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Can increased education help reduce the political opportunity gap?

Karl-Oskar Lindgren
Sven Oskarsson
Mikael Persson

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Can Increased Education Help Reduce the Political Opportunity Gap?[a]

by

Karl-Oskar Lindgren[b], Sven Oskarsson[c] and Mikael Persson[d]

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Abstract
It is well documented that voter turnout is lower among persons who grow up in families of low socio-economic status compared to persons from high-status families. This paper examines whether reforms in education can help to reduce the socio-economic gap in voting. We distinguish between reforms of two types that may lead to differences in the exercise of voting; (a) changes in the resources allocated to education between different socio-economic groups (reform effects) and (b) changes in return which relate to the impact of education on turnout in different groups. We use this framework to analyze a reform of the Swedish upper secondary school system in the 1990s. This reform increased the length and amount of social science education on vocational training programs. We find that the reform reduced the gap in voting mainly by means of its stronger influence among individuals from families of low socio-economic status.

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[b]IFAU, Department of Government, Uppsala University, and UCLS, karl-oskar.lindgren@statsvet.uu.se
[c]Department of Government, Uppsala University, and UCLS, sven.oskarsson@statsvet.uu.se
[d]Department of Political Science, University of Gothenburg, mikael.persson@pol.gu.se

Table of contents

1 Introduction

In a democracy, political participation is the most basic means of voicing political concerns and influencing public policy. It is therefore a problem if groups in society differ in their capacity or willingness to participate in politics. Passive groups risk having their interests neglected (Verba et al., 1995; Lijphart, 1997; Schlozman et al., 2012). Differences in political involvement related to family background are especially problematic because they violate the basic democratic principle of *equality of political opportunity*. Indeed, as Robert Putnam (2015) has pointed out, inherited political inequality brings us uncomfortably close to the type of political regimes at which democratic revolutions were once targeted.

Despite its importance, the relationship between social origin and political participation remains fairly unexplored territory (Brady et al., 2015). The research that does exist, however, indicates that children of more advantaged parents are considerably more likely to grow up to become politically active citizens than children from less privileged homes (Verba et al., 1995, 2003; Cesarini et al., 2014; Gidengil et al., 2016; Lindgren et al., 2017). This raises the question of what can be done to help alleviate the gap in political opportunity.

Traditionally, political scientists of various persuasions have placed great hopes in the equalizing impact of improved educational standards (Nie et al., 1996). This argument, in turn, draws on a long tradition of research that depicts formal educational attainment as the most important resource for political participation (Converse, 1972; Wolfinger and Rosenstone, 1980). Recently, this conventional wisdom has, however, been questioned by scholars suggesting that the relationship between educational attainment and political participation may be spurious rather than causal (Tenn, 2007; Kam and Palmer, 2008; Berinsky and Lenz, 2011; Persson, 2014). More specifically, education is said to operate as a proxy for pre-adult experiences and predispositions that are consequential but difficult to observe. According to advocates of this perspective changes to the education system will therefore do little to reduce political inequality. Or, in the words of Berinsky and Lenz (2011, 371):

> Previous research indicating that education increased participation suggested a policy prescription for leveling the playing field: more education. But, education levels have risen over the past generation, yet participation levels have failed to increase. Our findings indicate that education may not be entirely "the great leveler" and may partly be just "the great proxy" of preexisting characteristics.

In the last decade, much effort has gone into trying to determine which of these two perspectives provides the better description of reality. In the process, scholars have used increasingly sophisticated research designs to gauge whether political participation is causally related to educational attainment. The designs used include techniques such as matching (Kam and Palmer, 2008; Henderson and Chatfield, 2011; Mayer, 2011), instrumental variable estimation (Berinsky and Lenz, 2011; Milligan et al., 2004; Dee, 2004), field experiments (Sondheimer and Green, 2010), and regression-discontinuity analysis (Solis, 2013; Persson et al., 2016). The methodological advances notwithstanding, the results from these studies point in different directions and fail to provide a clear answer to the question of whether education causes political participation.

Despite all its merits, a common limitation in recent research on the education-participation nexus is that it has mainly been concerned with estimating homogeneous treatment effects. The implicit assumption underlying this approach is that education is a standardized commodity that affects all types of individuals similarly. However, given that children of different backgrounds tend to be unequally equipped with resources and motivations that foster political activity, education is likely to have a greater impact among some groups than others. This being so, the population-average effects that provide the main focus of previous research may conceal as much as they reveal. Most importantly, if the effect of education varies across groups this means that changes to the educational system may affect equality of participation even if education has no overall effect.

Whereas the issue of heterogeneous causal effects of education has attracted some attention from sociologists and economists in recent years (Brand and Xie, 2010; Carneiro et al., 2011), political science research on the topic is still rather scanty (for recent exceptions see Campbell and Niemi (2016) and Neundorf et al. (2016)). One likely reason for this is the methodological challenges associated with this type of analysis. First, obtaining sufficient precision in the estimates for particular subgroups often requires very large samples. Second, and as detailed by Breen et al. (2015), the usual problems of causal inference are further aggravated when examining the heterogeneity of effects because conventional selection bias is easily mistaken for heterogeneity of causal effects. Consequently, it could be argued that exogenous variation in educational attainment is particularly needed when studying the extent to which the returns from education differ between individuals or groups.

In an attempt to meet these requirements, this study uses unique population-wide administrative data from Sweden to examine the impact on voter turnout of a major school reform implemented in the early 1990s. Thanks to a recent effort to scan and digitize the complete electoral roll for the 2010 general election in Sweden we have access to high-quality individual-level turnout information for more than 95% of the electorate. This data is compared with data on a school reform that lengthened vocational training programs at the upper secondary level from two to three years and added more general theoretical content, including civic studies, to the curriculum. An attractive feature of this reform was that it was preceded by an extensive pilot scheme in which the new system was tried out in a number of carefully selected municipalities. There is thus an arguably exogenous variation across regions and over time in the implementation of the reform that can be used to identify the effects of interest (Hall, 2012).

Our results indicate that the education reforms led to an increase in voter turnout among individuals from the most disadvantaged homes, but did not affect turnout of individuals from more privileged social backgrounds. In consequence, the reform helped to reduce the overall voting gap related to family background by raising turnout at the very lowest end of the socio-economic distribution. More precisely, we find that the equalizing effect is mainly driven by what we refer to as a *return effect*, i.e. the effect of education on turnout is much larger among individuals from low socio-economic backgrounds. These results square well with recent research, which shows that the positive effect on political knowledge and interests of civics training in schools, mainly benefits politically marginalized groups (Campbell and Niemi, 2016; Neundorf et al., 2016).

The rest of the paper is organized as follows. First, we present theory and previous research. We then describe the reform, and discuss the methods and data being used. Finally, we report the empirical results and conclude by discussing the implications of our findings.

2 Family, Education, and Political Participation

Students of political socialization have long recognized the important role played by the family in shaping adolescents' political attitudes and behavior (Hyman, 1959; Jennings and Niemi, 1981). One family characteristic that has been found to be particularly strongly related to future political activity is parental social status. Available empirical evidence shows that children of high socio-economic status (SES) parents are considerably more likely to grow up to become politically engaged citizens than those from less advantaged backgrounds (Verba et al., 2003; Gidengil et al., 2016).

Theoretically, parents' social status can be expected to influence children's political participation in, at least, two different ways. First, parents can pass on their socio-economic status to their children. If high socio-economic status is conducive to political involvement, and the children of high-status parents are more likely to have a high socio-economic status themselves, then political participation will be related to family background. Second, parents' political involvement may directly affect that of their children. Politically engaged parents may raise their children to be similarly engaged and interested (Neundorf and Smets, 2017). Following Gidengil et al. (2016) we can refer to these two potential mechanisms as the *status transmission-* and *social learning theory*, respectively.

Another frequently discussed agent of political socialization is the school. There is a voluminous empirical literature demonstrating that formal education is among the most important predictors of political participation. Regardless of context and type of political participation, the better educated tend to be more active than the less educated (e.g., Verba et al., 1995).

A number of explanations have been offered for why education affects political participation. The traditional understanding is that the school is a place where children learn important participatory skills and abilities, which reduces the material and cognitive costs of political participation in the future (Wolfinger and Rosenstone, 1980). In addition, education is believed to instill in citizens the belief that political participation is a civic duty (Jackson, 1995, 280), and to place individuals in politically active networks where they are more likely to be mobilized into politics (Verba et al., 1995; Nie et al., 1996). Viewed from this perspective, education constitutes an essential resource for political participation.

An equally important corollary to this resource based account of political participation is that improved educational standards can also help to reduce inequalities in political participation between different socio-economic groups (Nie et al., 1996, 188). As Schlozman et al. (2004, 34), for instance, explain:

> Since education is such a powerful predictor of political engagement, rising absolute levels of education might be expected to facilitate the political activation of those at the bottom of the SES hierarchy and produce class convergence in participation.

According to this argument, one way to lessen the impact of parents' social status on children's future political engagement would therefore be to promote policies aimed at expanding educational opportunities. As it stands, however, this policy prescription leaves some central questions unanswered.

A first issue pertains to the importance of educational content. One frequently voiced view is that rising levels of education per se are unlikely to spur political engagement, but that it is primarily a "civic or social science curriculum that imparts the skills and resources necessary to be active in the political realm" (Hillygus, 2005, 28). Despite decades of research there still, however, remains great uncertainty both about the participatory effects of education in general and those of civics studies in particular.

Another lingering question concerns the specific reasons why rising levels of educational attainment can be expected to have a greater impact on political participation among those at the bottom of the SES hierarchy. There are basically two possibilities here, which can be seen more clearly by contemplating a simple formalized example. For reasons of concreteness, the example focuses on the participatory act of voting although the logic of the argument applies equally well to other forms of political participation.

To fix ideas, consider a country in which the electorate consists of two groups: individuals from *low*- and *high* SES backgrounds ($g \in l,h$). Let us further assume that the country is just about to implement some type of school reform aimed at increasing educational attainment. Under what conditions will such a reform facilitate class convergence in voting? To see this we can express the average probability of voting in each of the two groups as:

$$Pr(V)_s^g = \rho^g \bar{e}_s^g + \eta^g, \tag{1}$$

where $\bar{e}_s^g$ denotes the average level of education in socio-economic group g under school system s, and η^g is a group-specific constant capturing the joint effects of factors other than education. The differences in turnout for each of the two SES groups before and after a school reform can then be expressed as:

$$\Delta Pr(V)^l = \rho^l \Delta \bar{e}^l + \Delta \eta^l, \tag{2}$$

$$\Delta Pr(V)^h = \rho^h \Delta \bar{e}^h + \Delta \eta^h, \tag{3}$$

where Δ denotes the before and after difference in the variable of interest. A first thing to note is that the difference in turnout between the two points in time will not only depend on the change in average educational attainment ($\bar{e}$), but will also be affected by any simultaneous change in the group specific constant η. However, if we invoke the assumption that no other important changes occurred simultaneously with the school reform it is easy to characterize the impact of the reform on voting inequality. For instance, if we use the difference in the shares of the turnout between individuals from low and high SES homes the effect of the reform is:

$$\Delta Pr(V)^h - \Delta Pr(V)^l = \rho^h \Delta \bar{e}^h - \rho^l \Delta \bar{e}^l. \tag{4}$$

As is to be expected, the sign of this effect depends on the relative size of the overall reform effect in each of the two groups. To reduce inequality, the school reform must increase turnout more among individuals from low SES homes than among those from high SES homes (i.e., $\rho^l \Delta\bar{e}^l > \rho^h \Delta\bar{e}^h$).

More importantly, however, equation (4) highlights the fact that there are two different effects at work here. First there is what we will refer to as the *resource effect*, i.e., the reform may affect the allocation of education (the resource) between SES groups. Available empirical evidence suggest that both the sign and the magnitude of the resource effect may depend on the type of education reform being examined. Reforms that lengthen compulsory education, for instance, tend to have a larger effect on educational attainment of children from low SES homes ($\Delta\bar{e}^l > \Delta\bar{e}^h$) because they are less likely to go on to secondary education (Lindgren et al., 2017). In contrast, Blanden and Machin (2004) found that policies that expanded higher education in the UK served to widen the educational gap between children from rich and poor backgrounds (i.e., $\Delta\bar{e}^l < \Delta\bar{e}^h$). Depending on the nature of the reform, the resource effect can therefore contribute to an increase or a decrease in the voting gap.

However, even if both SES groups experience an equal increase in educational attainment as a result of the reform, so that the resource effect is zero, the voting gap could nevertheless change if the effect of education on voting differs across groups. We will refer to this as the *return effect*. If formal education and a stimulating socializing family environment are *substitutes* in the process of developing the type of skills, interests, and norms conducive to political participation a given increase in educational attainment should have a larger effect among individuals with low SES background (i.e., $\rho^l > \rho^h$). Or, conversely, if these two factors are *complements* in the production of political participation, increased schooling should have a more pronounced effect among individuals from high SES homes (i.e., $\rho^h > \rho^l$). In the literature on civic education these two possibilities have been referred to as the *compensation* and *acceleration* hypothesis, respectively (e.g., Campbell, 2008).

In a recent contribution, Neundorf et al. (2016) also discuss two special cases of the compensation hypothesis. The first is the *ceiling effect*, which suggests that individuals with higher levels of initial political engagement should benefit less from civic education since there is a natural upper limit on how politically active one can be. Second there is the *left-behind* effect stating that without the stimulus of civic education the political activity of children from less politically engaged families will lag behind that of children from more politicized homes for a very long time. However, in observational terms these two effects are similar to the compensation effect in that they imply that civic education helps children of disadvantaged backgrounds to "*catch up* with their peers who come from families with high levels of political socialization" (Neundorf et al., 2016, 927).

