

Can Increased Education Help Reduce the Socio-Economic Voting Gap?

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Abstract

It is well documented that voter turnout is lower among persons who grew up in families with low socio-economic status compared to persons from high status families. This paper deals with whether education reforms can help to reduce the socio-economic voting gap. We differentiate between two types of reform effects that can affect voting inequalities; (a) resource effects that changes the allocation of education between socio-economic groups and (b) return effects which refer to the size of the effect of education on turnout in different groups. We use this framework to analyze the reform of the Swedish upper-secondary schools system in the 1990s. This reform increased the length and amount of social science education on vocational tracks. We find that the reform decreased the voting gap mainly by a return effect indicating a stronger effect of the reform among those from families with low socio-economic status.

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Introduction

Few would, nowadays, question that the principle of “*one person, one vote*” form a cornerstone of representative democracy. Yet, in most developed democracies there are still huge differences in the extent to which members of different groups actually make use of their right to vote. To the extent that these differences have consequences for who gets what from the government, they may pose a threat to the functioning of democracy (Verba et al., 1995; Lijphart, 1997).

The present study is concerned with one particular source of voting inequality, namely family of origin. In comparison with the research on other types of participatory inequalities the voting gap related to family background remains fairly unexplored territory (Brady et al., 2015). Nonetheless, the research that do exist indicates that children from high socio-economic status (SES) homes are considerably more likely to be politically active citizens than children from less privileged homes (Verba et al., 1995, 2003; Gidengil et al., 2016). In an attempt to account for these results, Schlozman et al. (2012, 185) point to education as the main engine of intergenerational inequality in political participation. By passing on their high levels of education to their children, they argue, high SES parents simultaneously pass on many other attributes conducive to political activity (Schlozman et al., 2012, 185).

This argument, in turn, draws on a long and important tradition of political science research that depicts formal educational attainment as the most important resource for political participation (Converse, 1972; Wolfinger and Rosenstone, 1980). As Nie et al. explain, the adoption of this resource based account of political participation is also likely to affect views on how to best alleviate existing political inequalities.

A corollary of this resource metaphor is that if everybody gets more resources, the playing field for political voice and access will be leveled. Such a model expects that a more educated citizenry is likely to create a society with less participatory inequality (Nie et al., 1996, 188).

Following in the footsteps of progressive educational thinkers such as Jefferson, Mann, and Dewey, scholars working in this tradition thus tend to consider education reforms as a potent means for reducing political inequalities in general, and those associated with family origin in particular (cf., Brick, 2005).

In the last decade, this conventional wisdom has, however, been questioned by studies 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 believed to operate as a proxy for consequential, but difficult to observe, pre-adult experiences and predispositions. 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.

Despite all their merits, we think that both of these perspectives are incomplete in that they are based on an overly simplistic understanding of the role of education for political participation. In particular we find the implicit tendency of previous research to treat education as a homogeneous commodity that produces similar returns for all types of individuals problematic. Given that both the educational content varies between different educational levels or tracks, and that various types of individuals with differing interests, skills, and resources could benefit differently from education, the much discussed average causal effect of education on political participation may in fact conceal as much as it reveals. When analyzing the potential effects of various education reforms on political inequality it is therefore important to consider the possibility of heterogeneity both regarding the effect of education on political participation, and the effect of these

reforms on educational attainment in various socioeconomic groups.

Whereas the issue of heterogeneous causal effects of education have attracted some attention in sociology and economics in recent years (Brand and Xie, 2010; Carneiro et al., 2011), political science research on the topic is still largely lacking. 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 sample sizes. Second, and as detailed by Breen et al. (2015), the usual problems of causal inference are further aggravated when examining effect heterogeneity 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 to education differ between individuals or groups.

In an attempt to meet these requirements this study uses unique population-wide administrative data from Sweden to study 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 election 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 matched to a school reform that lengthened vocational tracks 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 interests (Hall, 2012).

Our results indicate that the education reform lead 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. As a consequence the reform helped to reduce the overall voting gap related to family background by raising turnout in the

very lower end of the socio-economic distribution. The argument of this study is thus that education is unlikely to be the universal solvent that does away with all participatory inequalities. At the same time, however, our results clearly indicate that it is much premature, as some scholars seems to have done, to write off education reforms as a viable means to reduce the socio-economic stratification of political activity.

