of political participation are socially based and affected by recruitment via social networks, we only have reason to expect that the relative education model is valid on the competitive forms of political participation. Likewise, in an early contribution to the debate Huckfeldt (1979) argued that socially based forms of political participation are strongly affected by contextual factors such as social network composition, whereas individually based forms of participation are not affected by contextual factors whatsoever.

In this article, the predictions derived from the relative education model are tested on four indicators of political participation: writing letters to political representatives, voting, political party activities and party membership. Support is found for the relative education model on voting and activities related to political parties, whereas the model is not supported when it comes to writing letters to political representatives. Hence, the relative education model is, at least partially, supported in the European context as well. The evidence for relative education effects on the socially based forms of participation under study—activities and membership in political parties—and the absence of relative education effects on an individually based form of participation—writing letters to political representatives—confirms the hypotheses proposed by previous research: socially based forms of participation are influenced by contextual factors, whereas individually based forms are not.

Third, in studies of the relative education model following NJS (1996), there has been considerable debate on how to operationalize relative education. Which is the relevant unit of aggregation for the educational environment? In relation to whom should each individual's education be compared? In this article, three different units of aggregation for the educational environment are evaluated: (a) narrow as regards both age and place, (b) wide as regards age and narrow as regards place, and (c) narrow as regards age and wide as regards place. By using these three definitions, we can trace where sorting processes operate. Census data are used to apply very precise information on mean levels of education in the social environment according to each of the three definitions, at the time of each original survey. Results show that different forms of political participation are affected by different forms of sorting

processes since the relative education hypothesis is only proven valid under specific operationalizations of the educational environment. The relative education model is supported for political party activities and party membership when defining the unit of aggregation for educational environment as wide as regards age and narrow as regards place. When it comes to voting, however, support for the sorting model is found when applying any of the three different units of aggregation for the educational environment.

In what follows, the theoretical explanation of the relative education model is first outlined. Thereafter, critique and improvements of the model are discussed. We then turn to the Swedish case and demonstrate why it constitutes a less likely case for the model. The data, statistical techniques and results are subsequently presented. A concluding section discusses implications of the findings.

The Relative Education Model

Research on political participation has most often failed to provide a proper explanation for the paradox of participation. For example, in their influential study on political participation Verba, Schlozman and Brady argue that "education is the prime factor in most analyses of political activity" (Verba et al. 1995, p. 433). Yet they emphasize that they "are not arguing that aggregate changes in the level of education of the population will be associated with commensurate changes in the aggregate level of participation" (Verba et al. 1995, p. 436). According to NJS there is no paradox to explain. Since they consider the effects of education to be relative rather than absolute they do not expect that an aggregate increase in mean levels of education should lead to a corresponding increase in political participation.

The idea behind the relative education model comes from an argument most famously put forward by Hirsch (1978), who argues that, as educational levels rise "the effect will be to push competition by hitherto qualified applicants down the hierarchy of jobs" (Hirsch 1978, p. 50). This means that, for example, jobs that previously required high school education would after educational expansion require college education. According to Hirsch, educational expansion will lead to an inflation of educational credentials as the positional competition increases. In other words, education works as a sorting mechanism.

NJS apply this argument to effects of education on political participation and develop it in sharp contrast to the mainstream view on effects of education, the so-called *absolute education model*. The widely held idea behind the absolute education model is that education improves civic skills and civic knowledge, which in turn leads to political participation. NJS rejects that civic skills and knowledge are the causal mechanisms linking education with political participation. Instead, NJS claim that the link between education and political participation is, what they refer to as, a positional pathway. Education matters to the extent that it determines individual's social network positions, which in turn influences political participation. This means that the impact of education is relative: the "value" of an individual's education depends on the level of education in the environment. In technical terms this means that the individual level effect of education is interacted

with the contextual level of education. As the level of education in the environment increases, the effect of each individual's education decreases. The core assumption of the relative education model is that education is simply a proxy for social network centrality. Persons with high social status are exposed to networks that encourage participation and are thus more likely to be recruited.[4] Hence, the relative education model implies that since education influences participation via social network centrality, the individual-level effect of education decreases when the level of education in the environment increases.

The Critique of the Relative Education Model

In addition to whether the relative education model spells out the correct relationship between education and political participation, the critique of the model has mainly concerned two points. The first question concerns the scope of the model: is the relative education model applicable to all forms of political engagement? The second question concerns the relevant unit of aggregation for the educational environment: in relation to whose level of education should each individual's education be compared?

Is the Relative Education Model Applicable to All Forms of Political Participation?

NJS assume that all forms of political engagement are equally affected by relative education since they consider political participation to be a competitive zero-sum game in which those with high social status have an advantage. However, Campbell (2009) argue that all forms of political participation are not equally competitive, and it is thus wrong to assume that support for the relative education model should be equivalent on all forms of political participation. Therefore, following Campbell, the relative education model should be valid only in relation to the truly competitive forms of political participation. NJS are, of course, correct that some forms of political participation are essentially competitive. For example, not everyone can be a political representative in parliament because there are only a finite number of seats. But not all forms of political participation are of this character. Take, for example, the act of writing letters to political representatives, which is a form of political participation that is not restricted or bounded in any zero-sum way. One individual's decision to write a letter does not decrease the ability of others to do the same thing, which would be the case if it were a zero-sum game. Likewise, writing letters does not seem to be as dependent on recruitment through social networks as the more competitive forms of political participation. Campbell (2009) argues that several forms of political participation are not at all competitive, and that it is not clear as to why the relative education model should apply to these forms. If social

[4] As Verba, Schlozman and Brady point out, one of the reasons why individuals participate in democratic activities is simply because they were asked. Consequently, the reason why individuals with low levels of education participate to a lesser extent in political activities may simply be that they are "outside of the recruitment networks that bring people into politics" (Verba et al. 1995, p. 269).

network centrality is the causal mechanism linking relative education and political participation, the model should only be valid on those forms of political participation that are dependent on recruitment from social networks.

If the relative education model is correct the individual level effect of education should be conditioned on the level of education in the social environment. But how can we distinguish between the forms of political participation that are likely to be affected by contextual factors in the social environment from those that are not? Huckfeldt (1979) introduces a distinction between individualistic versus collectivistic forms of political participation. The individualistic forms of political participation are not supposed to be affected by contextual factors. Collective forms of political participation, on the other hand, require individuals to be involved with other individuals or organizations in order to perform the acts. For that reason, collective forms of political participation are more likely to be affected by contextual factors such as social network centrality. Huckfeldt points out writing letters to political representatives as an example of an individualistic form of political participation while, for example, joining and being active in a political party are collectivistic forms of participation since they are hard to perform without interacting with other people.

However, Kenny (1992) has criticized this distinction by arguing that it is inappropriate to lump together acts such as voting and writing letters to political representatives. Kenny argues that while voting is an individualistic act, the "processes leading up to this act may well include interactions with members of various social contexts" (Kenny 1992, p. 260). Thus, even though it is difficult to make a distinction between purely individualistic acts and collectivistic socially based acts, we have reason to believe that forms of political participation that include high levels of interaction with other people are more influenced by contextual factors. Hence, the relative education model should receive stronger support for forms of political participation that are socially based than for those that are individually based.

Which is the Relevant Unit of Aggregation for the Educational Environment?

The second crucial question in the debate about the relative education model is the relevant unit of aggregation for measuring the educational environment. The crucial difficulty is: What kind of territorial boundaries should be drawn and to what age group should each respondent's education be compared? Previous research has employed very different units of aggregation for the educational environment and has shown contradictory results. Table 1 presents a summary of previous research on the relative education model.

Although NJS are theoretically sophisticated, in their empirical analyses they employ a quite peculiar definition of educational environment. NJS argue that it is not relevant to compare each individual's level of education to the mean levels of education among the entire population. Instead, they compare each respondent's level of education to the mean national level among individuals at age 25–50 when the respondent was 25. Helliwell and Putnam (2007) point out that NJS's measure of educational environment implies that everyone always competes with those older

Table 1 Summary of previous studies on the relative education model on participation

Study	Unit of aggregation for educational environment regarding *age*	Definition of educational environment regarding *place*	Support for the relative education model	Dependent variables in the analyses
Nie et al. (1996)	Mean national levels among individuals who were 25–50 years old, when the respondent were 25 years old	Wide (national mean levels)	Yes, on all indicators	Voting in national elections Political attentiveness Campaign activity
Tenn (2005)	Narrow (those born in the same year)	Wide (national mean levels)	Yes	Voter turnout
Helliwell and Putnam (2007)	Wide (all other living adults)	Narrow (census region)	No	Social engagement (number of memberships, club meetings, community projects, dinner parties)
Campbell (2006)	Narrow (four cohorts: 25–39, 40–54, 55–69, 70+)	Wide (national levels)	Strong support for the relative education model is found on *competitive political activity* (There is no support for the relative education model on the other indicators)	Competitive political activity (a: contacts with politicians/government/ local government official; b: working in a political party or action group) Expressive political activity (a: signed petition; b: taking part in lawful demonstration; c: boycotted certain products) Voting Working in voluntary associations

Table 1 continued

Study	Unit of aggregation for educational environment regarding *age*	Definition of educational environment regarding *place*	Support for the relative education model	Dependent variables in the analyses
Campbell (2009)	Narrow (four cohorts: 25–34, 35–44, 45–64, 65+)	Wide and narrow: State MSA/County Zipcode	The relative education model is only supported on *electoral activity* and only when the educational environment is measured narrowly regarding geography (i.e., as MSA/county or zipcode) No support for the relative education model is found on *the expressive index, the civic index, voting*	Electoral activity (a: persuading others; b: displaying button/signs/stickers; c: campaign contributions; d: volunteering for candidate or political organizations) Expressive index (a: contacting officials; b: contacting the print media; c: contacting the broadcast media, protesting) Civic index (a: community problem-solving; b: regular volunteering for non-electoral organization; c: active membership in association; d: raising money for charity) Voting

Comment: Dependent variables other than forms of political participation are excluded from the table

than oneself, but never with those younger than oneself. NJS's definition of the educational environment is indeed rather counter-intuitive; for that reason, Helliwell and Putnam use geographically narrower measures and compare respondents' education to "all other living adults, both older and younger" (Helliwell and Putnam 2007, p. 3). In relation to NJS, Helliwell and Putnam's definition is wide when it comes to age but narrow geographically. Their results show that "the contextual effects of education on social participation are generally positive, and never significantly negative" (Helliwell and Putnam 2007, p. 3). Thus, their results provide no support for the relative education model.[5]

Tenn (2005) presents an even finer measure of relative education. To overcome the definitional problems in previous studies, Tenn employs an intra-birth-cohort measure of relative education; each individual's education is compared to those born in the same year. Tenn's results support the relative education model: "relative education has far more explanatory power than does absolute education" (Tenn 2005, p. 279). However, Tenn uses data from the US Current Population Survey, and he is therefore only able to test the influence of relative education on one dependent variable—voter turnout.

Furthermore, Campbell (2009) criticizes the wide geographic scope of NJS's and Tenn's measures of the educational environment by arguing that since social networks are local rather than national, the educational environment should be measured using narrow geographic units. Consequently, it is not appropriate to assume, for example, that the relative impact of education among people living in New York is dependent on the levels of education in Alaska. To date, the only study that uses a narrow measure of both age and place is Campbell's (2009). Using relatively small age groups (divided into four cohorts) and zip codes, Campbell finds support for the relative education model on competitive forms of political participation, more precisely "electoral activities" such as persuading others, displaying buttons, making campaign contributions, and volunteering for candidates or for political organizations.

How to best measure the educational environment remains an open question since previous research employs very different units of aggregation for the educational environment and has shown contradictory results. Hence, in the empirical analyses I will separately test three alternative measures of the educational environment in order to obtain a better understanding of where and when sorting processes operate.

The Relative Education Model in the Swedish Context

Although this is a single-country study of Swedish data, the aim is to make a comparison with previous US studies on the relative education model. For that reason, it is essential to emphasize both the similarities and differences between the two national contexts. Initially it should be emphasized that Sweden and the US are

[5] However, as Campbell points out Helliwell and Putnam "have not accounted for the considerable differences in educational attainment across age cohorts" (Campbell 2009, p. 775). In addition, Helliwell and Putnam do not include electoral activities. In sum, this may explain the weak support of the sorting model in their analyses.

similar in that they are both post-industrial Western democracies. However, within this group of countries they are poles apart (cf. Granberg and Holmberg 1988, p. 3).

The most important difference between the Swedish and the US context is the greater amount of equality in Sweden. The level of equality is of interest in relation to the relative education model because it is reasonable to expect that the model receives less support in societies that have high levels of equality. NJS claim that what matters is the position in the educational hierarchy, and they do not provide any discussion as to how the distance between the positions in the hierarchy affects the applicability of the relative education model. I hypothesize that the relative education model should gain more support the more unequal a society is. In a society where there is a large amount of inequality, there are also larger distances between social networks—it is harder for those with low levels of education to access the social networks that are the most important for gaining political influence. Low levels of social stratification may make it easier for the disadvantaged to participate in politics. Thus, the support for the relative education model should be stronger in societies with higher levels of inequality. For that reason, I hypothesize that Sweden constitutes a harder test—a less likely case—for the relative education model, since the levels of equality are, in most respects, higher in Sweden than in the US.[6]

Yet there is no previous research on how the level of inequality affects the validity of the relative education model. However, we can draw some predictions from the literature on how inequality affects social networks. Bottero points out that one of the key features of inequality is that "it leads to 'social distance' in our personal relations" (Bottero 2007, p. 828). Furthermore, in unequal societies "[p]eople with different social attributes are less likely to interact and form social relationships because there is *already* 'social distance' between them" (Bottero 2007, p. 828). Hence, since the distance between social networks is larger in the US than in Sweden it is plausible to assume that social network centrality has a stronger effect on political participation in the US than in Sweden.

Here it should also be emphasized that a prominent difference between the contexts is the fact that the Swedish educational system has been explicitly designed to achieve egalitarian values, such as promoting social equality (Erikson and Jonsson 1996; Hout and Dohan 1996; Rothstein 1996; Meghir and Palme 2005). Furthermore, Eriksson and Jonsson claim that Sweden is exceptional since regarding trends in educational inequality Sweden is the only country where they find equalization over time (Erikson and Jonsson 1996, p. 8). Consequently, since the Swedish educational system has increased social mobility one could expect that social networks are less stratified in Sweden. Furthermore, since the Swedish educational system is more egalitarian, education may have a weaker impact on

[6] Regarding income there is a larger gap between the rich and the poor in the US than in Sweden (De Nardi et al. 2000). The Gini Coefficient is 0.368 for USA and 0.252 for Sweden (*source*: Luxembourg Income Study 2007). Additionally, Sweden has been shown to occupy a special place in studies of social mobility. Breen and Jonsson claim that "class origins […] appear to have a smaller influence on class destinations […] than in most other countries" (Breen and Jonsson 2007, pp. 1175–1776). Likewise, preferences in favor of egalitarian values are also less common in the US than in Sweden (Verba and Orren 1985).

social networks, which in turn would imply that the hypothesized pathway between relative education and political participation via network centrality is less evident in Sweden than in the US.

Moreover, the pathways of recruitment to political assignments in Sweden are very different compared to those in the US. As in most countries, the members of the Swedish parliament (Riksdag) constitute an elite that is more highly educated and more often has upper-middle class occupations compared with the entire electorate (Esaiasson and Holmberg 1996; Holmberg 1989; Narud and Valen 2000). However, in Sweden there has been a close link between the dominant party—the Social Democratic Party—and the working class movement, particularly the Swedish Trade Union (LO). For that reason, engagement in the trade union has been an important pathway for recruitment to political assignments in Sweden (Holmberg 1974, Chap. 10). The working class movement constitutes an alternative pathway to political participation that has no counterpart in the US context. Cross-national studies show that strong labor organizations have a significant mobilizing effect that increases levels of participation (Radcliff and Davis 2000). Most importantly, education is not crucial to achieve central positions in these networks. To conclude, in Sweden there are other non-educational routes to central social network positions that positively affect political participation. Hence, the impact of the working class movement on the Swedish political scene may decrease or balance the sorting effects of education. Thus, these factors suggest that the relative education model should receive less support in the Swedish context.

The general trends in Sweden concerning mean levels of education and the amount of political participation during the period 1985–2006 correspond to the general trends in most Western countries, i.e. the paradoxical relationship between education and political participation is valid in Sweden as well. During this period, the mean level of education has increased significantly, from less than 10 years to more than 11 years. The change in mean levels of education is even more prominent among 26–36 year olds, where the mean levels has increased from approximately 11.5 to 13 years. At the same time, all forms of political participation under study in this article declined during this period.[7]

Data

The analysis employs data from the Swedish National Election Studies (SNES) to test the relative education model.[8] The 1985 to 2006 SNES surveys were pooled,

[7] During 1985 to 2006 voter turnout in Sweden has declined from 90% in 1985 to 82.0% in 2006. Moreover, contact with political representatives has decreased from 16% to about 9%. Party membership and active participation in political parties have however remained quite stable around 8 and 3% respectively since the early 1990s.

[8] The pooled dataset consists of SNES 1985, 1988, 1991, 1994, 1998, 2002, and 2006. The SNES-studies are based on face-to-face interviews; Statistics Sweden (SCB) carries out the fieldwork. The response rates vary between 69.3% (2002) and 81.4% (1998). The SNES surveys are conducted at every Swedish election and are based on national representative samples. Principal investigators were Sören Holmberg and Mikael Gilljam (1985, 1988, 1991 and 1994), Sören Holmberg (1998) and Sören Holmberg and Henrik Oscarsson (2002 and 2006).

and all the analyses in this article were made based on the merged dataset. Indicators on both each individual's level of education as well as the mean level of education in the social environment are needed in order to test the relative education model. To meet the task of providing a full test of the relative education model and the alleged causal mechanisms, we would ultimately need individual level longitudinal data with reliable measures of social network centrality, in addition to the independent and the dependent variables. In the absence of such data, the aim of the study is to examine the central prediction derived from the relative education model, i.e. whether the effect of individual education is conditional on educational levels in the educational environment. And most importantly, whether higher mean levels of education in the environment are associated with a smaller effect of education at the individual level.

All SNES surveys include information about each respondent's highest achieved level of education.[9] The educational environment variables were calculated based on data from Statistics Sweden and merged on the pooled SNES dataset. Fortunately, Statistics Sweden annually supplies information about the number of individuals with different lengths of education in every municipality, organized along the different years in which people were born. This data is publicly available on the Statistics Sweden website.[10] Spreadsheets can be generated to include information about the number of people at different ages with different lengths of education in each municipality. Mean levels of education were calculated in accordance with the three definitions of educational environment explained below, respectively, for every respondent in the dataset using the spreadsheets generated from the website of Statistics Sweden. Thereafter, the educational environment variables were merged on the pooled dataset containing SNES data from 1985 to 2006. In total, the dataset consists of 20,063 individuals and contains information about the educational environment in accordance with the three definitions, respectively, for each individual.[11]

Since previous studies put forward contradictory suggestions for how to best define the educational environment, three different units of aggregation for the

[9] In SNES 2006 and 2002 we got exact information from Statistics Sweden about each respondent's educational attainment. In the older surveys, there is a single question on highest achieved education. Data from 2002 and 2006 are harmonized to follow the same scale as the measures in the earlier surveys in order to not produce distortion in the comparison between the surveys before and after 2002. Data on individuals' highest achieved education was transformed to a variable describing the length of education in years.

[10] All data on contextual levels of education can be obtained from http://www.ssd.scb.se/databaser/makro/start.asp.

[11] Following prior research in the field, respondents younger than 26 were excluded since a considerable amount of them have not yet finished their educations. Thus, the effects of education on social network centrality are yet to come. Likewise, respondents older than 74 years old are excluded since no information on mean levels of education among individuals over that age are available from Statistics Sweden.

educational environment were produced.[12] First, the measure *A*: *age and place* employs a unit of aggregation that is narrow as regards both age and place; it defines the educational environment as the mean level of education among individuals born in the same year as the respondent in the specific municipality where the respondents were living at the time of survey. This measure is the most narrowly defined measure of the educational environment considering *both* age and place tested so far in any study of the relative education model. Second, the measure *B*: *place only* is narrow as regards place but wide as regards age; it defines educational environment as the mean level of education in the municipality among individuals 26–74 years old. Third, the measure henceforth referred to as *C*: *age only* is narrow as regards age but wide as regards geography. Each individual's educational environment is defined as the mean level of education at the time of the survey among individuals born in the same year in the entire country.

The following indicators of political participation are used to test the relative education model: (a) voting in general elections for the parliament (Riksdag); (b) writing a letter to a political representative; (c) membership in a political party; and (d) active participation in a political party. All dependent variables in the analyses are dichotomous. A set of controls including dummies for gender, civil status, homeownership, church attendance, and residence in rural area as well as taxable income (five categories) are included in the analyses to balance for the influence of these factors. Furthermore age is included as a control, which is important since numerous studies show that political participation is related to position in the life cycle.[13] However, not only age but also generation has been shown to affect participation. Older people generally participate more at each point of time, but seen over time there is also a generation effect, for example, individuals born in the beginning of the 20-century generally participate more in traditional forms of political participation than individuals born later (e.g. Zukin et al. 2006). Since the dataset used in this article contains election studies from a period ranging over 20 years (1985–2006), age and generation is not perfectly correlated and it is possible to include both age and two dummy coded variables for generation in the model without risking multicolliniearity to distort the results. More precisely the generation variable contains three categories: born pre 1945, born 1946–1964, and born post 1965. In addition, a variable measuring time[14] (year of survey) is included in the analyses to account for the generally decreasing trend in participation over the time period that the surveys cover.[15]

[12] It is worth to mention that it is not possible to replicate the measure of educational environment employed by NJS—i.e. using the mean level of education among people 25–50 when the respondent was 25. To replicate this measure we would need detailed information about mean levels of education as far back as in the 1930s. Furthermore, to use a narrow area specific measure we would need to know where respondents lived at the time they were 25; unfortunately we only know where they lived at the time of the survey. For that reason, mean levels of education in the environment are calculated at the time of every specific survey.

[13] To reduce multicollinearity in the model, age is divided in 10 cohorts (26–30, 31–35, 36–40, 41–45, 46–50, 51–55, 56–60, 61–65, 66–70, 71–74).

[14] Substituting the "time" variable with a set of dummy variables for each specific election year does not significantly alter the results. Results available upon request from the author.

[15] Even though the models include both age cohorts, generation, time, and in addition two of the specifications of the educational environment define mean education as the level of education among

The Modeling Strategy

In previous studies, relative education has been modeled in several different ways. In this section I will discuss the pros and cons of the different modeling strategies. The first strategy is to create a measure of relative education and compare its impact in relation to the conventional measure of absolute education (in years). This strategy is employed by NJS, who use a relative education measure calculated as the ratio between absolute education for the individual and the mean level of education in the environment (see Jenkins 1996, p. 230). Such a modeling strategy might work well when it comes to comparing the goodness of fit between models employing absolute versus relative education measures. However, this strategy does not explain whether a sorting process actually takes place. Most importantly, it does not explain whether the impact of individual levels of education varies with different levels of education in the environment.

The second strategy, applied by NJS (1996) and Helliwell and Putnam (2007), is to add a variable measuring the educational environment into a multiple regression model together with the individual level of education. A significant negative effect of the educational environment is interpreted as support for the relative education model. However, this modeling strategy does not constitute a true test of the predictions from the model since it only takes into account the contextual effect of the educational environment under control for the individual level of education. This modeling strategy does not test whether the individual effect is conditional on the contextual effect; in other words, whether the individual-level effect of education decreases as the level of education in the environment increases. This strategy only estimates whether there is a contextual level main effect of the mean level of education in the environment. The relative education model actually posits an interactive hypothesis—the higher the level of education in the environment, the smaller is the effect of individual education. Likewise, the second strategy does not answer the crucial question; whether each year of additional education matters less in environments with high mean levels of education.

For these reasons, Campbell (2009) applies a third modeling strategy; to test the interaction between each individual's years of education and the educational environment (Individual Education × Educational Environment). Explicitly specifying the interaction effect in a regression model is, by all standards, the most

Footnote 15 continued
people at the same age, results are not distorted by multicollinearity. Models with educational environment defined as "A: Age and place" and as "B: Place only" includes no independent variables correlated higher than 0.706. None of the variables have a VIF above the critical value 10 or tolerance below 0.1. However, models with educational environment defined as "C: Age only" suffer from some multicollinearity since the educational environment measure and "age" has a correlation of −0.7949. As a consequence, the VIF for "Educational environment: Place only" is 10.56 and "Age" is 11.26. However, by dropping the "Generation" dummies the VIF decreases below the critical value 10 (VIF for "Educational environment" is 9.30 and "Age" is 7.03). Models without the generation dummies do not significantly alter the sizes, signs or significance of the main independent variables presented in the article. Most, importantly the interaction term in model 3 remain significant and the marginal effect of education decrease from a significant value of 0.0096 when the "Educational environment" is held constant two standard deviations below the mean to 0.0077 when the "Educational environment" is held constant two standard deviations above the mean.

accurate way to test of the relative education model, since it is the only one of these three modeling strategies that is up to the task of testing whether each year of education at the individual level matters less in environments with high mean levels of education. There is one obvious major argument in favor of including the interaction term to test the relative education model: the theory is about an interactive relationship. One cannot test whether the impact of individual education is conditioned on the education level in the environment without taking the interaction between individual level education and contextual education into account. For this reason I follow the strategy employed by Campbell (2009) and include an interaction term between individual education and contextual education in the models.