The upshot of this discussion is that the alleged link between rising educational levels and class-convergence in political participation is considerably more involved than the previously discussed quote from Schlozman et al. (2004) would lead us to believe. A first necessary requirement is obviously that there is a causal effect of education on political participation to begin with. However, as mentioned in the introduction, this assumption has been questioned by a number of scholars who argue that the correlation between education and political participation is spurious rather than causal (e.g., Tenn, 2007; Kam and Palmer, 2008; Berinsky and Lenz, 2011). Second, even if the effect is causal, rising absolute levels of education will only help reduce political inequality insofar as the increase in education is more pronounced among those from low SES backgrounds (the resource effect) or if education has a greater impact on political participation in this group than in others (the return effect).

Ultimately it is an empirical question whether, and if so to what extent, policies designed to increase educational standards can prove effective in mitigating the inequality in political participation. But, as should be clear from the discussion, this is also a very demanding question to answer. First, and most importantly, distinguishing correlation from causation requires access to some form of (plausibly) exogenous variation in educational attainment. Second, at least part of the extra time spent in school should be devoted to the study of civics. Finally, to be able to say anything about the relative importance of resource as well as return effects, we need to study a policy that has a greater impact on educational attainment among some socio-economic groups than others. In the next section, we argue that a major reform of Swedish upper secondary education meets these requirements, and thus offers a suitable testing ground for examining this important issue.

3 Institutional Background

In Sweden students enter the upper secondary school system the year they reach the age of 16 after nine years of compulsory schooling.[1] Although upper secondary education is non-mandatory, a majority of students go on to this level (about 90 percent of the students during the period under study). Students can choose from a number of either vocational training or academic programs. Typically students attend an upper secondary school in their municipality of residence. If the desired program is not available they may attend an upper secondary school in a nearby municipality.

In 1984 the government appointed a committee with a mandate to propose a reform of the upper secondary school system with a special focus on improving vocational education. Based on the committee's proposal the Swedish Parliament decided on a large-scale reform of the upper secondary school in 1991. In the pre-reform system students had been able to choose between a number of two-year vocational training or three-year academic programs.[2] The former had a strong focus on preparing students for working life and contained less theoretical study, whereas the latter were intended to prepare the students for higher education at university level. In the post-reform upper secondary school the length of all vocational training programs was extended to three years. Moreover, the reform also provided for a stronger theoretical content in the curriculum of these programs. In the pre-reform system Swedish had been the only mandatory theoretical subject provided in vocational training programs. After the reform these programs also included English, social science and an additional optional theoretical subject (mathematics being the most common choice). As a result of these changes, students graduating from vocational training programs were classified as meeting the basic entry requirements for admission to university.

One of the explicit political intentions behind the reform was to reduce the socio-economic gap between students from low SES homes who for the greater part took vocational training programs and students from high SES homes who primarily opted for

[1] This section is based on the detailed description of the Swedish upper secondary school system and the school reform in 1991 provided in Hall (2009) and Hall (2012).

[2] In addition a four-year vocational training program in technology was available.

theoretical programs. By opening up the possibility of going on to higher education at university for students from vocational training programs the reform of the upper secondary school was seen as a step towards the overriding goal, expressed in the common slogan “a school for everyone”. In a historical perspective the reform harmonizes with earlier educational reforms in Sweden, put forward primarily by the Social Democrats, with the intention of providing mass education for large parts of the population and thereby creating a more egalitarian society. In a parliamentary debate the minister of education and future prime minister, Göran Persson, defended the proposal to introduce a three-year upper secondary school with the following arguments:

> In the long run it is all about defending a democratic society. If we accept that some people will be left out, that some people need not be included —- well, then we have also said that we are abandoning one of the foundations of a democratic society, namely that we all have equal rights and are all of equal value. It is against this background that the Social Democrats has carried out its education reforms (Minutes of the Riksdag 1990/91:126).

These arguments were also reflected in the official curriculum after the reform, according to which one of the central goals for schools was to “develop [students’] will to actively contribute to a deeper democracy in working and civic life” (The Swedish National Agency for Education, 2006, p.15).

The reform was fully implemented in 1994 but was preceded between 1988 and 1990 by a pilot scheme in which the new three-year training programs were implemented in some municipalities for evaluation purposes. Prior to the implementation of the actual pilot scheme a limited pre-pilot, including only 500 student places in 22 municipalities, was implemented in the academic year of 1987/1988. In the first year of the real pilot scheme (i.e. the academic year of 1988/1989) this number was increased to 6,000 student places, whereas the corresponding numbers for the second and third year of the scheme were 10,000 and 11,200 student places respectively. The implementation of a pilot scheme class in a municipality was always accompanied by the withdrawal of a class in a corresponding two-year vocational training program in that same municipality. Thus, the reform did not increase the total number of available places on vocational training programs. Moreover, places were allocated proportionately in a way intended to ensure that two-year and three-year places were offered in the same proportions across different training programs. By the end of the period the pilot scheme included around 20% of the available places on vocational training programs.

The municipalities had to apply to participate in the pilot scheme and the National Board of Education decided which municipalities to allow. When making this decision the Board took several factors into account. First, it was important for the local labor market to be able to meet the demand for the extended working-life training included in the new three-year vocational training programs. Second, the board tried to implement the scheme in different geographical areas. Finally, a certain amount of variation regarding the extent to which different regions participated seemed desirable.

At the start of the scheme the demand to participate exceeded the supply of three-year places on vocational training programs provided by the National Board of Education. For example, during the first year the Board received applications for over 10 000 student places but could only accept 6000. Out of Sweden's 284 municipalities at the time, 113 participated in the scheme in the first year (1988/1989), 144 in the second and 147 in the third. Given that in 1990 vocational training was only offered in 193 municipalities, those eligible for inclusion in the pilot scheme were by this time a majority (SOU 1989:90).[3]

During the pilot period participating municipalities usually offered both two-year and three-year vocational training programs. Moreover, in some municipalities both two-year and three-year versions of the same program were available. In municipalities where only the three-year programs were offered it was possible to attend the corresponding two-year programs nearby. Hence, the pilot scheme setting did not represent a dramatic change, and no one who wanted to attend a vocational training program was forced into a three-year one. Our estimation strategy relies instead on the fact that, depending on one's year of birth and municipality of residence when commencing upper secondary school, there was variation in the degree to which students had a chance to attend a three-year rather

[3] In the pre-reform system students could choose between 17 different vocational training programs. The pilot scheme offered ten such programs in the first year and 17 in the last two. The most popular ones were caring services, industry and motor and transport engineering.

than a two-year vocational training program. The design and gradual implementation of the pilot scheme thus provides a source of exogenous variation in the availability of education that can be exploited to study the causal effects of schooling.

A number of studies have used the pilot scheme to study the effects of education on different outcomes. Ekström (2003) used a cross-sectional sample of students to compare those living in pilot scheme and non-pilot scheme municipalities and showed that while the reform increased the probability of going on to higher education it did not reduce the probability of being unemployed. However, based on population-wide data Hall (2012)) found the reform to have no significant effects on university enrollment rates and later-life earnings. The discrepancies between the two studies might be attributed to the fact that the dataset used by Hall (2012) provided better opportunity for control of unobserved differences between municipalities.

Using a similar modeling strategy as the one employed in Hall (2012), Grönqvist and Hall (2013) found no effects of the school reform on men's fertility rates whereas early-life fertility rates were significantly lower among women who took a three-year vocational training program. In a further study, Grönqvist et al. (2015) focused on the effects of the reform on criminal behavior. They concluded that keeping students in the school system for an additional year lead to a reduction in property crime.

While no previous study has looked at the effects of the pilot scheme on political participation, Persson and Oscarsson (2010) compared levels of political participation among students from vocational training and theoretical programs before and after the reform was implemented on a national scale in 1994. They concluded that differences in political participation between students from vocational training and theoretical programs did not disappear after the reform. However, this study was based on a small cross-sectional sample and did not analyze whether the reform had heterogeneous effects conditional on social background.

4 Empirical Framework

We will employ a difference-in-difference approach to identify the causal effect of completing a three-year vocational training program on voter turnout.[4] Consider, first, the following baseline specification:

$$V_{icm}^{g} = \alpha_0^{g} + \alpha_1^{g} D_{icm}^{g} + \boldsymbol{\lambda^{g} X_{icm}^{g}} + \theta_c^{g} + \eta_m^{g} + \varepsilon_{icm}^{g}, \tag{5}$$

where V_{icm}^{g} is a dichotomous indicator for voter turnout for individual i, starting upper secondary school in year c, and residing in municipality m. D_{icm}^{g} is a dummy taking on the value 1 for individuals who completed a three-year training program, $\boldsymbol{X_{icm}^{g}}$ is a vector of individual-level covariates, and θ_c^{g} and η_m^{g} are cohort and municipality fixed effects, respectively. The superscript g ($g \in l,h$) indicates that the effect of a third year of upper secondary education is evaluated separately for low (l) and high (h) socio-economic status groups.

If $\boldsymbol{X_{icm}^{g}}$ includes all relevant factors that may influence an individual's educational choices as well as his or her voting behavior, estimating Model 1 using Ordinary Least Squares (OLS) would lead to an unbiased estimate of the causal effect of completing an extra year of upper secondary schooling. However, as pointed out in a growing number of studies (Kam and Palmer, 2008; Henderson and Chatfield, 2011; Mayer, 2011) this is not likely to be the case since many of these factors are difficult or impossible to observe and measure correctly. Therefore we should expect OLS estimates of α_1^{g} to be biased due to a correlation between D_{icm}^{g} and ε_{icm}^{g}.

To circumvent this problem we will make use of the plausibly exogenous variation in the length of training programs introduced by the pilot scheme prior to the reform of the upper secondary school. As outlined in the previous section the pilot scheme was implemented gradually within municipalities and to different degrees across municipalities. Thus, depending on when they were born and where they resided when they completed

[4] For a similar empirical approach see Hall (2009) and Hall (2012). More precisely, we will estimate the effect of completing a three-year vocational program rather than a two-year vocational program or having no upper secondary schooling at all. In order to simplify the language we will refer to this as the effect of completing a three-year vocational program or as the effect of completing an extra year of upper secondary vocational schooling.

compulsory school, the students faced different opportunities. Some could choose from plenty of three-year vocational training programs whereas others were assigned to the shorter two-year ones.

In a first step we will estimate the following reduced form effect of the reform:

$$V^g_{icm} = \beta^g_0 + \beta^g_1 R_{cm} + \boldsymbol{\zeta^g X^g_{icm}} + \theta^g_c + \eta^g_m + \xi^g_{icm}, \tag{6}$$

where R_{cm} is a continuous measure of the extent to which the individual's municipality of residence was affected by the pilot scheme by the time he or she began upper secondary school. Consequently, β^g_1 is an estimate of the difference in turnout between students who had to attend a shorter two-year vocational training program ($R_{cm} = 0$) and those whose only option was the three-year program ($R_{cm} = 1$). As highlighted in the theoretical section (equation 4) the school reform needed to have a stronger positive impact on students from low SES homes ($\beta^l_1 > \beta^h_1$) in order to reduce inequality in turnout.

In the theoretical section we also pointed out that any reform effect that reduces inequality may be driven by a resource and/or a return effect. In order to decompose the overall reform effect into these potential pathways we will use the reform indicator as an instrument for completing a three-year program and estimate a Two Stage Least Squares (2SLS) model. The first and second stages take the following form:

$$D^g_{icm} = \gamma^g_0 + \gamma^g_1 R_{cm} + \boldsymbol{\tau^g X^g_{icm}} + \theta^g_c + \eta^g_m + \phi^g_{icm} \tag{7}$$

$$V^g_{icm} = \delta^g_0 + \delta^g_1 \widehat{D}^g_{icm} + \boldsymbol{\omega^g X^g_{icm}} + \theta^g_c + \eta^g_m + \psi^g_{icm} \tag{8}$$

where γ^g_1 is the effect of the reform indicator on completing a three-year training program and δ^g_1 is the effect of completing a three year program on turnout propensity.[5] The resource mechanism is concerned with the extent to which the effect of the reform on schooling choices differ across SES groups. Thus, even if the effect of education on turnout is equal across socio-economic groups ($\delta^l_1 = \delta^h_1$) the reform will reduce inequality

[5]Thus, γ^l_1 and γ^h_1 correspond to the parameters $\Delta\bar{e}^l$ and $\Delta\bar{e}^h$ from equation 4 in the theoretical section. Likewise, δ^l_1 and δ^h_1 correspond to the parameters δ^l_1 and δ^h_1.

if $\gamma_1^l > \gamma_1^h$ and increase inequality if $\gamma_1^l < \gamma_1^h$. However, a change in the turnout gap could also reflect a pure return effect if the resource effects are the same in the two groups ($\gamma_1^l = \gamma_1^h$) whereas the impact of an extra year of schooling is greater among low SES students ($\delta_1^l > \delta_1^h$) or among high SES students ($\delta_1^l < \delta_1^h$).

Our empirical framework rests on a number of identifying assumptions. The most important among these concerns the (conditional) exogeneity of the reform, i.e., conditional on the covariates included in the model R_{cm} should be uncorrelated with other factors influencing turnout propensities. Given that our model include municipality fixed effects the main concern is whether there were different trends in unobserved characteristics in municipalities with high and low reform intensity. Hall (2012) examined this issue at some length and found that reform intensity was unrelated to changes in important municipal characteristics, such as the unemployment rate, or to changes in various types of student characteristics such as immigrant background, parental education, and compulsory school GPA. Although not conclusive, Hall's results thus lend credibility to the conditional exogeneity assumption underlying our identification strategy. Moreover, in the robustness section we provide empirical evidence that further corroborates the plausibility of this assumption.

