

# The Effects of Having Peers Whose Parents Are Politicians

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## Abstract

We estimate the causal effect of having classmates during upper secondary school whose parents are politicians on individual political participation as an adult. In this paper we measure political participation as voting in the European parliament election in 2009, voting in the general election in 2010, ever being nominated to political office, and lastly as ever being elected to political office after upper secondary school. We use unique register data material from Sweden to link individuals to their parents. We estimate a causal effect by comparing individuals who were enrolled in the same upper secondary school and the same educational program but at different times with one another. Since students spend most of their time with other students of the same age, we argue that the number of politicians among their parents across different cohorts at the same school and program should be *as if* random. Our preliminary results indicate that voter turnout in 2009 and the probability of ever being nominated to run for political office increases if the number of politicians among the parents of classmates increases.

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# 1 Introduction

Spurred by the great boom of research on social capital (Putnam et al., 1993, Putnam, 2000), the few last decades have witnessed a resurgence of scholarly interest in the social dimensions of politics (McClurg, 2003, Zuckerman, 2005, Rolfe, 2012, Campbell, 2013). The basic intuition motivating this perspective is that citizens' decisions to participate in politics is not formed in a vacuum, but rather depends on their social surroundings (Lazarsfeld et al., 1944). The study of social networks, thus, becomes an integral part of understanding why individuals choose to exercise their fundamental democratic rights to vote or to run for political office.

Most research in this area so far has assessed only horizontal social networks evaluating how the discussion of politics with one's peers impacts one's political behavior and attitudes (Kenny, 1992, Huckfeldt and Sprague, 1995, La Due Lake and Huckfeldt, 1998, McClurg, 2003, Klofstad, 2007). Yet, as Smith (2016) notes, relatively less attention has been paid to empirically investigating the importance of vertical social ties connecting citizens to their elected representatives.

From a theoretical point of view, this state of affairs is somewhat surprising since it has long been assumed that personal acquaintance with elected politicians bolsters citizens' sense of political efficacy and political involvement. In its modern form this argument can be traced back to Dahl and Tufte's seminal contribution on the relationship between size and democracy (1973). One important reason citizens in smaller political units tend to be more politically active, they argue, is that individuals are more likely to both "know officials names and have attitudes about them" compared to those residing in larger political units (Dahl and Tufte, 1973, p. 64). More recently, Lassen and Serritzlew (2011) similarly found that internal political efficacy among citizens declined as Danish municipalities were amalgamated into larger units. A partial explanation for this, they argue, is that the reform made it less likely for citizens to have local politicians in their social network (Lassen and Serritzlew, 2011, 4). The plausibility of these arguments notwithstanding, systematic studies on the topic is largely lacking.

Whereas the lack of research on the participatory effects of political connections is surprising on theoretical grounds, it is more understandable from a methodological perspective. A first challenge concerns access to relational data. Studying whether social ties to elected officials increases political participation requires information on whether an individual is personally acquainted with a politician, which is rarely available in surveys that are typically used to study political participation. And even if available, the number of individuals having social ties to politicians are usually too few to allow for sufficient precision in the estimation of the effect of interest.

Another challenge comes from the fact that the group of citizens who know a politician are unlikely to constitute a random subset of the entire population. To argue that the difference in politically activity between those who do know a politician and those who do not is actually due to this factor, we must therefore employ more sophisticated research designs that make it credible, such as accounting for other observed and unobserved differences between the two groups. Achieving this is always very difficult, but

particularly so when working with small random samples.

In an attempt to overcome these challenges, we use unique Swedish population-wide administrative data to examine how adult political participation is affected by being acquainted with a politician during a person’s adolescence. More specifically, we estimate how voter turnout and the likelihood of running for office is affected by having had a classmate during upper secondary school whose parent was an elected politician.

One reason for focusing on the school context is that previous research has pointed to the school as an important arena for political socialization. For example, adult political engagement has been shown to be higher among those who participate in school politics (Fox and Lawless, 2005), those who discuss politics in class (Campbell, 2008), and among those who have been involved in extracurricular activities and youth voluntary associations (Beck and Jennings, 1982, Hanks, 1981, McFarland and Thomas, 2006, Smith, 1999, Verba et al., 1995).

There are also methodological reasons to study the importance of social networks within the school setting. Most importantly, we can utilize the fact that two individuals living in the same place are much more likely to be in the same class and thus spend time together if they are born in the same year. In this paper we will thus attempt to handle the selection problems discussed above by comparing students who went to the same school and educational program, but because they were enrolled in different years they were placed with different sets of classmates. Under the assumption, which we will argue for later, that the year-to-year variation in the number of politicians among the parents in a given program is “as good as random” this comparison will give us the causal effect of being socially linked to a politician through one’s class mates. Our estimates show that in fact political socialization in the school context has a profound impact on later political participation; every upper secondary school classmate whose parent was a politician results in a 12 percent higher probability of ever running for office and a small but notable increase in vote propensity.

The rest of the paper is organized as follows. First, we provide a brief overview of the theoretical argument forming the basis of our study. We then describe data and the institutional setting. Next we present the empirical framework and discuss our identification strategy. The paper then continues with a presentation and discussion of the main results as well as a robustness analysis. We end the paper with a conclusion.