The downside of this strategy is that models with interaction effects are less intuitive and harder to interpret, especially when employing logistic regression. To actually test whether each year of education matters less in environments with high mean levels of education, one could not merely examine the signs, values and significance level of the coefficient of the interaction term in the regression output (Norton et al. 2004; Ai and Norton 2003). Rather, one needs to examine the marginal effect or predicted probabilities of individual-level education while holding the educational environment constant at a range of relevant values (cf. Kam and Franzese 2007). For that reason, marginal effects of individual level education is calculated while holding contextual education constant at a range of relevant values.

Results

Tables 2, 3, 4, and 5 present results from logistic regression models on the impact of education and the educational environment on each of the four dependent variables using the three different operationalizations of the educational environment, respectively.[16] All models include the interaction term between individual education and the educational environment, as well as both constitutive terms and the relevant controls.

A closer look at the effects on voting in Table 2 reveals that the interaction term is significant when applying all three of the operationalizations of the educational environment. The effect of education at the individual level is interacted with the level of education in the environment. When it comes to communication with political representatives, we find no significant interaction effects whatsoever. Furthermore, with regard to membership in political parties and actively working in political parties, the coefficient of the interaction term is significant only when using *B*: *place only* as the unit of aggregation for the educational environment. Hence, results indicate that sorting processes take place in relation to both voting and activities in political parties. However, for voting the individual-level education

[16] Since the individuals are clustered within different educational environments the vce(cluster) option in STATA11 is used in order to cluster individuals within their educational environment and produce standard errors which allow for intragroup correlation.

Table 2 Effects of education and educational environment on *voting*. Logit Models

Generation 1 (Born pre-1945) = Base	(1) Unit of aggregation for educational environment: *A*: *Age and place* (mean level of education in municipality among people at the same age)	(2) Unit of aggregation for educational environment: *B*: *Place only* (mean level of education in municipality among 26–74 year olds)	(3) Unit of aggregation for educational environment: *C*: *Age only* (mean level of education among individuals at the same age in entire country)
Years of education	0.123*** (0.014)	0.129*** (0.015)	0.129*** (0.014)
Educational environment	0.060 (0.045)	0.039 (0.050)	0.068 (0.089)
Years of education × Educational environment	0.038*** (0.008)	0.024** (0.011)	0.047*** (0.009)
Gender (1 = male)	−0.278*** (0.071)	−0.274*** (0.075)	−0.276*** (0.069)
Civil status (1 = married)	0.732*** (0.072)	0.716*** (0.072)	0.734*** (0.075)
Taxable income	0.195*** (0.031)	0.182*** (0.029)	0.200*** (0.029)
Residence (1 = reside in rural area)	0.004 (0.075)	−0.002 (0.079)	−0.011 (0.072)
Homeownership (1 = homeowner)	0.425*** (0.074)	0.422*** (0.072)	0.428*** (0.074)
Church attendance (1 = attend church monthly)	0.081 (0.129)	0.085 (0.134)	0.079 (0.120)
Age	0.070** (0.031)	0.083*** (0.026)	0.065 (0.048)
Generation 2 (1 = born 1946–1964)	−0.300** (0.132)	−0.259** (0.131)	−0.325** (0.148)
Generation 3 (1 = born post 1965)	−0.409* (0.211)	−0.270 (0.204)	−0.470* (0.252)
Time (year of survey)	−0.029*** (0.008)	−0.032*** (0.008)	−0.028** (0.012)
Constant	58.772*** (15.437)	65.220*** (14.985)	57.933** (23.248)
Observations	13,440	13,440	13,440
Pseudo R^2	0.068	0.066	0.069

Comment: Unstandardized coefficients. Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

effect is interacted with all three measures of the educational environment. For party related political participation the mean level of education of all individuals at the local level (*B*: *place only*) is interacted with the individual-level effect.[17]

[17] In logistic regression, merely examining the significance level of the coefficient for the interaction term reported in the regression output cannot test the true significance of the interaction. Since each of the coefficients is conditional on the other variables in the model in logistic regression, the true sign of the interaction term as well as its level of significance may be different for different observations (Norton et al. 2004; Ai and Norton 2003). Additional analyses have been made to compute the correct effect of the interaction term by making use of Norton, Wang and Ai's STATA command inteff. Results from inteff for the models with significant coefficients of the interaction terms are supplied upon request from the author.

Table 3 Testing the impact of education and the educational environment on *writing a letter to a political representative*. Logit Models

Generation 1 (Born pre-1945) = Base	(4) Unit of aggregation for educational environment: *A*: *Age and place* (mean level of education in municipality among people at the same age)	(5) Unit of aggregation for educational environment: *B*: *Place only* (mean level of education in municipality among 26–74 year olds)	(6) Unit of aggregation for educational environment: *C*: *Age only* (mean level of education among individuals at the same age in entire country)
Years of education	0.161*** (0.019)	0.160*** (0.019)	0.161*** (0.019)
Educational environment	0.053 (0.062)	0.033 (0.073)	0.073 (0.105)
Years of education × Educational environment	0.008 (0.011)	0.010 (0.018)	−0.002 (0.012)
Gender (1 = male)	0.262** (0.104)	0.256** (0.103)	0.257** (0.110)
Civil status (1 = married)	−0.188* (0.113)	−0.187* (0.110)	−0.190 (0.122)
Taxable income	0.127*** (0.047)	0.129*** (0.047)	0.127*** (0.044)
Residence (1 = reside in rural area)	0.110 (0.105)	0.104 (0.112)	0.076 (0.102)
Homeownership (1 = homeowner)	0.006 (0.116)	0.005 (0.116)	−0.001 (0.115)
Church attendance (1 = attend church monthly)	0.687*** (0.145)	0.683*** (0.144)	0.686*** (0.158)
Age	0.099** (0.042)	0.078** (0.035)	0.112* (0.059)
Generation 2 (1 = born 1946–1964)	−0.203 (0.175)	−0.180 (0.163)	−0.177 (0.175)
Generation 3 (1 = born post 1965)	−0.006 (0.287)	−0.009 (0.274)	0.056 (0.313)
Time (year of survey)	−0.033*** (0.013)	−0.032** (0.014)	−0.036** (0.015)
Constant	62.671** (24.812)	60.298** (28.706)	67.897** (30.178)
Observations	5970	5970	5970
Pseudo R^2	0.048	0.047	0.047

Comment: Unstandardized coefficients. Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Now, let us see how the educational environment alters the effects of individuals' education by looking more closely at the marginal effects of individual level education while holding education in the environment constant at different levels. The crucial test of the relative education model is whether there is a decreasing marginal effect of an additional year of education on the probability of political participation when the educational environment is held constant at increasingly higher values. Table 6 reports the marginal effect of individual education and standard errors calculated by the delta method for models with a significant

Table 4 Testing the impact of education and the educational environment on *membership in political parties*. Logit Models

Generation 1 (Born pre-1945) = Base	(7) Unit of aggregation for educational environment: *A*: *Age and place* (mean level of education in municipality among people at the same age)	(8) Unit of aggregation for educational environment: *B*: *Place only* (mean level of education in municipality among 26–74 year olds)	(9) Unit of aggregation for educational environment: *C*: *Age only* (mean level of education among individuals at the same age in entire country)
Years of education	0.036*** (0.011)	0.048*** (0.011)	0.026** (0.012)
Educational environment	−0.130*** (0.038)	−0.273*** (0.047)	0.062 (0.049)
Years of education × Educational environment	0.000 (0.006)	0.016* (0.009)	−0.005 (0.008)
Gender (1 = male)	0.466*** (0.064)	0.465*** (0.064)	0.478*** (0.066)
Civil status (1 = married)	0.168** (0.074)	0.172** (0.075)	0.164** (0.068)
Taxable income	0.051* (0.027)	0.053* (0.029)	0.036 (0.028)
Residence (1 = reside in rural area)	0.484*** (0.062)	0.396*** (0.064)	0.547*** (0.057)
Homeownership (1 = homeowner)	0.250*** (0.073)	0.225*** (0.075)	0.252*** (0.077)
Church attendance (1 = attend church monthly)	0.822*** (0.082)	0.814*** (0.082)	0.838*** (0.086)
Age	0.019 (0.024)	0.071*** (0.019)	0.106*** (0.029)
Generation 2 (1 = born 1946–1964)	−0.154 (0.105)	−0.187* (0.102)	−0.161 (0.112)
Generation 3 (1 = born post 1965)	−0.557*** (0.184)	−0.525*** (0.181)	−0.409* (0.213)
Time (year of survey)	−0.031*** (0.007)	−0.018** (0.007)	−0.052*** (0.008)
Constant	59.654*** (13.012)	31.740** (14.880)	100.919*** (15.306)
Observations	13,465	13,465	13,465
Pseudo R^2	0.066	0.069	0.064

Comment: Unstandardized coefficients. Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

interaction term (Models 1, 2, 3, 8 and 11).[18] The marginal effects of education are calculated when holding the educational environment at its mean as well as plus and minus one and two standard deviations of the mean.[19] Thus we can trace how the marginal effect of education alters when moving from low to high levels of education in the environment. This is also shown graphically in Fig. 1.

[18] Marginal effects were calculated with the margins command in STATA11.

[19] Marginal effects are calculated while all controls are simultaneously held at their means.

Table 5 Testing the impact of education and educational environment on *actively working in a political party*. Logit Models

Generation 1 (Born pre-1945) = Base	(10) Unit of aggregation for educational environment: *A*: *Age and place* (mean level of education in municipality among people at the same age)	(11) Unit of aggregation for educational environment: *B*: *Place only* (mean level of education in municipality among 26–74 year olds)	(12) Unit of aggregation for educational environment: *C*: *Age only* (mean level of education among individuals at the same age in entire country)
Years of education	0.095*** (0.017)	0.107*** (0.018)	0.085*** (0.017)
Educational environment	−0.059 (0.056)	−0.260*** (0.065)	0.163 (0.109)
Years of education × Educational environment	0.008 (0.009)	0.024* (0.013)	−0.001 (0.011)
Gender (1 = male)	0.322*** (0.096)	0.316*** (0.095)	0.331*** (0.098)
Civil status (1 = married)	0.244** (0.114)	0.245** (0.117)	0.238** (0.113)
Taxable income	0.033 (0.041)	0.038 (0.043)	0.019 (0.039)
Residence (1 = reside in rural area)	0.550*** (0.093)	0.437*** (0.093)	0.574*** (0.088)
Homeownership (1 = homeowner)	0.380*** (0.120)	0.354*** (0.121)	0.375*** (0.126)
Church attendance (1 = attend church monthly)	1.093*** (0.108)	1.078*** (0.110)	1.106*** (0.102)
Age	0.039 (0.037)	0.066** (0.028)	0.147*** (0.054)
Generation 2 (1 = born 1946–1964)	−0.443*** (0.154)	−0.440*** (0.151)	−0.420** (0.169)
Generation 3 (1 = born post 1965)	−0.579** (0.271)	−0.536** (0.263)	−0.359 (0.296)
Time (year of survey)	−0.023** (0.010)	−0.004 (0.011)	−0.047*** (0.014)
Constant	40.746** (19.974)	4.137 (21.417)	89.768*** (27.674)
Observations	13,466	13,466	13,466
Pseudo R^2	0.064	0.068	0.065

Comment: Unstandardized coefficients. Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

First, all marginal effects reported are significant at the $p < 0.01$ level. It is important to note that under control for the level of education in the environment the marginal effects of individual education are small albeit significant. If the relative education model is to be proven right, the marginal effect of education should be larger in low education environments and smaller in high education environments. In Table 6 we can clearly see that this is the case: the marginal effect of education decreases when the level of education in the environment increases. The higher the level of education in the environment, the smaller the marginal effect of each individual's education. For voting, the marginal effect is decreasing with about 25%

Table 6 Marginal effects of individual-level years of education at different levels of education in the educational environment, using logit results

Dependent variable:	Voting	Voting	Voting	Party membership	Actively working in a political party
Marginal effects derived from: Unit of aggregation for educational environment:	Model 1 A: Age and place	Model 2 B: Place only	Model 3 C: Age only	Model 8 B: Place only	Model 11 B: Place only
Educational environment at mean level minus two standard deviations	0.0098*** (0.0015)	0.0094*** (0.0013)	0.0102*** (0.0025)	0.0064*** (0.0016)	0.0066*** (0.0014)
Educational environment at mean level minus one standard deviation	0.0091*** (0.0012)	0.0091*** (0.0011)	0.0095*** (0.0016)	0.0053*** (0.0013)	0.0053*** (0.0010)
Educational environment at mean level	0.0085*** (0.0010)	0.0088*** (0.0010)	0.0089*** (0.0010)	0.0043*** (0.0010)	0.0042*** (0.0007)
Educational environment at mean level plus one standard deviation	0.0079*** (0.0010)	0.0086*** (0.0011)	0.0082*** (0.0011)	0.0034*** (0.0008)	0.0032*** (0.0005)
Educational environment at mean level plus two standard deviations	0.0073*** (0.0012)	0.0083*** (0.0012)	0.0076*** (0.0015)	0.0027*** (0.0006)	0.0025*** (0.0005)

Comment: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

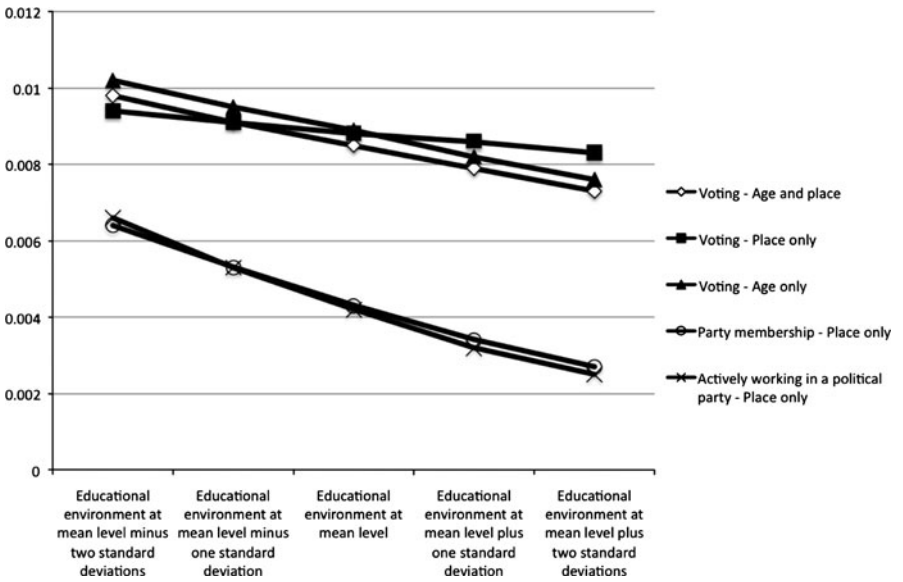

Fig. 1 Marginal effects of individual-level years of education at different levels of education in the educational environment, using logit results. *Comment*: See Table 6 for additional details

Table 7 Summary of result

Unit of aggregation	Socially based political participation		Voting	Individually based political participation
	Membership in political party	Active participation in political party		Writing letter to political representatives
A: Age and Place	–	–	Support for the relative education model	–
B: Place only	Support for the relative education model	Support for the relative education model	Support for the relative education model	–
C: Age only	–	–	Support for the relative education model	–

when educational environment measures taking age into account are applied (measure *A*: *age* and *C*: *age and place*). The marginal effect diminishes even more for political party activities and party membership; in these cases the marginal effect of individual education is less then half in the high education environment compared to the low education environments. Hence, as hypothesized by the relative education model, as the mean level of education in the environment increases, the effect of individual education decreases. Table 7 summarizes the results and shows on which dependent variables and using which units of aggregation the relative education model is supported.[20]

On the one hand, the results show that as regards participation related to political parties, sorting processes take place at the municipal level. Hence, the relative education model is supported for political party membership and party activities when the educational environment is defined as proposed by Helliwell and Putnam: geographically narrow and wide concerning age. On the other hand, the results show that sorting processes take place in another way in relation to voting. Using any of the three definitions of the educational environment, results show that sorting processes take place in relation to voting.

Conclusion

This paper provides the first in depth evaluation of the relative education model outside the US. A number of previous studies based on data from the US have

[20] One concern when using this pooled dataset from a period of over 20 years might be whether the simultaneous overall trends towards lower participation and higher education distorts the results. Indeed there is such a trend during the period (as explained in footnote 9). A continuous variable for "time" (year of survey) is included in the analyses to control for this negative trend. Another way to further make sure that this negative trend has not distorted the results is to run the models for each specific year respectively. Results from such models can be provided upon request from the author. In sum results from these models show no systematic distortion in the year-by-year coefficients and the amount of support for the sorting model does not vary over time.

shown support for the argument that the effects of education on political participation are relative rather than absolute. However, we have hitherto not known whether the relative education model is also supported in a more egalitarian country such as Sweden. Despite the fact the Swedish context is a harder test for the relative education model, the results partially support the relative education model. NJS's model gains support on voting and political participation related to political parties, but in the second case only when the educational environment is defined narrow as regards geography. Thus, the relative education model is only proven valid on the socially based forms of political participation when the educational environment is measured at the municipal level. In sum, the results presented in this article strengthen the support for the relative education model since it has been proven valid in a new and very different context that constitutes a harder test for the model. This study thereby confirms that the relative education model of education is not a unique feature of American political culture. Education has indeed an effect on the individual level, but the level of education in the social environment matters as well.

However, results also support Huckfeldt's argument that socially based forms of participation are affected by contextual factors while purely individually based forms are not. Results also lend support to Campbell's argument that the scope of the sorting model should be narrowed since it is found that the individual and non-competitive form of participation under study—writing letters to political representatives—is not affected by education in a relative way. For this forms of participation the level of education in the social environment does not alter the individual level effect of education. Thus, these results provide further support to Campbell's argument that the relative education model put forward by NJS should be revised; it is not reasonable to predict relative education effects on purely individually based and non-competitive forms of political participation.

To conclude, it is worth stressing the important implications of whether the relative education model or the absolute education model makes correct predictions. Determining this is crucial for whether we shall expect increased levels of political participation as a consequence of increased mean levels of education in society. Systematic inequalities in levels of political participation are often considered to be a democratic problem, since it will lead to unequal influence in the political process (cf. Lijphart 1997). On the one hand, if the absolute education model is correct, education can help address this problem, and inequalities in levels of political participation can be mitigated by raising the level of education. On the other hand, if the relative education model is correct, extending education as a means to increase political participation is a futile strategy that will only lead to educational inflation. Disappointingly for proponents of the absolute education model, as regards voting and political party activities, results presented in this article indicate that educational inflation will occur as a consequence of increased aggregated educational levels. Extended education among the citizenry has not lead to correspondingly higher aggregated levels of participation since the individual effect of education decreases as the aggregated level of education increases.

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Paper 2

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2

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Article

Social network position mediates the effect of education on active political party membership

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Mikael Persson
University of Gothenburg, Sweden

Abstract
Research has shown that education is positively correlated with political party membership at the individual level. It is thus puzzling that increased education at the aggregated level in most Western countries has not resulted in an aggregate increase in levels of party membership. One explanation for this paradox is provided by the sorting model of education, according to which there is no direct effect of education on political participation; education affects individuals' social network positions, which in turn affects political participation. Prior research on the sorting model has focused on the observable predictions derived from the model. The hypothesized causal mechanism, i.e. social network position, has not been sufficiently tested. This article employs Swedish data with comprehensive measures of social relations and utilizes structural equation modelling to test the hypothesized causal relationship. The results confirm that social network position mediates the effect of education on active political party membership.

Keywords
education, party membership, political participation, political socialization, social networks

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Corresponding author:
Mikael Persson, Department of Political Science, University of Gothenburg, Box 711, SE 40530, Gothenburg, Sweden
Email: mikael.persson@pol.gu.se

The problem

Research on participation in political parties has shown that active party members have generally had a substantially higher education than average citizens (e.g. Cross and Young, 2008; Scarrow and Gezgor, 2010; Whiteley, 2009, 2011).[1] However, research in this area seldom problematizes the possible causal mechanisms linking education with active party membership, i.e. exactly *why* do the high-educated participate in political parties to a greater extent than the low-educated? According to the conventional view, education increases civic skills and cognitive capabilities, factors that in turn increase the likelihood of participation in political parties (Lewis-Beck et al., 2008; Verba et al., 1995). One of the major puzzles in political behaviour research is therefore why increased education at the aggregated level in Western countries has not resulted in a corresponding aggregate increase in levels of political party participation (Brody, 1978).

Since the literature on party membership has widely ignored this paradox, we turn to the broader literature on political participation in order to find a theoretical explanation. One explanation of the paradox is suggested by the sorting model of education, originally proposed by Nie et al. (1996) (henceforth NJS) and refined by Campbell (2009). According to this model, social network position is the causal mechanism linking education with political participation: education functions as a sorting mechanism that influences individuals' social network positions, which in turn affects political participation.

NJS claim that education is the primary factor influencing social network position. By obtaining a higher level of education, individuals make contacts that form and facilitate high-status social network positions. Access to high-status social networks encourages political participation, mainly by increasing the likelihood of being recruited. In other words, high-educated people are more likely to be tied to networks that consist of active political participants, and are therefore more likely to be recruited to participate themselves. Education determines who the people with these important network ties are. There are several reasons why the most educated individuals in a population are the ones with the most high-status social network ties. NJS (1996: 49–53) claim that, in addition to the fact that individuals make contacts at educational institutions, education also affects occupational prominence, family wealth and membership in voluntary associations, which in turn affects social network status. Hence, through a series of interlinked factors, education promotes contacts with a wide range of persons.

This model stands in sharp contrast to the conventional view that education is a direct cause of political participation. Previous studies on the sorting model have investigated the predictions derived from the model. This article takes another approach. It does not focus on studying education inflation *per se*; instead it aims to test the causal mechanism pointed out by NJS, i.e. whether social network position mediates the relationship.

The article makes two contributions. First, it contributes to the literature on party membership by providing a better understanding of the relationship between education and participation in political parties. Finding out which model can correctly explain the relationship between education and participation has important theoretical implications. If the sorting model is correct, then the effect of education on party membership found in most political behaviour research is misinterpreted. Despite the severe implications for our understanding of one of the most central predictors as to why people become active

in politics, the sorting model has been widely ignored within research on participation in political parties. Second, it contributes to the wider debate on educational inflation by directly studying the causal mechanism in the sorting model (social networks), which has been widely ignored in previous research. The study presents a research approach that improves on approaches used in prior research on the sorting model of education. Although a small number of studies have re-examined and refined the sorting model following NJS, all of them focus on the observable implications derived from the sorting model, i.e. whether the effect of education at the individual level is conditioned on the level of education in the environment (Campbell, 2009; Helliwell and Putnam, 2007; Tenn, 2005). The hypothesized full causal path including the causal mechanism – social network position – is only modelled in NJS's original study. However, as I argue, both the data used by NJS and the modelling strategy they employ have serious shortcomings. Hence, the extent to which social network position actually mediates the relationship remains hidden in the 'black box'.

This study is the first to explicitly test whether the hypothesized causal path is valid in another context than that of the US. It is a hard test of the generalizability of the model by testing it in Sweden, a context where it is reasonable to expect that less sorting takes place than in the US. The analysis employs Swedish survey data with more comprehensive measures on social connections compared to the data employed by NJS on the US case. Instead of using traditional regression path analyses as do NJS, this study makes use of a more powerful and robust modelling strategy: structural equation modelling with bias-corrected percentile interval bootstrap tests for the indirect effects. The study focuses on one central form of political participation – active political party membership. Drawing on previous studies on the sorting model, active participation in political parties should be one of the forms of participation for which sorting applies the most (e.g. Campbell, 2009). The results show that most of the effect of education on active political party membership is mediated via social network position. When the indirect effect is taken into account, the direct effect of education is small and insignificant.

The outline of the article is as follows. The next section presents the theory and a literature review. Thereafter, data and techniques of analyses are discussed. Results are subsequently presented and the article concludes with a discussion on the implications of the results.

Theory

Whether education has a direct causal effect on political participation is a matter of dispute in political behaviour research (Berinsky and Lenz, 2011; Burden, 2009; Campbell, 2009; Dee, 2004; Henderson and Chatfield, 2011; Highton, 2009; Hillygus, 2005; Kam and Palmer, 2008, 2011; Milligan et al., 2004; Mayer, 2011; Nie and Hillygus, 2001; Niemi and Junn, 1998; Persson, 2011, forthcoming; Persson and Oscarsson, 2010; Sondheimer and Green, 2010; Tenn, 2005, 2007). While some argue that education is a direct *cause* for political participation, others argue that there is no direct causal effect and that education is just a *proxy* for other factors.