In addition to this, the IV-model also requires the assumption that the intensity of the reform had no direct effect on voter turnout, but influenced turnout only indirectly by affecting the likelihood of completing a three-year training program. While this assumption cannot be tested we nonetheless find it fairly plausible since it is difficult to come up with any good reasons why reform intensity should be directly related to voter turnout.[6]

Finally, despite the fact that our key dependent variable is binary, we will rely on a linear probability model to obtain our estimates. There are two main reasons for this. First, the difference-in-differences approach of the type used here loses much of its attractiveness and simplicity when applied to non-linear models (Blundell and Dias, 2009; Lechner, 2011). Stated in simple terms, the root of the problem is that the cohort and municipality

[6]The one reason that we can think of is if we can expect large spill-over effects from treated individuals to their friends or family and that these spill-over effects are particularly concentrated to individuals in the same cohorts as the treated individuals. Although the presence of such peer effects cannot be ruled out we find it unlikely that they will be sufficiently large to severely bias the IV-model.

effects (θ and η) in equations 6, 7, and 8 will not partial out if the model is estimated by a logit or probit model. That is, in non-linear models the inclusion of municipal and cohort fixed effects will not be sufficient to absorb the impact of unobserved factors affecting a particular municipality or cohort. Second, the instrumental variable approach becomes involved and requires much more stringent assumptions when applied to non-linear models. This is particularly true in a case like this when we also have a binary endogenous regressor (e.g., Freedman and Sekhon, 2010). We will, however, provide logit results as a robustness check.

5 Data and Measures

We use data from various administrative registers maintained at Statistics Sweden to construct our sample and to acquire information on several socio-economic and demographic variables. Our sample consists of all individuals born between 1970 and 1974. Since Swedish students normally finish compulsory schooling in the spring term of the year they reach the age of 16 our sample includes nearly all individuals who completed compulsory schooling between 1986 and 1990. We use the Multi-Generation Registry to match these individuals with their parents. The children and parents are matched with various administrative registers containing information regarding educational attainment, income, occupational status, and other demographic and socio-economic characteristics.[7]

To construct a pilot scheme reform indicator for each individual in our sample, we follow Hall (2012) and use information on the individual's municipality of residence according to the 1985 census together with information on vocational training programs available across municipalities.[8] More precisely, the indicator measures the number of three-year vocational training programs as a proportion of all vocational programs.[9]

Family socio-economic status constitutes another key variable in our analysis. Broadly

[7] See the Appendix for additional details on these registers and variables.

[8] We are grateful to Caroline Hall for sharing the code used to construct this indicator.

[9] Hall (2012) sets the reform indicator to zero for municipalities not offering any vocational training programs. However, students living in such municipalities could enroll in upper secondary schools in nearby municipalities. Therefore, for municipalities that lacked vocational training programs during the study period we use the reform score for the municipality in which most students from the 1970 cohort (the cohort preceding the first cohort after the reform) attended a vocational training program.

defined socio-economic status (SES) can be said to be related to "one's access to financial, social, cultural, and human capital resources" (NCES, 2012, 4). To capture these various dimensions of SES, researchers have traditionally relied on composite measures including family income, parental educational attainment, and parental occupational status.[10] The *PISA index of economic, social, and cultural status* (ESCS), developed by the OECD, represents a prominent recent example of this approach. The ESCS measure is derived from the following three indices: highest educational level of parents in years, highest occupational status of parents, and home possessions (OECD, 2010, 131).

In this study we will use a measure of socio-economic status that is closely related to the ESCS (it has a slight difference in that it uses parental income instead of home possessions). That is, our measure of family SES is based on a simple additive index of three items: *i)* highest parental education, *ii)* highest parental occupational status, and *iii)* average parental labor income. All items are assigned the same weight in calculating the SES index and if information on one of the indicators is missing the index is based on the two indicators for which data is available.[11] To adjust for differences in scales between the variables, all sub-items were initially standardized to have a mean of 0 and a standard deviation of 1.[12] Consequently, our measure of family SES will take on a value of 0 for an individual from a family with an average score on each of the three items, and a value of 1 for an individual from a family that is situated one standard deviation above the mean on all items.

Whereas information on parental education and labor income are gathered directly from the registers our measure of occupational status is based on census occupation codes. More precisely, we use the occupation codes to compute three well-known measures of occupational status: the *International Socio-Economic Index* (ISEI, Ganzeboom et al., 1992)), the *Standard International Occupational Prestige Scale* (SIOPS, Treiman, 1977),

[10] The authors of a recent overview on the topic refer to parental income, education, and occupational status as the big 3 variables of SES measurement (NCES, 2012, 13).
[11] Complete data on all three indicators are available for 94% of the cases.
[12] The scale reliability of this index is .78. To reduce the skewness of the additive index, and reduce the risk that some of the really large incomes are due to measurement error, parental income was top coded at the 99th percentile before it was standardized. However, all substantive results remain very similar if parental income is not top coded or if it is log-transformed.

and the *International Cambridge Scale* (ICAMS, Meraviglia et al., 2016; Prandy and Jones, 2001).[13] As shown by Meraviglia et al. (2016), despite the differences in conceptual underpinnings these three measures are very highly correlated and appear to reflect a single underlying dimension. We therefore use the average of these three indicators to measure mothers' and fathers' occupational status.[14]

Turning to the dependent variable, the supply of data from Statistics Sweden is less satisfactory for electoral participation. The public registers do contain validated information on voter turnout from relatively large samples in connection to each election from 1991 and onwards. However, our research design requires us to have access to samples that are preferably population-based. Therefore we have collected population data on voter turnout in the 2010 general election (N $\approx 7,000,000$) by scanning and digitizing the information in the publicly available election rolls (we provide a detailed description of the procedures we have used to scan and digitize them in the Appendix). The resulting dataset is unique in both scope and quality. The reliability of the digitized individual-level turnout data is very high. Quality checks suggest that the digitized information on electoral participation conforms with the manual coding of Statistics Sweden in 99.7% of the cases.

Table 1 presents summary statistics for individual background variables (Panel A) and outcomes (Panel B) separately for two groups of municipalities based on the extent to which they participated in the pilot scheme. Municipalities with above median participation in the pilot scheme in 1990 are considered high intensity municipalities. Comparing across the columns it is evident that the two groups are very similar in terms of background characteristics.[15]

As expected the share of students with at least three years of upper secondary educa-

[13] See the Appendix for more detailed descriptions of these measures. The code for translating census occupation codes into ICAMS, ISEI, and SIOPS was downloaded from http://www.harryganzeboom.nl/isco88/index.htm.

[14] The scale reliability of this index is .96 for fathers and .93 for mothers. For a small number of individuals that have two non-employed parents, parental occupational status has been set at its sample minimum value. Because the ISEI measure is constructed on the basis of information on education and income it may be objected that including this measure in the Family SES measure is somewhat superfluous since these variables are already included in the SES measure. However, excluding the ISEI measure from the analysis does not change the substantive results.

[15] Immigrant background is a dummy equal to 1 if the individual or at least one parent is born abroad.

tion at age 20 is larger in municipalities with a high pilot intensity, and these differences are evident for all quartiles of the family background variable. Turning to voter turnout we see that the probability of voting is fairly closely related to family background. For instance, there is a difference of almost 10 percentage points in turnout between individuals from the highest quartile (Q4) and those from the lowest quartile (Q1). However, this simple cross-tabulation does not show any differences in turnout between individuals from high and low reform intensity municipalities. The question is whether this will change as more systematic analyses of the data are undertaken.

Table 1: Summary Statistics

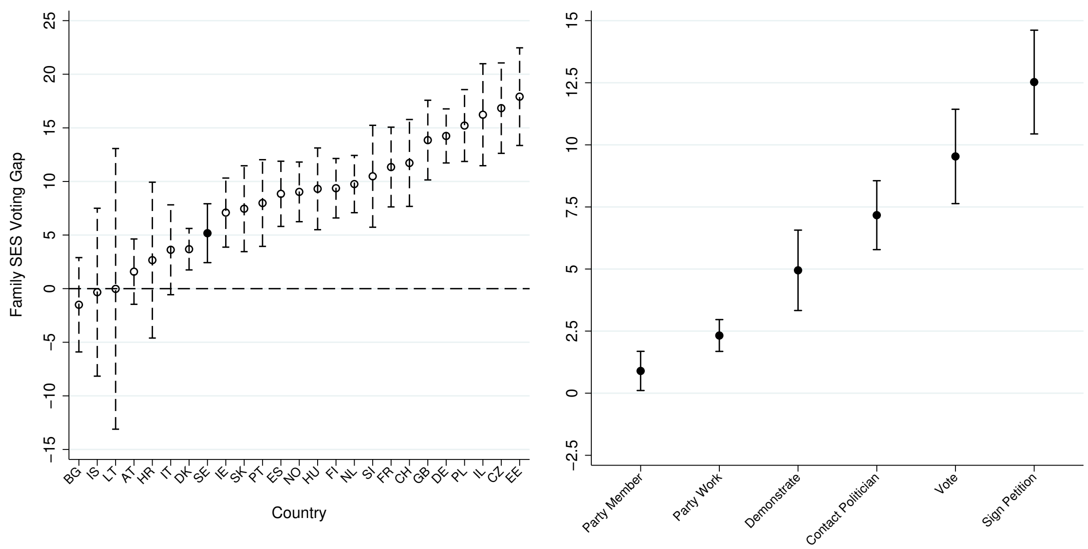
	Low level pilot intensity	High level pilot intensity
Panel A. Background variables.		
Female	0.49	0.49
Immigrant background	0.16	0.15
Student's year of birth	1972.00	1972.01
Mother's year of birth	1945.48	1945.49
Father's year of birth	1942.81	1942.76
Family SES	−0.00	−0.01
Panel B. Outcomes by family background.		
Share completing three-year programs Q1	0.28	0.31
Share completing three-year programs Q2	0.36	0.39
Share completing three-year programs Q3	0.52	0.54
Share completing three-year programs Q4	0.72	0.74
Turnout Q1	0.83	0.83
Turnout Q2	0.87	0.87
Turnout Q3	0.90	0.90
Turnout Q4	0.92	0.92
Number of individuals	241,626	272,621

Note: High level pilot scheme intensity municipalities are defined as municipalities where the share of three-year programs was above the median (=0.20) in 1990. The sample consists of all individuals who completed compulsory school 1986-1990.

6 Situating the Swedish Case

The empirical focus of this study is on political participation through voting in Sweden. To situate our study, and improve the understanding of the generalizability of the results, we will, however, begin with a brief descriptive analysis of the inequality of political voice in 25 modern democracies using data from the European Social Survey (ESS).[16] A first

Figure 1: Family Background and Political Participation, ESS data



Note: The graphs are based on the results from various regression analyses in which the dependent variable of interest, e.g., voting, is regressed on dummies for family SES quartiles together with a set of controls including gender, survey year,year of birth fixed effects (left graph), and country by year of birth fixed effects (right graph). We then plot the expected differences in outcomes between the highest and lowest quartile of the family SES distribution. All individuals included in the analyses are aged 25–65. Standard errors are clustered at the country level and post-stratification weights are used make the samples representative. The confidence intervals are calculated at the 95% level.

important question concerns the representativeness of the Swedish case with regard to the relationship between family background and voter turnout. Towards this end, the leftmost graph in *Figure 1* displays the *interquartile difference*, i.e., the difference in expected turnout between an individual coming from a home in the highest quartile (Q4) of the family SES distribution and one coming from a home located in the lowest quartile (Q1),

[16] A few countries included in the ESS survey have been excluded from the analysis, since they are either not fully democratic or have compulsory voting laws. See the Appendix and the table notes for details on data and methods

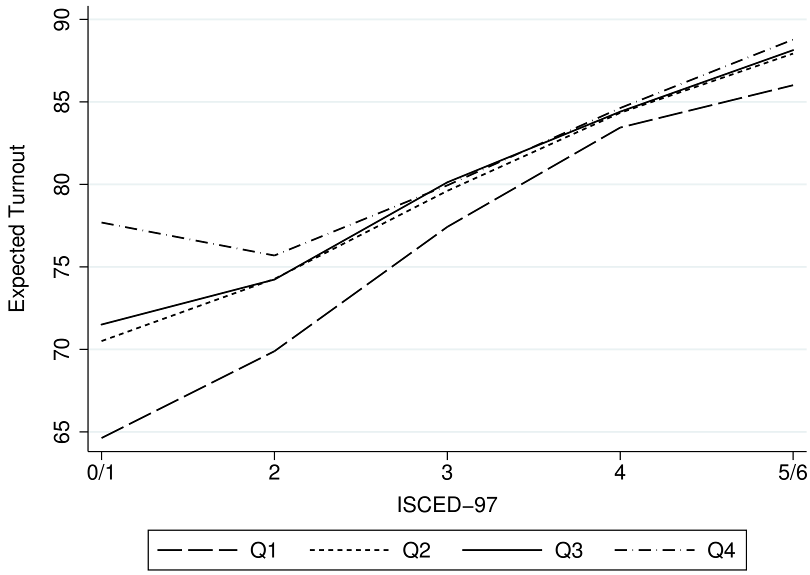
for each of the 25 countries.[17] In most countries individuals from more advantaged social backgrounds vote, or at least claim to vote, to a much higher extent than individuals from less well off backgrounds. The interquartile difference in the sample as a whole is about 9.5 percentage points whereas the corresponding figure for Sweden (labeled *SE*) is 5.2 percentage points. Although slightly more politically equal than the average European country, Sweden is not exceptional as can be seen from the fact that the confidence interval for the Swedish point estimate overlaps with those of most other countries.