## 2 Why should knowing a politician make citizens more politically active?

To reiterate a famous statement from Verba et al. (1995), when attempting to explain why some citizens are more politically active than others it is often more practical to turn the question around and ask why people *do not* take part in politics. Doing so, the authors continue, three main explanations immediately come to mind: “because they can’t; because they don’t want to; or because nobody asked” (Verba et al., 1995, 14). In thinking about why being personally acquainted with a politician may matter for political activity it is therefore fruitful to structure the discussion around these different

explanations.

Starting with the “can’t” explanation, the main reason why citizens feel they cannot participate in politics is that they lack the necessary resources in terms of time, money, and civic skills (Verba et al., 1995, 271). In well-functioning democracies we should not expect that merely knowing a politician will have an effect on resources such as time and money, whereas it is much easier to conceive how it could impact civic skills. In particular, it seems likely that social ties to elected officials can make citizens more aware of and knowledgeable about politics. Applied to the present case, there are, at least, two different ways in which having a classmate whose parent is an elected official can increase political knowledge. A first possibility is that fellow students acquire political knowledge directly from the parent in question, for instance through political discussions, when visiting the home of their classmate. Another possibility is that the effect is mediated through the child of the politician, i.e., it is the child who teaches his or her friends about politics through discourse at school.

A second reason for not participating in politics is that citizens “don’t want to” because they lack psychological engagement with politics (Verba et al., 1995, 269). An important argument in the literature on descriptive representation is that the presence of visible political role models can enhance political interest and efficacy among politically marginalized groups. For instance, there are studies showing that the presence of women or ethnic minorities in important political roles may render the members of these groups to increase their likelihood of paying attention to politics and, in turn, to experience that they are capable of affecting political outcomes. Analogously, it can be hypothesized that having a friend during adolescence whose parent holds political office may affect future political participation by increasing a student’s psychological engagement with politics.

Finally, a third way in which social networks can affect participation is through recruitment or by “being asked” (Verba et al., 1995, Teorell, 2003). Consequently, citizens who personally know a politician may be more politically active because they are more likely to receive requests for participation in various types of political events or activities. Again, this effect can once again be conceptualized as being either direct or indirect. That is, the request to take part in political action may come directly from the politically involved parent or it may be the child of the politician who persuade his or her classmates to engage in politics.

As this short discussion indicates there are a number of reasons why knowing a politician can impact citizens’ political participation. Based on the impressionable years model, advanced by prominent scholars of political socialization, we may further expect this effect to be particularly pronounced prior to adulthood when individuals are more politically malleable (e.g., Jennings, 2007, Stoker and Bass, 2011). Despite this, we still know relatively little about the effects on future political participation of maintaining social ties to elected politicians during adolescence. In the remaining parts of this study we therefore set out to shed more empirical light on this central and underexplored question.

### 3 Data

We use several data sources, all of which come from Swedish registers. A burgeoning scholarship has begun to use Swedish population-wide data in the study of political science and economics (e.g. see [Dancygier et al. \(2015\)](#) and [Folke et al. \(2016\)](#) for recent examples). These registers provide us with population-wide administrative data containing information on a robust range of individual-level characteristics, such as age, gender, family status, education, income, and residential location of the universe of the Swedish population in a given year. This data also allows us to draw relational conclusions. Because we can locate relationships between individuals and their relatives and individuals in classrooms with one another, we are able to ascertain who may or may not have had a relationship. We then combine this data with data on individual-level voter turnout, nomination, and elected politician data.

Our main variable of interest is the share of politicians among the parents of students. Because we have data on nominated and elected politicians beginning in 1982, this would optimally have been the first year in our dataset. However, a reform in the 1990s changed the upper secondary school system in Sweden. Prior to the reform, upper secondary school consisted of different educational tracks. After the reform, 16 national programs were instead introduced making it difficult to compare cohorts graduating before and after 1997. As a result, our main analyses focuses instead on the years after 1997.

We take advantage of the Swedish registers by first linking students to their parents using a multigenerational register. Then we link students to one another by using information from the upper secondary school application records, where we observe which school a particular student attended in the fall semester and which program within that school the students was enrolled in. In future iterations of this project, we will also run the entire analyses for the earlier cohorts as a robustness analysis.<sup>1</sup>

Our four dependent variables in the main analysis are voter turnout in the European Election in 2009, voter turnout in the general election in 2010, the probability of ever being nominated to political office, and the probability of ever being elected to political office after graduating from upper secondary school. The first two variables have been digitalized from printed election rolls meaning that we have individual voter turnout for almost the entire Swedish population. The last two variables are measured in the dataset of *elected and nominated* politicians where we have detailed individual information on which type of election a candidate was running and elected in.

We also control for an individual's future socioeconomic status by adding additional data on their place of residence at the time of the European election in 2009 and the general election in 2010, and their labor market status which we use in the robustness analysis. This data comes from the a labor market register from Statistics Sweden called LISA. We use the LISA data to include individual covariates and covariates for the parents at the time of graduation from upper secondary school.

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<sup>1</sup>The reader should also note that we have discovered a break in the time series for school codes in 2001. We are currently waiting for a key from SCB. The results in this paper will hence be based on a weighted average between the years prior and post 2001.