The rest of the paper is organized as follows. First, we develop a simple theoretical framework that will guide the empirical analysis. We then describe the reform, and discusses the methods and data being used. Thereafter, we present the empirical results and conclude by discussing the implications of our findings.

A Theoretical Framework

In this section we develop a simple formal model that allows us to explore the circumstances under which education reforms can affect the social inequality in voting evident in most developed democracies (Lijphart, 1997). We will take the well-known rational voting model of Riker and Ordeshook (1968) as our point of departure. In this model voters are faced with a choice between two political candidates (parties) and they have to calculate whether it is worthwhile to cast a vote for either of the two contenders. To allow for inequality in voting we will further assume that voters come from two types of family background: *low* and *high* socioeconomic-status (SES) homes ($g \in l, h$). Likewise we allow for two different states of the world with respect to the school system. Either the system has been subject to a school reform ($s = 1$) or it has not ($s = 0$). The utility of voting for an individual i , in socio-economic group g , attending school system s can then be expressed as:

$$U_{i,s}^g = pB_{i,s}^g + D_{i,s}^g - C_{i,s}^g, \tag{1}$$

where p is the perceived likelihood that one's vote will be pivotal in the election, B is the expected benefits if one's preferred candidate wins, D is the expressive benefits from

voting, and C is the costs of voting. The expressive benefits of voting has been said to emanate from the satisfaction associated with things such as complying with the norm of voting, expressing one's political identity, showing one's support for democracy, and affirming one's efficacy in the political system (e.g., Riker and Ordeshook, 1968; Fiorina, 1976; Schuessler, 2000). The costs of voting, on the other hand, is thought to be related to the acquisition of the information necessary to make an informed choice on who to vote for as well as the opportunity costs of attending the polls rather than doing something else (e.g., Frey, 1971).

As discussed at length in the literature on the paradox of voting, given the size of the electorate the probability of being pivotal in a national election will be effectively zero in most developed democracies (Riker and Ordeshook, 1968). This fact have led many researchers to argue that the probability of voting will mainly depend on the net cost of voting, i.e., $D - C$. That is, rather than considering voting as a means to an end it is better considered as an end in itself (Ashenfelter and Kelley, 1975; Schuessler, 2000).

Following this line of reasoning, we will focus our attention on the impact of education on the net costs of voting. Reviewing previous research in the area Jackson (1995, 280) distilled three general mechanisms linking education to turnout: *i)* Education enables more efficient collection and processing of political information, *ii)* Education instills in citizens the belief that voting is a civic duty, and *iii)* Education enhances both the belief that the voter can influence policy (*external efficacy*) and the belief that the voter has the competence to understand and participate in politics (*internal efficacy*).¹ With the partial exception of the effect of education on the external efficacy of voters all these mechanisms are closely related to the net cost of voting.

To analyze these issues more formally, let us proceed by approximating the net cost

¹In addition Jackson (1995) notes that education may promote skills and interests that reduce the costs of registration, which can contribute to higher turnout among well-educated in systems that require voters to register prior to the election.

of voting by means of the following equation:

$$D_{i,s}^g - C_{i,s}^g = c_s^g + a_{i,s}^g + \delta^g e_{i,s}^g - \theta_i, \quad (2)$$

where c is a group specific constant, a denotes an individual's innate political ability (interest), e is his or her level of education, δ is the group specific effect of education, and θ , finally, is an idiosyncratic component capturing the importance of haphazard events such as, for instance, bad weather or sudden illness.

From equation (2) it follows that an individual will vote as long as the net cost of voting is not negative, i.e., $c_s^g + a_{i,s}^g + \delta^g e_{i,s}^g \geq \theta_i$. The probability of voting (V) for an individual i in group g can thus be written:

$$Pr(V)_{i,s}^g = F(c_s^g + a_{i,s}^g + \delta^g e_{i,s}^g). \quad (3)$$

where F is the cumulative distribution function of θ . For reason of tractability, we assume θ to be uniformly distributed over the interval $-m$ to m , in which case the probability of voting takes the following simple form:

$$Pr(V)_{i,s}^g = \frac{c_s^g + a_{i,s}^g + \delta^g e_{i,s}^g + m}{2m}. \quad (4)$$

Equation (4) can then be used to calculate the average turnout for each combination of socio-economic status and school system, as is done in Table 1 (the bars denote mean values).