According to the education as a cause view, education increases civic skills, which functions as the causal mechanism triggering participation (e.g. Lewis-Beck et al.,

2008; Verba et al., 1995; Wolfinger and Rosenstone, 1980). In their seminal work, Verba et al. (1995: 305) claim that: 'Education enhances participation more or less directly by developing skills that are relevant to politics – the ability to speak and write, the knowledge of how to cope in an organizational setting.' Furthermore, according to the education as a cause view, education also increases political knowledge and political interest. As Lewis-Beck et al. (2008: 102) put it: 'With more formal education comes a stronger interest in politics, a greater concern with elections, greater confidence in playing one's role as a citizen, and a deeper commitment to the norm of being a good citizen.' The education as a cause view has been confirmed in numerous studies on political participation in a wide array of democracies around the world. However, an important implication of the education as a cause view is that aggregate increases in education should lead to commensurate increases in political participation, which clearly has not been the case. Hence, despite the vast number of studies that show support for the conventional view, it fails to explain the paradoxical relation between education and participation at the individual and aggregate levels.

NJS claim that the conventional view is mistaken and that there are no direct effects of education on political participation. According to the sorting model, education is only a proxy for social position (NJS, 1996).[2] NJS claim that it is the social network position gained through education and not the skills and capabilities received during education that increases participation. Persons with high social status are exposed to social networks that encourage participation and are thus more likely to be recruited to political activities. Similarly, individuals with low education are outside recruitment networks that mobilize individuals into political activities.

An important implication of the sorting model is that the effect of individual education is conditioned on the level of education in the environment. In a low-education environment, less education at the individual level is needed in order to gain a high-status social network position. Hence the impact of education on political participation is relative rather than absolute.

Previous research on the sorting model

There are two major controversies in the literature on the sorting model: the scope of the model and the relevant unit of aggregation for the educational environment. As regards the scope of the model, NJS (1996) claim that the sorting model is valid for all forms of political participation. In Campbell's (2009) further development of the model, he argues that if social network position is the causal mechanism, the model should not be valid on the individualistic forms of participation. Campbell shows that only the forms of political participation that require individuals to be involved with other individuals or organizations in order to perform the acts are subject to sorting through social networks. For political activities that one can perform without interaction with other people, social network position does not seem to matter. Hence, according to Campbell the sorting model has a narrower scope than NJS claim.

The modelling strategies employed in previous studies have mainly differed with respect to how the educational environment is defined. NJS compare each respondent's levels of education to the mean national levels among individuals aged 25–50 when the

respondent was 25. Tenn (2005) uses intra-birth cohort measures of the educational environment. Helliwell and Putnam (2007) use geographically narrower measures in order to better capture the local context. Campbell (2009) uses even finer measures for both age and place (zip codes). And Persson (2011) uses data on mean educational levels in municipalities. Subsequent to NJS, all studies on the sorting model rely on empirical tests of the relationship between individual-level education and the level of education in the environment (Campbell, 2009; Helliwell and Putnam, 2007; Persson, 2011, forthcoming; Tenn, 2005). To test this, data with variation in the contextual mean levels of education are needed, either over-time data and/or data with geographical variation in mean levels of education. However, if we have data on the causal mechanism, it is possible to use cross-sectional data – without information about the contextual levels of education – to test the sorting model.

Modelling the relation between individual level of education and the educational level in the environment is a feasible strategy for testing the implications of the sorting model. It facilitates tests of educational inflation over time and estimation of the effect of individual education in environments with different mean levels of education. However, it is a crude way of measuring an individual's social network position and does not allow for direct examination of the causal mechanism. In this study, I do not aim at testing the educational inflation hypothesis directly. The aim is much more modest – to test the causal mechanism pointed out by NJS, i.e. whether social network position mediates the relationship between education and political participation.

The main reason for the lack of research on the full relationship is the absence of high-quality data on social network connections together with measures of the dependent and independent variables. To date, the only study examining the indirect effect of education via social network position is NJS's (1996: ch. 4) original study, in which they use data from the 1991 Current Population Survey. They use an additive scale constructed of the number of high-status people that the respondents claim known them.[3] Regression path models are used to estimate the indirect effects of education via social network connections. They find that social network position (and verbal cognitive proficiency) explains almost the entire relationship between education and voting, and that social network position is the main factor determining participation in 'difficult political activities'.[4]

However, NJS's path analyses are problematic for several reasons. First, NJS do not perform any significance tests of the indirect effects and thus leave it an open question whether education has a significant indirect effect via social network position. Even though the path from an independent variable to a mediator (a) and the path from a mediator to a dependent variable (b) are statistically significant, it does not follow that the indirect effect (ab) is statistically significant. The indirect effect needs to be significance tested in order to draw such conclusions (cf. Preacher and Hayes, 2008).

A second problem is that NJS's measure of social network position includes only connections with elected officials and persons working with news media. This problem concerns the causal direction in the model. It is not obvious that these connections are consequences of education (as the sorting model states); they can also (perhaps even more likely) be consequences of political participation. When you get active in politics, you obviously expand your own social network (Quintelier et al., forthcoming) and become more likely to get to know people such as elected officials. If social network

position instead were measured by social connections that are not as likely to be consequences of political participation, this problem would be less severe.

A third problem is that in NJS's analysis, social network position is treated as a manifest variable defined as a simple additive index rather than a latent variable. Social network position is a complex phenomenon that is obviously not possible to directly measure with only a few items, since it is unobservable. NJS's modelling strategy forces each of the items to contribute equally to the social network position measure, although this is likely not realistic. The more sound approach employed in this article is to treat social network position as a latent variable in a structural equation model that allows the different indicators to vary in their contribution to the measure. Hence, structural equation modelling can be used to estimate the underlying dimensionality of the factors. Structural equation modelling also has the benefit of taking measurement error into account.

Social networks and participation

Previous studies on the sorting model provide very little information about how social networks mediate the relationship between education and participation and what aspects of social networks are important. NJS (1996: 44) define social network centrality as 'proximity to governmental incumbents and political actors who make public policy and to those in the mass media who disseminate and interpret issues, events, and activities of people in politics'. This is a very narrow definition of social network position, since it only takes into account relations to two sorts of people. Even if relations to persons working with media and politics are crucial for inducing political activity, relations to other people might also trigger participation.

Research on social networks, social capital and political participation can help us refine this part of the sorting model. The size and composition of the social networks are seen as central in determining the effects on political participation. Usually, research in this field emphasizes social connections to a wide range of people in each individual's surroundings (e.g. Mutz, 2002; McClurg, 2003; Siegel, 2009). Three factors of social networks are suggested to have a positive impact on political participation: information, recruitment and mobilization (e.g. Kotler-Berkowitz, 2005). Research has found that large networks have a strong positive effect on participation: the more people you know, the broader the opportunities for recruitment.[5] As Kotler-Berkovitz (2005: 152) puts it:

> [T]ies to a diverse set of people – or to a set of people located across a diverse set of social groups – facilitate greater access to varied and non-redundant types of information that in turn enhance opportunities for undertaking social action, engaging in social activities, and reaping social and personal benefits.

Another important distinction in the literature is the impact of strong versus weak social ties. How strong do social network ties need to be in order to boost political participation? Is it enough to have a large social network of remote acquaintances or does one need to have strong friendships? Most research arrives at the latter conclusion – strong social ties have a greater impact on political participation than weak ties (Brady et al.,

1999; Verba et al., 1995). The rationale behind this conclusion is twofold. First, recruitment proposals come more frequently from people one is closely acquainted with than from those one is only weakly acquainted with. Second, individuals are more likely to accept invitations to participate from close acquaintances, while invitations from remotely acquainted people or strangers are more likely to be turned down.

These ideas are reinforced by the recent developments in studies on social networks and social capital. According to social capital theory, social networks help build social capital. Lin (1999a, b) focuses on social capital as the 'access to and use of resources embedded in social networks'. These resources include factors such as information and certification of social credentials, and reinforcement of identity and recognition. The access and use of social capital resources are determined by the position in the hierarchical structure. Extensive meta-studies on the effects of education on social capital conclude that education has strong and robust effects on social capital (Huan et al., 2009).

While little previous research exists on the links between social networks and party activism, some research focuses on the effects on social movement activity. This research generally concludes that diverse networks increase social movement activism, yet some studies also indicate that the causality runs in the opposite direction, i.e. that social movement activities increase the network size (Tindall et al., forthcoming). However, within research on social networks and social capital, studies on political outcomes such as party activities are pointed out as a largely ignored issue (cf. Erickson, 2003).

Hence, our understanding of the causal mechanism in the sorting model can be refined by drawing on recent research on social networks, social capital and participation. More precisely, we arrive at the refined hypothesis that having strong ties to a large network of high-status persons mediates the relationship between education and political participation.

Sweden as a test case

Compared to the US, Sweden is a hard case for testing the sorting model. Several factors in the Swedish context indicate that the hypothesized causal relationship should be less prominent in Sweden than in the US. First, the labour movement constitutes an influential alternative pathway to participation with no counterpart in the US. Verba et al. (2005: 108) point out that in democracies 'where there are strong labor unions or competitive labor or social democratic parties, the links between social class and political participation are weaker than they are in the United States'. In Sweden, the Social Democrats and the Labour Union dominated the political scene from the 1930s to the early 2000s. Second, the level of economic inequality is lower in Sweden, which indicates that the social distance between individuals with different socioeconomic status is smaller (cf. Erikson and Jonsson, 1996). When there is a great social distance between individuals with different social networks, the sorting model is more likely to be supported. Third, the Swedish educational system was designed with the objective of promoting social equality (Meghir and Palme, 2005), resulting in for example free higher education for all citizens. To conclude, if the sorting model is valid in a hard test case like Sweden, it is most likely valid in democracies with less egalitarian educational systems, such as the US, as well.

In a comparative perspective, Sweden has experienced high levels of political participation. For example, Sweden ranks 21 in the world league table of voter turnout 1945–2001 (mean 84.1 percent), while the US ranks 138 (47.7) (Lopez Pintor et al., 2002). More importantly for this research is that Sweden has also had a relatively high level of political party membership. At the time the data used in this article were collected, about 5.5 percent of the total Swedish electorate were members of a political party. The European mean level at the time was somewhat lower (5 percent) (Mair and van Biezen, 2001). However, while the levels of participation in political parties have declined during the past few decades, the mean level of education has increased. Since the mid-1980s, the mean level of education has increased from less than 10 years to more than 11 years. The change in mean levels of education is even more prominent among 26–36 year olds, where it has increased from approximately 11.5 to 13 years.

Data

The analysis employs the Swedish Society, Opinion and Media (SOM) survey from 2001, which draws on a representative sample of 6000 Swedish adults (15–85 years old residing in Sweden), and the response rate was 67.2 percent.[6] The survey, distributed as a postal questionnaire, includes a battery of items on the respondents' relations to persons with different occupations – usually referred to as the 'position generator' (Lin, 2001), which is intended to measure social network position.[7] Lin (1999b) claims that this technique has important advantages compared to other ways of measuring social networks. For example, it can be used with representative samples and measures both strong and weak social ties. The strong utility of the position generator has been confirmed in a large number of studies on social capital in a wide range of contexts (see the Lin, 1999a, b for overviews of the field). The position generator items construct the measure of social position for this study. To my knowledge, this is the only survey conducted in a Western democratic country that draws on a nationally representative sample and includes position generator items as well as measures of political party membership. Unfortunately, the lack of corresponding data from other countries makes comparative studies impossible.[8]

The respondents were asked the following question: 'Do one or more persons among your friends and acquaintances have the following occupations?' The list includes 20 different occupations, such as lawyer, member of national parliament, professor, journalist, doctor, etc. (see Appendix A for a complete list). These indicators were used to construct the latent variable for social network position in the analyses.

It could be argued that these items do not measure exactly what the sorting model states. According to the sorting model, education affects social network *centrality.* Drawing on these data we do not know whether the individuals have central positions in the networks, but only whether they have connections to persons with these occupations. The key here is how many connections individuals have to people in occupations that are likely to draw them toward participation. In other words, certain people know others who are more involved in political participation, and this draws them to participate.

An explanatory factor analysis was conducted to evaluate the dimensionality of the items on social network position. The RMSEA value was used as a criterion to judge the model fit. As it turned out, treating the social network position items as a one-factor model yields an acceptable model fit.[9] The measure of social network position has a bias towards high-status people, which is in accordance with the theoretical foundation of the sorting model. It also measures the size of the social network in a reliable way since it takes into account relationships with persons in 20 different occupations. NJS's empirical analyses had no reliable measure of network size, despite the fact that network size is heavily emphasized by social network research. Hence, these indicators have a wider scope than the NJS measure and are thus not as vulnerable for critique about reversed causality.

In the original data, these variables are coded so that 0 reflects no acquaintanceship, 1 represents weak acquaintanceship and 2 represents strong acquaintanceship.[10] Since the hypothesis to be tested states that strong ties mediate the relationship, the variables in the main models are recoded so that 1 reflects strong ties while 0 reflects weak or no acquaintanceship. However, as a robustness check the additional impact of weak ties is tested in a subsequent model and the coding with three values for each variable is then used.

As for education, a question on highest achieved education is used, and is recoded so that it corresponds to years of education.[11] The models also include controls for a number of factors that previous studies have shown influence participation: age, age squared, immigrant status, urban/rural residence, gender and marital status (see, e.g., Verba et al., 1995).

The dependent variable measures active political party membership, defined as having some kind of assignment in a party. This is a form of political participation that should be subject to sorting via social networks regardless of whether we depart from NJS's (1996) original version or Campbell's (2009) refined version of the sorting model. Unfortunately, items measuring other forms of political participation were not included in the survey. The dependent variable is a dichotomous variable coded 1 for members who have some kind of assignment in the party and 0 for non-members and non-active members.[12]

Results

Structural equation modelling (SEM) is used to test the indirect effect of education via social network position on active political party membership.[13] Compared to traditional path regression analyses, SEM is more powerful to use for path analysis. SEM facilitates simultaneous estimation of the different regression paths in the model via latent mediators and tests of the indirect effects (cf. Muthen and Muthen, 1998–2010).

The results section proceeds as follows. First, Model 1 estimates the effect of education on active political party membership without taking social network position into account. This is to show whether there is a significant relationship between education and political party membership to begin with, i.e. whether an effect that possibly could be mediated exists. Model 2 introduces the latent variable and the indirect path. Model 3 offers a robustness check for the argument about reversed causality; the model is estimated without the network connections (political representatives) that are most likely consequences of party membership. Model 4 corroborates the effect of strong and weak ties. In this model, weak ties are added to the model to gauge the additional contribution.

Model 5 is a final robustness check that estimates the combined effect of strong and weak ties without indicators for relationships with political representatives.

Model 1 – The effect of education on active party membership

Model 1 estimates the direct effect of education on the dependent variable under control for the covariates without taking social network position into account. Probit regression is used in all models since the dependent variable is dichotomous, and weighted least square estimation (WLSMV) is the estimator employed. The upper left part of Figure 1 presents the standardized estimates from Model 1 (unstandardized estimates and standard errors from the model can be found in Table 1).[14] The results reveal that education has a significant positive effect at the 95 percent significance level (all estimates reported are two-tailed). Besides urban/rural residence, this is the only independent variable that has a significant effect. This initial model confirms what previous studies have demonstrated: education appears to be the strongest predictor of active participation in political parties.

Model 2 – The indirect effect via strong network ties

The middle left part of Figure 1 presents the results from Model 2, which is a full model that estimates the indirect effect of education via strong social network ties on active political party membership. The graph only illustrates the focal relationship, while estimates for the controls can be found in Table 2. In this model, the control variables are estimated on both the dependent variable and the mediating variable in order to get as accurate estimates as possible for the main paths of interest. The reason for this is that we have reason to believe that the same controls that influence participation also affect social network position.

Most importantly, however, when adding social network position we find that there is no longer a significant direct effect of education on active political party membership. However, the effect of education on social network position is statistically significant. Likewise, the effect of social network position on active party membership is of considerable size and statistically significant.

This provides a first indication that the effect of education is mediated via social network position. Yet even though both paths are significant, it does not follow that the indirect effect is significant (Mallinckrodt et al., 2006). Thus, the next step is to compute and significance test the indirect effect. The standardized indirect effect, which is simply the product of the two path coefficients, is 0.117. This indicates that about 68 percent of the total effect of education (0.171) is mediated via social network position. The significance test of the indirect effect is performed using the bias-corrected percentile interval bootstrap test (2000 bootstrap sample draws) as proposed by Shrout and Bolger (2002). Simulation studies have shown that the bias-corrected bootstrap method performs better than other methods (such as the Sobel test) when it comes to statistical power (Mallinckrodt et al., 2006). More precisely, it has been found that when using the standard normal theory method for significant tests of indirect effects, the confidence intervals are 'too wide in the direction of the null hypothesis but too narrow in the direction of the alternative

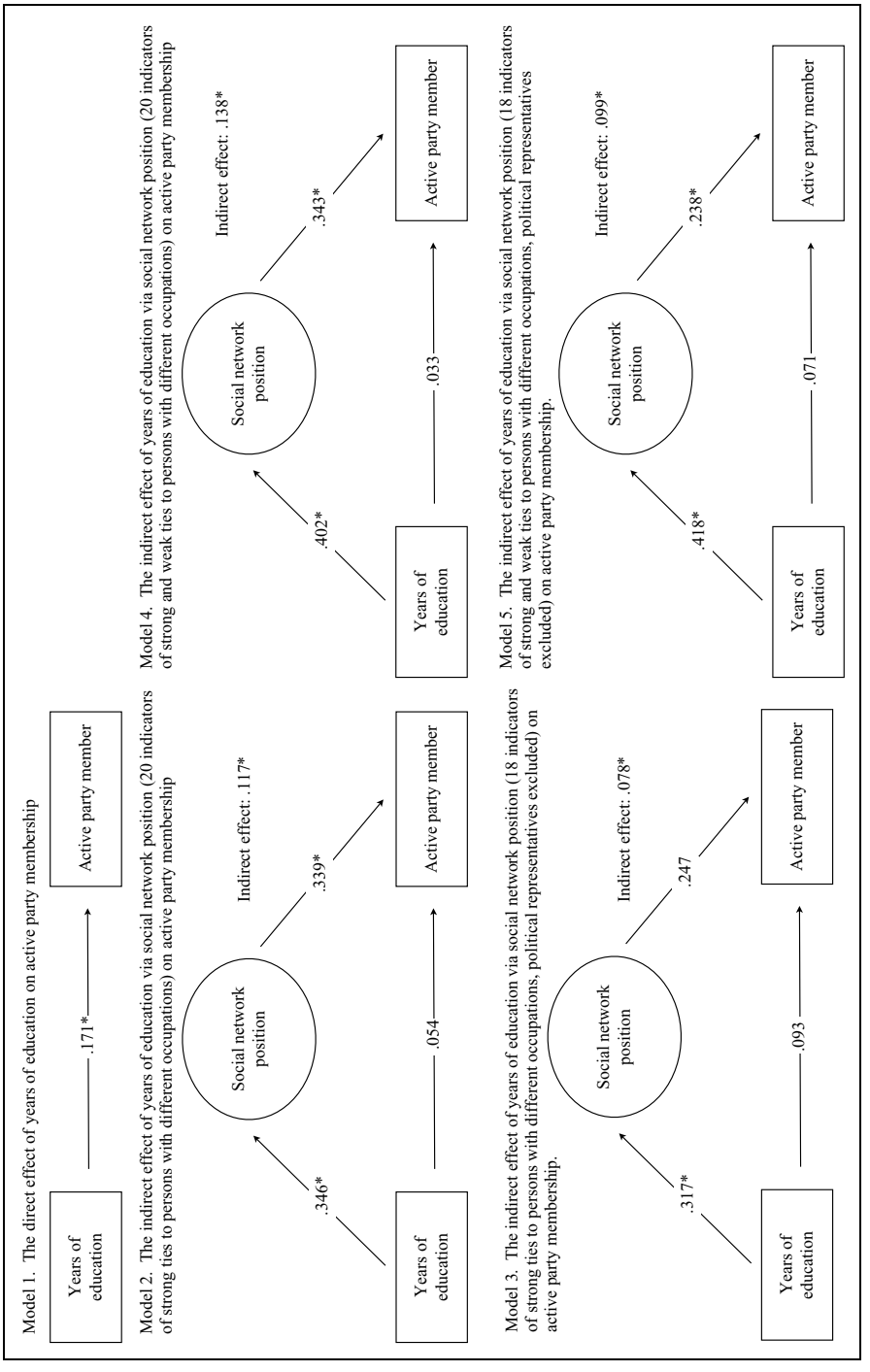

Figure 1. Illustrations of the direct and indirect effects from models 1-5

Table 1. Estimates from model 1

	Standardized estimate	Unstandardized estimate	Standard error of unstandardized estimate
Education → Active party member	0.171	0.068	0.029
Age → Active party member	0.507	0.036	0.030
Age^2 → Active party member	–0.211	0.000	0.000
Urban → Active party member	–0.187	–0.128	0.049
Immigrant → Active party member	–0.022	–0.088	0.275
Married → Active party member	0.003	0.007	0.171
Gender → Active party member	–0.072	–0.154	0.140
n	1254		
Weighted root mean square Residual	2.508		

hypothesis' (Shrout and Bolger, 2002: 426). Whether the indirect effect is significant or not is judged by whether the percentile confidence interval includes zero. In this case, the confidence intervals run from 0.05 (lower 2.5 percent) to 0.15 (upper 2.5 percent) and we can thus conclude that social network position indeed significantly mediates the effect of education on active political party membership.

Model 3 – Robustness check 1

As always when using cross-sectional survey data, one cannot completely rule out reversed causality or alternative causal paths. In this case it could be that the causal path between social network position and political party membership is, at least partially, reversed. Social network position could be an outcome of active party membership, i.e. as one becomes active in a political party the social network expands. To some extent this must be true; of course one gets to know new people when becoming active in a political party. Yet without longitudinal data it is not possible to estimate the relative strength of the two causal paths.[15]

Hence, drawing on this kind of cross-sectional data, we cannot tell which social connections are consequences of education and which are consequences of political party activities (or of any other factor). However, two of the occupations in the data are more likely to be consequences of political party activities than the others: acquaintanceship with a member of the local parliament and/or a member of the national parliament. To make sure that these occupations do not drive the entire relationship between active party membership and social network position, the previous model was estimated once more as a robustness check, but this time without the variables measuring relations to persons with these two occupations.

Model 3 is identical to Model 2 except for the fact that the latent variable is measured by 18 indicators (political representatives excluded) instead of 20. Standardized estimates from the main relationship in Model 3 are shown in the lower left part of Figure 1 (all estimates can be found in Table 3). As can be seen, they are substantially similar to the results from Model 2. The direct effect of education remains insignificant, although

Table 2. Estimates from model 2

	Standardized estimate	Unstandardized estimate	Standard error of unstandardized estimate
Social network position → Active party member	0.339	3.614	1.261
Education → Social network position	0.346	0.013	0.002
Age → Social network position	0.765	0.005	0.002
Age^2 → Social network position	–1.095	0.000	0.000
Urban → Social network position	0.022	0.001	0.002
Immigrant → Social network position	–0.028	–0.010	0.010
Married → Social network position	0.055	0.013	0.007
Gender → Social network position	–0.015	–0.003	0.006
Education → Active party member	0.054	0.022	0.028
Age → Active party member	0.248	0.017	0.042
Age^2 → Active party member	0.160	0.000	0.000
Urban → Active party member	–0.194	–0.133	0.051
Immigrant → Active party member	–0.013	–0.051	1.756
Married → Active party member	–0.016	–0.039	0.180
Gender → Active party member	–0.067	–0.143	0.142
Indirect effect: Education → Social network position → Active party member	0.117	0.047	0.013
Latent variable indicators (social network position)			
Police	0.254	1.000	0.000
Doctor	0.382	1.655	0.406
Officer	0.385	1.406	0.332
Banker	0.389	1.769	0.389
Company owner	0.345	1.546	0.371
Journalist	0.503	1.823	0.449
Union ombudsman	0.279	1.260	0.356
Headmaster	0.457	1.593	0.383
Lawyer	0.381	1.237	0.328
Member of local parliament	0.494	1.908	0.473
Member of national parliament	0.392	0.639	0.192
Professor	0.396	1.198	0.315
Farmer	0.343	1.643	0.381
Priest	0.469	1.599	0.412
Actor	0.293	0.709	0.251
Librarian	0.355	0.971	0.331
Communications officer	0.323	0.982	0.309
Social worker	0.317	1.193	0.390
Employee at the employment office	0.199	0.506	0.191
Employee at the social insurance office	0.290	0.860	0.247
n	1254		
Weighted root mean square residual	1.187		

Table 3. Estimates from model 3

	Standardized estimate	Unstandardized estimate	Standard error of unstandardized estimate
Social network position → Active party member	0.247	2.336	1.358
Education → Social network position	0.317	0.013	0.003
Age → Social network position	0.639	0.005	0.002
Age^2 → Social network position	–1.179	0.000	0.000
Urban → Social network position	0.042	0.003	0.002
Immigrant → Social network position	–0.024	–0.010	0.010
Married → Social network position	0.040	0.011	0.007
Gender → Social network position	–0.019	–0.004	0.005
Education → Active party member	0.093	0.037	0.030
Age → Active party member	0.349	0.025	0.042
Age^2 → Active party member	0.080	0.000	0.000
Urban → Active party member	–0.197	–0.135	0.051
Immigrant → Active party member	–0.017	–0.065	1.756
Married → Active party member	–0.007	–0.017	0.181
Gender → Active party member	–0.068	–0.145	0.143
Indirect effect: Education → Social network position → Active party member	0.078	0.031	0.017
Latent variable indicators (social network position)			
Police	0.283	1.000	0.000
Doctor	0.522	2.124	0.520
Officer	0.436	1.454	0.366
Banker	0.457	1.908	0.453
Company owner	0.393	1.597	0.409
Journalist	0.584	1.994	0.529
Union ombudsman	0.308	1.252	0.382
Headmaster	0.527	1.706	0.445
Lawyer	0.436	1.293	0.360
Professor	0.470	1.310	0.355
Farmer	0.378	1.641	0.423
Priest	0.522	1.647	0.468
Actor	0.350	0.769	0.274
Librarian	0.392	0.974	0.359
Communications officer	0.404	1.120	0.358
Social worker	0.408	1.405	0.451
Employee at the employment office	0.217	0.495	0.197
Employee at the social insurance office	0.311	0.829	0.254
n	1254		
Weighted root mean square residual	1.162		

the indirect effect is slightly reduced. The standardized estimate for the indirect effect is 0.078, which corresponds to about 45 percent of the total effect of education. The 95 percent confidence interval from the bootstrap test indicates significant mediation

(0.00, lower 2.5, to 0.16, upper 2.5). Hence, it should be noted that the amount of mediation via strong social ties is reduced from 68 to 45 percent when political representatives are excluded from the social network measure. Still, this robustness check shows that when excluding the social ties that most likely are not causes but consequences of political party activities, the results are substantially the same as in the original model: the direct effect of education is insignificant and the significant part of the effect is mediated via strong ties.