## 4 Institutional background

The Swedish educational system can be roughly divided into three different parts: elementary school, upper secondary school, and university education. Elementary school is mandatory. For the period that we focus on in this paper, elementary school in Sweden spanned nine years and students' ages ranged between 7 and 16. Although upper secondary school is voluntary in Sweden, around 98% of all students attend the three-year long upper secondary school, meaning that it is a large and important part of the Swedish education system (Skolverket, 2017).<sup>2</sup>

To pursue their education after elementary school and enter upper secondary school, which is roughly equatable to high school in the US, students apply to upper secondary school in the spring semester of the ninth grade in elementary school. At the time of application, students can choose which school to attend and which program to enter within that school. For the time period studied, students had the option of entering 16 national programs. There were also additional local programs that were only offered at specific schools.<sup>3</sup> Because some programs are more competitive than others, in many cases this means that students need to qualify with their grades from elementary school for a slot at upper secondary school. Both public and private schools function under a voucher system, meaning that students do not pay for their education. Private schools generally accept applicants from all over Sweden, whereas public schools offer education to those residing in the municipality where the school is situated. There are, however, some exceptions to this rule where a student may attend a public school in another municipality if the public schools in the home municipality do not offer a specific educational program. Urban municipalities often offer various public and private schools where the competition to be accepted to highly-regarded programs at certain schools is difficult. In the countryside, it is common that there is only one public school in a municipality and all students receive a place there after applying (although not necessarily in one of the popular educational programs). Certain private schools located in the country side also have high application numbers.

Students in upper secondary school enroll in one of the 16 national educational programs (known as tracks before 1997) where the programs are divided into two groups: those programs that prepare students to attend university and vocational programs. Examples of university preparatory programs are the natural science program and the social science program. The industry program and the construction program are examples of

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<sup>2</sup>There were also two year and four year programs in available for students, but not for the time period that we focus on in the main analysis.

<sup>3</sup>In order for us to make a comparison over the years, we use codes for the national programs and the local programs, but we restrict local programs codes to those programs where at least 100 students have attended. We choose this in order to avoid a scenario where we treat programs as separate programs because they have different program codes, but the programs are essentially the same. This could for instance happen if they renamed a local program.

vocational programs.<sup>4</sup> Generally, higher grades from elementary school are needed to be accepted to a university preparatory program in comparison to a vocational program.

For ease of interpretability, we refer to a specific program at a specific school as a class. We note that in large upper secondary schools, however, certain educational programs are divided into several classes. All students often share some education together, but some lectures are conducted in smaller groups. In order to facilitate interpretation of our empirical results, we rescale our variable of interest as the number of politicians among the parents in a class of 25 students.

Given that students apply to upper secondary school based on their grades, there is a great deal of self-selection into educational programs and into specific schools. The same problem arises, however, when considering other types of institutional settings. Self-selection into schools is a general problem which exists regardless if students compete with their grades from elementary school or not. Students are almost never randomized to schools meaning that all papers studying the peer effect of having classmates that are politicians needs to apply a proper identification strategy to study the causal effects. We return to this issue in the identification strategy section below.

## 5 Empirical framework

In this section we present our identification strategy, the model specification, and descriptive statistics.

### 5.1 Identification strategy

To understand how adult political participation is affected by being acquainted with a politician during a person's adolescence and to overcome the methodological limitations previewed in the introduction, we estimate the causal effect of having peers during upper secondary school whose parents are politicians on an individual's political participation as an adult. We are interested in the effect of having classmates where there is one additional parent politician among the parents of these classmates where the class consists of a small group of students.

Let  $i$  denote an individual and let  $X_i$  denote the number of politicians among the parents within a class that individual  $i$  attends. We would like to observe political participation,  $Y_i$ , if  $X_i$  is increased by one politician, meaning that we would like to observe  $Y_i(X_i + 1) - Y_i(X_i)$ , which is essentially the individual partial treatment effect.

Nevertheless, we may not observe both  $X_i$  and  $X_{i+1}$  for the same person. Just comparing political participation among students who were enrolled in a class where there were many politician parents to those students who attended a class where there were few or zero politician parents would not yield a causal effect. It is likely that students

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<sup>4</sup>In the beginning of the time period we examine, students who graduated from a vocational program were able to apply to university, but they had to compete with those who graduated from preparatory programs. This was later altered. After the reform, those who attended vocational program had to take extra classes before they could apply.

whose parents are politicians are more interested in politics and that they overall perform better in school. As a result, it is likely that they self-select into certain schools and certain programs when they apply to upper secondary school. Other students in those programs are also likely to have higher cognitive ability and higher SES status which we in turn expect to have positive effects on political participation. Because students are not randomly assigned to upper secondary schools and to particular programs within an upper secondary school, we are unable to separate the causal effect from selection bias without a proper identification strategy.

Our strategy is to compare students that attended the same upper secondary school and the same program within that upper secondary school with each other. Our variable of interest would as a result only vary over the different cohorts. Our general idea is that students spend most of their time together with other students of the same age given that education at a upper secondary school is organized around each cohort separately. The identification strategy assumes that it is *as if* random which specific cohort a person belongs to for a given upper secondary school and for a given program within that upper secondary school. The potential spillover effect from having a parent that is a politician should as a result affect the students at the same age.

## 5.2 Model specification

We estimate the following regression equation in the main analysis:

$$Y_i = \beta_0 + \beta_1 X_{spc} + \beta_2 W_i + \beta_3 W_{spc} + \beta_4 W_p + f_{sp} + u_i$$

$Y_i$  is the dependent variable outcome for individual  $i$  and  $u_i$  is the error term. All dependent variables are expressed in percentage points on a scale between 0 and 100 in order to facilitate interpretation.