Another issue concerns the representativeness of the participatory act of voting. The rightmost graph of *Figure 1* therefore displays how family background relates to other forms of political participation, such as working for a political party, contacting elected politicians, or signing petitions. As can be seen, individuals from privileged social backgrounds are more likely to undertake all these political acts than those from less privileged backgrounds. The large variation in the baseline probabilities of performing these different acts makes it difficult to compare the relative importance of family background across different types of political participation. The important point, however, is that individuals of low social origin are less likely to exercise political voice, regardless of participatory channel. The positive relationship between family background and voting thus seems to hold true for political participation more generally. To judge from this simple analysis, the lessons drawn in this study can thus be expected to travel beyond the particular case of voting in Sweden. Moreover, in the appendix (*Figure A5*) we use the ESS data to show that there are clear differences in political attitudes between individuals of different family background. For instance, individuals of more disadvantaged social background are more likely to favor economic redistribution and oppose immigration. Given the closeness of many national elections it therefore seems likely that the observed differences in political participation between individuals of different social origin can have important real-world consequences.

The main interest in this study, however, concerns the degree to which increased educational attainment can help alleviate the political opportunity gap rather than the impor-

[17] The family SES measure used here is very similar to that used in the main analysis except that data on parental income is not available in the ESS. To increase comparability across space and time we have standardized all the socio-economic indicators in the ESS by country and cohort.

Figure 2: Expected Turnout by Education and Family SES



Note: The graph is based on the predictions from regressing voting on gender and a full set of dummies for survey year, country by year of birth, and ISCED-97 codes. The ISCED codes are as follows: 0/1) less than secondary education, 2) lower secondary education, 3) upper secondary education, 4) post-secondary non-tertiary education, 5/6) first and second stage tertiary education.

tance of family background for voting *per se*. As a brief prelude to this analysis *Figure 2* uses ESS data to show how voter turnout varies with educational attainment—using the simplified 5-category version of ISCED-97 included in ESS—for each quartile of the family SES distribution.

One interesting thing to note is that the relationship between education and voting is most pronounced in the lowest quartile of the family distribution. As a direct implication of this, the voting gap between the individuals in the lowest quartile and those in the upper three quartiles decreases as we move up the educational ladder. This is consistent with the view that eduction can reduce the political inequalities by compensating for the civic disadvantages associated with growing up in low SES homes. However, the pattern displayed in the graph could also be due to a selection effect whereby the individuals from Q1 going on to higher levels of education are more positively selected than those of the other groups. Alternatively, the result could be driven by a higher tendency to overreport voting among the better educated from less privileged families (Solis, 2013) since the room for overreporting is higher in groups with lower actual turnout. To address these caveats we now turn to the analysis of the Swedish case.

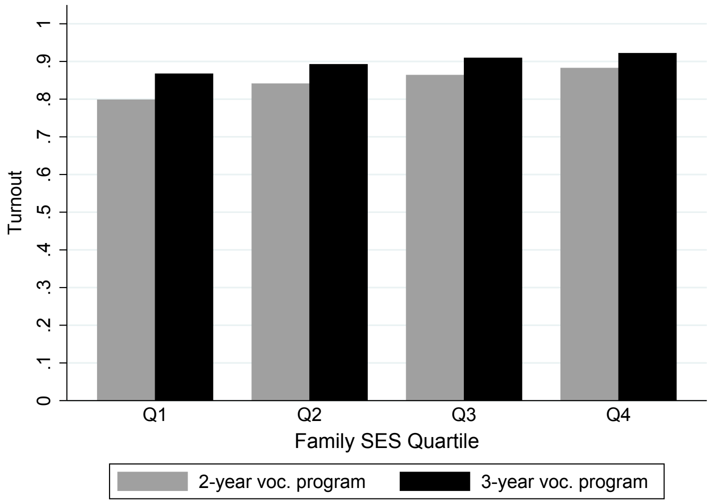
7 Analyzing the Swedish Case

The purpose of this section is to examine how the lengthening of vocational upper secondary education from two to three years affected voter turnout. *Figure 3* therefore displays voter turnout by track length and family SES quartile for those attending vocational training programs.[18] Two things can be noted. First, for all quartile groups turnout is higher among those attending 3-year programs than among those on 2-year training programs. Second, the voting gap between the two educational groups is smaller for individuals from more advantaged backgrounds. For individuals from the lowest quartile of the family distribution (Q1) the difference in turnout is 6.9 percentage points, whereas the corresponding figures in the other three quartile groups are 5.1, 4.5, and 3.9 percentage points. To judge from these results the lengthening of the vocational training programs may thus have helped increase and equalize voter turnout. However, one problem with this analysis is that it is likely to suffer from endogeneity bias because the individuals choosing 3-year vocational training programs are likely to have been different from those choosing 2-year ones.

To mitigate this issue we now proceed to use the more exogenous variation in program length induced by the pilot scheme implemented in the late 1980s. To this end, *Table 2* reports how the availability of 3-year vocational training programs in an individuals home municipality at age 16 affected the probability of voting in the 2010 election. In the first panel of the table the dichotomous indicator for voter turnout is regressed on the measure of reform intensity—i.e., the share of three-year vocational training programs in a municipality—and a number of controls including gender, immigrant and family background, year of birth, parent's year of birth, and municipality of residence. These reduced-form coefficients give us the *total* effect of the reform for different groups. The first column provides the effect for the full sample of individuals born between 1970 and

[18] More precisely, we exclude individuals attending theoretical programs in the analysis whereas those that did not complete any secondary education are lumped together with those attending 2-year programs.

Figure 3: Turnout by Family background and program length



1974. As can be seen we find no evidence that the reform raised turnout in the student group as a whole. Although the effect of the reform intensity variable is positive, it is small in magnitude and not statistically significant.

However, as highlighted in the theoretical section there are reasons to believe that the reform effect could differ between socio-economic groups. In columns 2–5 we therefore estimate separate models for each quartile of the family background variable. To judge from these results there was, indeed, an effect of the reform among children of low socio-economic status. For individuals growing up in homes of the lowest quartile of the family SES distribution the reform is associated with a rather large, and statistically significant, increase in voter turnout. Increasing the share of three-year vocational programs from 0 to 1 is estimated to increase voter turnout by almost 3.6 percentage points in this group, whereas we find no statistically significant effect in any of the other quartiles. With respect to the differences in coefficients between groups we find that three out of six differences have p-values below .05. These are Q2 vs. Q1 ($p = 0.002$), Q3 vs. Q1 ($p = 0.006$), and Q4 vs. Q1 ($p = 0.023$).

To further clarify the meaning of these results for the socio-economic voting gap, the leftmost graph in *Figure 4* displays how the expected turnout rates in the four groups vary

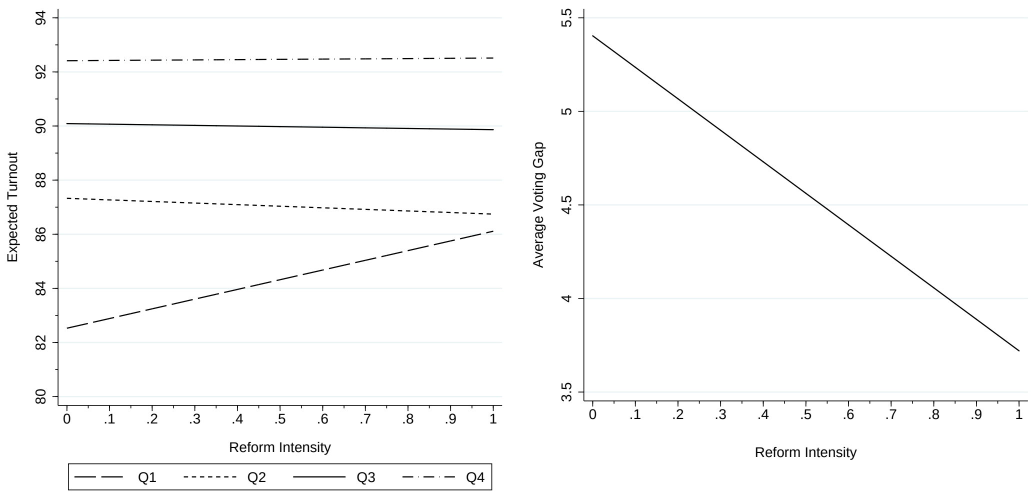
Table 2: Reform Effect on Voter Turnout

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
(*Reduced form*)					
Reform intensity	0.80	3.58***	−0.58	−0.22	0.10
	(0.51)	(1.06)	(0.86)	(0.95)	(0.94)
Gender	2.39***	3.91***	3.23***	1.78***	0.57***
	(0.11)	(0.20)	(0.20)	(0.17)	(0.13)
Immigrant background	−6.43***	−8.29***	−6.18***	−5.03***	−4.92***
	(0.26)	(0.40)	(0.33)	(0.38)	(0.28)
Family SES	3.85***				
	(0.10)				
Panel B. Dependent variable: $\geq$ 3 years of post-primary educ. at age 20					
(*First- stage 2SLS*)					
Reform intensity	17.15***	21.26***	19.80***	17.37***	6.09***
	(1.89)	(2.57)	(2.52)	(2.04)	(1.99)
Gender	2.99***	3.79***	3.75***	3.57***	0.88***
	(0.30)	(0.39)	(0.43)	(0.44)	(0.32)
Immigrant background	0.57	2.37***	1.85***	−0.55	−4.67***
	(0.42)	(0.57)	(0.65)	(0.59)	(0.40)
Family SES	19.31***				
	(0.14)				
Panel C. Dependent variable: Voting.					
(*Second-stage 2SLS*)					
Completed 3-year program	4.68	16.86***	−2.95	−1.29	1.61
	(3.06)	(5.24)	(4.27)	(5.45)	(15.35)
Gender	2.25***	3.27***	3.34***	1.83***	0.56***
	(0.14)	(0.29)	(0.26)	(0.27)	(0.19)
Immigrant background	−6.46***	−8.68***	−6.12***	−5.04***	−4.84***
	(0.26)	(0.45)	(0.33)	(0.38)	(0.76)
Family SES	2.95***				
	(0.59)				
Observations	514,247	128,561	128,561	128,563	128,562

Notes: All models include a full set of fixed effects for birth year, home municipality, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

with reform intensity.[19] As can be seen, there are remaining substantial inequalities in voting as the share of three-year programs starts to increase. In particular, the differences between the three highest socio-economic groups hardly change at all as a result of the reform (these lines are more or less parallel). The relative turnout of those from the most disadvantaged homes, however, clearly improved as a result of the reform. According to these estimates, in the absence of any three-year vocational programs the expected voting gaps between individuals in the lowest quartile and those in the other three quartiles would be 4.8 (Q2 vs Q1), 7.6 (Q3 vs Q1), and 9.9 (Q4 vs Q1) percentage points, whereas the corresponding figures would be 0.6, 3.8, and 6.4 percentage points when all vocational training programs are three years in length.

Figure 4: Voting Gaps by Reform Intensity



In the rightmost graph of *Figure 4* we use the same predictions to show how the aggregate socio-economic voting gap varies with reform intensity. That is, the graph shows the average absolute difference in voting probability across the six possible quartile comparisons for different values of reform intensity.[20] More substantively, we can think of these differences as the expected (absolute) difference in turnout between two randomly

[19] Expected turnout is calculated on the basis of the results presented in *Table 2* averaging over the sample values of all other variables in the model.

[20] Q1 vs Q2; Q1 vs Q3; Q1 vs Q4; Q2 vs Q3; Q2 vs Q4; and Q3 vs Q4. Put differently, the line in the rightmost graph represents the expected average pairwise distance between the four lines in the leftmost graph for different values of reform intensity.

selected individuals representing two different quartile groups. To judge from our results the average voting gap decreases from 5.4 to 3.7 percentage points as reform intensity increases from 0 to 1. However, and as the leftmost graph makes clear, this overall reduction is mainly driven by the fact that the relative position of the lowest quartile group improved as a result of the reform.

Given this, the next question is what accounts for this reduction in the voting gap. Is it mainly due to a resource or a return effect? To answer this question the second two panels of *Table 2* report the results from a 2SLS model where reform intensity is used as an instrument for having completed at least three years of post-primary education by age 20.

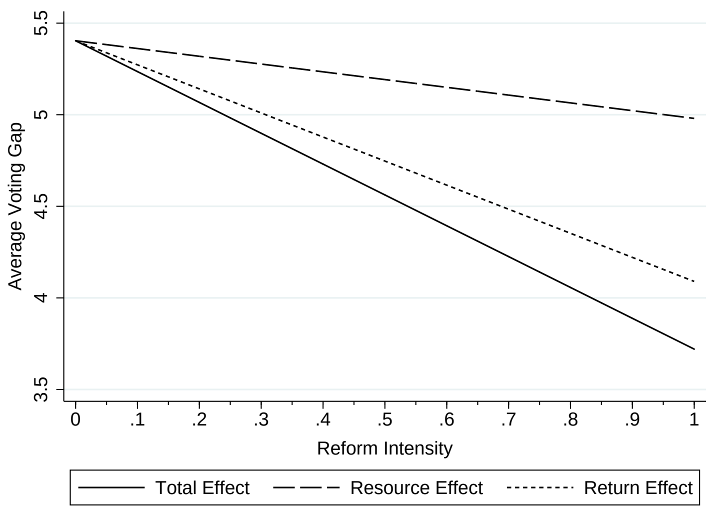
The first-stage results presented in Panel B provide direct evidence on the resource effects in the different socio-economic groups. The results indicate that the resource effect is more pronounced for children in the three lowest family SES quartiles. For children from the lowest quartile of the family distribution the likelihood of completing three years of post-primary education is estimated to increase by more than 21 percentage points as all vocational programs in a municipality are lengthened from two to three years. The corresponding figure for children in the highest quartile is about 6 percentage points, i.e., just slightly more than one fourth of the effect found in the most disadvantaged group. The reason why the resource effect decreases as we move up the social ladder is obviously that children of higher social background are less likely to pursue vocational studies, and as such they were less likely to be affected by this reform.