The differences in turnout for each of the two SES groups before and after the school reform will thus be given by the column differences in Table 1. That is:

$$\Delta Pr(V)^l = \frac{\Delta c^l + \Delta \bar{a}^l + \delta^l \Delta \bar{e}^l}{2m}, \quad (5)$$

$$\Delta Pr(V)^h = \frac{\Delta c^h + \Delta \bar{a}^h + \delta^h \Delta \bar{e}^h}{2m}, \quad (6)$$

Table 1: Turnout by group and school system

		School System	
		<i>Non-Reformed</i>	<i>Reformed</i>
SES	<i>Low</i>	$\frac{c_0^l + \bar{a}_0^l + \delta^l \bar{e}_0^l + m}{2m}$	$\frac{c_1^l + \bar{a}_1^l + \delta^l \bar{e}_1^l + m}{2m}$
	<i>High</i>	$\frac{c_0^h + \bar{a}_0^h + \delta^h \bar{e}_0^h + m}{2m}$	$\frac{c_1^h + \bar{a}_1^h + \delta^h \bar{e}_1^h + m}{2m}$

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 increase in average educational attainment (\bar{e}), but will also be affected by any simultaneous changes in the group specific constant c or innate political ability (\bar{a}). This problem is obviously closely related to the methodological problems of identifying the causal effect of education on political participation, which has attracted considerable scholarly interest in recent years (see Persson (2015) for an overview).

However, if we invoke the assumption that no other important changes occurred simultaneously with the school reform any differences in turnout across the columns of Table 1 will be due solely to the reform. In this case it is also straightforward to characterize the impact of the reform on voting inequality. For instance, if we use the difference in turnout shares between individuals from low and high SES homes to measure inequality the change in voting inequality induced by the reform is:

$$\Delta Pr(V)^h - \Delta Pr(V)^l = \frac{\delta^h \Delta \bar{e}^h - \delta^l \Delta \bar{e}^l}{2m}. \quad (7)$$

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. For inequality to reduce the school reform must increase turnout more among individuals from low SES homes than among those from high SES homes (i.e., $\delta^l \Delta \bar{e}^l > \delta^h \Delta \bar{e}^h$).

More importantly, however, this model 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 can depend on the type of education reform being examined. Reforms that lengthens compulsory education, on the one hand, 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 pursue secondary education (Lindgren et al., 2014). Blanden and Machin (2004), on the other hand, find that policies that expand higher education in the UK have 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 increasing or decreasing 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, voting inequality 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., $\delta^l > \delta^h$). 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., $\delta^h > \delta^l$). In his study of the impact of civic education on political engagement, Campbell (2008) refers to these two possibilities as the *compensation* and *acceleration* hypothesis, respectively (cf., Langton and Jennings, 1968; Neundorf et al., 2015).

The extent to which education reforms can help mitigate political inequality will thus depend on both the resource and the return effects associated with particular reforms. When we now proceed to the examination of a large scale reform of secondary education in Sweden we will therefore pay close attention to these different effects.

Institutional Background

In Sweden students enter the upper-secondary school system the year they turn 16 after nine years of compulsory schooling.² Although upper-secondary education is non-mandatory, a majority of students attend this level (about 90 percent of the students during the period under study). Students can choose from a number of vocational and theoretical tracks. Typically students attend an upper secondary school in their municipality of residence. If the desired track is not available they may attend upper secondary school in a nearby municipality.

In 1984 the government appointed a committee with the mission to propose a reform of the upper secondary school system with a special focus on improving and making the vocational education more flexible. Based on the committee proposal the Swedish Parliament decided on a large-scale reform of the upper secondary school in 1991. In the pre-reform upper secondary school students could choose between attending a number of two-year vocational and three-year theoretical tracks.³ The former had a strong focus on preparing students for work life and included little theoretical academic training whereas the latter were intended to prepare the students for higher education at the university level. In the post-reform upper secondary school the length of all vocational tracks were extended to three years. Moreover, the reform also stipulated a stronger theoretical content in the curriculum of the vocational tracks. In the pre-reform upper secondary school Swedish was the only mandatory theoretical subject provided at vocational tracks. After the reform the vocational tracks included also English, social science and an additional optional theoretical subject (most students chose mathematics). As a result of these changes students graduating from vocational tracks were were classified as meeting the basic entry requirements for admissions to universities.