Model 4 – Robustness check 2

Having concluded that strong social network ties mediate most of the relationship between education and participation, we now move forward to evaluate the additional effect of weak ties. Do additional weak ties increase the amount of mediation? The impact of weak ties is tested by including all of the 20 indicators with variable values for weak ties (0 = no acquaintanceship, 1 = weak acquaintanceship, 2 = strong acquaintanceship). Model 4 is identical to Model 2 except for the coding of the social network items. The standardized estimates from the main relationship are presented in the upper right part of Figure 1 and all estimates from the model are presented in Table 4.

When including both weak and strong network ties, the standardized estimate of the indirect effect increases from 0.117 to 0.138 (the 95 percent confidence interval runs from 0.08 to 0.20). Comparing Models 2 and 4, the difference in mediation corresponds to an increase in mediation from 68 to 80 percent of the total effect of education. One important conclusion can be drawn from this result: strong social network ties account for the majority of the indirect effect via social network position. Adding weak ties increases the amount of mediation only marginally. When taking the combined effect of weak and strong ties into account, the direct effect of education accounts for only about 20 percent of the total effect of education and is insignificant.

Model 5 – Robustness check 3

Having concluded that the combined effect of weak and strong ties mediates almost the entire relationship, a last model is estimated as a robustness check in order to gauge the influence of political representatives on the latent variable in the previous model. Hence, in Model 5 we re-estimate the previous model with political representatives excluded from the model. The standardized coefficients are presented in the lower right part of Figure 1, while all estimates can be found in Table 5. Again we find that the level of mediation decreases when political representatives are excluded. The amount of mediation is about 58 percent when political representatives are excluded from the combined measure of weak and strong ties. Yet the indirect effect is significant (standardized estimate 0.099, the 95 percent confidence interval goes from 0.03 to 0.17) and the direct effect of education remains insignificant.

Table 6 summarizes the amount of the total effect that is mediated via social network position when different operationalizations of the latent variable are applied. We can conclude that strong ties account for the majority of the indirect effect. Moreover, weak social ties increase the amount of mediation only marginally. Yet, the important

Table 4. Estimates from model 4

	Standardized estimate	Unstandardized estimate	Standard error of unstandardized estimate
Social network position → Active party member	0.343	1.248	0.279
Education → Social network position	0.402	0.044	0.005
Age → Social network position	1.050	0.020	0.004
Age^2 → Social network position	–1.041	0.000	0.000
Urban → Social network position	0.003	0.001	0.005
Immigrant → Social network position	–0.060	–0.064	0.029
Married → Social network position	0.064	0.044	0.019
Gender → Social network position	–0.010	–0.006	0.016
Education → Active party member	0.033	0.013	0.027
Age → Active party member	0.147	0.010	0.042
Age^2 → Active party member	0.146	0.000	0.000
Urban → Active party member	–0.188	–0.128	0.050
Immigrant → Active party member	–0.002	–0.008	1.755
Married → Active party member	–0.019	–0.047	0.180
Gender → Active party member	–0.069	–0.147	0.142
Indirect effect: Education → Social network position → Active party member	0.138	0.055	0.012
Latent variable indicators (social network position)			
Police	0.380	1.000	0.000
Doctor	0.505	1.360	0.121
Officer	0.425	1.078	0.111
Banker	0.477	1.372	0.125
Company owner	0.389	0.921	0.097
Journalist	0.537	1.337	0.135
Union ombudsman	0.343	0.999	0.117
Headmaster	0.531	1.282	0.129
Lawyer	0.433	0.986	0.109
Member of local parliament	0.547	1.428	0.141
Member of national parliament	0.478	0.691	0.084
Professor	0.445	0.953	0.108
Farmer	0.405	1.161	0.119
Priest	0.524	1.268	0.125
Actor	0.422	0.787	0.102
Librarian	0.439	0.908	0.118
Communications officer	0.418	0.899	0.114
Social worker	0.415	1.075	0.125
Employee at the employment office	0.342	0.663	0.091
Employee at the social insurance office	0.401	0.872	0.106
n	1254		
Weighted root mean square residual	1.615		

Table 5. Estimates from model 5

	Standardized estimate	Unstandardized estimate	Standard error of unstandardized estimate
Social network position → Active party member	0.238	0.844	0.298
Education → Social network position	0.418	0.047	0.005
Age → Social network position	1.034	0.020	0.004
Age^2 → Social network position	–1.048	0.000	0.000
Urban → Social network position	0.033	0.006	0.006
Immigrant → Social network position	–0.057	–0.063	0.030
Married → Social network position	0.062	0.043	0.020
Gender → Social network position	–0.020	–0.012	0.016
Education → Active party member	0.071	0.029	0.027
Age → Active party member	0.261	0.018	0.042
Age^2 → Active party member	0.039	0.000	0.000
Urban → Active party member	–0.195	–0.133	0.051
Immigrant → Active party member	–0.009	–0.035	1.755
Married → Active party member	–0.012	–0.029	0.181
Gender → Active party member	–0.068	–0.144	0.142
Indirect effect: Education → Social network position → Active party member	0.099	0.040	0.014
Latent variable indicators (social network position)			
Police	0.389	1.000	0.000
Doctor	0.555	1.470	0.128
Officer	0.426	1.056	0.107
Banker	0.494	1.391	0.125
Company owner	0.385	0.892	0.094
Journalist	0.549	1.340	0.134
Union ombudsman	0.335	0.951	0.113
Headmaster	0.538	1.271	0.127
Lawyer	0.442	0.985	0.108
Professor	0.446	0.933	0.107
Farmer	0.397	1.111	0.115
Priest	0.522	1.234	0.124
Actor	0.421	0.768	0.100
Librarian	0.441	0.892	0.115
Communications officer	0.435	0.916	0.113
Social worker	0.425	1.078	0.124
Employee at the employment office	0.336	0.635	0.089
Employee at the social insurance office	0.396	0.842	0.102
n	1254		
Weighted root mean square residual	1.586		

conclusion is that all models show that the entire significant effect of education is mediated via social network position whichever operationalization is applied. The direct effect of education is insignificant in all models when the indirect effect is taken into account.[16]

Table 6. Summary of results

Specification of the latent variable for social network position	Percent of the total effect of education mediated via social network position
Strong social ties (20 occupations)	68
Strong social ties (18 occupations)	46
Strong social ties + weak social ties (20 occupations)	80
Strong social ties + weak social ties (18 occupations)	54

Conclusion

This article explicitly tests a central claim in the sorting model that has not previously been sufficiently tested: whether social network position mediates the relationship between education and active party membership. The results confirm this central hypothesis from the sorting model. While the study does not supply a full test of the sorting model, it focuses on the causal mechanism and thereby refines our understanding of how the sorting model functions by showing what kind of social networks are important. Drawing on research on social networks and the empirical analyses, we arrive at a refined conclusion on how sorting works: strong ties to a large social network of high-status persons mediate the relationship between education and active party membership. Since the results indicate that the causal path proposed by NJS is confirmed in the Swedish case, we can also expect that it is relevant in other countries, such as in the US where social inequality is higher.

This study moves beyond previous research by confirming the causal path proposed by NJS. It uses the best data available on social network position and analyses them with a more accurate technique than has previously been employed. However, all studies have their limitations and this is of course no exception. In this case, the problem is that this study is limited to one single country, Sweden, and one form of political participation, i.e. active political party membership. Further research is needed in order to evaluate whether the conclusions are generalizable to other forms of participation in other contexts. Moreover, as always in research drawing on cross-sectional data, there is a problem with possible reversed causality: not only education but also political activities are likely to affect social network position. Panel data are needed to establish the relative strength of these paths. Hence, this study brings some important insights to our knowledge of how the relationship between education and participation functions, yet the black box remains far from totally illuminated.

The results have important policy implications: providing more education will not *per se* get more people active in political parties. It is their networks that are of primary importance for the probability of getting active in political parties, not their skills and capabilities gained through education. Isolated individuals are not very likely to participate in political parties no matter how well educated they are. This conclusion stands in sharp contrast to the conventional view on the effects of education on political participation. For example, Lewis-Beck et al. (2008: 102) claim that ‘effective citizen participation depends on the operation of a nation’s educational system’. The results presented here indicate that this might be a false claim; the content of education has

negligible importance – what matters is the social position you gain by obtaining education. This is indeed disappointing news for social engineers hoping to increase participation by education reforms.

Given the positive effect of education on political participation reported in numerous studies, researchers have found it paradoxical that political party membership has declined in almost all Western countries despite the fact that the aggregate levels of education have increased. This article does not solve the paradox as such, since it is still an open question why party membership is declining. However, the study clarifies the relationship between education and active party membership and indicates that these two factors might not be as closely related as is often assumed. Education is not a direct cause of participation in political parties; social network position mediates the relationship.

Appendix A – The SOM study 2001

Design: Postal survey to a national representative random sample of 6000 persons. Principal investigators for the study were Sören Holmberg, Lennart Weibull and Lennart Nilsson. Principal investigators for items measuring social network position were Bo Rothstein and Henrik Oscarsson. Additional information available at http://www.snd.gu.se/sv/catalogue/study/431, study id: SND0797.

Social network position, original question wording: 'Among your friends and acquaintances, are there some belonging to the following occupations?

0 = 'No there is no one with that occupation in my acquaintanceship'
1 = 'Yes, remotely'
2 = 'Yes, closely'
3 = 'I have this occupation myself'

List of occupations:
Police
Doctor
Officer
Banker
Company owner
Journalist
Union ombudsman
Headmaster
Lawyer
Member of local parliament
Member of national parliament
Professor
Farmer
Priest
Actor
Librarian
Communications officer
Social worker

Employee at the employment office
Employee at the social insurance office

Education: Coded as years of education: 9 = compulsory only, 11 = two-year gymnasium or equivalent, 12 = three-year gymnasium, 14 = unfinished post-gymnasium studies, 16 = graduated from university.

Active political party member: Coded 1 for active member with some kind of commission, coded 0 for inactive members and non-members.

Age: Age in years.

Gender: 0 = women, 1 = men.

Marital status: 1 = married/living with partner, 0 = living alone.

Urban/rural: 1 = rural, 2 = small population centre, 3 = suburb to city, 4 = city, 5 = suburb to metropolitian area city, 6 = metropolitian.

Immigrant status: 0 = grew up outside Sweden, 1 = grew up in Sweden.

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Notes

1. In a recent study, Scarrow and Gezgor (2010) show that party members have higher education than the general population in most European countries. However, some studies have suggested that the high-educated are less likely to join political parties in some contexts; see, e.g., Togeby (1992) for a study of the Danish case.
2. Not all researchers who adhere to education as a proxy view embrace the sorting model. The pre-adult socialization model suggests that education is a proxy for pre-adult factors that affect both educational choice and political participation in adulthood (e.g. Jennings and Niemi, 1974; Kam and Palmer, 2008; Langton and Jennings, 1968; Sears, 1989). Education works as a proxy only for factors such as family socio-economic status, parents' level of political participation, parents' political orientations and the discussion climate at home (Achen, 2002; Alwin and Thornton, 1984; Andolina et al., 2003; Beck and Jennings, 1982, 1991; Jennings and Niemi, 1968; McIntosh et al., 2007; Westholm, 1999). Other researchers argue that pre-adult factors such as intelligence (Deary et al., 2008; Luskin, 1990), genetic factors (Alford et al., 2005; Fowler et al, 2008) or personality types affect political participation in adulthood (Mondak and Halperin, 2008; Mondak et al., 2010).

3. In total, six different occupations: member of congress, member of state legislature, member of local council, member of other local official board, someone who works for the local media or someone who works for the national media.
4. More exactly, 'difficult political activities' cover working on political campaigns, community work, serving on local government boards and contacting officials.
5. In contrast to NJS's focus on high status social network connections, an important strand of research claims that politically diverse social networks increase participation. Previous research shows mixed results regarding this issue, yet results from panel data suggest a cross-lagged effect (Quintelier et al., forthcoming).
6. Evaluations of the representativeness of the survey concluded that, on the whole, the composition of respondents and the Swedish population only differ marginally in terms of age, gender and occupation (Nilsson, 2002).
7. However, as most questions in the SOM survey, the questions used in this study were only distributed to half of the sample, since the questionnaire would have been too long if everybody was to answer all questions. Hence the subset consists of 3000 respondents with a response rate of 69.2.
8. For example, the US General Social Survey 2008 includes position generator items but lacks items on party membership. Likewise, the Canadian Election Studies also include position generator items, while questions about party membership were not put to the same respondents.
9. Hu and Bentler (1999) suggest that a value below 0.06 indicates an acceptable model fit. This model's fit is 0.058, which indicates that it loads cleanly on one dimension.
10. In the original dataset, a fourth category was also included: whether one has the occupation oneself. However, since we are interested in acquaintanceship and not respondents' own occupations, this category was excluded as a separate category from the analyses. We assume that respondents have strong acquaintanceship with at least someone with the same occupation as oneself, and the variables are coded accordingly.
11. Following NJS (1996), respondents under 25 years of age are excluded since education is not supposed to have had full effects on social network position earlier in life.
12. The question wording is: 'Please indicate below which associations you are a member of, and how active you are in them.' Then follows nine associations including political parties. Response options are 'Not member', 'Member but have not been to any meetings in the last 12 months', 'Member and have been to meetings in the last 12 months', 'Member who has some kind of assignment'. About 4 percent of the respondents were classified as active party members.
13. Mplus 6.2 was used to fit the models.
14. All standardized estimates reported are based on the variances of both the latent and observed variables.
15. However, it is not very likely that individuals join political parties without having any social network at all and form their entire network based on the relations acquired through activities in the party. Previous research supports the primacy of social context over political participation due to the fact that choices of social networks and context derive primarily from non-political factors (see, e.g., Huckfeldt and Sprague, 1993; Kotler-Berkowitz, 2005).
16. Late in the process of writing this article, the SOM Institute released a survey conducted in 2011 in which the survey questions used in this article were replicated. The SOM survey of 2011 included 17 of the occupations that were also included in the 2001 survey. I have

estimated models equivalent to those presented in the article on the 2011 data and the results are strikingly similar. The effect of education is significant when social network centrality is not taken into account. When the indirect effect is taken into account the direct effect becomes insignificant, while the indirect path is significant. Hence, the results presented from the 2001 survey are substantially reconfirmed in an independent study 10 years later.

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Author Biography

Mikael Persson is a PhD candidate in political science at the Department of Political Science, University of Gothenburg, Sweden. His main areas of interest are political behaviour and public opinion. His recent publications include 'An Empirical Test of the Relative Education Model in Sweden', *Political Behavior* (2011) and 'Does Type of Education Affect Political Participation?', *Scandinavian Political Studies* (2012). Other articles have appeared in *European Journal of Political Research, European Political Science Review*, *Legislative Studies Quarterly, Scandinavian Political Studies* and *Politics*.

Paper 3

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3

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Is the Effect of Education on Voter Turnout Absolute or Relative? A Multi-level Analysis of 37 Countries

MIKAEL PERSSON
University of Gothenburg, Sweden

ABSTRACT *While it is well established that education is positively correlated with voter turnout at the individual level, the increased educational levels in most western countries have not caused increased voter turnout at the aggregate level. The relative education model suggests one explanation: education is only a proxy for social status and has no direct causal effect. The individual-level effect of education is conditional on the level of education in the environment. Whereas previous research on the relationship between relative education and voter turnout has largely focused on the U.S. case, this article uses comparative survey data on voter turnout to test the relative education model. It combines data from the CSES and ESS covering about 275,000 individuals in 173 country-years in 37 countries. The analysis applies a definition of relative education operationalized as each individual's education rank position in relation to the level of education of those born in the same five-year cohort in the same country. The results show that relative education has a much larger effect on voter turnout than absolute education. Moreover, relative education has a stronger effect when aggregate turnout is low.*

The Problem

The relationship between education and voter turnout is a major puzzle in political behavior research. On the one hand it is well established that education is positively correlated with voter turnout at the individual level. On the other hand, the increased educational levels in most western countries have not caused an aggregate increase in voter turnout (e.g., Brody, 1978). How can this puzzle be solved?

The relative education model put forward by Nie, Junn and Stehlik-Barry (1996) (NJS hereafter) proposes an explanation: education has no direct causal effect on voter turnout; it works only as a proxy for social status. The individual-level effect of education is thus conditional on the level of education in the environment. This model stands in sharp contrast to the conventional view, sometimes referred to as the absolute education model, according to which education increases civic skills

Correspondence Address: Mr. Mikael Persson, University of Gothenburg, Department of Political Science, Box 711, SE 405 30 Gothenburg, Gothenburg, Sweden. Email: mikael.persson@pol.gu.se

and cognitive capabilities, factors that in turn increase political participation irrespective of the level of education in the environment.

According to the relative education model, or the sorting model as it is sometimes referred to, education functions as a sorting mechanism that influences individuals' social network positions, which in turn affect voter turnout. If this model is correct, educational inflation is the explanation for why aggregate increases in education have not resulted in higher voter turnout. As the level of education increases, more education for each individual is needed to retain the same social status position.

Previous research on the relationship between relative education and voter turnout has mostly employed single country study designs.[1] Following NJS, a number of studies have re-examined the relative education model (Campbell, 2009; Helliwell & Putnam, 2007; Persson, 2011; Tenn, 2005). NJS claim that their model is universally applicable, yet do not provide empirical evidence that their model resolves the paradox between education and voter turnout in other countries than the United States. The present article tests the wider generalizability of the model and goes beyond previous research by using comparative survey data. It combines data from the Comparative Study of Electoral Systems and European Social Survey covering about 275,000 individuals over 173 country-years in 37 countries, and applies a definition of relative education defined as each individual's education relative to the level of education of those born in the same five-year cohort in the same country at each point in time. The results show that relative education has a much larger effect on voter turnout than absolute education.

The article brings the following contributions to the field. First, it provides a test of the relative education model on voter turnout using country comparative data. Hence, it constitutes a crucial test for the generalizability of the model. Second, it presents a refined modeling strategy for relative education relying on country-comparative intra-birth cohort measures. Third, it brings an important substantive message to voter turnout scholars since it shows that the effect of education is relative rather than absolute. The conclusion suggests that most studies on voter turnout misinterpret the effect of education.

The outline of the article is as follows. The next section presents the theory and literature review. Thereafter, data and techniques of analyses are discussed. Results are subsequently presented, and the article concludes with a discussion on the implications of the results.

Theory

How can the puzzling relationship between education and voter turnout be explained? Whether education has any direct causal effects on any form of political participation is a highly contested issue (Berinsky & Lenz, 2011; Burden, 2009; Campbell, 2009; Dee, 2004; Henderson & Chatfield, 2011; Highton, 2009; Hillygus, 2005; Kam & Palmer, 2008, 2011; Mayer, 2011; Milligan et al., 2004; Sondheimer & Green, 2010; Nie & Hillygus, 2001; Niemi & Junn, 1998; Persson, 2012; Persson & Oscarsson, 2010; Tenn, 2005, 2007). According to the conventional view, education is a

direct *cause* of political participation. This view, sometimes referred to as the absolute education model, states that education positively affects voter turnout directly since it supposedly increases skills relevant to understanding politics, increases political interest, and increases the sense of civic duty and concern for the importance of elections (e.g., Lewis-Beck et al., 2008; Verba et al., 1995; Wolfinger & Rosenstone, 1980). The fact that education and turnout are strongly correlated in most countries has been considered to provide support for this view.

However, the conventional view has failed to explain anomalies such as the puzzling relationship between education and political participation. In his seminal work on voter turnout, Franklin estimates the average effect of higher education on turnout to be about 15% (2004: 17). At the same time, he acknowledges the puzzling fact that most countries used to have higher levels of turnout when they had fewer highly-educated citizens and expresses doubts about the true causal effect of education. However, few attempts have been made by researchers dealing with comparative voter turnout to solve this paradox.

To handle the anomaly of the relationship between education and political participation in the United States, researchers have started to question the conventional view and instead argue that there is no direct causal effect of education on voter turnout. Hence, education could be a *proxy* for other factors (cf. Kam & Palmer, 2008, Nie et al., 1996). NJS draw on a theory generally attributed to Hirsch (1976) according to which education is a positional good with a value that depends on how many others possess it. According to the relative education model, education has no value per se but rather serves as a proxy for social position. It is the social status position gained by education that increases turnout, not what the educational experience actually does with individuals. According to the relative education model, the same amount of education has a stronger effect in a low education context than in a high education context. In a low education context, less education at the individual level is needed in order to gain a high social status position. Hence the impact of education on political participation is relative rather than absolute.[2]

According to NJS, social network position is the causal mechanism connecting relative education and political participation. People who are close to the center of important social networks are more likely to vote than people in the periphery, irrespective of level of absolute education. Franklin's analysis of the costs and benefits of voting supports the idea that having a favorable social network position increases voter turnout: "People in social networks would also incur costs of nonvoting because other members of their group care whether they vote or not … So, the benefits of voting and the costs of nonvoting are higher for socially connected people" (Franklin, 2004: 51). NJS also contend that education is important since it strongly influences a person's social network status, but it is not important in itself. This causal mechanism is, however, not possible to test with the dataset used in the present article; see Persson (forthcoming) for an investigation of the causal mechanism. Instead we focus on the observable predictions of the model, i.e., that persons with a high rank in the education hierarchy are more likely to vote than persons with a low rank, irrespective of their absolute levels of education.

Previous research on relative education and voter turnout has disagreed on how to model relative education. NJS compare each respondent's education level with the mean national levels among individuals aged 25–50 when the respondent was 25, and find support for the relative education model across a wide range of political participation indicators. However, their measure of relative education has been criticized for not being able to separate the effect of the educational environment from the effect of age and year of birth (Tenn, 2005).

Helliwell and Putnam (2007) use geographically narrower measures in order to better capture the local context. They compare each respondent's education to "all other living adults, both older and younger" within the same geographical region (Helliwell & Putnam, 2007: 3). Hence, compared to NJS, Helliwell and Putnam's definition of relative education is wide regarding age but narrow as to geography. Helliwell and Putnam find no support for the relative education model, but it should be noted that their focus is only on social capital and that they do not analyze voter turnout. The problem with their definition is that it makes it impossible to control for state-level variations, such as registration requirements, opportunities for early voting, etc., since the relative education measure is correlated with geographic region.

To overcome these problems, Tenn (2005) uses intra-birth cohort measures of the educational environment in his study of the relative education model and voter turnout in the United States. Tenn employs a measure of relative education according to which each individual's education is compared with the mean level of education of those born in the same year in the entire United States. Tenn's results give strong support for the relative education model over the absolute education model. The problem with Tenn's measure, however, is that it does not consider regional differences in educational levels. Campbell (2009) argues that there are strong geographical variations in educational levels, and since social status is shaped in relation to one's closest surroundings, any measure that does not take the local geographical context into account will be biased. Thus, he defines the educational environment narrowly in terms of both age and place. Campbell (2009) finds support for the relative education model on competitive forms of political participation, including "electoral activities." In a similar vein, Persson (2011) uses mean educational levels in Swedish municipalities and finds support for the relative education model on voting and activities related to political parties.

The modeling strategy applied in this article follows Campbell (2009) in the sense that it uses a relative education measure that varies according to both age and place. As for age, relatively small cohorts are used (five years). As for place, the analysis exploits the cross-national variation in educational levels at different times. Following Campbell it would be preferable to define the educational environment at a geographical level that is as low as possible. But since comparable subnational measures of aggregate educational levels do not exist, and we do not know the exact geographical location of each respondent within countries, it is not possible to construct such a measure with the data used here.

One further issue that previous research on the relative education model has not dealt with is whether the effect of relative education is different in different contexts. According to the so-called "law of dispersion" formulated by Herbert Tingsten in his seminal work *Political Behavior* (1937; see also Rosenstone & Hansen, 1993) the level of equality in political participation is higher when the level of voter turnout is higher. Consequently, political inequality will increase as voter turnout decreases. If this theory holds it would suggest that differences in turnout between citizens with different levels of relative education should be larger when aggregate turnout is lower and that the differences should be smaller when aggregate turnout is higher. The analysis in this article will add to our previous knowledge on the relative education model by testing this idea.