Turning instead to the return effects these are given by the second-stage results presented in Panel C of *Table 2*.[21] In this setting, the coefficients give us the marginal change in the propensity to vote associated with completing at least three years of post-primary education (rather than *less* than three years). As can be seen from the table it is only among children from the most disadvantaged family background that we find a statistically significant effect of completing three years of post-primary education on voter turnout. In this group completing a three-year program is estimated to increase the prob-

[21] Alternatively, we could have obtained these coefficients by dividing the reduced form coefficients in Panel A by the first-stage coefficients in Panel B.

ability of voting by almost 17 percentage points. For the other three quartile groups the IV-estimates are considerably smaller in magnitude and not statistically significant. As is often the case with instrumental variable models, precision is an issue here. Yet, if we compare the difference in coefficients across groups we find that both the differences between Q2 and Q1 ($p = 0.002$) and Q3 vs. Q1 ($p = 0.017$) are statistically significant at the .05 level, whereas the difference between Q4 and Q1 ($p = 0.359$) despite being large in magnitude does not reach conventional levels of statistical significance.[22] Overall, our results thus appear to be consistent with the *compensation hypothesis*. That is, that at least to some extent, education is a means to compensate for various types of civic disadvantages associated with growing up in low SES homes (e.g., Campbell, 2008).

Figure 5: Decomposing the Reduction in the Voting Gap



The results presented in *Table 2* imply that the return effect was more important than the resource effect in explaining the reduction in the socio-economic voting gap pictured in *Figure 4*. To characterize the relative importance of these two factors in accounting for the reduction in the overall voting gap, *Figure 5* presents the results from two empirically informed thought experiments.

We first imagine a situation in which the return to education is set to the sample average for all socio-economic groups, but where the effects of the reform on educational

[22] However the IV-estimate for Q4 is very imprecisely estimated due to a weak first stage.

attainment (the resource effects) are the ones previously estimated from the data. The development of the voting gap in this scenario is shown by the dashed line in the figure. The dotted line instead shows how the voting gap would vary with reform intensity in a situation where we leave the return effects of the different groups intact, but assign the average resource effect to all four groups.

In line with what is expected based on the results in *Table 2*, the differences in return effects across groups are more important than differences in resource effects in explaining the reduction in the voting gap. Under the assumption of equal return effects in all groups an increase in reform intensity from 0 to 1 would only have reduced the average voting gap by 0.4 percentage points, whereas the corresponding decrease under the assumption of equal resource effects is 1.3 percentage points.

8 How Robust are the Results?

So far we have been studying all individuals born between 1970 and 1974, although the pilot did not affect the length and content of the academic training programs in upper secondary school. The main advantage with this is that we need to be less worried that the results are driven by compositional changes between different types of programs. Yet, given that students in vocational training programs were those primarily affected by the reform we should expect the effect on voting to be more pronounced in this group. In panels A and B of *Table 3* we therefore present separate results for individuals who had not completed an academic upper secondary program by the age of 20 (Panel A) and those who had (Panel B). Admittedly, if the introduction of the reform affected the probability of completing an academic program this means that we will condition on an outcome of the reform, which could bias the results. However, as is shown in *Table A3* in the Appendix, whereas reform intensity has a large effect on the likelihood of completing a 3-year rather than a 2-year vocational program, we only find very marginal effects of reform intensity on the probability of obtaining theoretical rather than vocational education. On average, increasing the share of three-year vocational programs from 0 to 1 is estimated to have reduced the probability of taking a theoretical program at upper secondary level by about

2 percentage points. Given the limited magnitude of this coefficient it seems relatively unproblematic to perform the type of analysis that we do in *Table 3*

A first thing to note is that the results for the sample focusing on individuals with vocational degrees are very similar to those obtained for the full sample. The main difference is that the coefficient of the reform intensity variable for the lowest quartile group (Q1) increases from 3.6 to 5.0, which is to be expected since it was the vocational programs that were affected by the reform. Furthermore, because we find no corresponding effect of reform intensity for this group in Panel B, we can also conclude that the reform effect for individuals of low socio-economic background is entirely driven by those attending vocational training programs in upper secondary school. For the other quartile groups we find no statistically significant effects of the reform among either vocational or theoretical students.

The fact that we find no reform effect among those attending theoretical programs can be interpreted as support for the common trend assumption underlying our identification strategy. To the extent that our findings are driven by unobserved trends at the municipality level we would expect those to affect students of vocational and theoretical programs alike. An alternative way to check for the presence of such unobserved time trends in the data is to pre-date the treatment of interest and examine whether there is also evidence of an effect in the pre-reform period. In Panel C of *Table 3* we therefore artificially change the date of the pilot scheme and assume that it was implemented four years earlier than it actually was.[23] In practice, this means that we examine how reform intensity affected turnout of individuals born between 1966 and 1970 and therefore were too old to be affected by the pilot scheme. If we were to find an effect of this "placebo reform" it would suggest the presence of important pre-reform trends in the data, i.e. that the results are driven by unobserved differences between municipalities. Consequently, it is reassuring to find that the coefficient of the artificial reform intensity variable is small in magnitude and statistically insignificant in all quartile groups. The results presented in *Table 3* thus provide strong support for the common trend assumption underlying our

[23] The reason why we pre-date the reform by four years is that we want to make sure that all cohorts included in this analysis were unaffected by the actual reform, and the youngest unaffected cohort is those born in 1970.

Table 3: Sensitivity Analyses

	All	Q1	Q2	Q3	Q4
Panel A. Individuals with vocational degree					
Reform intensity	1.13	5.01***	−1.17	−1.08	−0.60
	(0.78)	(1.34)	(1.02)	(1.51)	(2.03)
Gender	3.01***	3.80***	3.36***	2.34***	1.21***
	(0.14)	(0.25)	(0.27)	(0.27)	(0.31)
Immigrant background	−7.39***	−8.74***	−6.87***	−6.03***	−5.97***
	(0.30)	(0.47)	(0.38)	(0.49)	(0.49)
Family SES	4.20***				
	(0.13)				
Observations	284,087	97,215	85,992	64,635	36,245
Panel B. Individuals with theoretical degree					
Reform intensity	0.81	0.14	2.03	1.26	0.54
	(0.66)	(1.63)	(1.51)	(1.16)	(0.84)
Gender	0.51***	0.98***	1.01***	0.39*	0.16
	(0.11)	(0.34)	(0.28)	(0.20)	(0.14)
Immigrant background	−5.35***	−8.27***	−5.56***	−3.97***	−4.14***
	(0.32)	(0.62)	(0.56)	(0.40)	(0.32)
Family SES	1.13***				
	(0.13)				
Observations	230,160	31,346	42,569	63,928	92,317
Panel C. Pre-dating reform intensity with 4 years					
Reform intensity	−0.09	−0.63	−0.34	−0.28	1.02
	(0.51)	(0.93)	(0.85)	(0.92)	(0.77)
Gender	3.09***	4.50***	3.99***	2.72***	1.16***
	(0.10)	(0.21)	(0.19)	(0.17)	(0.14)
Immigrant background	−5.04***	−6.36***	−5.36***	−4.14***	−3.51***
	(0.19)	(0.40)	(0.30)	(0.28)	(0.28)
Family SES	3.92***				
	(0.09)				
Observations	524,318	131,078	131,081	131,076	131,083

Notes: All models include a full set of fixed effects for birth year, home municipality, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

difference-in-difference strategy.

We have also performed a number of additional robustness checks. Because of shortage of space, we only provide a brief summary of these results here, but full details are given in the Appendix. First, as mentioned earlier, it is not obvious how to define reform intensity in municipalities where no vocational programs were offered. To examine how sensitive the results are to our choice of method of handling this problem we have re-estimated the main specification including only the 193 (out of 284) municipalities offering vocational training programs at the upper secondary level. This does not affect the results (see *Table A2* in the Appendix).

Moreover, for reasons provided above, we have relied on a linear probability model for estimation even though our main outcome variable is binary. We have, however, estimated both the reduced form and the first-stage equations by means of logit regression, and in terms of average marginal effects the results are very similar to those from the linear probability model (see *Table A1* in the Appendix). On a more substantive note, the finding that the positive effect of the reform is restricted to individuals from low SES-homes holds true also when interpreting the logit coefficients in terms of odds-ratios. This suggests that the lower return to education in higher SES-groups is not primarily due to a ceiling effect since odds-ratios, unlike probabilities, are not affected by the mean of the dependent variable (Mare, 1980).

Finally, we have examined the sensitivity of our results with respect to our measure of family background. First, to check that our results are not unduly driven by the choice to split the SES measure into quartiles we have re-estimated the reduced form equation for each decile of the family SES distribution. Although this lessens the precision of the estimates, this more fine-grained analysis clearly supports the view that the positive effect of the reform on voting is to be found at the bottom of the family SES distribution (see *Figure A3*). Second, we have estimated separate models for each of the three sub-items making up our family SES measure. Although the reform effect shrinks somewhat in magnitude—it ranges from 2.2 to 2.9 percentage points—when considering the different indicators in isolation, the overall results of this disaggregated analysis closely mimics the results we obtain with the composite index (see *Table A4* in the Appendix).

9 What Accounts for the Effect?

Thus far we have learned that the reform reduced the voting gap primarily through a return effect such that the positive impact of education on turnout is greater among individuals of low socio-economic background. A natural next question to ask is why the reform-induced additional schooling increased turnout in this group. As mentioned in the theory section a number of mechanisms have been suggested as explanations for why education may influence political participation. First, education may have a direct effect on individuals' propensity to engage in the political sphere. According to this model it can be hypothesized that the lengthening of vocational training programs from two to three years—and the increased focus on civic education—strengthened attitudinal factors shown to predict voter turnout in earlier studies such as political knowledge (Galston, 2001), interest in politics (Verba et al., 1995), internal as well as external political efficacy (Finkel, 1985), and support for the norm of voting (Blais and Young, 1999). Second, education may have a more indirect effect on political participation by influencing individuals' social and economic status. These intervening factors will in turn determine social and political network centrality. Thus, individuals with higher education will be more closely connected and exposed to networks that boost participation (Verba et al., 1995; Nie et al., 1996).

Due to data constraints, a more in-depth analysis of the causal mechanisms underlying the observed relationship between reform status and turnout is unfortunately outside the scope of the current article, but we will use available data to try to shed at least some light on this important issue. Above all, our data facilitates a simple test of the second and more indirect link between education and political participation through possible mechanisms such as income, occupation, family status, and political activity in surrounding social networks. In *Table 4* we therefore present results from a mediation analysis in which we sequentially control for a number factors. This analysis is based on a somewhat smaller sample than before because we have invoked the requirement that we have complete data on all potential mediators. In the first column we therefore re-estimate the reduced form model for individuals of low social background (Q1) with complete data records. In this restricted sample the reform is estimated to increase the probability of voting by about 3.5 percentage points, which is very close to the effect previously found for the larger sample (3.6).

Table 4: Mediation Analysis: Social Position

	(1)	(2)	(3)	(4)	(5)	(6)
Reform intensity	3.53***	3.41***	3.45***	3.37***	2.93***	2.92***
	(1.00)	(1.02)	(1.02)	(1.02)	(0.97)	(0.98)
Controls						
Gender	4.58***	2.47***	2.28***	2.20***	2.96***	2.68***
	(0.20)	(0.25)	(0.24)	(0.24)	(0.24)	(0.24)
Immigrant background	−5.85***	−5.89***	−5.49***	−5.84***	−4.15***	−3.99***
	(0.34)	(0.34)	(0.33)	(0.33)	(0.30)	(0.30)
Earnings		0.14***	0.13***	0.11***	0.14***	0.11***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Marital status		4.31***	4.07***	4.32***	−3.20***	−3.21***
		(0.27)	(0.27)	(0.27)	(0.28)	(0.28)
Number of children		2.14***	1.98***	2.13***	−2.49***	−2.53***
		(0.11)	(0.11)	(0.11)	(0.12)	(0.12)
Turnout neighbors			36.27***			12.59***
			(2.21)			(2.24)
Turnout colleagues				5.23***		4.16***
				(0.48)		(0.45)
Turnout family members					27.10***	26.89***
					(0.35)	(0.35)
Occupational dummies	No	Yes	Yes	Yes	Yes	Yes
Family SES	Q1	Q1	Q1	Q1	Q1	Q1
Observations	101,722	101,722	101,722	101,722	101,722	101,722

Notes: All models include a full set of fixed effects for birth year, home municipality, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

In Column 2 we add controls for marital status (1=married), the number of children below 19 years of age, monthly labor income (in 1,000 SEK), and occupational dummies (based on four-digit occupational codes). Although the results suggest that all these variables are related to voting the effect of increasing the share of three-year training programs only decreases by about 8 percent when controlling for these factors. In the remaining columns we also add controls for the political activity of surrounding networks. More precisely, in columns 3–5 we, in turn, control for the average turnout level among other individuals living in the same voting district (neighbors), the average turnout level of other individuals working at the same establishment (colleagues), and the average turnout of other eligible voters belonging to the same household (family members). In the last column we control for all these factors simultaneously. As is to be expected there is a positive association between the probability of voting and the political activity of one's social networks. Nonetheless, to judge from these results, much discussed factors such as income, occupation, and social networks can only account for one fifth of the overall educational effect.