$X_{spc}$  is the variable of interest which is a variable equal to the number of politicians in office among the parents in a class for a given upper secondary school,  $s$ , a given program  $p$  within that upper secondary school and a given cohort  $c$  attending that program. The number of students in a class is on average 64 (we return to this in more detail in the descriptive statistics section) meaning that we need to rescale this variable in order to make it interpretable across different classes. We choose to express this variable as the number of politician parents per 25 students. We are not interested of the intergenerational effect of having a parent that is a politician. When running the analysis, we therefore drop all students whose parents are elected politicians and focus the entire empirical analyses on the spill-over effects to other students in a class.

It could be that the number of politicians in a class picks up other treatments because elected politicians are so different from other people. For instance, they could be wealthier and better educated. In order to take this into account we include a vector of covariates,  $W_{spc}$ , for average income, average years of education, average employment and average prevalence of welfare recipients among the parents in a class. We also control for the same variables for the parents of individual  $i$  in the vector  $W_p$ . Lastly, we

include individual covariates in  $W_i$  which consists of dummy variables for gender and whether the individual is an immigrant or not.

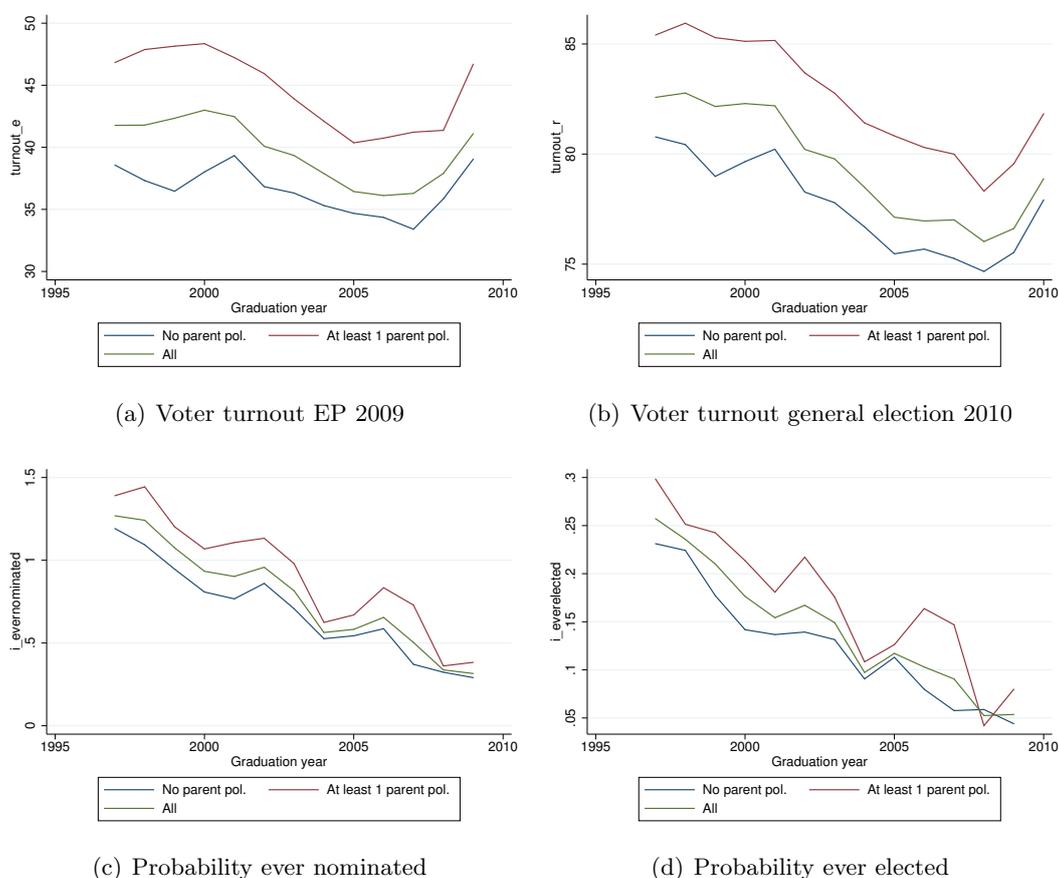
Most importantly in terms of identification, we include fixed effects where  $f_{sp}$  are unique indicators for each school-program combination. We also add separate cohort fixed effects and municipal fixed effects for the municipal of residence in 2009 (for voter turnout in 2009), municipal of residence in 2010 (for voter turnout in 2010), and municipality of residence at the year of graduation (for the last two outcome variables) in auxiliary model specifications.

The standard errors are clustered at the same level as the fixed effect, the school-program level. This clustering level will be used throughout the paper.

### 5.3 Descriptive statistics

The first descriptive statistic that we display in figure 1 is the mean of our dependent variable for different cohorts split by those who had at least one classmate whose parent was a politician and those who had no classmates whose parents were politicians. We also display the average in all four subfigures. From figure 1 it is clear that there is a positive association between having politician-parents and for all outcome variables. What is also clear is that voter turnout is higher for first time voters in the 2009 and 2010 election respectively in comparison to almost first time voters. For older cohorts, voter turnout is also higher. Unsurprisingly, the probability of being either nominated or elected is higher for older cohorts. While this of course does not translate any evidence for any causal effects, it sets the stage for our main analysis later on.