Data

When modeling relative education, it is crucial to use data with enough variation in the educational levels in the environment. With data from only one election and one place, absolute and relative education would strongly correlate. To get necessary variation in the contextual levels of education, this study combines individual-level data from the Comparative Study of Electoral Systems (modules 1, 2 and 3) and the European Social Survey (rounds 1 to 5).[3] Following previous studies on the relative education model, only adult citizens are included in the analyses (25–70 years old), since earlier in life education has not yet had a chance to have its full effects. Including all respondents would result in involving a lot of respondents who have not yet finished their educations.

The 37 countries and the 173 country-years are presented in Table 1. In all these countries, the mean levels of education have increased during the last 60 years, while the development of voter turnout is slightly negative in most countries. Hence, in all countries we see a weak or negative relationship between education and voter turnout at the aggregate level.

By combining the ESS and CSES, we get variation in both time (1996–2010) and geographic context (37 countries). To ensure that the electoral behavior of the respondents is performed without governmental coercion, the analysis is restricted to countries that are considered free according to the Freedom House Index.[4,5]

As individual-level control variables, only a small set of controls for which items are equivalent in the ESS and CSES are used. More specifically, the following control variables – which previous research repeatedly has shown to be correlated with voting (e.g., Franklin, 2004; Lewis-Beck et al., 2008; Verba et al., 1995) – are included in the analysis: age, age squared, gender[6] and marital status.[7] This article does not explore the effects of these variables; the purpose of including them is only to control for their influence on the main independent variables.[8] In addition, controls for five contextual-level factors that are likely to affect voter turnout are included in the analysis: compulsory voting,[9] democratic system,[10] electoral system,[11] type of ballot[12] and registration requirements.[13]

Table 1. Countries and years included in the analysis

Country	Year of survey
Austria	2008
Belgium	1999, 2002, 2003 2004, 2006, 2008, 2010
Brazil	2002, 2006
Bulgaria	2001, 2006, 2008, 2010
Canada	1997, 2004
Croatia	2007, 2008
Cyprus	2006, 2008
Czech Republic	1996, 2002, 2004, 2006, 2008, 2010
Denmark	1998, 2001, 2002, 2004, 2006, 2008, 2010
Estonia	2004, 2006, 2008, 2010
Finland	2002, 2003, 2004, 2006, 2007, 2008, 2010
France	2002, 2004, 2006, 2007, 2008, 2010
Germany	1998, 2002, 2004, 2005, 2006, 2008, 2009, 2010
Greece	2002, 2004, 2008
Hungary	1998, 2002, 2004, 2006, 2008, 2010
Iceland	1999, 2003, 2004, 2007, 2009
Ireland	2002, 2004, 2006, 2007, 2008
Israel	1996, 2002, 2003, 2006, 2006, 2008, 2010
Italy	2002, 2004, 2006,
Japan	2004, 2007
Latvia	2006, 2008
Lithuania	1997
Mexico	1997, 2000, 2003, 2006, 2009
Netherlands	1998, 2002, 2004, 2006, 2008, 2010
New Zealand	1996, 2002, 2008
Norway	1997, 2001, 2002, 2004, 2005, 2006, 2008, 2010
Peru	2006
Poland	1997, 2001, 2002, 2004, 2006, 2007, 2008, 2010
Portugal	2002, 2004, 2005, 2006, 2008, 2009, 2010
Romania	1996, 2004 2006, 2008
Slovenia	1996, 2002, 2004, 2006, 2008, 2010
Spain	1996, 2000, 2002, 2004, 2006, 2008, 2010
Sweden	1998, 2002, 2004, 2006, 2008, 2010
Switzerland	1999, 2002, 2003, 2004, 2006, 2007, 2008, 2010
Taiwan	1996, 2001, 2004
United Kingdom	1997, 2002, 2004, 2005, 2006, 2008, 2010
United States	1996, 2004

Indeed there are additional factors that could affect turnout at both the individual level (such as sense of civic duty, political knowledge, etc.) and election context level (such as the competitiveness of the elections, the number of parties, the campaign activity, etc.) (cf. Franklin, 2004). As for the individual-level variables, equivalent measures are unfortunately not available in the merged dataset. Concerning the context-level variables, there should be no reason to expect additional factors to affect the estimates for the absolute and relative education coefficients, and hence the context-level controls are restricted to these five factors. In addition, the multilevel structure of the models includes random intercepts that will account for the fact that the turnout levels vary between countries and country-years. However, there is still of course a theoretic possibility that omitted variables at the country or country-year level bias the results. To handle this problem, a model with country-year fixed effects that remove all second- and third-level variance will be presented as a robustness check.

As for the dependent variable, it measures reported voting in the most recent election and the variable is coded as a dichotomy (0 = not voted, 1 = voted). The item construction follows different national standards in the CSES.[14] In the ESS, the item construction for the voter turnout question is "Some people don't vote nowadays for one reason or another. Did you vote in the last [country] national election in [month/year]?" In addition, while the CSES is carried out in conjunction with elections, the ESS might be carried out when considerable time has passed since the last election. Moreover, the turnout item measures reported voting, and it is well known that some respondents over-report voting (cf. Granberg & Holmberg, 1991; Persson & Solevid, forthcoming). However, recent research has shown that while different item constructions of the turnout question can produce different predicted levels of turnout, different item constructions correlate strongly. Most importantly, the same independent variables have been found to show significant effects using different items measuring intended, reported and validated voting (Achen & Blais, 2010). While these shortcomings regarding the measurement of the dependent variable should be acknowledged, they are irreparable at this stage. Despite these problems, the dataset constitutes the best available opportunity to study individual-level variation in turnout in a large number of countries.

As for the education variables, data on both individual-level educational attainment and aggregate levels of education is used. Regarding the individual-level variables, items on reported education in CSES and ESS are used. Most previous studies on relative education have used data on *years* of education at the individual level. However, since the length of specific educations differ between countries (and over time), it is not possible to construct a valid measure of years of education drawing on the combined CSES and ESS data. Thus, four categories of educational attainment that exist in most countries are used: (1) incomplete primary, (2) primary education completed, (3) secondary education completed, and (4) higher education completed. While it would have been preferable to further distinguish between different levels of education, country-specific differences in how education is organized and measured make it impossible.

Data on aggregate levels of education comes from the Barro & Lee Educational Attainment Dataset.[15] The data reports the share of citizens with different educational levels within five-year age cohorts in each country during every five-year period from 1950 to 2010. To construct the relative education measure, data on the share of citizens with (1) incomplete primary, (2) primary education completed, (3) secondary education completed, and (4) higher education completed within each cohort in each country at each point in time is used in combination with the individual education measures.

Modeling Strategy

Previous studies of effects of relative education have differed in their modeling strategies. NJS and Helliwell and Putnam (2007) estimate regression models that include both years of education at the individual level and aggregate mean levels of years of education. They interpret a negative effect of the aggregate measure as support for the relative education model. Campbell (2009) and Persson (2011) treat relative education as the interaction between years of education at the individual level and the level of education in the environment. This is a more feasible strategy since the theoretical model suggests an interactive relationship; as the level of education in the environment increases, the individual-level effect of education decreases. However, none of these strategies are feasible in the present article. The reason is the lack of a valid measure of years of education that is equivalent in the 37 countries. In order to model relative education using this data, we need a measure that relies on the four categories of education.

Thus, a modeling strategy similar to the one applied by Tenn (2005) is used. A measure of each respondent's rank position in the education hierarchy within each respondent's five-year cohort in each country at the time of each survey is calculated.[16] Each person is assigned a value on the relative education variable that corresponds to the midpoint of the percentile range of the person's educational level. To illustrate how the measure is calculated, take a person for whom 20% of the people in his/her cohort have no primary education, 30% have completed primary education and 20% have completed secondary education. If this person has completed secondary education, he/she is assigned the value 60 on the percentile-ranking variable. If the respondent has only completed primary education, he/she is assigned the value 35, etc. Hence, it reflects the share of people in the same birth cohort in the same country with higher, lower and similar levels of education. This measure is of course not perfect since we only know the distributions of the four main educational categories. However, it is reasonable to assume that it roughly proxies each person's relative education position. And even a rough measure allows us to evaluate whether what matters for turnout is having achieved a specific level of education or relative position in the education hierarchy.

An important advantage of this measure is that it is not perfectly correlated with birth year or age (as is NJS's relative education measure) or geographical area (as is Helliwell and Putnam's measure). We can thus control for both age and factors

at the country level without having to worry about multicollinearity. This modeling strategy produces more valid estimates than the strategies employed by NJS as well as Helliwell and Putnam since they cannot separate their relative education measures from confounding factors related to age and place. Our measure comes closer to capturing the true effect of relative education separated from these confounding factors.

The fact that individuals are clustered within different countries and country-years with different levels of voter turnout violates one of the general assumptions of regression analysis – that the residuals are uncorrelated with each other. Employing a modeling strategy that does not take the clustered structure of the data into account would likely produce inaccurate standard errors (e.g., Gelman & Hill, 2007; Goldstein, 1995). Thus, a multi-level regression model is employed in which the nested three-level structure – individuals (i), within country-years (j), within countries (k) – is explicitly modeled. Logistic multi-level regression is used as the estimator since the dependent variable is dichotomous.[17]

Results

We begin by estimating a random intercept-only model to focus on the variation at the country and country-year level (Model 1 in Table 2). The variances at both these levels are statistically significant, and multi-level modeling is thus needed to take the nested structure of the data into account. Model 2 estimates the effects of the absolute education measures together with the control variables. As regards the controls, they all run in the expected directions. Age has a positive effect, yet the quadratic effect suggests that the probability of voting decreases in old age. Men vote more than women, as do married compared to non-married (albeit this effect does not reach statistical significance). As for the contextual-level control variables, only the difference between compulsory voting with sanctions and non-compulsory voting reaches statistical significance. However, the primary purpose of this model is to see whether it shows the expected significant relationship between absolute education and voter turnout. The results clearly show that education has significant effects when modeled using the conventional absolute education approach.[18]

Having established that there is a significant relationship between education and voter turnout when education is modeled in the conventional absolute way, we now move forward to analyze the relative impact of education. Model 3 in Table 2 adds nine dummy variables indicating each respondent's relative education rank position (with the lowest decile rank position as reference category), in addition to the variables included in Model 2. Including both the relative and absolute education variables is a hard test for the relative education model. If the absolute education measures show strong significant estimates while the relative education variables are insignificant, we can conclude that what matters in relation to turnout is which specific level of education a person has and not the position in the education hierarchy. Yet if the relative education measures are significant while the absolute education measures are not, we can conclude that what matters is not the level of education as such, but rather the position in the education hierarchy.

Table 2. Effects of absolute and relative education on voter turnout (multi-level logistic models)

	(1)	(2)	(3)
Individual level controls			
Age		0.084*** (0.003)	0.089*** (0.003)
Age^2		-0.001*** (0.000)	-0.001*** (0.000)
Civil status		0.018 (0.011)	0.018 (0.011)
Gender		0.027** (0.011)	0.027** (0.011)
Contextual level controls			
Democratic system		0.085 (0.254)	0.161 (0.253)
Voting system		-0.100 (0.250)	-0.099 (0.249)
Compulsory voting, weak sanctions/without sanctions		-0.620 (0.558)	-0.674 (0.555)
Non-compulsory voting		-1.485*** (0.486)	-1.482*** (0.484)
Registration requirements		-0.291 (.693)	-0.079 (0.689)
Type of ballot		0.163 (0.279)	0.152 (0.278)
Absolute education (reference category =not completed primary)			
Primary education completed		0.163*** (0.022)	-0.154*** (0.037)
Secondary education completed		0.612*** (0.022)	-0.033 (0.060)
Higher education completed		1.177*** (0.024)	0.237*** (0.083)
Relative education rank position (reference category = 0–10)			
Rank position 11–20			0.193*** (0.033)
Rank position 21–30			0.309*** (0.038)
Rank position 31–40			0.370*** (0.043)
Rank position 41–50			0.515*** (0.047)
Rank position 51–60			0.568*** (0.053)
Rank position 61–70			0.650*** (0.058)
Rank position 71–80			0.701*** (0.066)

Rank position 81–90			0.754*** (0.069)
Rank position 91–100			0.959*** (0.079)
Constant	1.843*** (0.109)	-0.133 (0.549)	-0.191 (0.546)
Standard deviation of intercept at country level	0.651*** (0.084)	0.554*** (0.076)	0.571*** (0.077)
Standard deviation of intercept at country-year level	0.379*** (0.024)	0.439*** (0.028)	0.433*** (0.027)
Number of countries	37	37	37
Number of country-years	173	173	173
Number of individuals	275439	275439	275439
Log likelihood	-118465.	-113802.9	-113721.8

Note: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The results show that when taking the relative education measures into account, the coefficients for the absolute education variables change considerably. The significant coefficient for secondary education turns insignificant, and the size of the coefficient for higher education is considerably reduced. In contrast, the relative education measures have strong and significant effects. Since substantive interpretations are hard to draw from logistic coefficients, Table 3 presents the marginal effects of absolute and relative education derived from the fixed part of Models 2 and 3 respectively (while all other variables are held at their mean values). The marginal effects are also presented graphically in Figure 1. The estimates show that absolute education has limited importance in relation to voter turnout. Completed higher education has a modest marginal effect of 0.027. The effect of being among the 10% with highest education has an effect about three times as large (0.092). The effect of relative education is increasingly higher within every decile. To conclude, when modeling education as a relative position defined as the rank within a cohort within a country, and taking variation in both time and between countries into account, relative education has far greater explanatory value than absolute education. What matters in relation to turnout does not seem to be the specific level of education, but rather the relative rank position in the education hierarchy. Moreover, the decreasing log likelihood values across the models in Table 2 shows that the goodness of fit increases when including the relative education variables.

Table 3. Marginal effects of absolute and relative education on voter turnout

	Estimates derived from Model 2	Estimates derived from Model 3
Absolute education		
Primary education completed	0.019*** (0.003)	-0.019*** (0.005)
Secondary education completed	0.070*** (0.006)	-0.004 (0.008)
Higher education completed	0.112*** (0.009)	0.027*** (0.011)
Relative education rank position		
Rank position 11–20		0.022*** (0.005)
Rank position 21–30		0.034*** (0.007)
Rank position 31–40		0.040*** (0.009)
Rank position 41–50		0.053*** (0.004)
Rank position 51–60		0.060*** (0.005)
Rank position 61–70		0.065*** (0.005)
Rank position 71–80		0.067*** (0.006)
Rank position 81–90		0.073*** (0.007)
Rank position 91–100		0.092*** (0.007)

Note: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

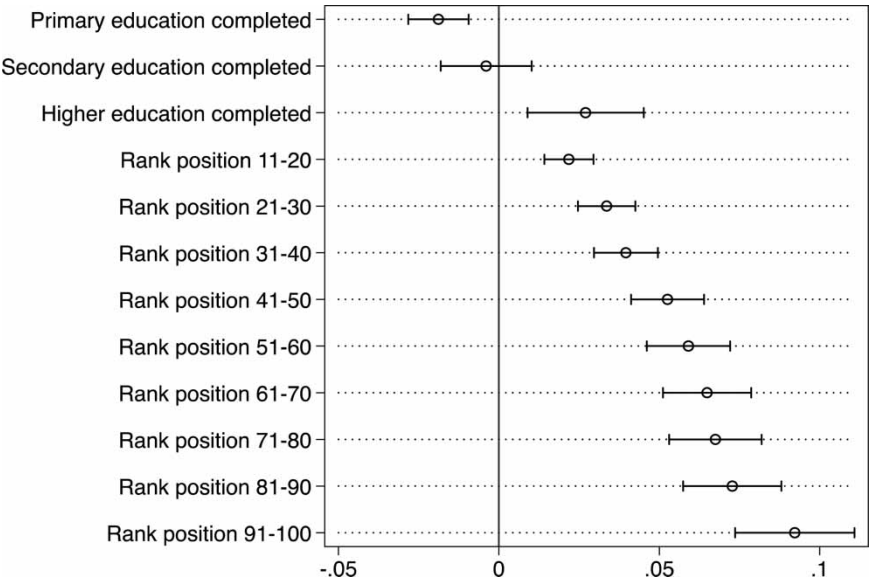

Figure 1. Marginal effects of absolute and relative education on voter turnout.

While the results presented in Tables 2 and 3 indicate support for the relative education model over the absolute education model, there are several reasons to be cautious before drawing decisive conclusions. This is especially the case when using this kind of data, compiled from different surveys in different countries and years. The results from the data presented in Table 3 are not weighted with respect to any potential form of bias. This section continues by testing for four potential forms of bias and presents robustness checks that show that the general pattern of the results hold after correction for potential biases. The four potential forms of bias are (a) bias due to sampling error within each survey, (b) bias as a consequence of the overrepresentation of respondents from some countries, (c) bias as a result of overrepresentation of respondents from some country-years, and (d) bias as a result of omitted variables at the country-year level.

To correct for the first three potential biases, three forms of weights will be applied in three separate models. We begin with internal sampling error within each survey. In some of the surveys, particular populations within countries are oversampled and the distribution of socio-demographic characteristics might not closely resemble the characteristics of the populations. Weights are provided with some, but far from all, of the surveys to correct for such bias.[19] Model 4 in Table 4 presents estimates from a model with the survey weights applied.[20] When these weights are applied, the coefficients for the absolute education variables all turn insignificant. However, the relative education measures remain statistically significant.[21]

Table 4. Robustness checks

	(4) Survey weights model	(5) Country level weights model	(6) Country-year level weights model	(7) Country-year fixed effects model
Individual level controls				
Age	0.092*** (0.007)	0.089*** (0.002)	0.087*** (0.002)	0.089*** (0.005)
Age^2	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)
Civil status	-0.074 (0.060)	0.003 (0.007)	0.015*** (0.006)	0.022 (0.043)
Gender	0.052** (0.023)	0.012* (0.007)	0.015*** (0.005)	0.027 (0.019)
Contextual level controls				
Democratic system	0.564*** (0.130)	-0.187* (0.101)	0.413*** (0.062)	-
Voting system	-0.125 (0.132)	-0.314*** (0.105)	0.161** (0.066)	-
Compulsory voting, weak sanctions/without sanctions	-0.671** (0.317)	-0.854*** (0.179)	-0.121 (0.154)	-
Non-compulsory voting	-1.490*** (0.230)	-1.577*** (0.168)	-1.099*** (0.126)	-
Registration requirements	-0.083 (0.289)	-0.565** (0.265)	0.406* (0.210)	-
Type of ballot	0.232* (0.118)	0.136 (0.137)	0.081 (0.069)	-
Absolute education (reference category =no completed primary)				
Primary education completed	-0.103 (0.159)	-0.239*** (0.022)	-0.158*** (0.018)	-0.154** (0.065)
Secondary education completed	-0.037 (0.257)	-0.405*** (0.035)	-0.051* (0.030)	-0.025 (0.102)
Higher education completed	0.399 (0.332)	-0.391*** (0.050)	0.235*** (0.041)	0.251* (0.144)
Relative education rank position (reference category = 0–10)				
Rank position 11–20	0.398*** (0.132)	0.278*** (0.021)	0.185*** (0.017)	0.192*** (0.058)
Rank position 21–30	0.489*** (0.173)	0.481*** (0.023)	0.319*** (0.019)	0.307*** (0.078)
Rank position 31–40	0.440** (0.194)	0.547*** (0.026)	0.363*** (0.021)	0.368*** (0.085)
Rank position 41–50	0.434** (0.188)	0.694*** (0.029)	0.496*** (0.024)	0.513*** (0.080)
Rank position 51–60	0.598*** (0.220)	0.768*** (0.031)	0.557*** (0.026)	0.564*** (0.090)
Rank position 61–70	0.710*** (0.260)	0.903*** (0.034)	0.632*** (0.029)	0.644*** (0.098)

Rank position 71–80	0.865*** (0.290)	1.085*** (0.038)	0.699*** (0.033)	0.693*** (0.109)
Rank position 81–90	0.637** (0.287)	1.219*** (0.041)	0.732*** (0.034)	0.745*** (0.120)
Rank position 91–100	0.856*** (0.321)	1.453*** (0.048)	0.943*** (0.039)	0.947*** (0.139)
Constant	-0.633* (0.362)	0.568*** (0.186)	-0.905*** (0.155)	0.198 (0.134)
Standard deviation of intercept at country level	-	0.534*** (0.029)	-	-
Standard deviation of intercept at country-year level	-	-	0.708*** (0.018)	-
Country-year fixed effects	NO	NO	NO	YES
Number of countries	37	37	37	37
Number of country-years	173	173	173	173
Number of individuals	275439	275439	275439	275439
Log likelihood	-124479.1	-286130.0	-456755.0	-113294.03

Note: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The second potential form of bias stems from the fact that the 37 countries included in the analysis contribute with unequal numbers of respondents to the dataset. This is troublesome if respondents are overrepresented from countries where absolute education has particularly weak or strong associations with voting. The results from Model 5 presented in Table 4 show estimates from a model in which each country is weighted so that it contributes equally to the distribution of respondents.[22] We see that this weighting procedure slightly affects the results. However, and most importantly, the estimates for absolute education show small negative effects while relative education has strong and positive significant effects.

The third potential source of bias is the fact that some countries contribute with a large number of surveys while others only contribute with one or two. Hence, Model 6 of Table 4 is estimated with weights that correct the sample so that each country-year has an equal contribution of respondents to the dataset. Again we find that the general pattern holds when correcting for this potential bias. The absolute education measures show small effects, while the relative education measures show stronger and significant effects. Hence, while it is evident that these three forms of bias affect the estimates to different degrees, they do not alter the general conclusion: the relative education model gains more support than the conventional absolute education model.

As a fourth robustness check, Model 7 includes country-year fixed effects to make sure that omitted variables at the country or country-year level do not bias the results. Including the country-year fixed effects variables increases the model fit, which indicates that a number of factors at the country and country-year level that are omitted from the previous models affect voter turnout.[23] However, they have a negligible effect on the absolute and relative education coefficients. The size and significance of the coefficients for the main variables of interest are essentially the same as in Model 3.

At this point, one final issue remains to be analyzed and that is whether the effect of relative education is different in different contexts. In order to test this a model including a random coefficient for relative education was estimated. Since including the 10 relative education dummy variables are impractical for computational reasons, I use the original relative education rank variable (indicating the relative rank position on a scale from 0 to 10). Hence, Model 8 in Table 5 is a two-level model testing whether relative education has different effects in the 37 different countries by including the random coefficient for relative education (for computational reasons the country-year level is omitted in this model). The results show that the random slope is significant and the effect of relative education indeed varies between the countries (and the log likelihood value is significantly lower in this model compared to a model without the random coefficient for relative education).

Can the varying effect of relative education be explained by differences in the aggregate turnout rates? In order to test this a variable measuring the aggregate level turnout at each country year was included in the next model and interacted with the relative education variable.[24] Results from Model 9 show that this interaction is indeed significant. To better interpret the interaction effect Figure 2 illustrates the

Table 5. Testing random effects of relative education

	(8) Random coefficient model	(9) Cross level interaction model
Individual level controls		
Age	0.087*** (0.003)	0.086*** (0.003)
Age^2	-0.001*** (0.000)	-0.001*** (0.000)
Civil status	-0.085*** (0.010)	-0.026** (0.010)
Gender	0.035*** (0.011)	0.029*** (0.011)
Contextual level controls		
Democratic system	0.200 (0.300)	-0.138 (0.123)
Voting system	-0.061 (0.296)	0.134 (0.119)
Compulsory voting, weak sanctions/ without sanctions	-0.419 (0.645)	0.000 (0.277)
Non-compulsory voting	-1.611*** (0.566)	-0.454* (0.244)
Registration requirements	-0.453 (0.799)	-0.436 (0.321)
Type of ballot	0.193 (0.335)	0.052 (0.132)
Education variables		
Primary education completed	0.036 (0.035)	0.080** (0.035)
Secondary education completed	-0.217*** (0.061)	0.079 (0.061)
Higher education completed	-0.092 (0.085)	0.348*** (0.085)
Relative education rank position (0-10)	0.137*** (0.014)	0.263*** (0.033)
Aggregate turnout		7.832*** (0.225)
Relative education rank position X Aggregate turnout		-0.213*** (0.037)
Standard deviation of random slope: relative education	0.069*** (0.009)	0.057*** (0.007)
Standard deviation of intercept at country level	0.731*** (0.087)	0.277*** (0.038)
Number of countries	37	37
Number of country-years	173	173
Number of individuals	275439	275439
Log likelihood	-113565.02	-113565.02

Note: Standard errors in parentheses, * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

predicted level of voter turnout for individuals with low and high relative education at different levels of aggregate turnout. As could be theoretically expected, the difference between individuals with high and low relative education is largest when turnout is low and the difference is smaller when turnout is high (albeit the difference remains statistically significant also at the highest levels of aggregate turnout).[25] Hence, we can conclude that relative education matters more in low turnout contexts than in high turnout contexts. It should also be noted that the standard deviation of the

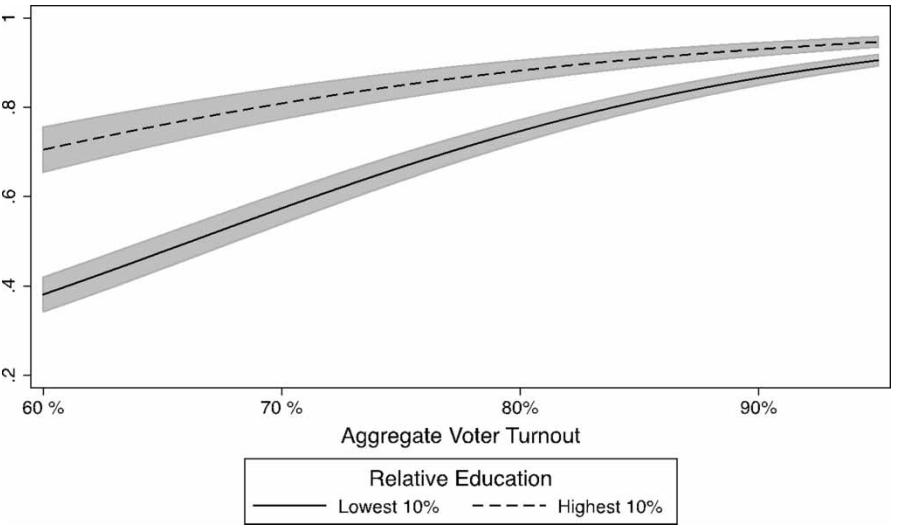

Figure 2. Predicted probability of voting for individuals with lowest and highest relative education across different levels of aggregate voter turnout (estimates derived from Model 9).

random slope for relative education is reduced when relative education is interacted with aggregate turnout. This implies that differences in aggregate turnout account for at least some of the difference in the effect of relative education between different countries.