Thus, the lion's share of the reform effect seems to be mediated via other pathways. As already mentioned a likely possibility is that the effect is driven by various factors more directly related to the nature and content of education, such as skills and norms the individuals learn in school. Unfortunately our data do not permit any direct tests of the degree to which the reform effect on turnout is mediated by such factors. However, in the Appendix we present results on how the reform effect varies by birth-cohorts that provide some indirect insights into and support for the attitudinal pathway. Above all, the pattern of cohort-specific reform effects suggest two things. First of all, the effect of the reform does not seem to be driven by the increase in average age at which the individuals typically moved out of their parents' home. Second, the reform had a markedly larger effect on turnout when the additional year of schooling coincided with an election. Under the reasonable assumption, which we substantiate in the Appendix, that schools put extra focus on civics education and, especially, the importance of voting in connection with the general elections this result is consistent with a scenario in which the reform had downstream effects on later-life turnout propensities mediated by skills and norms learned in school.

10 Conclusion

By using population based data on voter turnout and exploiting the exogenous variation in educational supply brought by the reform of the Swedish upper secondary school system in the 1990s we provide a more detailed and nuanced account of education effects than previously appreciated. The research on effects of education on political participation has focused on debating whether education has a causal impact or whether the relationship is spurious and can be explained with reference to unmeasured pre-adult factors. We suggest that this is a simplified way of thinking. Instead of focusing on whether there is an effect or not we focus on potential heterogeneities and provide a framework for analyzing how education reforms can affect inequalities in voting related to the socio-economic status of one's family origin.

More specifically we show that education reforms can affect voting by resource effects that change the allocation of education between socio-economic groups and return effects which refer to the size of the effect of education in different groups. We find that the Swedish reform decreased the voting gap mainly via a return effect indicating a stronger effect of education on electoral participation among those from families of low socio-economic status.

This finding is important since it suggests that education can have a compensatory effect on students from families of lower socio-economic status. It seems that what these students lack in terms of a stimulating home environment conducive to political engagement can be at least partly made up for by strengthening the school environment.

Some researchers have worried that education reforms will serve to accelerate socio-economic differences due to a larger return effect among the most advantaged. Our data show little support for such worries. On the contrary, while increased education does not seem to raise participation levels for everyone it appears beneficial for those from the most disadvantaged backgrounds and can thereby be a means to reduce socio-economic inequalities in voting.

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11 Appendix

11.1 Data Availability

In this paper we use individual level information obtained from various administrative registers. The data are stored on an encrypted server at Statistics Sweden and all our analysis have been conducted through a remote desktop application. We are under contractual obligation not to disseminate these data to other individuals.

For interested researchers there are, however, two ways to get access to the administrative data used in this paper for replication purposes. The first possibility is to order the data directly from Statistics Sweden. Currently, Statistics Sweden require that researchers obtain a permission from a Swedish Ethical Review Board before data can be ordered (a description, in Swedish, of how to order data from Statistics Sweden is available at: http://www.scb.se/sv_/Vara-tjanster/Bestalla-mikrodata). We will also make available a complete list of the variables that we ordered from Statistics Sweden for this project.

The second possibility to replicate our analyses for interested researchers is to come to Uppsala and reanalyze these data through the same remote server system that we used for our analyses. Any researcher interested in using this option needs to contact us before coming to Sweden so that we can arrange with Statistics Sweden that the researcher is temporarily added to our research team, which is required in order to get access to the remote server system.

11.2 Variables and Data Sources

Voter Turnout

Beginning in 1991, Statistics Sweden has collected information on individual voter turnout for a random sample of about 1 percent of the electorate after each general election by manually checking the electoral rolls. Population data on the entire electorate have, however, not hitherto been available. We therefore decided to collect that data ourself for the 2010 general election.

In Sweden the electoral rolls are still maintained in paper form, and each roll lists all eligible voters living a particular voting district. The electoral rolls contain preprinted information on the full name and a unique social security number (*personnummer*) for all eligible voters, and hand-written information, filled in by the election officials, on whether particular individuals chose to vote in each of the three different elections at the municipal, county and national levels. Whereas abstention is indicated by an empty box for the relevant election, voting can be indicated by either of three hand-written symbols: a *P* for early postal voting, a *V* for late postal voting, and a */* (slash) for voting in a polling place at the actual day of the election.

After the elections the electoral rolls are archived at the municipality level. The first task in the data collection process was therefore to contact all 290 municipalities in Sweden and ask them if they could scan or copy the 2010 electoral rolls for us. In those cases where the municipalities were unable to do the work for us, our research assistants went to the municipalities to scan the material.[24] Using this strategy we were able to obtain digital copies of the electoral rolls for 282 out of the 290 municipalities. In 7 municipalities they were unable to locate the electoral rolls for the 2010 election, and in one municipality they would not let us scan or copy the electoral rolls. In addition, in a few cases the electoral rolls for specific electoral districts were missing, and, in a somewhat larger number of cases, individuals pages were accidentally neglected in the process of scanning.[25]

The next step in the process was to retrieve the information of interest from the scanned images. First all images were straightened and converted to have the same resolution. Then the preprinted parts of the electoral rolls were digitized using standard techniques for optical character recognition (OCR), employing the open source OCR engine Tesseract.

Digitizing the handwritten information on actual voting was somewhat more challenging. To do this, all the boxes in which the election officials keep their records were converted to binary images using a thresholding algorithm. That is, every pixel in these boxes were assigned either a value of 0 or 1 depending on their relative darkness. In an

[24]We are grateful to our research assistants Edwin Sönnergren and Oskar Hultin Bäckersten for their help in the data collection process.
[25]We are currently in the process of trying to acquire as much as possible of the missing material.

ideal world empty boxes, indicating voting abstention, would then be represented by all zeros, whereas the images for voters would contain a larger number of ones, and the location of these ones should represent the pencil strokes associated with each of the three different symbols used for indicating voting. In practice, however, there could be some black pixels (ones) also in empty boxes, due to stains on the paper or to imperfect scanning, and both the number and location of the black pixels associated with a particular symbol, such as *P*, will be highly dependent on the individual handwriting of the different election officials keeping the records. We therefore designed a procedure for making an initial classification of the content of all the individual boxes, by counting the number of black pixels in different directions of the individual images. In cases where the original image was of good quality this initial classification proved highly accurate. To improve the accuracy even further, we also developed a graphical user interface that was used to view and, when necessary, manually correct the automated classifications.[26] To save labor, we decided to focus on the classification of *voting* vs. *abstention*. Consequently, in the manual correction stage we did not attempt to correct misclassifications with respect to type of voting, e.g. voting in a polling place vs. postal voting.

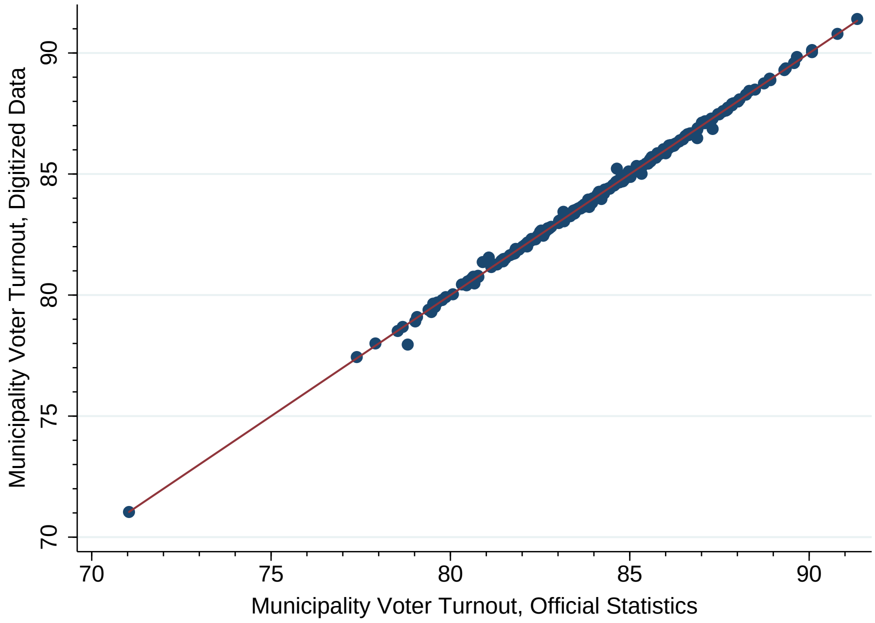
Following this procedure we were able to retrieve information on voter turnout for 96.5 percent of those eligible to vote in the election to the national parliament in 2010 (6,873,661 out of 7,123,651 individuals). *Figure A1* provides a first check of the quality of these data, by comparing aggregate voter turnout on the municipality level in our data with official election statistics.[27] As can be seen, aggregate turnout are virtually the same in the two data sources, and the only observation that is located visibly below the 45 degree line is one of the municipalities for which we, currently, lack information on a number of electoral districts.

Another way to check the quality of our data is to compare our indicator of voter turnout with that of the random sample collected by Statistics Sweden (SCB) after the 2010 election. By using the unique social security number included in both data sets we

[26]The software solution used for the digitalization was designed and developed by Anders Larsson www.ormbunkar.se.

[27]The reason why we do not use the electoral district as the unit of analysis in this graph is that the late postal votes are aggregated up to the municipality level in the official records.

Figure A1: Voter Turnout at the Municipality Level



were able to compare our classification with that of SCB for a total sample of 85,449 individuals. As can be seen from *Figure A2*, our digitized information on voter turnout conforms with SCB's manual codings in 99.7 percent of the cases (85,235/85,449). In practice, this means that very little, if anything, is lost by using our automated, and much less labor intensive, procedure for collecting data on voter turnout.

Figure A2: Comparing Classifications at the Individual Level

		SCB's Classification	
		Abstained	*Voted*
Our Classification	*Abstained*	13,869	86
	Voted	128	71,366

Data from Administrative Registers

In the main analysis we make use of data from various administrative registers. In this subsection we describe this data in somewhat more detail.

`Reform intensity` — The share of available vocational programs in a municipality that were three years long. For municipalities not offering any vocational programs during the study period we use the reform intensity score for the municipality in which most students from the 1970 cohort (the cohort preceding the first reform cohort) attended a vocational program. For instance, if municipality *A* were not offering any vocational programs and most of the individuals born in 1970 from municipality *A* chose to attend their vocational studies at the upper secondary level in the nearby municipality *B*, the reform intensity score in municipality *A* will be the share of three-year vocational programs in municipality *B*. The data necessary to construct the reform intensity measure were obtained from the Upper Secondary School Application Record (*Gymnasieskolans sökanderegister*). We are very grateful to Caroline Hall for sharing the stata code used to create this indicator.

`Home municipality` — Code for municipality of residence in 1985. Information is retrieved from the 1985 census.

`Birth month` — Information is retrieved from the Swedish Population Register.

`Gender` — Equal to 1 if female. Information is retrieved from the Swedish Population Register.

`Immigrant background` — Equal to 1 if the individual or at least one parent is born abroad. Information is retrieved from the Swedish Population Register.

`Completed three-year program` — Equal to 1 if the individual has completed at least three years of post-primary education at the age of 20.

`Labor income` — Individual monthly labor income in 2010 (in 1,000 SEK). The variable is retrieved from the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

`Martial status` — Equal to 1 if an individual is married or in a civil union. Information is retrieved from the LISA database for the year 2010.

`Number of children` — Number of children under the age of 18 living in an individual's household. Information is retrieved from the LISA database for the year 2010.

`Turnout neighbors` — Average turnout in the electoral district in which the individual was living in 2010.

`Turnout colleagues` — Average turnout among the other individuals employed in the same establishment (arbetställe). Information on establishment codes was obtained from the LISA database for the year 2010.

`Turnout family members` — Average voter turnout among the other individuals belonging to the same household. Information for identifying families was retrieved from the LISA database for the year 2010.

`Occupation code` — Four digit occupation code (SSYK-96) similar to the international ISCO-88 code. Information is retrieved from the LISA database for the year 2010.

`Parental income` — Average labor income of mother and father. Information is retrieved from the 1985 census.

`Parental education` — Highest education, in years, of mother and father. Information is retrieved from the 1985 census.

`Parental occupational status` — This variable is based on the occupational codes for mothers and fathers in the 1985 census (NYK-85). We converted NYK-85 codes into ISCO-88 format by using conversion keys developed by Statistics Sweden and Erik Bihagen (2007). In the next step, the occupational codes for mothers and fathers were translated into three different, but highly correlated, measures of occupational status: the *International Socio-Economic Index* (ISEI, Ganzeboom et al., 1992), the *Standard International Occupational Prestige Scale* (SIOPS, Treiman, 1977), and the *International Cambridge Scale* (ICAMS, Meraviglia et al., 2016; Prandy and Jones, 2001).

The SIOPS scale was constructed by Treiman (1977) through averaging the prestige scores of about 60 national prestige scales and then mapping the resulting scores into ISCO-68 occupational titles. The ISEI indicator is based on a different rationale and attempts to capture the process that translates educational credentials into income (Ganzeboom et al., 1992). More technically, the measure was constructed through an optimal scaling procedure in such a way as to maximize the role of occupation as a mediator between education and income. Lastly, the ICAMS score uses detailed information on inter-occupational marriage patterns to statistically estimate the "social distance" between different types of occupations (Prandy and Jones, 2001). The indicator thus measures occupational stratification. For reasons of international comparison, we here use the international CAMSIS scale developed by Meraviglia et al. (2016) based on information available in surveys of the International Social Survey Programme (ISSP) for the years 2001 to 2007.