One of the explicit political intentions behind the reform was to decrease the socioeconomic gap between students from low SES homes who dominantly attended vocational

²This 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).

³In addition a four-year track in technology was available.

tracks and students from high SES homes who primarily attended theoretical tracks. By opening up the possibility to enter higher education at universities for students from vocational programs the reform of the upper secondary school was seen as a step towards the overarching goal, manifested in the common slogan “a school for everyone”. In a historical perspective the reform harmonizes with earlier education reforms in Sweden, put forward primarily by the Social democrats, with the intention to provide mass education to large shares of the population thereby creating a more egalitarian society. In a parliamentary debate the minister of education and would-be 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 social democracy 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 by a pilot scheme between 1988 and 1990 in which the new three-year tracks were implemented in some municipalities for evaluation purposes. As described below this pilot scheme generated plausibly exogenous variation in education by providing some students the choice of attending a three-year vocational track instead of the old two-year track. We will use this pilot scheme to study the effects of education on participation among students from different social backgrounds.

The pilot scheme was initiated in the academic year of 1988/1989 by involving 6000

students. In the second year (1989/1990) the scheme was expanded to include 10 000 students and in the third year (1990/1991) it expanded further to include 11 200 students.⁴ The implementation of a pilot scheme class in a municipality was always accompanied by the withdrawal of a class in a corresponding two-year track in that same municipality. Thus, the reform did not increase the total number of available places on vocational tracks. Also, the allocation of places was based on proportionality such that the share of two- and three-year places was supposed to be the same across different tracks. By the end of the period the pilot scheme included around 20% of the available places on vocational tracks.

The municipalities had to apply for participation in the pilot scheme and the National Board of Education took the final decision on which schools should be allowed to participate. When deciding which municipalities to include the Board took several factors into account. First, it was important that the local labor market would be able to take care of the extended work-life training included in the new three-year vocational tracks. Second, the board aimed at implementing the pilot scheme across different geographical areas. Finally, the aim was also to achieve a certain amount of variation regarding the extent to which different regions participated in the scheme.

At the start of the pilot scheme period the demand for participation in the pilot scheme among the municipalities was greater than the supply of three-year places on vocational tracks provided by the National Board of Education. For example, during the first year the Board received applications for over 10 000 student places and could only allow 6000. Out of Sweden's 284 municipalities 93 participated in the pilot scheme the first year (1988), 145 in the second year and 147 in the third year. Given that vocational education was only offered in 193 municipalities in 1990, a majority of the eligible municipalities was by this point included in the pilot scheme (SOU 1989:90).⁵

⁴On top of this, in 1987/1988 there was a very limited pre-pilot scheme only including 500 student places.

⁵In the pre-reform gymnasium students could chose 17 different vocational tracks. The pilot scheme offered ten tracks the first year and 17 tracks the last two years. The most popular ones were caring services, industry and motor and transport engineering.

During the pilot period participating municipalities as a rule offered both two- and three-year vocational tracks. Moreover, in some municipalities both two- and three-year versions of the same vocational track were available. In municipalities where students were only offered the three-year tracks it was possible to attend the corresponding two-year tracks in nearby municipalities. Hence, the pilot scheme setting did not represent a sharp change were everyone in a pilot scheme municipality who wanted to attend a vocational track was forced into a three-year track. Our estimation strategy instead relies on the fact that, depending on birth year and current municipality of residence, there was variation in the degree to which students had a chance to attend a three-year rather than a two-year vocational track. 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 education.

A number of studies have used the the pilot scheme to study the effects of education on different outcomes. Ekström (2003) uses a cross sectional sample of students to compare those living in pilot scheme and non-pilot scheme municipalities and show that while the reform increased the probability of getting higher education it did not decrease the probability of being unemployed. However, based on population-wide data Hall (2012) finds no significant effects of the reform 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) provides a better opportunity to control for unobserved differences between municipalities.