Figure 1: Long term trends outcome variables



The variable of interest in this paper varies for each upper secondary school, for each program and for each cohort. An important descriptive statistics is to see how many students there are in such a group. Our basic idea is that people are affected by their peers, but if the numbers of peers are too large is unlikely that a student,  $i$ , is friends with the particular students whose parents are politicians. Simply put, it is unlikely that we may capture the true spillover effect of having a classmate that is a politician. Table 1 displays the summary statistics for the variable of interest. The mean number of students is 64 with a maximum of 388. As already discussed, we express our variable of interest as the number of politicians among the parents of 25 students. Figure 2 shows that the number of students for a given school, a given program and a given cohort has decreased over time. Figure 2 displays the distribution of number of students for a given school, program and cohort in a histogram. In Table 2 we display descriptive statistics for the variables of interest.

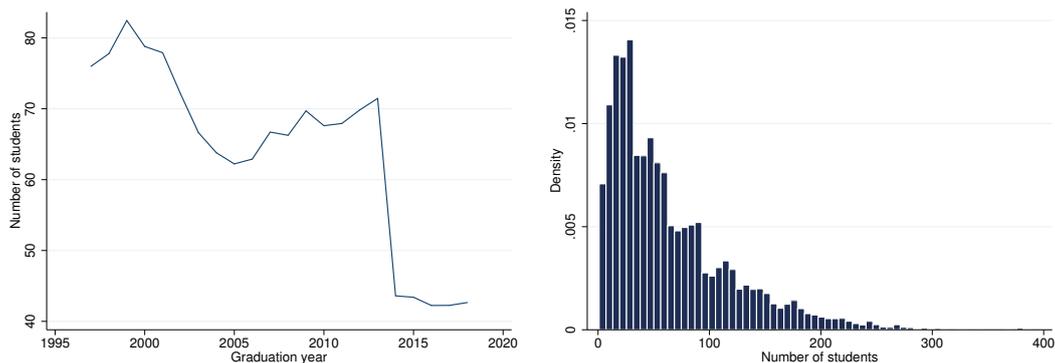
Table 1: Number of students in a cohort, in a program for a given highschool (a class)

	Mean	SD	Max	Min
TreatmentgroupNS	64.2	54.8	388.0	1.0
Observations	2106561			

Table 2: Share politician parents in a class, summary statistics

	Mean	SD	Max	Min
Share parents in pol. office	0.004	0.009	0.5	0.000
Share parents that was ever elected	0.009	0.015	0.50	0.000
Share parents in a class elected during highschool	0.002	0.007	0.50	0.000
Observations	2097645			

Figure 2: Descriptive figures



(a) Number of students in treatment group over time (b) Histogram number of students in a "class"

## 6 Main Results

In this section, we present the main results of the paper. Because we examine voter turnout (mass participation) and the probability of being nominated and elected, we have divided the tables into two, where the results for the dependent variables on voter turnout are presented in table 3 and the results for elite participation are presented in table 4. The structure of each table is the same for all outcomes where the first column is the simplest specification and then we add additional covariates and fixed effect sequentially. The mean of the outcome variable is presented in the bottom to facilitate a discussion concerning practical significance – an issue we return to in the end of this section. The last column includes all fixed effects and covariates. This last specification also includes the lagged and the lead value of the variable of interest, i.e. the number of politicians in a class for a given program for the cohort above and below the cohort in question.

Let us start with our results for voter turnout in the 2009 European Election. We may conclude that the variable of interest is positive, statistically significant, and fairly stable in magnitude across different specifications. In terms of magnitude, an increase of one politicians among the parents of a group of 25 students increases voter turnout by approximately 0.3 percentage points in 2009. Moving on to voter turnout in the general election in 2010, we estimate coefficients that are smaller in magnitude and the coefficients are neither statistically significant for more restrictive specifications. One important difference between the European election and the general election is that the voter turnout rate in general is much higher in the general election, meaning that we have less variation in our dependent variable. Given that voter turnout in Sweden is higher than 80% in general elections, but only around 40% in European elections, one could argue that people in general vote in general elections in Sweden, but that their choice whether to vote or not in European elections is more likely to be influenced by other factors – in this case having peers whose parents are politicians during upper secondary school.

The last two outcome variables are closely connected to each other. Being nominated to political office is naturally more common than being elected to political office and given that these individuals graduated between 1997 and 2009 the probability that they are either nominated or elected to political office is small on average. Table 4 displays the results for both these outcomes. The estimated coefficient for the variable of interest with regards to the probability of being nominated is positive. If we consider column 5, the probability that an individual is ever nominated to political office increases by 0.087 percentage points if number of politicians among the parents of 25 students is increased by one politician. In order for this to make sense, we need to make a relative comparison in relation to the average probability that someone in these cohorts ever is being nominated to political office which is 0.742 percentage points. In relative terms, the estimated increase is around 12 percent in comparison to the mean value of the Y-variable. With regards to the last outcome, the estimated coefficients are generally positive but smaller in comparison to the results for nominated. Considering column