The code to translate census occupational codes into ICAMS, ISEI, and SIOPS was downloaded from http://www.harryganzeboom.nl/isco88/index.htm. We then computed the occupational status of fathers and mothers, respectively, as the average of these three indicators (they all vary between 0 and 100). Finally, parental occupational status is the maximum of father's and mother's occupational status. For a small number of individuals that have two non-employed parents, parental occupational status have been set to its sample minimum value.

`Family SES` — This is a simple unweighted average of parental earnings, parental education, and parental occupational status. To adjust for differences in scales between the variables, all three sub-items were initially standardized to have a mean of 0 and a standard deviation of 1 in the sample under study. Parental earnings were top coded at the 99th percentile before being standardized. In case information on one of the indicators is missing the index is based on the two indicators for which data is available (complete data on all three sub-items are available for 94% of the observations).

ESS Data

The analysis of data from the European Social Survey is based on data from rounds 1-5 of this survey for the following 25 countries: Austria, Bulgaria, Croatia, Czech Republic, Denmark, Estonia, Finland, France, Germany, Hungary, Iceland, Ireland, Israel, Italy, Lithuania, Netherlands, Norway, Poland, Portugal, Slovakia, Slovenia, Spain, Sweden, Switzerland, and the United Kingdom.

Three of the countries that are included in the ESS—Turkey, Russia, and Ukraine—were dropped from the analysis because they are not coded as *Free* by Freedom House in all survey years, and four countries—Cyprus, Luxembourg, Greece, and Belgium—were dropped from the analysis because voting is compulsory by law in these countries. Moreover, to ensure that all respondents included in the analysis were eligible to vote individuals born outside their country of residence are excluded from the data. Finally, we decided to focus on individuals between 25 and 65 years of age.

`Occupational status` — The average of the ICAMS, ISEI, and SIOPS scores for each individual (see the discussion above). These scores are computed using the ISCO-88 codes included in the ESS.

`Income` — Total household income, after tax and compulsory deductions, from all sources measured in deciles of the national income distribution. This indicator is only available four rounds 4–5 of ESS, and it is based on the variable named HINCTNTA in the original ESS data set.

`Education` — Years of completed education. The indicator is based on the variable named EDUYRS in the original ESS data set.

`Gender` — Equal to 1 for female respondents.

`Parental education` — Highest education, in years, of father and mother. In the original ESS file educational attainment of mothers and fathers are coded using the five-level ISCED-97 code, but we have translated these codes into years of education following the procedure devised by Okbay et al. (2016, Supplementary Table 1.2): Less than lower secondary education (7 years); Lower secondary education (10 years); Upper secondary education (13 years); Post-secondary non-tertiary education (15 years);

Tertiary education (20 years).

`Parental occupational status` — In the standard ESS data, occupational codes for parents—when the respondent was 14 years old—is reported on a scale with only 9 categories. However, the raw data also include free text strings with information on parental occupation. Together with his research team, Harry Ganzeboom have converted these free text codes into ISCO-88 codes for ESS rounds 1–5 (Ganzeboom, 2013). Using the ISCO-88 codes provided by Ganzeboom we then computed the average of the ICAMS, ISEI, and SIOPS scores for the mother and father, respectively. Finally, parental occupational status is the maximum of father's and mother's occupational status so computed.

`Family SES` — This is a simple unweighted average of parental education and parental occupational status. To adjust for differences in scales between variables, cohorts and countries both sub-items were initially standardized by cohort and country to have a mean of 0 and a standard deviation of 1.

`Vote` — Equal to 1 for respondents who reported to have voted in the most recent election in their country. The indicator is based on the variable named VOTE in the original ESS data set.

`Reduce income differences` — Equal to 1 for respondents who answered either *agree strongly* or *agree* to the statement that "The government should take measures to reduce differences in income levels". The indicator is based on the variable named GINCDIF in the original ESS data set.

`Allow large immigration` — Equal to 1 for respondents who answered either *Allow many to come and live here* or *Allow some* to the question "To what extent do you think [country] should allow people of the same race or ethnic group as most [country] people to come and live here?". The indicator is based on the variable named IMDFETN in the original ESS data set.

`Gays free to live as they wish` — Equal to 1 for respondents who answered either *agree strongly* or *agree* to the statement that "Gay men and lesbians should be free to live their own life as they wish". The indicator is based on the variable named

FREEHMS in the original ESS data set.

`Ban undemocratic parties` — Equal to 1 for respondents who answered either *agree strongly* or *agree* to the statement that "Political parties that wish to overthrow democracy should be banned". The indicator is based on the variable named PRTYBAN in the original ESS data set.

`Science can save environment` — Equal to 1 for respondents who answered either *agree strongly* or *agree* to the statement that "Modern science can be relied on to solve environmental problems". The indicator is based on the variable named SCNSENV in the original ESS data set.

`Contacted politician` — Equal to 1 for respondents who had contacted a politician or a government official during the last 12 months. The indicator is based on the variable named CONTPLT in the original ESS data set.

`Demonstrated` — Equal to 1 for respondents who had taken part in a lawful public demonstration during the last 12 months. The indicator is based on the variable named PBLDMN in the original ESS data set.

`Signed petition` — Equal to 1 for respondents who had signed a petition during the last 12 months. The indicator is based on the variable named SGNPTIT in the original ESS data set.

`Worked for party` — Equal to 1 for respondents who had done party work during the last 12 months. The indicator is based on the variable named WRKPRTY in the original ESS data set.

`Member of party` — Equal to 1 for respondents who were party members. The indicator is based on the variable named MMBPRTY in the original ESS data set.

11.3 Additional Analyses and Sensitivity Checks

Using a logit model

For reasons discussed in the main text, we decided to use a linear probability model for our empirical analysis. However, in *Table A1* we estimate both the reduced form and the first-stage equations by means of a logit regression. The coefficients reported in the table are odds-ratios. Moreover average marginal effects—which are directly comparable to the coefficients of the linear probability model estimated in the main text—are presented within parentheses.

As can be seen from comparing these results with those of the main text, we obtain very similar results when using a logit model instead of a linear probability model. Unfortunately, there are no easy analog to the 2SLS model in the case when both the outcome of interest and the endogenous variable are binary. Available options, such as the bivariate probit, rest on very stringent identification assumptions and have proved to be difficult to estimate due to their numerical instability (Freedman and Sekhon, 2010). In line with these theoretical results, when attempting to estimate a bivariate probit model we had problems to get the models to converge for some groups and the results that we did obtain tended to be highly sensitive to different model specifications and sample restrictions (unlike the first-stage and reduced form equations). We therefore decided not to report these results here.

Exlcuding municipalities without vocational programs

As discussed in the main text, it is not obvious how to define reform intensity in municipalities where no vocational programs were offered. To examine how sensitive the results are to our choice of method to handle this problem *Table A2* displays the results when including only the 193 (out of 284) municipalities offering vocational programs at the upper secondary level. The results for this restricted sample is very similar to those obtained for the larger sample in the main analysis.

The Effect of the Reform on Educational Choices

In the main text we discuss the issue of whether the reform affected the likelihood of attending different types of education. One way to analyze this issue is to study the effect of the reform on different types of educational choices. *Table A3* displays the effect of the reform on not completing secondary education (Panel A), completing 2-year vocational education (Panel B), completing 3-year vocational education (Panel C), or completing theoretical upper secondary or some type of tertiary education at age 20. In each case

Table A1: Logit Results, Odds ratios and Marginal Effects

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
Reform intensity	1.08	1.30***	0.95	0.97	1.01
	(0.76)	(3.65)	(−0.57)	(−0.23)	(0.09)
Gender	1.26***	1.33***	1.34***	1.22***	1.09***
	(2.39)	(3.93)	(3.24)	(1.79)	(0.57)
Immigrant background	0.59***	0.59***	0.61***	0.61***	0.55***
	(−5.29)	(−7.40)	(−5.42)	(−4.35)	(−4.12)
Family SES	1.50***				
	(4.11)				
Observations	514,204	128,532	128,524	128,540	128,491
Panel B. Dependent variable: $\geq$ 3 years of post-primary educ. at age 20.					
Reform intensity	2.25***	2.52***	2.31***	2.23***	1.45***
	(17.14)	(18.07)	(18.67)	(19.09)	(7.13)
Gender	1.15***	1.21***	1.18***	1.16***	1.05***
	(2.98)	(3.78)	(3.74)	(3.57)	(0.88)
Immigrant background	1.03	1.13***	1.09***	0.98	0.79***
	(0.58)	(2.34)	(1.84)	(−0.54)	(−4.50)
Family SES	2.49***				
	(19.30)				
Observations	514,227	128,543	128,546	128,554	128,551

Notes: All models include a full set of fixed effects for birth year, home municipality, and father's and mother's birth years. Numbers without parentheses are odds-ratios and numbers in parentheses are average marginal effects. Standard errors allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

Table A2: Restricting the analyses to municipalities with vocational programs.

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
Reform intensity	0.64	3.36***	−0.87	0.68	−0.54
	(0.54)	(0.99)	(1.07)	(1.08)	(0.94)
Gender	2.37***	3.90***	3.37***	1.78***	0.49***
	(0.12)	(0.23)	(0.22)	(0.19)	(0.14)
Immigrant background	−6.43***	−8.31***	−5.89***	−4.74***	−4.80***
	(0.29)	(0.45)	(0.35)	(0.38)	(0.34)
Family SES	3.91***				
	(0.11)				
Panel B. Dependent variable: At least 3 years of post-primary educ. at age 20.					
Reform intensity	17.24***	20.96***	20.12***	18.40***	5.96***
	(2.22)	(3.07)	(3.06)	(2.12)	(2.07)
Gender	2.77***	3.53***	3.56***	3.29***	0.85**
	(0.34)	(0.42)	(0.50)	(0.50)	(0.34)
Immigrant background	0.90**	3.15***	2.34***	0.09	−4.59***
	(0.44)	(0.58)	(0.68)	(0.69)	(0.45)
Family SES	19.30***				
	(0.15)				
Panel C. Dependent variable: Voting.					
Completed 3-year program	3.94	16.04***	−4.32	3.68	−8.98
	(3.22)	(5.28)	(5.30)	(5.86)	(16.37)
Gender	2.26***	3.33***	3.52***	1.66***	0.57***
	(0.15)	(0.32)	(0.28)	(0.27)	(0.21)
Immigrant background	−6.43***	−8.82***	−5.79***	−4.74***	−5.21***
	(0.29)	(0.51)	(0.35)	(0.37)	(0.80)
Family SES	3.11***				
	(0.62)				
Observations	419,748	101,437	103,887	106,040	108,384

Notes: All models include a full set of fixed effects for birth year, home municipality, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

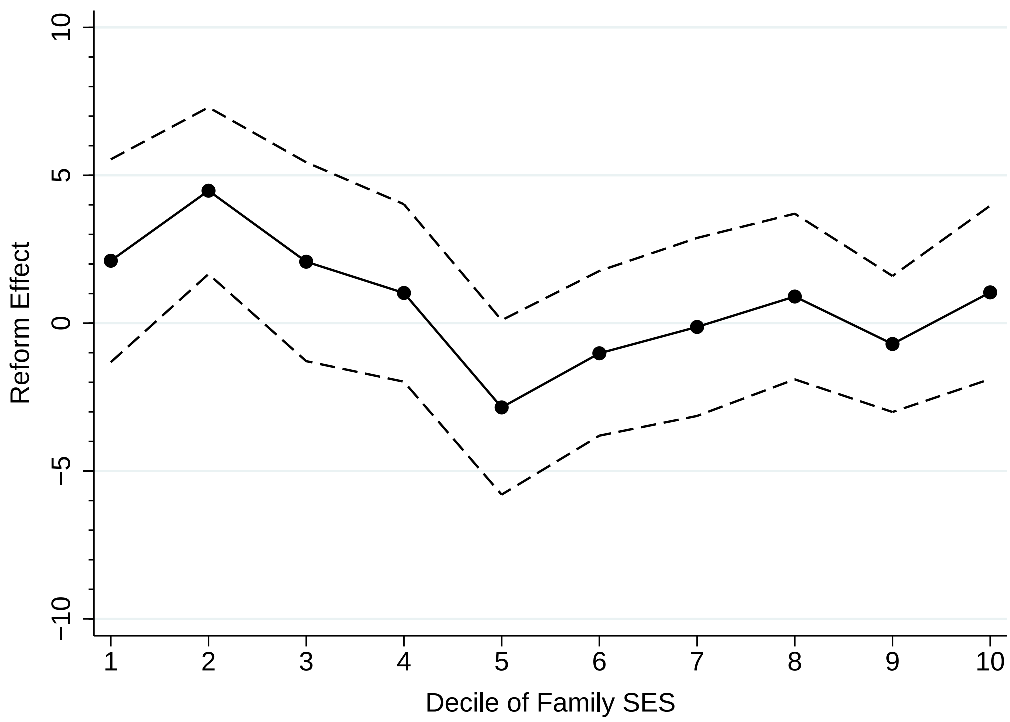
all remaining educational categories are lumped together, e.g., in Panel B we estimate the effect of reform intensity of completing a 2-year vocational program rather than *not* completing such a program.

As expected, the main effect of the reform was to decrease the share of individuals completing 2-year vocational programs and increase those completing a 3-year vocational program. We find no evidence that the reform affected the likelihood of pursuing post-primary education (Panel A). However, for some socio-economic groups we find a small decrease in the probability of completing theoretical upper secondary education.