Conclusion

This article deals with the paradoxical relationship between education and voter turnout. While the relationship is well established at the individual level, it does not seem to hold at the aggregate level. The article is the first to use intra-birth cohort measures of relative education on cross-country survey data to test the relative education model as a possible solution to the paradox. The results confirm that what matters is not so much people's absolute level of education as their rank position in the education hierarchy. The relative education model solves the paradox concerning the relationship between education and turnout; if there is little direct causal effect of education on turnout, we have no reason to expect that increased levels of education at the aggregate level should result in higher levels of voter turnout. One important conclusion from this study is that one must be careful when drawing inferences about causal relationships from cross-sectional data. Education has often been considered to be a direct cause of voter turnout, yet the results presented here suggest that the direct effect is quite marginal.

While this study does not include any data on the alleged causal mechanism, a few words ought to be said about it. According to the relative education model, social

network centrality connects education with political participation. By obtaining higher education, individuals get in contact with other highly-educated people and form social networks. Access to high-status social networks encourages political participation, mainly by increasing the likelihood of getting recruited. In other words, highly-educated people are more likely to be tied to networks that consist of active political participants, and they are therefore more likely to be recruited to participate themselves. Relative education determines who are the people with these important network ties.

If we regard the estimates for relative education as the effect of education via social status and the estimates for absolute education as the "direct" causal effect of education (via the mechanisms civic skills, political interest, etc.), we should reach the conclusion that the relative effect via social status is much stronger than the direct effect. The education effect on voter turnout is not a phenomenon operating solely at the individual level; rather the effect of education depends on how many others have which levels of education. This conclusion adds further evidence to a larger literature suggesting that education has no direct causal effect (e.g., Berinsky & Lenz, 2011; Kam & Palmer, 2007). Policy implications differ greatly depending on whether the absolute or relative education model is correct. While many scholars have attributed a central role to the education system in determining a country's level of voter turnout, this role is likely overestimated if the relative education model is correct.

While the results clearly favor the relative education model, this article is not likely to be the last word in the debate. Instead, we should anticipate further studies drawing on cross-country data. In the future, there will hopefully be better data that covers larger time spans and more countries. A promising way to accomplish this would be to harmonize the many existing election studies around the globe. Clearly, more robust results could also be obtained if advances were made in the way education is measured in different countries, providing more reliable and equivalent measures of years of education. Yet until further evidence is presented, the most reasonable conclusion is that education is first and foremost a proxy for social status and the social network that surrounds higher status individuals, not a variable that has direct absolute effects on voter turnout.

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Notes

1. However, a previous comparative analysis using the ESS was presented in a working paper by Campbell (2006). Campbell presents support for relative education model by modeling the interaction between the individual level years of education with the aggregate level of education in each country.
2. Yet other researchers argue that education is a proxy for pre-adult factors that affect both educational choice and political participation in adulthood, most importantly early political socialization in the

home environment (e.g., Achen, 2002; Alwin & Thornton, 1984; Andolina et al., 2003; Beck & Jennings, 1982, 1991; Jennings & Niemi, 1968, 1974; Kam & Palmer, 2008; Langton & Jennings, 1968; McIntosh et al., 2007; Westholm, 1999). Yet others claim that education is a proxy for intelligence (Deary et al., 2008; Luskin, 1990), genetic factors (Alford et al., 2005; Fowler et al., 2008) or personality types (Mondak & Halperin, 2008; Mondak et al., 2010).

3. Information about the surveys and data is available at http://www.europeansocialsurvey.org/ and http://www.cses.org/.
4. Information about the Freedom House Index is available at http://www.freedomhouse.org/.
5. Moreover, Australia and Thailand were excluded since in these two countries 98 and 99% of all respondents in the surveys report that they voted and there is hence no meaningful variation in the dependent variable.
6. The variable is coded 0 for females and 1 for males.
7. The variable is coded 1 for those who are married and 0 for those who are single/divorced/widowed.
8. The most important individual level control variable missing from the analysis is household income. There are several reasons why this item is not included. First, it is measured in different ways in the different countries. Second, the variable includes a large number of non-responses (54,000), so including this variable would substantially decrease the number of respondents included in the analysis. As a robustness check, all models have been re-estimated with income as control variable. In these models, absolute education has an even weaker effect and relative education has a stronger effect than in the models presented in the article. Of course it would be preferable if more controls could be included. However, for variables such as occupation, urban/rural residence and political interest, no equivalent variables are available in all datasets.

   However, two additional individual level variables that might be related to education are included in a majority of the surveys: church attendance and party identification. Yet including these variables reduces the number of country-years by 14, from 173 to 159. All models have been re-estimated with controls for church attendance and party identification. The size and significance of the absolute and relative education coefficients are nearly identical.
9. The variables indicate whether each country has compulsory voting with strong sanctions, with weak/no sanctions, or no compulsory voting. The source of the coding is http://www.idea.int/vt/compulsory_voting.cfm. Compulsory voting with strong sanctions is the reference category in the models.
10. The variable indicates whether the country has parliamentarism or presidentialism. The source of the coding is http://www.bu.edu/sthacker/data.htm. The variable is coded 0 for presidentialism/semi-presidentialism and 1 for parliamentarism.
11. The variable indicates whether the parliamentary system is proportional or majoritarian. The source of coding is http://www.bu.edu/sthacker/data.htm. The variable is coded 0 for majoritarian voting and 1 for proportional voting.
12. The variable indicates whether the type of ballot is open or closed. The source of the coding is Gallego (2010). The variable is coded 0 for open ballot and 1 for closed ballot.
13. The variable indicates whether registration requirements are absent (coded 0), compulsory (coded 0.5), or voluntary (coded 1). The source of the coding is Gallego (2010).
14. Additional information about how the turnout variable is constructed in different countries can be found in the codebooks at http://www.cses.org.
15. Data and documentation are available at http://www.barrolee.com/.
16. The Barro and Lee data only reports levels of education in each cohort in each country every five years. Hence, when calculating the relative education measure, the Barro and Lee data from 1995 is used for the surveys from 1996 to 1999, the data from 2000 is used for the surveys from 2000 to 2004, the data from 2005 is used for the surveys from 2005 to 2009, and the data from 2010 is used for the surveys from 2010.
17. The results are produced by the STATA command XTMELOGIT.
18. However, additional analyses show that there is substantial variation in how much education is related to voter turnout in the 37 countries. In most of the countries, there is a significant difference between the low and high educated. Analyses from separate logit models in each country show that in the

United States, Norway, the Netherlands, Poland and Switzerland, the differences in turnout are very large between the low and high educated, while in countries such as Belgium, Spain and Sweden the differences are much smaller (albeit significant). Yet the high educated do not vote to a higher degree than the low educated in all countries. As a matter of fact, there are no significant differences in the predicted probability of voting between low and high educated individuals in Bulgaria, Croatia, Cyprus, Greece, Iceland, Italy and Romania. Future research is encouraged to explore why education does not seem to have any significant relationship with turnout in these countries.

19. See the ESS and CSES web pages for more information on the weights supplied. For CSES, a combination of the "Polity weight: Sample" and "Polity weight: demographic" is used, and for ESS the "Design weights" is used.
20. Due to the complexity of applying survey weights in multi-level models (see, e.g., http://www.stata.com/bookstore/stata12/pdf/xt_xtmixed.pdf), the weights are applied for illustrative purposes in an ordinary logit model with clustered standard errors at the country level.
21. Marginal effects of the education variables are not shown for the robustness checks due to space constraints. However, the marginal effects tell a similar story as the logit coefficients in Table 4: the absolute education variables show weak effects while the relative education variables show stronger and significant effects.
22. Models 5 and 6 are estimated using the XTLOGIT command in STATA12. The models are two-level models. Model 6 is takes into account the country level and its corresponding weights and Model 7 takes into account the country-year level and its corresponding weights.
23. The estimator is logistic regression with clustered standard errors at the country-year level.
24. Aggregate voter turnout was calculated as the share of voters at each country-year in the dataset and might thus deviate from the official levels of turnout in the elections.
25. Note that two countries with virtually universal reported turnout were omitted from the data.

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Paper 4

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4

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ORIGINAL PAPER

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

Mikael Persson

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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M. Persson (✉)
Department of Political Science, University of Gothenburg, Box 711, 405 30 Göteborg, Sweden
e-mail: mikael.persson@pol.gu.se
URL: http://www.pol.gu.se/staff/mikaelpersson

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

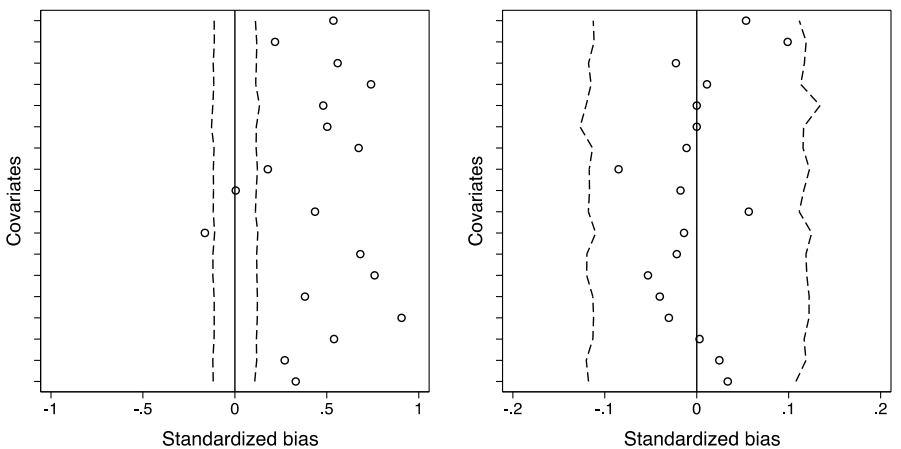

Fig. 1 Standardized bias in the unmatched sample (*left*) and the matched sample (*right*)

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

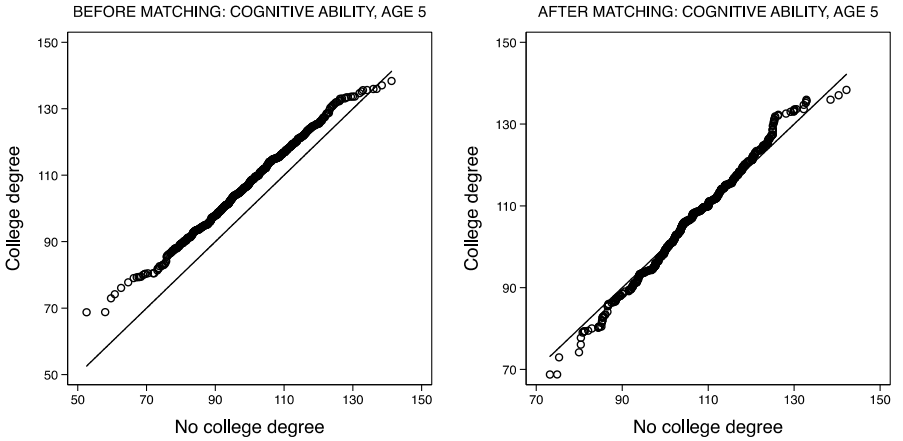

Fig. 2 Cognitive ability at age five before and after matching

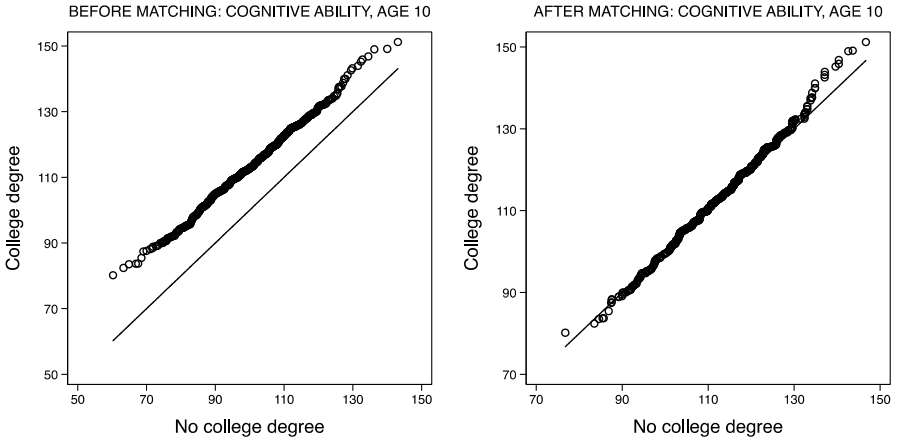

Fig. 3 Cognitive ability at age ten before and after matching

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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Appendix. Paper four: Robustness checks and additional analyses

Table 1: Covariate balance of all items in the family activity index and cultural activities index as well as the indices for total cognitive ability at age 5 and 10.

	Unmatched baseline		Matched ATT 1:1	
	p-value	KS p-value	p-value	KS p-value
Cultural activities 1: Reading books	.000	.000	.807	1
Cultural activities 2: Goes to a club or organization	.000	.000	.611	.998
Cultural activities 3: Goes to museums	.000	.000	.735	1
Cultural activities 4: Goes to libraries	.000	.000	.328	.909
Cultural activities 5: Plays a musical instrument	.000	.000	.081	.158
Family activities 1: Go for walks	.000	.010	.220	.408
Family activities 2: Goes to outings together	.000	.014	.127	.408
Family activities 3: Go to holidays together	.000	.000	.667	1
Family activities 4: Go shopping together	.732	1	.244	.451
Family activities 5: Chat for at least five minutes	.000	.205	.849	1
Family activities 6: Go to a restaurant together	.047	.146	.019	.329
Cognitive ability, age 5	.000	.000	.893	.496
Cognitive ability, age 10	.000	.000	.480	.367

Figure 1: Standardized bias in the unmatched sample (left) and the matched sample (right). Bias for all items in the family activity index and cultural activities index as well as the indices for total cognitive ability at age 5 and 10.

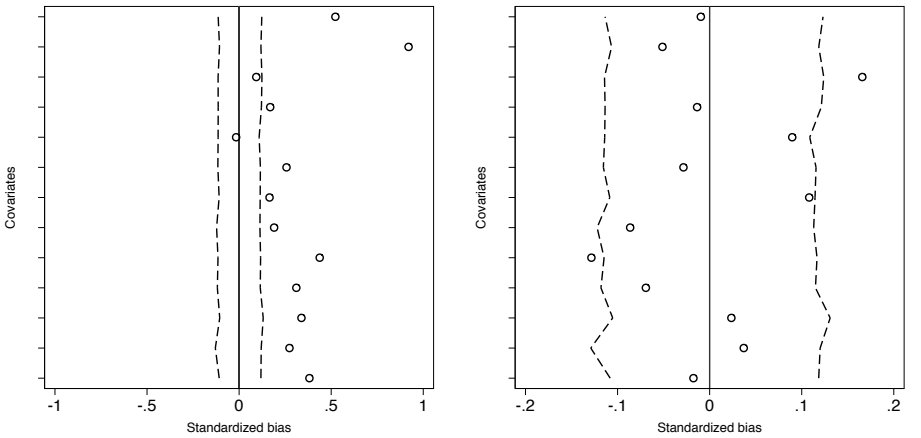

Table 2: Robustness checks of the effects of higher education on political participation. Genetic matching (1:2) with replacement.

	1:2	
	Estimate	p-value
Voted in 2001 election	.034	.216
Demonstration	.010	.546
Signed a petition	.023	.473
Contacted MP	.013	.486
Attended public meeting or rally	.024	.192

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

	1:1		1:2	
	Estimate	p-value	Estimate	p-value
Voted in 2001 election	.072	.006	.108	.000
Demonstration	.009	.427	.017	.054
Signed a petition	.051	.055	.055	.016
Contacted MP	.012	.397	.026	.022
Meeting/rally	.023	.092	.026	.018

Table 4: Robustness check: matching on an imputed data set. Genetic matching (1:1) with replacement.

	Imputed dataset	
	Estimate	p-value
Voted in 2001 election	.039	.053
Demonstration	.001	.947
Signed a petition	.015	.525
Contacted MP	.016	.151
Attended public meeting or rally	.017	.199

Paper 5

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Does Type of Education Affect Political Participation? Results from a Panel Survey of Swedish Adolescents

Mikael Persson*

In several countries it is apparent that individuals with academic gymnasium (upper-secondary) education show significantly higher levels of political participation than individuals with vocational education. However, previous research on this issue draws exclusively on one-shot cross-sectional data. This article utilizes a Swedish panel survey to gauge whether there is a direct causal link between type of education and political participation. Results demonstrate that differences in political participation are already present when students enter different types of education. The analyses show no significant effects of education; instead results support the education-as-a-proxy view: pre-adult factors predict political participation as well as educational choice.

Introduction

Research from several countries has found differences in political participation related to type of education (Hillygus 2005; Nie & Hillygus 2001; Niemi & Hanmer 2010; Kahne & Sporte 2008; Paterson 2009). However, previous research on this issue draws almost exclusively on one-shot cross-sectional data. This article utilizes a Swedish panel survey to gauge whether there is a direct causal link between the type of education and political participation. More specifically, this article evaluates why individuals with academic (theoretical) gymnasium (upper-secondary) education show significantly higher levels of political participation than adolescents with vocational education in Sweden. Do different educational tracks lead to different levels of participation, or is the correlation between type of education and political participation the result of self-selection?

Results show that differences in political participation related to type of education are already present before students enter different types of education. No evidence is found that type of education directly causes

* Mikael Persson, Department of Political Science, University of Gothenburg, Box 711, SE 40530 Gothenburg, Sweden. E-mail: mikael.persson@pol.gu.se

difference in political participation. Pre-adult factors predict political participation as well as educational choice.

The article is structured as follows. First, the rival theoretical expectations with regard to the effects of education on political participation are explained and previous research on effects of type of education on political participation is reviewed. Subsequently, data and techniques of analyses are presented. Finally, we turn to results from the panel survey and end by discussing implications of the findings.

Education: A Cause or a Proxy for Political Participation?

Years of education are of central importance in research on political participation (e.g., Converse 1972; Oscarsson & Holmberg 2004; Niemi & Junn 1998; Verba et al. 1995; Wolfinger & Rosenstone 1980). Recently, research has increasingly focused on whether years of education is a direct *cause* for political participation or merely works as a *proxy* for other factors (e.g., Berinsky & Lenz 2011; Burden 2009; Campbell 2009; Highton 2009; Kam & Palmer 2008; Sondheimer & Green 2010; Nie et al. 1996; Tenn 2005, 2007).

There are two possible explanations for the relationship between education and political participation. First, education might cause greater participation (the education-as-a-cause view). Or, second, the relationship could occur due to self-selection – that is, if the same kinds of people who seek higher education also would be 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 affects individuals' civic skills and cognitive capacity, which in turn increase political participation (e.g., Verba et al. 1995; see Campbell (2006) for a literature review). The education-as-a-proxy view states that education takes credit for other factors related to educational choice, such as the political socialization process early in life (e.g., Sears & Funk 1999; Searing et al. 1976; Alwin & Krosnick 1991; Cassel & Lo 1997; Jennings & Niemi 1974; Langton & Jennings 1968; Sears 1989). Factors such as family socio-economic status, parents' level of political participation, the discussion climate at home, and parents' political orientations are the basis of the early socialization process (Beck & Jennings 1982, 1991; Achen 2002; Andolina et al. 2003; Lauglo 2011; Jennings & Niemi 1968; Alwin & Thornton 1984; McIntosh et al. 2007; Andolina et al. 2003; Westholm 1999).[1] According to the education-as-a-proxy view these factors not only encourage political participation, they also determine the choice of education (cf. Kam & Palmer 2008).

The education-as-a-proxy view was supported in Langton and Jennings's (1968) seminal study, which showed the effects of civic education on political participation to be non-existent. It was then the dominating view during

the subsequent decades (cf. Niemi & Hepburn 1995). However, years of education repeatedly showed a strong impact on participation in cross-sectional studies, which led scholars to regard education as being one of the major factors influencing political participation (Converse 1972; Verba et al. 1995; Wolfinger & Rosenstone 1980).[2]

More recently, a number of studies have shown support for the education-as-a-proxy view. Kam and Palmer (2008) use propensity score matching to evaluate the effects of education. They find that differences in political participation related to education are diminished post-matching. 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 participation related to education were in place already before education was acquired.

However, there are also a number of recent sophisticated studies that show support for the education-as-a-cause view. The most interesting example is Sondheimer & Green (2010), who employ an experimental approach in which educational attainment is altered exogenously. In this study, strong support is found for the education-as-a-cause view. Likewise, Milligan et al. (2004) use both American and British data with variation over time to show that education has a positive impact on voting in the United States (but not in the United Kingdom). Moreover, Dee (2004) uses geographical distance to college and the adoption of child labour laws as instrumental variables to gauge the causal effects of education. Dee also concludes that education causes higher levels of political participation. In sum, previous research show contradictory results, and there is no agreement on whether education is a direct cause or a proxy for political participation.

Type of Education and Political Participation

The literature on whether education functions as a cause or proxy for political participation has largely focused on the effects of years of education. Indeed, in many countries 'higher education' means completing high school and going on to university. However, in some countries students select different educational tracks. In these places, different educational tracks lead to different patterns of political behaviour (cf. Stubager 2008, 2009).

Repeated studies using Swedish data have demonstrated that a main difference in political participation related to type of education consists in the significantly higher levels of participation among individuals from

theoretical gymnasium programmes (academic upper-secondary tracks aiming to prepare for further studies at universities) compared with students with education from vocational gymnasium programmes. Results drawing on several different data sources such as CivEd (the Civic Education Study) administrated by the International Association for the Evaluation of Educational Achievement, the Swedish National Election Studies, the SOM-surveys, as well as the Citizen Involvement and Democracy project, demonstrate that students in theoretical study programmes show significantly higher levels of intended and reported political participation than students from vocational programmes (Persson & Oscarsson 2010; Ekman 2007). In addition, Westholm et al. (1990) showed that students from vocational programmes had significantly lower levels of political knowledge than students from the theoretical programmes. Previous research has also shown that the differences in participation between students from theoretical and vocational programmes persisted after the reform of the Swedish educational system in 1990s when vocational programmes were extended from two to three years in order to equalize the educational system (Persson & Oscarsson 2010). Hence, the fact that the type of education gap in political participation exists is well established. However, due to the lack of panel studies it is uncertain if this gap is a direct cause of type of education.

A similar pattern is occurring in several other countries. Significant differences in levels of political participation among students from theoretical and vocational educational programmes in upper-secondary schools have also been documented in Norway (Lauglo & Øia 2006). In Italy, Losito and D'Apice (2004) find significant differences in conceptions of good forms of citizenship related to type of education. Drawing on data from Belgium, Quintelier (2008, 145–6) shows that general education students are encouraged to participate in politics more than those in technical and vocational tracks. And in a cross-national study, Van de Werfhorst (2007) uses data from 17 countries to show that students from vocational programmes were less politically active than students with theoretical educations.

Other studies focusing more broadly on type of education also find significant correlations with political participation. Using British longitudinal data, Paterson (2009) finds positive effects on political participation of taking social science courses. In a study on the impact of college education in the United States, Hillygus (2005, 38) finds that 'students who concentrated their studies in biology, chemistry, engineering and the like appear less inclined to participate politically, while those in the social sciences and humanities are more likely to vote and participate in other forms of political activity. Different levels of participation related to type of education are also found in Niemi and Hanmer's (2010, 319) study of voter turnout among American college students: 'Students majoring in math, science, and engineering voted less often.'