Table 3: Voter turnout

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Voter turnout in 2009</i>					
Parents in office	0.405*** ( 0.116)	0.398*** ( 0.115)	0.315*** ( 0.119)	0.305** ( 0.118)	0.187 ( 0.166)
Constant	39.208*** ( 0.028)	42.756*** ( 0.140)	6.887*** ( 1.396)	8.506*** ( 1.731)	6.842*** ( 2.215)
<i>Panel B: Voter turnout in 2010</i>					
Parents in office	0.156 ( 0.099)	0.189* ( 0.097)	0.157 ( 0.101)	0.122 ( 0.096)	0.096 ( 0.133)
Constant	79.567*** ( 0.024)	80.262*** ( 0.112)	64.580*** ( 1.219)	67.762*** ( 1.348)	67.551*** ( 1.721)
Mean DV Panel A	39.306	39.307	40.001	40.728	40.964
Mean DV Panel B	79.604	79.608	80.387	81.568	81.588
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Obs. Panel A	1 082 071	1 082 027	985 911	962 036	624 610
Obs. Panel B	1 071 243	1 071 199	979 058	955 212	620 263
Adjusted R2 Panel A	0.075	0.082	0.096	0.120	0.123
Adjusted R2 Panel B	0.059	0.069	0.073	0.140	0.138

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

5, the point estimates equals 0.007, meaning that the probability of ever being elected increases by 0.007 percentage points if the variable of interest increases by one politician. Given the small probabilities overall that ever being elected for those belonging to these cohorts, the relative size of the point estimated in comparison to the mean value of the outcome variable is around 4.5%.

Table 4: Probability that the individual is nominated to political office

	(1)	(2)	(3)	(4)	(5)
<i>Panel A: Ever becoming nominated</i>					
Parents in office	0.024 ( 0.026)	0.043* ( 0.026)	0.058** ( 0.027)	0.057** ( 0.028)	0.087** ( 0.041)
Constant	0.737*** ( 0.006)	1.053*** ( 0.028)	0.681*** ( 0.250)	0.582** ( 0.295)	0.526 ( 0.380)
<i>Panel B: Ever becoming elected</i>					
Parents in office	-0.002 ( 0.011)	0.001 ( 0.011)	0.002 ( 0.011)	0.001 ( 0.011)	0.007 ( 0.014)
Constant	0.138*** ( 0.003)	0.187*** ( 0.012)	0.287*** ( 0.105)	0.361*** ( 0.133)	0.362** ( 0.155)
Mean DV Panel A	0.743	0.743	0.742	0.751	0.740
Mean DV Panel B	0.137	0.137	0.137	0.139	0.133
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Obs. Panel A	1 082 071	1 082 027	985 911	962 036	624 610
Obs. Panel B	1 082 071	1 082 027	985 911	962 036	624 610
Adjusted R2 Panel A	0.002	0.003	0.003	0.004	0.004
Adjusted R2 Panel B	-0.001	-0.001	-0.001	0.000	0.000

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

## 6.1 Are our results significant in practice?

We generally find statistically significant results for two of our outcome variables, but we need to assess further whether these estimates are practically significant. Statistical significance is not a sufficient condition for refuting a null hypothesis and several researchers have argued that statistical significance testing is misused in the social sciences (McCloskey and Ziliak, 1996, Ziliak and McCloskey, 2004). This subsection therefore contains a discussion about the size of our coefficients and a comparison to the earlier literature. We focus our discussion on the estimated effect for voter turnout in the European election in 2009 given that elite participation is not that common when studying political participation.

To begin with, we are considering a rather weak treatment. Our treatment is weak in the sense that we measure the outcome variable several years after the treatment take

place meaning that we are in fact studying the long term effects of having peers whose parents are politicians. Our time horizon is between 0–12 years long considering all included cohorts. A valid comparison to the earlier literature should therefore especially be on long term effect on voter activation measures. And we note that there are most likely many other sorts of treatments with much larger effects on voter turnout in an election.

Consider the point estimates table 3 which are around 0.3 percentage points. Let us compare this to GOTV-treatments that are often recommended as a way of increasing voter turnout. Green et al. (2013) reviews this literature. They conclude that the average effect of sending out a mail reminding people to vote in an upcoming election is 0.162 percentage points increase in voter turnout with a 95 % confidence interval of 0.078–0.247. Our results are thus larger in comparison to this treatment effect.

Moreover, and importantly, the effect we document is a long-term effect in comparison to the mail effect, which is a short-term effect. Green et al. (2013) also demonstrate that the average direct effect of door-to-door canvassing is an increase in voter turnout by 2.5 percentages points. Prerecorded telephone GOTV treatments seems on average to increase voter turnout by 0.156 percentage points which is hence about half of our estimated long-term effect.

Gerber et al. (2003) investigate the long-term effects on voter turnout in their field experiment in New Haven where voters received treatment in the form of mailing or door-to-door-canvassing. Voter turnout was measured in the elections of 1998 and 1999. The one-year effect on voter turnout in the subsequent election in 1999 was 1.1 percentage points increase by door-to-door-canvassing in 1998 and 1.9 percentage points increase by receiving three mailed postcards encouraging the individual to vote in 1998 election.

In sum, our estimates of the effect on voter turnout are not at all negligible in light of the previous literature’s findings.