Dividing the family distribution into finer groups

In the main analysis we study the effect of the reform by family SES quartile. To check that our main findings are not driven by this, admittedly rather arbitrary, grouping we have re-estimated the reduced form equation for each decile of the family SES distribution. The results from this exercise is shown in *Figure A3*. Although the coefficients are

Figure A3: Reform Effect by Family SES Decile



rather imprecisely estimated the overall pattern of the coefficients are well in line with the

Table A3: The Reform and Educational Choices

	All	Q1	Q2	Q3	Q4
Dependent Variable: Less than secondary education at age 20					
Reform intensity	0.52	1.04	0.71	1.09	−0.57
	(0.63)	(1.38)	(1.01)	(0.99)	(0.94)
Dependent Variable: 2-year vocational education at age 20					
Reform intensity	−17.89***	−22.58***	−21.20***	−17.96***	−5.66***
	(1.94)	(2.61)	(2.56)	(2.21)	(1.60)
Dependent Variable: 3-year vocational education at age 20					
Reform intensity	20.15***	24.70***	23.72***	19.34***	8.50***
	(2.00)	(2.34)	(2.39)	(2.18)	(1.51)
Dependent Variable: Theoretical upper secondary education or tertiary educ. at age 20					
Reform intensity	−2.33**	−2.62**	−3.06**	−1.82	−2.12
	(0.94)	(1.33)	(1.46)	(1.71)	(1.52)
Observations	514,247	128,561	128,561	128,563	128,562

Notes: All models control for gender and immigrant background and include a full set of fixed effects for birth year, home municipality, and father's and mother's birth years. In addition, family background is included as a control in column 1. Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

findings reported in the main text. Clearly, the positive effect of the reform on voting is most marked in the bottom of the family SES distribution.

Disaggregating the family SES measure

In line with much previous research we have used a composite measure of SES. Although we believe that there are good theoretical reasons for doing so, it can nevertheless be interesting to disaggregate the effect of family SES into its different parts. Towards this end, we have estimated separate models for each of the three sub-items making up our family SES measure. The results are reported in *Table A4*.

As can be seen the reform effect in the first family background quartile shrinks somewhat in magnitude (the coefficients range from 2.2 to 2.9 percentage points) when considering the different indicators in isolation. This could be taken to indicate that the individuals that are situated in the bottom quartile on one of these variables, e.g., parental earnings, is on average less disadvantaged than an individual that are situated in the bottom quartile of the composite measure (since the negative effect of low parental earnings may be offset by high parental education or occupational status). This being said, the overall pattern of results remains very similar also when disaggregating the family SES measure into its constituent parts.

Cohort-specific reform effects

It can be hypothesized that the lengthening of vocational programs from two to three years—and the increased focus on civic education—strengthened attitudinal factors shown to predict voter turnout in earlier studies such as political knowledge (Galston, 2001), interest in politics (Verba et al., 1995), internal as well as external political efficacy (Finkel, 1985), and support for the norm of voting (Blais and Young, 1999).

Previous studies on the effects of civic education and educational attainment on these precursors to turnout behavior lend support to this putative causal mechanism (Jackson, 1995; Verba et al., 1995; Galston, 2001; Green et al., 2011). Unfortunately our data does not permit a direct test of the degree to which the reform effect on turnout is mediated by these factors. However, a closer look at if and how the reform effect on turnout varies by birth-cohorts can provide some insights into the attitudinal pathway.

Table A4: Alternative Measures of Family Background

	All	Q1	Q2	Q3	Q4
Panel A. Quartiles based on parental education.					
Reform intensity	0.82	2.20**	0.19	0.26	0.70
	(0.52)	(1.02)	(0.96)	(1.09)	(0.99)
Gender	2.41***	3.90***	2.91***	1.81***	0.67***
	(0.11)	(0.21)	(0.19)	(0.17)	(0.14)
Immigrant background	−6.95***	−8.53***	−6.84***	−6.12***	−5.95***
	(0.28)	(0.48)	(0.31)	(0.33)	(0.43)
Family Educ.	0.94***				
	(0.03)				
Observations	513,263	123,894	144,807	146,719	97,843
Panel B. Quartiles based on parental occupational status.					
Reform intensity	0.89*	2.90**	−0.10	0.02	0.56
	(0.52)	(1.14)	(1.16)	(0.93)	(0.92)
Gender	2.36***	3.59***	3.66***	1.88***	0.68***
	(0.10)	(0.20)	(0.23)	(0.16)	(0.16)
Immigrant background	−6.42***	−8.65***	−6.08***	−4.46***	−5.47***
	(0.27)	(0.45)	(0.35)	(0.31)	(0.35)
Family occ. status	0.18***				
	(0.00)				
Observations	474,739	117,957	93,623	144,304	118,855
Panel B. Quartiles based on parental earnings.					
Reform intensity	0.89*	2.46**	0.60	0.08	0.25
	(0.52)	(1.02)	(1.01)	(0.80)	(1.17)
Gender	2.38***	3.36***	2.63***	2.44***	1.05***
	(0.11)	(0.20)	(0.20)	(0.17)	(0.17)
Immigrant background	−6.59***	−8.33***	−6.43***	−6.05***	−5.21***
	(0.27)	(0.43)	(0.37)	(0.35)	(0.29)
Family earnings	0.01***				
	(0.00)				
Observations	514,247	128,425	128,375	128,880	128,567

Notes: All models include a full set of fixed effects for birth year, home municipality, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

Above all, the 1973 cohort is set apart from the other birth cohorts by the fact that the extra year in upper secondary school among the treated individuals (autumn 1991 to spring 1992) coincided with the general election in September 1991 whereas the untreated students in this birth-cohort completed their two years of schooling (autumn 1989 to spring 1991) in between the two elections in 1988 and 1991. The treated individuals in the other three treated cohorts—born in 1971, 1972 and 1974—completed their third year in upper secondary school in off-election academic years (1989/1990, 1990/1991 and 1992/1993). Under the reasonable assumption that schools put extra focus on civics education and, especially, the importance of voting in connection to the general elections with potential downstream effects on later-life turnout propensities we should therefore expect the reform effect to be stronger among those born in 1973.

One important example of such measures of enhanced civics education is the mock elections held in a large share of the Swedish secondary schools a couple of weeks before the general elections. Mock elections and other election related activities such as political debates and workshops have been prominent elements in upper secondary civics education since the late 1960's with the explicit aim to encourage students to vote in the elections and increase their political knowledge and efficacy (Ungdomsstyrelsen, 2011). Evaluations suggest that these mock elections increase interest in politics (Ungdomsstyrelsen, 2007). Moreover, a study by Öhrvall (2016) found that Swedish upper secondary students enrolled in schools that organized mock elections in 2010 had a higher probability of voting in the real elections in both 2010 and 2014.

However, an increase in the reform effect on voter turnout among those born in 1973 may also reflect another possible mechanism related to the increase in average age at which the individuals typically moved out of their parents' home. Previous studies have shown that young adults living with their parents vote more often than those who have left the nest (Bhatti and Hansen, 2012). Furthermore, research on the persistence in turnout suggests that voting is habitual (Plutzer, 2002; Denny and Doyle, 2009). Voting in one election increases the probability of voting in subsequent elections. In line with these findings it could be expected that the reform led to a boost in turnout among individuals that, as a consequence of the reform, were still living with their parents at the time of their first election in the beginning of the 1990's and that this resulted in an initial increase in voting probability that persisted into the 2010 election.

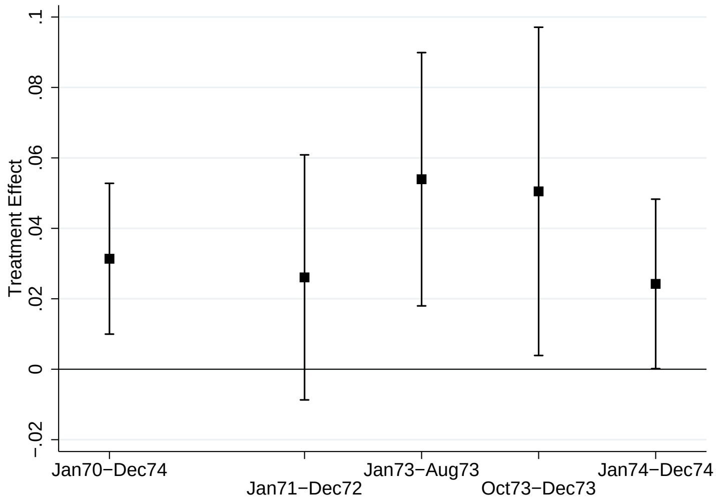
More precisely we should expect individuals born between 1/1 and 9/19 1973 to be affected by this mechanism. These individuals were first-time voters in the election in 9/19 1991. At this date the treated individuals had just begun the third and last year in school and were highly likely to still live together with their parents. Untreated individuals born in the same months in 1973, on the other hand, had finished upper secondary school in June 1991 and by the time of the election in September at least some of them had moved out on their own. Thus, the previous studies on first-time voting and turnout inertia suggest that the reform effect should be larger in magnitude among individuals born in 1973 and eligible to vote for the first time in the 1991 election. For all other treated cohorts in the sample this mechanism should make less of a difference. They were first-time voters in the election in September 1991 (those born 1971 and 1972) or in September 1994 (those born between 9/20 1973 and 12/31 1974) at a time when both treated and untreated individuals already had completed upper secondary schooling.

Consequently, the civics education mechanism implies an increase in the reform effect among all individuals born in 1973 whereas the pathway assuming long-term effects of living with one's parents at the time of the first election should lead us to expect a stronger influence of reform intensity among individuals turning 18 before the election in 1991. To separate between these distinct hypotheses *Figure A4* plots point estimates and 95% confidence intervals from a model in which the reform effect is allowed to vary by birth-cohort and, for the 1973 cohort, by voting eligibility in the 1991 election.[28] As a benchmark the leftmost point displays the average reform effect in the full sample.

Although less precisely estimated the pattern of cohort-conditional effects is clear. Consistent with the civics education hypothesis the results in *Figure A4* suggest an enhanced and approximately equally sized effect of the reform among all individuals born in 1973. The influence of an extra year in upper secondary school is twice as large among those born in 1973 compared to individuals born in 1971, 1972 or 1974. Thus, we argue

[28] Individuals born in September 1973 have been dropped from the sample. We only have data on birth month and therefore cannot tell whether those born in September 1973 were eligible to vote for the first time in the 1991 or the 1994 election.

Figure A4: Reform effect by Cohort



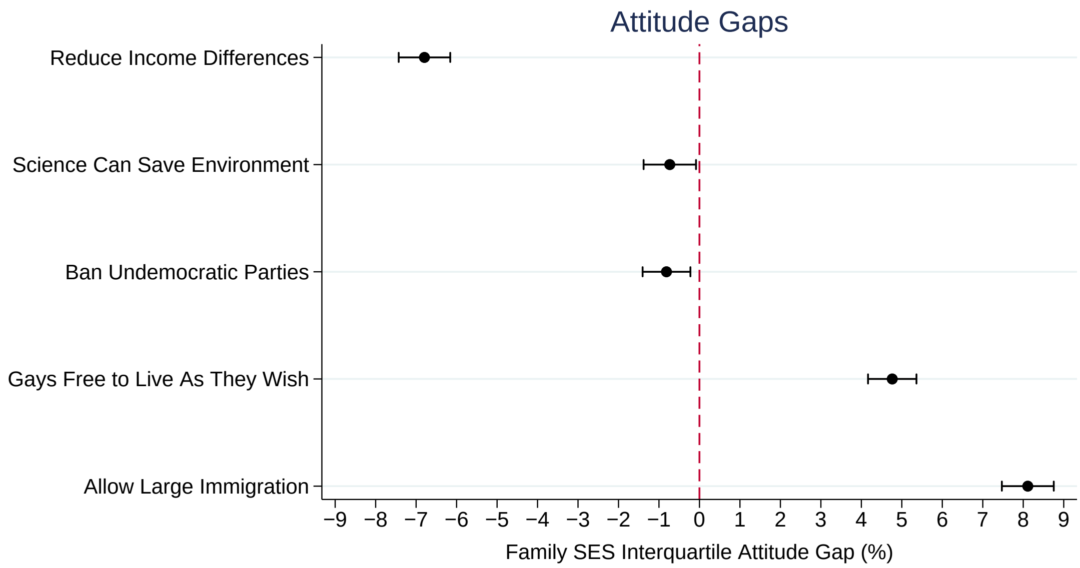
that the results in *Figure A4* reflect an effect of being enrolled in the school system at the time of an election irrespective of being eligible to vote or not rather than the effect of living with one's parents at the time of the first election.

Family Background and Political Attitudes

The focus on initial social circumstances could be questioned on the ground that the principle of political equality does not require "that all individuals be equally active, only that participant publics be representative in their politically relevant characteristics" (Schlozman et al. 2012:178). So the question then becomes whether family background is such a politically relevant characteristic. One way to attempt to answer this question is by examining whether individuals of different social origin hold conflicting political attitudes. In *Figure A5*, we therefore report the interquartile gap in family background for five attitudinal questions included in the ESS.

We find evidence of attitudinal gaps for all five indicators. Most importantly, individuals from less privileged social backgrounds are about seven percentage points *more* likely to agree to the statement that the government should take measures to reduce differences in income levels, whereas they are eight percentage points *less* likely to support the right for individuals of different ethnic belonging to settle in their country. We also see that individuals from more advantaged backgrounds are considerably more likely to support the

Figure A5: Gaps in Political Attitudes



Note: See the note to *Figure 1* for a description of the method used for creating this graph.

statement that gays and lesbians should be free to live their lives as they wish. Although smaller in magnitude, there are also discernible differences with respect to the probability of agreeing with the statements that science can be trusted to solve environmental problems and that undemocratic political parties should be banned.