The problem is that almost all of the studies on the impact of type of education draw on one-shot cross-sectional data – a problem that has plagued much research on political socialization (cf. Amnå et al. 2009; Campbell 2006).[3] The few exceptions that use longitudinal data do not control for pre-treatment levels of the dependent variable (Hillygus 2005; Paterson 2009). So, although differences in political participation related to type of education are repeatedly confirmed in a number of studies in several contexts, none of them apply a study design appropriate to draw conclusions about causal effects of type of education. None of these studies can exclude reverse causation as an alternative explanation to the fact that type of education correlates with political participation. Despite the well-known risks of drawing conclusions on the basis of one-shot cross-sectional data (Astin & Lee 2003), all evidence on how type of education affects political participation rests on such doubtful foundations.

Data and Study Design

This article reports findings from a one-year panel survey designed to gauge whether there is a direct causal effect of type of education on political participation. The first wave of the survey was conducted after the respondents graduated from comprehensive school, wherein they all shared the same curriculum. Hence, the panel study takes advantage of a crucial moment of educational choice that allows us to compare intended political participation even before different types of education were acquired.

Approximately 500 Swedish students were followed during their first year in the gymnasium. In the data there is a panel component on the individual level (there is a possibility to follow a single individual over time). Three gymnasium schools in three different municipalities in Sweden were recruited for the study, and we aimed to cover all first-year students (the tenth year of school in Sweden).[4] The reason to choose these gymnasiums was that they are public schools that include the majority of all students in each of the municipalities. As the main aim of the study is to capture students' initial levels of intended political participation before different types of education were acquired, the study was limited to individuals in the first year of the gymnasium. The advantage of this research design is that we can trace the possible causal effect of different educational routes as the students make progress through their educational institution.

The sample is not nationally representative; however, the aim is to trace whether levels in different groups change over time. Since the research question aims to evaluate a claim concerning a causal relationship, the panel design is more appropriate than one-shot cross-sectional designs. Still, the best research design would be a representative sample that could be followed over time. In the absence of such a study, the present data is

the best available data source and we have no particular reason to assume that the individuals in this sample are significantly different from the students in a general national sample of students at the same age. The students at the three schools have mean grades fairly close to the national mean. Swedish gymnasium mean grades range from 0 to 20. In 2009, the national mean was 14.1, while the three schools in the study range from 13.7 to 14.7. The mean levels of education among the adult population in the three municipalities are slightly higher than the national mean. While the national mean among adults 26–74 years old were 11.8 years of education in 2006, mean levels in the three municipalities range from 11.9 to 13.2. All three municipalities are located in the suburban areas of one of Sweden's largest cities. The population sizes vary between 35,000 and 75,000. The income levels in the municipalities are about the same as the national average in one municipality and well above the national average in two of the municipalities.

The first wave of the survey took place at the start of the first year in the gymnasium, which was the end of August 2008. By the end of the first year, we conducted the second wave, which was at the end of May and early June 2009. The survey was conducted in the students' classrooms during regular lessons. The two-wave sample includes 530 respondents;[5] 17 of the classes were from theoretical programmes and 21 were from vocational programmes;[6] and 231 of the students came from vocational programmes and 299 students from theoretical programmes.[7] In the empirical analyses, only students who answered each of the specific questions at both T1 and T2 are included, which reduces the sample further.[8]

One possible objection is that the panel only lasted during the first year of the gymnasium and cannot capture the full effects of education. However, it is important to note that the main aim of the study is to evaluate whether the differences related to type of education are due to self-selection effects or if they emerge when students make progress through the educational institution. For that reason, the most important feature of the study is to measure students' initial attitudes towards participation as they enter different educational programmes.[9] In previous research, panel designs are very rare and few of them lasted considerably longer than this one. For example, John and Morris's (2004) panel was conducted for one year, Westholm et al.'s (1990) for a year and a half, and the study employed by Hooghe and Dassonneville (2012) as well as that by Quintelier and Hooghe (2012) lasted for two years.

In total, 976 student in 44 classes in the three schools participated in the first wave of the survey. Panel mortality was about 46 percent, which is about what could be expected and roughly equivalent to studies with similar designs (cf. John & Morris 2004, 97). A major part of the sample loss was due to the fact that six classes were not able to participate in the second wave.[10]

Additional missing data in the individual analyses is due to the use of the 'don't know' option. The proportion of respondents choosing this option varies between 7.4 percent (spray-painting, T2) and 29.5 percent (demonstration, T1). There are a larger amount of non-responses in the indices covering non-parliamentary and traditional forms of participation than for voting and illegal forms of participation. When analyzing the patterns in the non-responses we also find that the vocational students are overrepresented among respondents providing non-responses. Whether this reflects that vocational students have less knowledge about forms of participation or are less interested in answering survey questions is unfortunately impossible to know. However, it is obvious that vocational students' intentions to participation are harder to capture.

So what does this imply for the validity of the results? One implication is that the results should be interpreted as more uncertain for the dependent variables with more non-responses. At the same time, the higher levels of 'don't know' answers for forms of participation that are less frequently occurring show that respondents have indeed reflected over their response options and not just picked a random alternative. At the end of the day, we need to accept that adolescents' intentions to participation are harder to measure for some forms of participation than others. This fact should be taken into account when interpreting the results.

To maximize comparability with previous research, the questionnaire replicates the items used by the dominating research programme in the field: the Civic Education Study (CivEd) carried out by IEA (International Association for the Evaluation of Educational Achievement). As for the dependent variables, the individual intentions to participate in the different forms of political participation were measured on a four-point scale, ranging from 'I will certainly not do this' to 'I will certainly do this'.

While the use of these items facilitates comparability with previous research drawing on the IEA data, one could question the external validity of the measures – that is, does intended participation transform into actual participation? This issue is depending on when adolescents' attitudes crystallize. On the one hand, if adolescents continue to develop their patterns of political behaviour after the age of 16, the items might capture a snapshot with small relevance for adult political behaviour. On the other hand, if the political socialization process does not continue to develop after the time of the survey, the measures are likely to give a glimpse of the respondents' future political behaviour. One way to assess the external validity of items measuring intentions to participation is to validate the results against another source of data containing comparable measures of performed participation (cf. Hooghe & Wilkenfeld 2008). In the concluding section I will discuss the findings from this study in relation to other studies of Swedish youths relying on measures of reported participation.

Most of the previous research on adolescents' political behaviour and the influence of school-related factors have relied on the CivEd data and the items measuring intentions to participation (e.g., Hooghe & Wilkenfield 2008; Campbell 2007, 2008; Wolbrecht & Campbell 2007; Metz et al. 2003; Torney-Purta et al. 2001; Torney Purta 2002; Torney-Purta & Amadeo 2003). In fact, they have become the standard indicators of youth political participation in the field. However, none of these studies follow individuals over time into adulthood. Unfortunately, only such studies could evaluate whether intentions to participation actually transform into performed participation (cf. Hooghe & Wilkenfield 2008, 156).

Looking more broadly at political behaviour research it is evident that there is a lack of studies comparing intention to participate, reported participation and validated participation. One of the few studies dealing with this issue is a recent paper by Achen and Blais (2010), which shows that intention to vote, reported voting and validated voting correlate strongly. Most importantly, the same independent variables show significant impact on all three measures (although there are some differences in the sizes of the coefficients due to the fact that not all intentions result in actual participation). Achen and Blais (2010, 7) conclude that 'researchers will rarely be misled by using any one of the three sources'. However, while using the items measuring intentions to participate might seem unproblematic drawing on the study of Achen and Blais, more research is called for in this area. Moreover, their data covers only adults and it is unclear whether these findings are generalizable to youths as well. Hence, results presented here should be interpreted only as in indication of students' likely future behaviour rather than a deterministic prediction.

The analyses cover 13 forms of participation. From previous research, we know that different forms of participation have different causes and differ in frequency (e.g., Verba et al., 1995). To sort out relevant categories of participation, factor analyses were conducted that revealed three distinct categories of participation, which were used to construct three indices. First, the *Traditional Participation Index* includes party membership, contacting political representatives, writing letters and candidating for a political party. Second, the *Non-Parliamentary Participation Index* includes boycotting, buycotting, wearing a political badge, signing a petition and demonstrating. Third, the *Illegal Participation Index* includes spray-painting, blocking traffic and occupying buildings. The factor loadings and eigenvalues of the indices are presented in Appendix Table 1, and descriptive statistics are presented in Appendix Table 2.[11] The indices are additive and are constructed by simply adding the respective variables together. To facilitate interpretation, the indices are rescaled to vary between 0 and 10. Following previous research, voting is treated as a separate category (cf. Verba et al. 1995; Campbell 2009).

The students in the survey are not statistically independent, since they belong to specific classes in three specific schools. Due to the nested structure of the data, this dependency between observations needs to be taken into account in the computation of standard errors. For that reason, multilevel modeling is employed. More specifically, two-level multilevel models (students within classes) that explicitly take into account the clustered structure of the data are applied. Performing nonhierarchical regression that ignores the dependency would likely produce biased estimates (cf. Goldstein 1995; Hox 2002; Snijders & Bosker 1999).[12] However, when analyzing whether there is a significant change in the theoretical–vocational gap between T1 and T2, I apply a 'difference-in-differences' approach, testing the interaction between gymnasium programme and time. To accomplish this test, the datasets containing T1 and T2 were stacked so that the unit of analyses is time of observation (within individuals). This reshaping of the dataset adds another level to the structure of the data. Hence, the models testing the interaction term (programme X time) are three-level models (observation within individuals within classes) while the models focusing on T1 or T2 separately are two-level models (individuals in classes).

The empirical analyses will proceed in four steps. First, we investigate whether there is a significant difference in levels of political participation already when students enter the gymnasium (at T1) and we repeat the test of this difference after one year (at T2). We use a dummy variable for educational programme (0 = vocational, 1 = theoretical) in the multilevel models to measure the difference between students at respective point of time.

Second, we estimate if there is a significant change in the vocational–theoretical gap during the first year of study in the gymnasium. Even if significant differences occur already at T1, the difference can significantly increase or decrease between T1 and T2. We model the change of the vocational–theoretical gap as the interaction between educational programme and time. A significant interaction term signals a significant change in the vocational–theoretical gap between T1 and T2.

Third, we investigate which other non-educational factors that might have caused the vocational–theoretical gap. If education is a proxy for other factors, which are the factors that have caused the gap in the first place? These analyses make use of the T1 sample only (with individuals not participating in the second round excluded). Only a small set of variables measuring background factors are used in order to minimize multicollinearity. The SES of the family environment is, of course, of central importance for the intention to participate in political activities (cf. Ekman & Zetterberg 2010). However, previous studies show that this information is hard to acquire from youth respondents. In international comparative studies such as CIVED and TIMSS, the number of books at home is frequently used as a proxy to measure the SES of the family (cf. Lopes et al. 2009; Wolbrecht & Campbell

2007; Campbell 2008). This item correlates strongly with parents' education (Evans et al. 2010). However, number of books has an advantage in that it yields considerably lower levels of non-response than questions about the parents' education (Beaton et al. 1996; Torney-Purta et al. 2001). Two questions are also included that measure the amount of political discussions with teachers and within the family. Discussion of politics within the family has repeatedly been shown to increase civic development among youths (McIntosh et al. 2007). Since attitudes towards participation might also be affected by different previous experiences of social science education in comprehensive school, political discussion with teachers is included to control for the impact of different earlier educational experiences related to social science education. Finally, we also include a control for gender.

And fourth, to further corroborate the education-as-a-proxy view, we investigate whether the same factors as predict political participation also predict educational choice? From previous research we know that parental resources and social status affect educational choices (cf. Tieben 2011). If education is a proxy for non-educational factors, the same non-educational factors that predict political participation should also drive the choice of education.

Results

Is there a Significant Vocational–Theoretical Gap in Political Participation?

First we compare the levels of intended participation among students from vocational and theoretical programmes at T1 (when they enter the gymnasium) and repeat this comparison at T2 (the end of the year). Since the dependent variables are continuous, linear mixed models are applied throughout the analyses. We use a dummy for whether each student takes part in a theoretical or a vocational programme to estimate the size of the vocational–theoretical gap. Obviously, a statistically significant difference between students from theoretical and vocational programmes at T1 cannot be a cause of type of education since students have just entered their educational programmes. In that case, type of education must consequently be a proxy for other factors.[13]

Table 1 presents results from multilevel regression models for each of the four dependent variables at T1 and T2.[14] Model 1 show that there are significant differences between students from theoretical and vocational programmes as regards intention to vote already at T1. This significant difference remains at T2. At both points, students from theoretical programmes show considerably higher levels of intention to vote than vocational students. For the Traditional Participation Index we find no significant differences between the students, neither at T1 nor at T2. As for the

Table 1. The Impact of Educational Programme (Theoretical or Vocational) on Indicators of Political Participation at the Beginning and the End of the First Year of Gymnasium (Multilevel Models, Maximum Likelihood Estimation)

	(1) Voting T1	(2) Voting T2	(3) Traditional Participation Index T1	(4) Traditional Participation Index T2	(5) Non-parliamentary Participation Index T1	(6) Non-parliamentary Participation Index T2	(7) Illegal Participation Index T1	(8) Illegal Participation Index T2
Fixed part:								
Educational programme (0 = vocational; 1 = theoretical)	0.893*** (0.204)	0.688*** (0.254)	–0.023 (0.269)	0.282 (0.227)	0.950** (0.426)	1.397*** (0.409)	–0.450* (0.264)	–0.847*** (0.291)
Intercept	7.521*** (0.158)	7.651*** (0.190)	2.770*** (0.205)	2.807*** (0.177)	4.479*** (0.327)	4.102*** (0.318)	1.991*** (0.201)	2.403*** (0.220)
Random part:								
Class level: Standard deviation of u_j	0.179 (0.249)	0.491*** (0.135)	0.473** (0.163)	0.132 (0.391)	0.700 (0.279)	0.416 (0.390)	0.368* (0.199)	0.462* (0.191)
Individual level: Standard deviation of e_{ij}	1.966*** (0.070)	1.895*** (0.067)	1.699*** (0.076)	1.812*** (0.081)	2.107*** (0.129)	2.371*** (0.142)	2.185*** (0.083)	2.286*** (0.087)
Number of groups	37	37	37	37	36	36	36	36
Number of individuals	433	433	283	283	168	168	379	379
BIC (Bayesian information criterion)	1841.938	1827.391	1142.017	1163.713	761.790	791.988	1701.014	1738.454

Notes: Standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Non-Parliamentary Participation Index, in models 5 and 6 we once again find a significantly higher level of intended participation among the theoretical students than the vocational students, both at T1 and T2. Here it should be noted that the results for traditional and non-parliamentary participation are more uncertain, due to the low response rates. Concerning the results from the Illegal Participation Index, we also find a significant gap between students from vocational and theoretical programmes, but here it is the vocational students that show considerably higher levels than the theoretical students.

In sum, we find mixed support for the hypothesis that there are significant differences between vocational and theoretical students: statistically significant differences in intention to participate are found on three of four dependent variables under study. However, these differences appear as students *enter* different educational programmes, and type of education cannot reasonably have had any effect at this time. How can these mixed findings be explained? As for voting and non-parliamentary participation, the findings confirm what is expected from previous research: statistically significant differences of substantial size (from about 0.7 to 1.4 on the 0–10 scale). Regarding the traditional participation index, the levels are low for both vocational and theoretical students. The findings show us that both vocational and theoretical students have low intention to participate in activities related to political parties. The expected vocational–theoretical gap is thus most evident in forms of participation that students intend to perform more frequently.

The perhaps most unexpected results is that intention to perform illegal acts of participation are higher among vocational students. Students on theoretical programmes are less likely to choose illegal forms of participation. Their intended participative repertoire consists primarily of legal forms of participation which reflects their higher degree of commitment to the established norms in society.

In sum, results do not show a general gap in participation between students with different types of education. For forms of participation that students intend to perform more frequently, students from theoretical programmes score higher. For traditional participation, we observe low levels among both vocational and theoretical students. And only for forms of participation that are illegal and concur with the norms of the established society do we observe higher intention among the vocational students.

Is there a Significant Change in the Vocational–Theoretical Gap in Participation during the First Year at the Gymnasium?

The second step takes a 'difference-in-differences' approach (cf. Donald & Lang 2007; Bertrand et al. 2004), which is employed to measure whether the vocational–theoretical gap significantly increases during the first year of

the gymnasium. Even if a significant gap exists as students enter the gymnasium, it might significantly increase or decrease as students make progress through different types of education. The difference-in-differences approach allows us to examine whether education has caused a significant change in the size of the differences. It is operationalized as the interaction effect of educational programme and time. If the education-as-a-cause view is to be proven valid, we should observe a significant increase in the vocational–theoretical gap during the first year in the gymnasium – that is, a significant interaction term. This design could be compared to an experimental design in which the difference between a treatment group and control group is measured at two points of time, and a significance test is made on the difference-in-differences.

To accomplish this test, the datasets containing T1 and T2 were stacked so that the unit of analyses is time of observation (within individuals). Then models are estimated that includes time, the vocational–theoretical dummy and the interaction between these two terms. For this reason, models 9–12 are three-level models that take into account that observations are clustered within individuals within classes and estimates standard errors at each of these three levels. Results reveal that there is no significant change in the magnitude of the vocational–theoretical gap during this year since the interaction terms in models 1–4 in Table 2 are not significant. The size of the gap in intention to participate in political activities between students from theoretical and vocational programmes does not change significantly during the first year of study in the gymnasium.[15]

Which Non-educational Factors Influence the Vocational–Theoretical Gap?

Models 1–4 in Table 3 present estimates from models analyzing factors that might affect intention to participate in political activities before individuals enter different types of education in the gymnasium.[16] These models include measures of the amount of discussion about politics at home and with teachers, respectively; the number of books at home; and gender. The vocational–theoretical dummy is also included in these models to see how this coefficient performs under control for other factors. Results show that in these models the vocational–theoretical dummy is only significant at the 95 percent level on voting. However, since it is unlikely that type of education should have had any effect as students enter the educational institution, this variable probably account for some unobserved pre-adult factors. For the non-parliamentary participation index we found a strong and significant effect of educational programme in the bivariate analysis presented in Table 1. However, when adding controls for books at home and the amount of discussion at home, the coefficient for type of education turn insignificant, which indicates that the relationship can be explained with reference to self-selection effects.

Table 2. Interaction Effects of Theoretical–Vocational and Time (Multilevel Models, Maximum Likelihood Estimation)

	(1) Voting (T1 + T2)	(2) Traditional Participation Index (T1 + T2)	(3) Non-parliamentary Participation Index (T1 + T2)	(4) Illegal Participation Index (T1 + T2)
Fixed part:				
Educational programme (0 = vocational, 1 = theoretical)	0.138 (0.167)	0.071 (0.165)	–0.349 (0.264)	0.440** (0.194)
Time (0 = T1, 1 = T2)	1.041*** (0.377)	–0.279 (0.385)	0.546 (0.638)	–0.067 (0.449)
Educational programme × Time	–0.176 (0.214)	0.267 (0.210)	0.394 (0.337)	–0.396 (0.251)
Intercept	7.388*** (0.292)	2.684*** (0.300)	4.840*** (0.496)	1.547*** (0.344)
Random part:				
Class level: Standard deviation of v_k	0.336*** (0.118)	0.314** (0.166)	0.616 (0.259)	0.396** (0.161)
Individual level: Standard deviation of u_{jk}	1.181** (0.084)	1.274*** (0.087)	1.649*** (0.147)	1.461*** (0.100)
Observation level: Standard deviation of e_{ijk}	1.535*** (0.052)	1.217*** (0.051)	1.504*** (0.082)	1.699*** (0.062)
Number of classes	37	37	36	36
Number of individuals	433	283	168	370
Number of observations	866	566	336	758
BIC (Bayesian information criterion)	3600.291	2211.477	1487.279	3358.082

Notes: Unstandardized regression coefficients. Standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table 3. Effects on Political Participation at the Beginning of the Gymnasium (Multilevel Models, Maximum Likelihood Estimation)

	(1) Voting T1	(2) Traditional Participation Index T1	(3) Non-parliamentary Participation Index T1	(4) Illegal Participation Index T1
Fixed part:				
Discussing politics in the family	0.920** (0.387)	1.517*** (0.393)	1.679*** (0.542)	0.276 (0.444)
Discussing politics with teachers	–0.776** (0.367)	–0.239 (0.384)	–0.616 (0.543)	–0.243 (0.433)
Watch television news	0.328 (0.370)	0.121 (0.390)	–0.372 (0.540)	–0.024 (0.455)
Books at home	0.660 (0.446)	0.821* (0.455)	2.537*** (0.636)	0.878* (0.519)
Gender (male)	0.090 (0.218)	0.060 (0.224)	–0.727** (0.320)	1.263*** (0.259)
Educational programme (0 = vocational, 1 = theoretical)	0.789*** (0.224)	–0.070 (0.261)	0.365 (0.413)	–0.524* (0.311)
Intercept	6.655*** (0.456)	1.391*** (0.458)	2.895*** (0.668)	0.808 (0.534)
Random part:				
Class level: Standard deviation of u_j	0.172 (0.319)	0.326* (0.200)	0.681 (0.262)	0.515 (0.219)
Individual level: Standard deviation of e_{ij}	1.923*** (0.078)	1.658*** (0.082)	1.776*** (0.120)	2.060*** (0.091)
Number of groups	37	36	33	34
Number of individuals	347	242	147	298
BIC (Bayesian information criterion)	1492.151	986.289	638.945	1337.435

Notes: Unstandardized regression coefficients. Standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Regarding the other independent variables, the amount of discussion within the family about politics has a positive and significant effect on all indices except for the illegal forms of political participation. Moreover, the number of books at home has a strong and large effect on non-parliamentary participation, while the effect of books at home on traditional and illegal participation only reaches the 90 percent significance level. Looking at the effects on illegal participation, we also find that men have higher intention to participate in such acts than women.

To conclude, the results in Table 3 show that the effect of type of education is reduced under control for other factors. Instead of factors related to education, the amount of political discussion at home and/or the number of books at home positively affects all four forms of participation.

Do the Same Factors that Predict Political Participation also Predict Educational Choice?

In the fourth and final step of the analysis, we analyze whether the variables that correlate with participation predict educational choice as well. Model 1 in Table 4 uses the dummy for vocational–theoretical programme as the dependent variable and estimates the impact of number of books at home, gender and the amount of discussion of politics at home. As expected, we find a strong effect of the number of books at home (while gender and amount of discussion in the family are insignificant). Translating the effect of books at home into predicted probabilities shows that there is a probability of 0.28 for adolescents with zero books at home to attend a theoretical programme while the probability for adolescents with more than 200 books at home is 0.67. Hence, the socio-economic status of the family affects both intention to participate in politics and educational choice. However, while the previous model showed that political discussion correlate with political participation, it does not seem to influence educational choice. Hence, politi-

Table 4. Effects on Choice of Educational Gymnasium Programme (0 = Vocational, 1 = Theoretical): Results from Logistic Regression

	(1) Choice of educational programme
Discussing politics in the family	0.281 (0.319)
Books at home	1.642*** (0.393)
Gender (male)	–0.123 (0.189)
Intercept	–1.002*** (0.339)
Number of individuals	498
Pseudo R^2	0.030

Notes: Unstandardized logistic regression coefficients. Standard errors in parentheses. *$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

cal discussion affects political participation independently. At the same time, books at home seem to be more important for educational choice than it is for some forms of political participation, such as voting.

Conclusion

Results from the analyses reveal that differences in levels of political participation related to type of education is not likely to be caused by type of education. When differences occur, they can be observed before different types of education might possibly have had any effects. Taken together, results do not support the idea that type of education causes differences between students with vocational and theoretical educations. Instead, results indicate that the vocational–theoretical gap in levels of political participation is caused by factors outside of school. Family socio-economic status and the amount of political discussion within the family are more robust predictors of political participation.

The main theoretical implication of the results is that previous studies might exaggerate the impact of education as a consequence of the limitations of the conventional cross-sectional correlational approach. Hence, further research would concentrate on disentangling the different factors in the pre-adult socialization process for which education might be a proxy. In particular, it is important to focus on pre-adult factors that are not considered in this study such as personality type, genetic factors or social networks.

However, no studies are without shortcomings and it should be acknowledged that this study has several limitations. First of all, the sample size is quite small and it could be the case that a larger study could detect patterns that are not visible in this data. Second, this study follows the respondents during one year only. It might be the case that effects of education on political participation begin to show after several years of secondary schooling, or after enrollment in further education. Hence, although education does not seem to have any effect on political participation drawing on these data, we cannot rule out the possibility that education causes political behaviour in the long run. Taking into account that research has shown that social network position can function as the causal mechanism linking education and participation (cf. Campbell 2009; Nie et al. 1996; Persson 2011) it might not be possible to detect effects of education until later in life. Third, the respondents do not represent a random sample drawn from the total population of individuals attending gymnasium in Sweden. Hence, in a statistical sense we cannot generalize the results to the entire population. However, it is important to note that the differences between theoretical and vocational students observed in this study closely resemble observed patterns in national representative surveys (cf. Persson & Oscarsson 2010; Ekman 2007). This study adds that these differences seem to be in place

before different types of education are acquired and that they do not change over time. However, further replications drawing on national representative samples following individuals over longer time periods would be needed in order to completely rule out the contention that type of education has any effect on political participation. Fourth, the items in this study measure intention to political participation and not reported participation. This is another reason for not drawing too far-reaching conclusions from this study. Although previous research shows that intention to participate and performed participation correlate strongly, we do not know for sure to what extent the intention to participate among the adolescents in this study will translate into political participation.