## 7 Robustness analysis

In this section, we present a robustness analysis addressing several concerns. The reader should note that this robustness section is work in progress. First, we run an analysis where we consider the parents who have been elected to political office, but not when their children attends upper secondary school. This specification addresses the mechanism whether the treatment effect really takes place when a student attends upper secondary school. Second, we run a placebo tests to assess our identification strategy where we focus on GPA from ninth grade to see if there is any remaining self-selection effect left. Next, we present the results for more restrictive specifications where we interact our school-program fixed effect with family fixed effects and place of residence fixed effects.

## 7.1 Placebo analysis: Elected but not during upper secondary school

Table 5: Voter turnout EU election in 2009

	(1)	(2)	(3)	(4)	(5)
Par. elec. but not during highschool	0.1967** (0.0986)	0.0940 (0.0959)	0.0427 (0.1002)	0.0560 (0.1006)	0.0569 (0.1378)
Constant	39.1805*** (0.0305)	43.9346*** (0.1415)	8.7340*** (1.3565)	8.7506*** (1.7300)	8.2846*** (2.2037)
Mean dep.var.	39.241	39.243	39.951	40.022	40.368
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.077	0.084	0.098	0.102	0.106
Observations	1178624	1178578	1074016	1069512	722079

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

Table 6: Voter turnout in general election in 2010

	(1)	(2)	(3)	(4)	(5)
Par. elec. but not during highschool	0.2850*** (0.0862)	0.1503* (0.0853)	0.1046 (0.0885)	0.1061 (0.0839)	0.0721 (0.1148)
Constant	79.1280*** (0.0267)	80.0757*** (0.1103)	64.1800*** (1.1778)	66.9895*** (1.3194)	66.5947*** (1.6499)
Mean dep.var.	79.216	79.219	79.994	81.294	81.209
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.060	0.070	0.075	0.147	0.143
Observations	1165988	1165942	1066181	1038436	702010

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

Table 7: Probability that the individuals is nominated to political office

	(1)	(2)	(3)	(4)	(5)
Par. elec. but not during highschool	0.0564*** (0.0186)	0.0301 (0.0185)	0.0330* (0.0191)	0.0317* (0.0191)	0.0343 (0.0257)
Constant	0.6340*** (0.0058)	0.9269*** (0.0250)	0.7251*** (0.2266)	0.9964*** (0.2830)	0.9863*** (0.3624)
Mean dep.var.	0.651	0.651	0.649	0.650	0.627
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.002	0.003	0.003	0.003	0.003
Observations	1178624	1178578	1074016	1069512	722079

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

Table 8: Probability that the individuals is elected to political office

	(1)	(2)	(3)	(4)	(5)
Par. elec. but not during highschool	0.0015 (0.0075)	-0.0036 (0.0075)	0.0001 (0.0078)	-0.0000 (0.0079)	0.0077 (0.0109)
Constant	0.1181*** (0.0023)	0.1666*** (0.0108)	0.2787*** (0.0927)	0.3655*** (0.1315)	0.4361*** (0.1692)
Mean dep.var.	0.119	0.119	0.118	0.118	0.111
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	-0.001	-0.000	-0.000	-0.000	0.000
Observations	1178624	1178578	1074016	1069512	722079

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

## 7.2 Placebo analysis for educational outcomes

In this section we run our analyses on placebo outcomes to assess whether there is any remaining self-selection left after we have implemented our identification strategy. Our prior is not to find any substantial effect on GPA from ninth grade since the variable is measured before our treatment takes place.

We standardize GPA with mean 0 and standard deviation 1 for each cohort in order to compensate for grade inflation. In table 9 we find some statistically significant results

which is a bit puzzling. The coefficients are however small. If we consider column 5 in the table, it means GPA is increased by 0.0035 standard deviations if there is one additional parent in a class of 25 students in upper secondary school.

Table 9: Standardized GPA from ninth grade

	(1)	(2)	(3)	(4)	(5)
Parents in office	0.0071*** (0.0025)	0.0083*** (0.0024)	0.0044* (0.0023)	0.0045* (0.0023)	0.0035 (0.0032)
Constant	-0.0019*** (0.0006)	-0.0666*** (0.0035)	-1.0483*** (0.0324)	-0.9844*** (0.0369)	-0.9341*** (0.0449)
Mean dep.var.	-0.000	-0.000	0.026	0.026	0.038
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.461	0.486	0.507	0.508	0.508
Observations	1176721	1176677	1077648	1073164	724918

Standard errors in parenthesis are clustered on school-program. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

### 7.3 Family fixed effect interaction

In the following tables we restrict the analyses to siblings who have attended the same upper secondary school and the same program, but at different points in time. Interestingly, we estimate larger coefficient for the first three outcome variables in tables 10–12. For the last outcome in table 13 – probability of being elected after upper secondary school to political office, the point estimate jumps around for different specifications. All of these estimates should be interpreted with caution given that they are not statistically significant which is probably a result of having less variation in our variable of interest.

Table 10: Voter turnout EU election in 2009

	(1)	(2)	(3)	(4)	(5)
Parents in office	0.4177 (1.9071)	0.5581 (1.9165)	0.7090 (2.3196)	0.7071 (2.3732)	0.8046 (3.5435)
Constant	39.3558*** (0.4446)	41.5941*** (2.2295)	23.4491 (60.6788)	31.6759 (73.9175)	50.8171 (106.8638)
Mean dep.var.	39.453	39.454	40.167	40.238	40.583
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.247	0.249	0.299	0.300	0.326
Observations	1193116	1193070	1087831	1083273	731043

Standard errors in parenthesis are clustered on school-program. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 11: Voter turnout general election 2010

	(1)	(2)	(3)	(4)	(5)
Parents in office	0.0960 (1.6807)	0.0822 (1.6897)	0.1469 (2.0140)	0.0889 (1.9720)	0.2730 (3.0284)
Constant	79.2994*** (0.3929)	77.3439*** (1.8291)	82.4614* (49.6602)	85.7339* (51.0647)	105.7819 (80.3583)
Mean dep.var.	79.322	79.325	80.097	81.397	81.312
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.234	0.237	0.340	0.407	0.410
Observations	1180433	1180387	1079959	1051803	710724

Standard errors in parenthesis are clustered on school-program. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 12: Probability of being nominated

	(1)	(2)	(3)	(4)	(5)
Parents in office	0.1085 (0.6011)	0.1318 (0.6048)	0.2605 (0.7674)	0.2806 (0.7830)	0.5868 (1.4739)
Constant	0.6730*** (0.1401)	0.8765** (0.4268)	-1.0287 (13.5150)	-1.0195 (13.8817)	3.2126 (23.2214)
Mean dep.var.	0.698	0.698	0.698	0.700	0.675
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.022	0.023	0.056	0.054	0.041
Observations	1193116	1193070	1087831	1083273	731043

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

Table 13: Probability of being elected

	(1)	(2)	(3)	(4)	(5)
Parents in office	-0.0081 (0.1552)	-0.0098 (0.1571)	0.0058 (0.2043)	0.0048 (0.2076)	0.0087 (0.3041)
Constant	0.1303*** (0.0362)	0.1483 (0.1789)	0.5509 (6.3588)	0.9895 (6.2655)	-0.3344 (13.1680)
Mean dep.var.	0.128	0.128	0.128	0.128	0.120
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	-0.058	-0.059	-0.035	-0.033	0.030
Observations	1193116	1193070	1087831	1083273	731043

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

## 7.4 SAMS interacted

In the following analysis we interact SAMS area of residence together with our class indicator. A SAMS area is the smallest geographical unit that can be found in the Swedish registers. It could be that students that attends the same school and the same program, but also live close to each other spend more time together in comparison to those who live in another neighborhood. We use the information of place of residence

on the SAMS level and interact it with our school-program fixed effect.

Table 14: Voter turnout EU election in 2009

	(1)	(2)	(3)	(4)	(5)
Parents in office	0.4625** (0.1824)	0.5683*** (0.1829)	0.4895** (0.1942)	0.4902** (0.1944)	0.4374 (0.2771)
Constant	39.3453*** (0.0425)	43.7716*** (0.1950)	9.3990*** (2.2582)	9.4734*** (2.2609)	9.1659*** (3.0481)
Mean dep.var.	39.453	39.454	40.167	40.238	40.583
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.091	0.096	0.107	0.107	0.111
Observations	1193116	1193070	1087831	1083273	731043

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

Table 15: Voter turnout general election 2010

	(1)	(2)	(3)	(4)	(5)
Parents in office	0.0974 (0.1597)	0.1342 (0.1601)	0.1576 (0.1681)	0.1203 (0.1637)	0.0930 (0.2290)
Constant	79.2991*** (0.0373)	79.8121*** (0.1548)	65.6021*** (1.9972)	68.4489*** (2.3226)	65.2590*** (3.0362)
Mean dep.var.	79.322	79.325	80.097	81.397	81.312
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	0.128	0.135	0.137	0.197	0.200
Observations	1180433	1180387	1079959	1051803	710724

Standard errors in parenthesis are clustered on school-program. \* p<0.1, \*\* p<0.05, \*\*\* p<0.01

Table 16: Probability of being nominated

	(1)	(2)	(3)	(4)	(5)
Parents in office	0.0251 (0.0421)	0.0425 (0.0422)	0.0567 (0.0457)	0.0541 (0.0458)	0.0813 (0.0664)
Constant	0.6925*** (0.0098)	1.0216*** (0.0389)	0.8196* (0.4351)	0.8187* (0.4356)	1.2858** (0.5609)
Mean dep.var.	0.698	0.698	0.698	0.700	0.675
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	-0.012	-0.012	-0.008	-0.007	-0.013
Observations	1193116	1193070	1087831	1083273	731043

Standard errors in parenthesis are clustered on school-program. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

Table 17: Probability of being elected

	(1)	(2)	(3)	(4)	(5)
Parents in office	-0.0101 (0.0178)	-0.0075 (0.0178)	-0.0029 (0.0193)	-0.0040 (0.0193)	-0.0080 (0.0221)
Constant	0.1308*** (0.0041)	0.1806*** (0.0176)	0.2637 (0.1869)	0.2656 (0.1872)	0.3053 (0.2460)
Mean dep.var.	0.128	0.128	0.128	0.128	0.120
I.covs	No	Yes	Yes	Yes	Yes
P.covs	No	No	Yes	Yes	Yes
Cohort FE	No	Yes	Yes	Yes	Yes
Muni FE	No	No	No	Yes	Yes
LagLead share	No	No	No	No	Yes
Adjusted R2	-0.010	-0.010	-0.005	-0.003	0.003
Observations	1193116	1193070	1087831	1083273	731043

Standard errors in parenthesis are clustered on school-program. \*  $p < 0.1$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

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