Taking these caveats into account, results do indicate that we ought to reconsider the view that type of education has a causal effect on political participation. Rather than support for a causal effect of education, results indicate that we might need to return to the conclusions made already in the seminal study by Langton and Jennings (1968) over forty years ago: the effects of civic education on political socialization are marginal at best and can be explained with reference to factors operating even before individuals gain education.

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NOTES

1. Other researchers adhere to the education-as-a-proxy view for other reasons than pre-adult socialization. For example, Luskin (1990) argues that education takes credit for factors such as intelligence (cf. Deary et al. 2008). Moreover, a recently emerging literature recognizes that genetic factors influence political participation (Alford et al. 2005; Fowler et al. 2008). Yet others focus on how personality types affect political participation (Mondak & Halperin 2008; Mondak et al. 2010). Furthermore, according to the relative education model, there is no direct causal link between education and political participation; education affects participation via social network centrality (e.g., Nie et al. 1996; Tenn 2005; Campbell 2009).
2. In order to gauge causal effects from a cross-sectional design it needs to be complemented with instrumental variable techniques or an experimental set up.
3. There are a number of studies using panel designs to estimate effects of education on related dependent variables. However, none of these studies estimate effects of type of education on political participation. Prominent examples of panel studies in this field are John and Morris (2004), who estimate effects of education on social capital; Westholm et al. (1990), who estimate the impact of type of education on political knowledge; and Oscarsson and Holmberg (2004), who use the panel structure of the Swedish National Election Surveys to evaluate if increased education between elections increases electoral participation.
4. In Sweden, students start the gymnasium in August at age 16.

5. However, due to the fact that some students marked a 'don't know' option on some questions, the number of individuals is not equal in all of the analyses.
6. I use the following classification: theoretical programmes in the sample are the Natural Science Programme, the Social Science Programme and the International Baccalaureate Programme; vocational programmes in the sample are the Athletics Programme, the Child and Recreation Programme, the Construction Programme, the Arts Programme, the Entrepreneurship Programme, the Health Care Programme, the Technology Programme, the Information Technology Programme, the Vehicle Programme, and the Business and Administration Programme.
7. The fact that there are more students from theoretical programmes in the survey, even though there are fewer theoretical classes, is due to two factors: larger class sizes among the theoretical programmes and higher response rates among the students from theoretical programmes. A potential problem is that the lower response rate among vocational students biases the results. One could object that the effect of type of education might be underestimated due to the fact that those who did not answer are more likely to be politically inactive.
8. It is important to evaluate whether students included in both waves are significantly different from those who were only included in the first wave. A comparison of the characteristics of all the students in the first wave with students included in both waves shows that they do not significantly differ from each other. This test included gender; immigrant status; number of books at home; the amount of political discussion with friends, teachers and family; and watching television news. Individuals included in both waves do not have any of these characteristics to a larger or smaller extent than students who only participated in the first wave.
9. Although we surveyed the students as early as possible after they entered the gymnasium, it is of course a theoretical possibility that differences between the two types of programmes might have arisen during the very first days of school. However, I find it quite counterintuitive that type of education should have had this rapid effect after only a couple of days, and then no significant change is found for an entire year.
10. An important question is of course whether panel mortality biases the results. When comparing the levels of participation at T1 for the 530 individuals included in both waves with the 446 panel dropouts we find that the panel dropouts do not show significantly different levels of political participation. Hence, the estimates presented in this article are not likely to be biased as a consequence of panel mortality.
11. Using the factor loadings as dependent variables instead of the additive indices provides substantively identical results.
12. An alternative approach to handle the nested data structure is to estimate OLS models with clustered standard errors at the class level. Coefficients and standard errors from such models are substantially identical to those presented from the multilevel models (available upon request from the author.)
13. It is important to note that the strategy in the first step is to make two separate analyses at T1 and T2 to evaluate, first, whether there is a significant difference between theoretical and vocational students, and second, whether this gap is maintained after one year of study. Hence, it is not the change scores between T1 and T2 that are used; instead there are two separate analyses as to whether there are gaps related to type of education at T1 and T2, respectively.
14. In models 1–4, linear multilevel regression models are applied to the four dependent variables (voting and the three indices), although the participation measures are originally all four-point ordinal scales. However, ordinal logit models (with clustered standard errors taking the dependency within classes into account) produce substantially similar results.
15. In models 9–12, the main effect of type of education (theoretical/vocational) is not statistically significant due to collinearity with the interaction term. In models 9, 10 and 12, the significant coefficients for type of education are restored when removing the interaction terms in each of these models.
16. All independent variables are recoded so that they vary between 0 and 1 to facilitate interpretation.

Appendix Table 1. Classification of the Forms of Political Participation

	Questions in questionnaire (When you are an adult, what do you expect that you will do?)	Factor loading (principal components)
Traditional Participation Index		
Party membership	Join a political party	T1: 0.7279 T2: 0.7731
Contact political representatives	Contact a political representative to express your opinion	T1: 0.7888 T2: 0.8369
Write letters	Write letters to a newspaper about social or political concerns	T1: 0.7943 T2: 0.8137
Candidate for a political party	Be a candidate for a political party	T1: 0.7188 T2: 0.7043
Eigenvalue		T1: 2.2997 T2: 2.4563
Non-parliamentary Participation Index		
Boycotting	Buy some products for political, moral or environmental reasons	T1: 0.8045 T2: 0.8120
Boycotting	Boycott some products	T1: 0.8222 T2: 0.7810
Wear a political badge	Wear a political badge	T1: 0.6984 T2: 0.7849
Sign a petition	Sign a petition	T1: 0.7251 T2: 0.8156
Demonstrating	Participate in a peaceful demonstration	T1: 0.6997 T2: 0.7503
Eigenvalue		T1: 2.8264 T2: 3.1135
Illegal Participation Index		
Spray-paint	Spray-paint protest slogans on walls	T1: 0.7537 T2: 0.8220
Block traffic	Block traffic as a form of protest	T1: 0.8797 T2: 0.9107
Occupy buildings	Occupy public buildings as a form of protest	T1: 0.8727 T2: 0.9224
Eigenvalue		T1: 2.8264 T2: 2.3560
Voting	Vote in national elections	

Notes: All item scales originally range 1–4: 1 = Certainly not do this; 2 = Probably not do this; 3 = Probably do this; 4 = Certainly do this.

Appendix Table 2. Descriptive Statistics

	Mean	Standard deviation	N	Minimum	Maximum
Discussing politics in the family	0.604	0.294	507	0	1
Discussing politics with teachers	0.385	0.385	428	0	1
Watch television news	0.631	0.296	517	0	1
Books at home	0.725	0.723	519	0	1
Gender (male)	0.402	0.491	520	0	1
Voting T1	8.068	2.025	433	0	10
Voting T2	8.099	1.991	433	0	10
Traditional Participation Index T1	2.744	1.764	283	0	10
Traditional Participation Index T2	2.980	1.826	283	0	10
Non-parliamentary Participation Index T1	5.056	2.281	168	0	10
Non-parliamentary Participation Index T2	4.948	2.515	168	0	10
Illegal Participation Index T1	1.706	2.227	379	0	10
Illegal Participation Index T2	1.911	2.366	379	0	10

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Summary in Swedish

Sammanfattning på svenska

Forskningsproblemet

Enligt en rad studier är utbildning den faktor som har starkast effekt på politiskt deltagande (Rosenstone och Hansen 1993; Schlozman, Verba och Brady 2012, Verba, Schlozman och Brady 1995; Wolfinger och Rosenstone 1980). Vanligtvis framhålls att utbildning påverkar kunskap och kognitiv förmåga, vilket i sin tur påverkar politiskt deltagande. På senare år har dock en rad studier ifrågasatt hur det kausala sambandet ser ut. Frågan gäller närmare bestämt om utbildning bör ses som en direkt *orsak* till politiskt deltagande eller om det snarare är en *proxy* för andra faktorer som är relaterade till utbildning (Berinsky och Lenz 2010; Burden 2009; Campbell 2009; Dee 2004; Kam och Palmer 2008; Milligan, Moretti och Oreopoulos 2004; Nie, Junn och Stehlik-Barry 1996; Sondheimer och Green 2010). Avhandlingen syftar till att bidra till debatten genom en serie analyser av hur utbildning är relaterat till politiskt deltagande.

I avhandlingen definieras politiskt deltagande som aktiviteter med avsikt att påverka politiska beslut. Fokus ligger vid traditionella former av politiskt deltagande så som valdeltagande och engagemang i politiska partier. Vad det gäller undersökningens oberoende variabel avses formell utbildning. Delstudierna undersöker effekter av utbildningslängd och utbildningsinriktning.

Teori

I litteraturen finns tre teoretiska modeller som erbjuder olika alternativa förklaringar till sambandet mellan utbildning och politiskt deltagande. Enligt den första modellen har utbildning i sig effekt på politiskt deltagande. Utbildning påverkar kognitiv förmåga och kunskap som i sin tur påverkar politiskt deltagande. Den kausala mekanismen är alltså enligt detta synsätt läroprocesser på individnivå.

Enligt den andra modellen, som ibland kallas sorteringsmodellen, har utbildning en relativ effekt på politiskt deltagande (Nie, Junn och Stehlik-Barry 1996). Utifrån detta synsätt har inte utbildning i sig någon direkt kausal effekt utan endast en indirekt effekt via sociala nätverk. Utbildning fungerar som en sorteringsmekanism som påverkar individers sociala

nätverk vilket i sin tur påverkar politiskt engagemang. Utbildning är enligt den här modellen alltså enbart en proxy för social position. Personer med hög social status är mer exponerade för sociala nätverk som uppmuntrar politiskt engagemang och har också större chans att bli rekryterade. Och vice versa, individer med relativt låg utbildning är ofta utanför rekryteringsnätverken till politiska aktiviteter. Modellen implicerar att effekten av utbildning är relativ snarare än absolut; vilken statusposition som utbildning genererar är beroende av utbildningsnivån i omgivningen.

Enligt den tredje modellen har utbildning inte någon effekt. Snarare är det den socialisationsprocess som sker tidigt i livet, och medfödda förmågor, som påverkar både utbildning och politiskt deltagande (Kam och Palmer 2008). Sambandet mellan utbildning och deltagande drivs alltså av en eller flera bakomliggande variabler. Särskilt viktigt i sammanhanget är faktorer så som uppväxtfamiljens socioekonomiska status, samtalsklimatet i uppväxtfamiljen och individens kognitiva förmåga.

De tre modellerna ger alltså olika förklaringar till hur sambandet ser ut och kan sammanfattas enligt följande: a) sambandet mellan utbildning och deltagande är en konsekvens av selektionseffekter; utbildning är endast en proxy för faktorer som tidig socialisation och medfödda förmågor, b) utbildning har en kausal effekt på deltagande, huvudsakligen genom de kausala mekanismerna kunskap och kognitiv förmåga, eller c) utbildning har ingen effekt i sig men påverkar social status som i sin tur påverkar politiskt deltagande. Med andra ord kan sambandet förklaras med hänvisning till a) selektionsprocesser som sker *före* individer genomgår utbildning, b) förmågor och kunskaper som tillskansats *under* tiden man genomgått utbildning, eller c) den sociala nätverksposition man uppnått *efter* genomgången utbildning.

Tidigare forskning

Metodologiskt är det svårt att korrekt mäta den kausala effekten av utbildning. Utifrån tvärsnittsstudier finns sällan möjligheten att estimera exakt vad som är effekten av utbildning, och vilken del av sambandet som kan förklaras av andra, oftast inte direkt mätta, faktorer. Randomiserade experiment utgör standarden för att estimera kausala effekter (Gerber och Green 2012). Men i det här fallet är det inte möjligt att genomföra ett randomiserat experiment eftersom det skulle innebära att olika individer slumpmässigt skulle behöva tilldelas olika utbildningar. Av etiska skäl är det naturligtvis inte möjligt att göra sådana ingrepp i människors liv.

Vad som då återstår är att utifrån forskningsdesign som är utformade för att pröva kausalitet och/eller medieringsanalys försöka undersöka effekter av utbildning. Ett antal studier har använt sig av sådana forskningsdesign och inte funnit några effekter av utbildning på politiskt deltagande. Några exempel är studier av naturliga experiment i form av utbildningsreformer (Pelkonen 2012; Persson och Oscarsson 2010), studier av lottning till militärtjänstgöring (vilket utgör en exogen chock på utbildningsnivån) (Berinsky och Lenz 2011), samt studier som använder sig av så kallad matchning för att jämföra individer med högre utbildning gentemot individer som är så lika dessa på alla relevanta variabler utom just högre utbildning (Kam och Palmer 2007).

Men det finns också ett antal studier som använder sig av forskningsdesign lämpade att pröva kausala effekter och som visar att utbildning faktiskt har effekt på politiskt deltagande. Sondheimer och Green (2009) utnyttjar tre fältexperiment där olika program för att öka studieresultat genomförts (mindre klasser, extra stödundervisning, etc). Elever som deltog i dessa aktiviteter tillskansade sig i genomsnitt därefter en högre grad av utbildning och som vuxna röstade de också i större utsträckning. Milligan, Moretti, och Oreopoulos (2004) använder förändrade skollagar vilka reglerar hur länge unga måste gå i skolan som så kallade instrumentvariabler för att mäta effekten av utbildning. De kommer till slutsatsen att utbildning påverkar valdeltagande i USA, men inte i Storbritannien. En liknande studie av Dee (2004) använder förutom skollagar också geografisk distans till högre utbildning som instrumentvariabel för att mäta effekten av utbildning. Även denna studie påvisar positiva effekter av utbildning på politiskt deltagande. Det finns alltså avancerade studier som kommit fram till att utbildning saknar kausal effekt, men det finns även studier som visar nollresultat.

De studier som tidigare nämnts behandlar utbildningslängd, men ett antal studier argumenterar också för att utbildningsinriktning – i första hand samhällsvetenskapliga utbildningsinriktningar – positivt påverkar politiskt deltagande (Hillygus 2005, Niemi och Hanmer 2010). De allra flesta av dessa studier angående utbildningsinriktning är dock tvärsnittsstudier utifrån vilka man inte kan dra slutsatser om huruvida sambandet är kausalt eller ej. Behovet av studier som undersöker de kausala effekterna av utbildningsinriktning är stort.

Vad säger då tidigare forskning om relativa utbildningseffekter? Nie, Junn och Stehlik-Barry (1996) prövade i sin studie sorteringsmodellen på ett antal indikatorer på politiskt deltagande. Deras slutsats är att ökad

utbildning i befolkningen inte leder till mer politiskt deltagande utan endast till utbildningsinflation. Deras resultat har dock ifrågasatts i en rad efterföljande studier. Ett område där oenighet råder är på vilka former av politiskt deltagande som utbildning har en relativ effekt. Nie, Junn och Stehlik-Barry menar att alla former av politiskt deltagande påverkas på samma vis. Bland andra Campbell (2009) hävdar dock att det finns skäl att tro att modellen endast är tillämpbar på de former av deltagande som är sociala och kräver interaktion med andra människor. Det råder också oenighet om hur relativ utbildning bör definieras. De flesta studier på området testar effekten av individuell utbildning i relation till omgivningens utbildning. Om effekten av individens utbildning avtar då omgivningens utbildningsnivå stiger tolkas det som stöd för tesen om relativa utbildningseffekter. Frågan är då hur omgivningens utbildning ska definieras. Även här råder oenighet inom fältet: vissa menar att omgivningens utbildning ska definieras snävt geografiskt och brett åldersmässigt medan andra argumenterar för det motsatta (Tenn 2005, Helliwell och Putnam 2007, Campbell 2009).

Artiklarna i avhandlingen

Samtliga tre teoretiska modeller är svåra att pröva i samma studie eftersom det inte existerar någon undersökning som mätt alla relevanta faktorer vilka hade varit nödvändiga för att utföra ett sådant test. Av det skälet undersöker de fem artiklarna i avhandlingen olika aspekter av de tre teoretiska modellerna. De inledande tre artiklarna undersöker sorteringsmodellen om relativa utbildningseffekter och de senare två undersöker om utbildning har någon kausal effekt eller om sambandet kan förklaras med hänvisning till selektionseffekter.

Artikel 1.

Den första artikeln – *An empirical test of the relative education model in Sweden* – undersöker om effekten av utbildning är relativ i den svenska kontexten (Persson 2010). Trots att det paradoxala sambandet mellan utbildning och deltagande – mer utbildning på makronivå tycks inte ha lett till högre aggregerade nivåer av politiskt deltagande, oavsett det starka sambandet på individnivå – gör sig gällande i de flesta länder i västvärlden, har modellen inte tidigare testas utanför USA. Analysen bygger på data från de svenska valundersökningarna från 1985 till 2006 i kombination med data om utbildningsnivåer i svenska kommuner. Den svenska kontexten utgör ett hårt test på modellen i och med den högre graden av social

mobilitet och jämlikhet i Sverige jämfört med i USA. Dessutom finns det i den svenska kontexten fler alternativa vägar in i politiken där utbildningsnivå inte är helt central, exempelvis via arbetarrörelsen. I artikeln undersöks fyra former av politiskt deltagande: valdeltagande, partimedlemskap, aktivt partiengagemang, och att skriva brev till politiker. Resultaten visar visst stöd för sorteringsmodellen på röstning och aktiviteter relaterade till politiska partier. Den relativa utbildningseffektsmodellen får därmed alltså, åtminstone delvis, stöd även i den svenska kontexten.

Artikel 2.

Den andra artikeln – *Social network position mediates the effect of education on political participation* – presenterar ett mer detaljerat test av den kausala mekanism som sorteringsmodellen pekar ut (Persson 2015). Forskningen i kölvattnet av Nie, Junn och Stehlik-Barrys studie har endast undersökt de observerbara implikationerna av modellen men inte direkt studerat den mekanism som pekas ut, det vill säga social status definierat som central nätverksposition (Campbell 2009; Helliwell och Putnam 2007; Tenn 2005). Den huvudsakliga anledningen till detta är troligen bristen på tillförlitlig data om individers sociala nätverk. Den enda studie som hittills testat den mellanliggande variabeln är Nie, Junn och Stehlik-Barrys studie från 1996. Deras resultat pekar på att sociala nätverk medierar majoriteten av sambandet mellan utbildning och politiskt deltagande. Deras analyser är dock problematiska av flera skäl. För det första använder de en relativt enkel statiskt metod (stiganalys) som inte ger information om huruvida den indirekta effekten är statistiskt signifikant. För det andra är de nätverk som undersöks kontakter till personer som arbetar med politik och media, en typ av nätverk som i själva verket mycket väl kan vara en konsekvens av politiskt engagemang och inte en orsak till detsamma. Det här är ett svårlösligt metodologiskt problem, men ett bredare och bättre mått på sociala nätverk skulle göra problemet mindre.

Analyserna i artikeln baseras på SOM-undersökningen från 2001. Den beroende variabeln i artikeln är aktivt partiengagemang. I 2001 års SOM-undersökning inkluderades ett unikt frågebatteri angående relationer till individer med olika yrkestillhörighet. Listan omfattar yrkeskategorier så som polis, professor, journalist, etc. I analyserna används 20 stycken indikatorer för att skapa en latent variabel i en strukturell ekvationsmodell. Resultaten visar att starka band till ett brett nätverk av högstatusindivider medierar sambandet mellan utbildning och deltagande. Studien ger därmed stöd till sorteringsmodellen så till vida att den bekräftar den kausala mekanism som

modellen pekar ut.

Artikel 3.
Den tredje artikeln – *Is the effect of education on voter turnout absolute or relative? A multi-level analysis of 37 countries* – är den första länderkomparativa studien av relativa utbildningseffekter (Persson 2013). Den kombinerar data från The Comparative Study of Electoral Systems och European Social Survey som tillsammans omfattar omkring 275 000 individer i 173 land–år i 37 länder. Studien fokuserar på valdeltagande som beroende variabel. Ett problem med komparativa data är att det saknas standardiserade mått på utbildningslängd i de olika länderna. Därför presenterar artikeln ett alternativt sätt att pröva relativ utbildning, närmare bestämt genom ett mått på varje individs relativa utbildningsranking inom respektive femårskohort inom varje land. Resultaten visar att när det relativa utbildningsmåttet inkluderas i modellerna så reduceras kraftigt den absoluta effekten av utbildning. Det tycks alltså inte vara så att vissa utbildningar har stabila effekter över tid och i olika länder, effekten av utbildningsnivå är snarare beroende av omgivningens utbildningsnivå. Undersökningen visar också att skillnaderna i valdeltagande mellan individer med hög respektive låg relativ utbildning är som störst när nivån på valdeltagandet är låg.

Artikel 4.
Den fjärde artikeln – *Testing the relationship between education och political participation using the 1970 British Cohort Study* – undersöker effekten av högre utbildning genom matchning (Persson 2014). Idén med matchning är att efterlikna en experimentell situation. Genom matchning skapas en jämförelsegrupp med personer med lägre utbildning men som i övrigt är så lika personerna med högre utbildning som möjligt.

Artikeln använder data från The British Cohort Study vilket är en undersökning som följer alla som föddes i Storbritannien under en vecka 1970. Studien innehåller bland annat variabler som mäter kognitiv förmåga vid fem och tio års ålder, familjens socioekonomiska status och sociala och kulturella aktiviteter. Därmed ger dessa data en möjlighet att matcha på en rad relevanta variabler som tidigare forskning inom området inte har haft tillgång till.

Analyserna visar att före matchning bekräftas den gängse bilden av sambandet; det finns signifikanta skillnader i politiskt deltagande mellan individer med högre utbildning jämfört med individer med lägre

utbildningsnivåer. Efter matchning reduceras dock skillnaderna mellan individer med olika utbildning kraftigt och inga statistiskt signifikanta skillnader kvarstår. Resultaten ger därmed stöd till idén om att utbildning i sig saknar kausal effekt och att sambandet i första hand kan förklaras med hänvisning till de selektionsprocesser som föregår utbildningsval.

Artikel 5.

I den femte artikeln – *Does type of education affect political participation? Results from a panel survey of Swedish adolescents* – riktas fokus mot utbildningsinriktning (Persson 2012). I den svenska kontexten finns en betydande skillnad i politiskt deltagande mellan individer som gått på teoretiska respektive yrkesförberedande gymnasieprogram. Frågan är dock om denna skillnad i deltagande är ett resultat av utbildningarna i sig eller om den kan förklaras i termer av selektionseffekter. För att undersöka detta följdes ett antal elever under deras första år i gymnasiet. Den första mätningen skedde strax efter att de just börjat gymnasiet och deras utbildningsinriktning inte ännu kunnat ha effekt och den andra mätningen skedde i slutet av det första läsåret. Skillnader mellan individer med olika typer av gymnasieutbildning existerar redan när de började på gymnasiet. Under det första året ökar inte skillnaderna mellan eleverna på teoretiska respektive praktiska program. Det mesta tyder alltså på att skillnaderna kan förklaras av andra faktorer än sådant som har med skolan att göra. Vissa faktorer i hemmiljön visar sig istället påverka både utbildningsval och politiskt deltagande.

Slutsatser

De tre första artiklarna i avhandlingen visar stöd för den relativa utbildningseffektsmodellen medan de sista två artiklarna visar stöd för att utbildning är en proxy för andra faktorer som föregår utbildningsval. Resultaten kan uppfattas som motstridiga och vidare studier bör utsätta de teoretiska modellerna för hårdare tester mot varandra. Överlag ger studierna svagt stöd till hypotesen att den läroprocess som sker genom deltagande i utbildning direkt påverkar politiskt deltagande. Utifrån sorteringsmodellen antas att social status främst påverkas av utbildning, men en möjlig förklaring till de till synes motstridiga resultaten i avhandlingen skulle kunna vara att även social status och relativ utbildning framförallt påverkas av tidig socialisation och medfödda förmågor. På så vis skulle hela sambandet kunna drivas av faktorer som opererar före utbildningsval. Och om detta stämmer skulle det vara så att personer från

gynnsamma förhållanden skaffar sig relativt sett hög utbildning i relation till dem i sin omgivning.

Resultaten har betydelsefulla implikationer för forskning om politiskt deltagande. Utbildning är en av de vanligaste kontrollvariablerna inom forskningsfältet, men det saknas en förståelse för exakt vad det är en kontroll för. Även om studierna presenterade här ger några pusselbitar så saknar vi fortfarande en heltäckande förståelse för hur utbildning påverkar politiskt deltagande.

Resultaten har också relevanta policyimplikationer. Ojämlikt politiskt deltagande pekas ofta ut som ett demokratiskt problem eftersom det innebär ojämlikt inflytande på politiken. I diskussioner om hur man skulle kunna höja medborgarnas politiska deltagande är ett vanligt förekommande argument att öka och förbättra medborgarnas utbildning. Om den konventionella synen på utbildningseffekter vore riktig skulle det också implicera att nivån på politiskt deltagande skulle kunna höjas genom att förlänga och förbättra medborgarnas utbildning. Men om utbildning endast är en proxy för andra faktorer och utbildning därmed i sig saknar direkta effekter på politiskt deltagande är det mindre troligt att förbättrad och förlängd utbildning genererar mer politiskt engagemang i samhället.

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