Using a related modeling strategy as the one employed in Hall (2012), Grönqvist and Hall (2013) find no effects of the school reform on men's fertility whereas early-life fertility rates were significantly lower among women who attended a three-year vocational track. In a further study, Grönqvist et al. (2015) focus the effects on the effect of the reform on criminal behavior. They conclude that keeping students in the school system for an additional year had a significant negative effect on committing property crime. However, they found no effect on violent 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 and theoretical tracks before and after the reform was implemented on a national scale in 1994. They conclude that differences in political participation between students from vocational and theoretical tracks were not equalized 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. As we will show in the next sections a more nuanced analysis of the effects of the reform of the upper-secondary school system reveals heterogeneous effects not detectable without the kind of large scale data that we employ.

Empirical Framework

We will employ a difference-in-difference approach to identify the causal effect of completing a three-year vocational track on voter turnout.⁶ Consider, first, the following baseline specification:

$$V_{icm}^g = \alpha_0^g + \alpha_1^g D_{icm}^g + \boldsymbol{\lambda}^g \mathbf{X}_i^g + \theta_c^g + \eta_m^g + \epsilon_{icm}^g, \quad (8)$$

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 vocational track, \mathbf{X}_i^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) socioeconomic-status groups.

⁶For a similar empirical approach see Hall (2009) and Hall (2012). More precisely, we will estimate the effect of completing a three-year vocational track rather than a two-year vocational track 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 track or as the effect of completing an extra year of upper secondary vocational schooling.

If \mathbf{X}_i^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 positive correlation between D_{cm}^g and ϵ_{icm}^g .

To circumvent this problem we will make use of the plausibly exogenous variation in track length introduced by the pilot scheme preceding 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 compulsory school, the students faced different opportunities. Some could choose among plenty of three-year vocational tracks whereas others were allotted the shorter two-year tracks.

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

$$V_{icm}^g = \beta_0^g + \beta_1^g R_{cm}^g + \boldsymbol{\zeta}^g \mathbf{X}_i^g + \theta_c^g + \eta_m^g + \xi_{icm}^g, \quad (9)$$

where R_{cm}^g is an indicator for 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_1^g is an estimate of the difference in turnout between students who had to attend a shorter two-year vocational track ($R_{cm}^g = 0$) and those who were only faced with the three-year option ($R_{cm}^g = 1$). As highlighted in the theoretical section (equations 5, 6 and 7) the school reform must have a stronger positive impact on students from low SES homes ($\beta_1^l > \beta_1^h$) 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 vocational track and estimate a Two Stage Least Squares (2SLS) model. The first and second stages take the following form:

$$D_{icm}^g = \gamma_0^g + \gamma_1^g R_{cm}^g + \boldsymbol{\tau}^g \mathbf{X}_i^g + \theta_c^g + \eta_m^g + \phi_{icm}^g \quad (10)$$

$$V_{icm}^g = \delta_0^g + \delta_1^g \widehat{D}_{cm}^g + \boldsymbol{\omega}^g \mathbf{X}_i^g + \theta_c^g + \eta_m^g + \psi_{icm}^g \quad (11)$$

where γ_1^g is the effect of the reform indicator γ on completing a three-year vocational track and δ_1^g is the effect of completing an extra year of upper secondary vocational schooling on turnout propensity.⁷ 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 socioeconomic groups ($\delta_1^l = \delta_1^h$) the reform will reduce inequality 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 such that 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 ($\delta_1^l < \delta_1^h$) while the resource effects are the same in the two groups ($\gamma_1^l = \gamma_1^h$).

Our empirical framework rests on a number of identifying assumptions. The most important among these concern the (conditional) exogeneity of the reform, i.e., conditional on the covariates included in the model R_{cm}^g should be uncorrelated with other factors influencing turnout propensities. Moreover, to interpret the 2SLS results we also have to assume that any impact of the instrument on turnout is completely mediated by the reform effect on upper secondary schooling. We will return to these assumptions when examining the sensitivity of our results with respect to different model assumptions.

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

⁷Thus, γ_1^l and γ_1^h correspond to the parameters $\Delta \bar{e}^l$ and $\Delta \bar{e}^h$ from equations 5, 6 and 7 in the theoretical section. Likewise, δ_1^l and δ_1^h correspond to the parameters δ_1^l and δ_1^h .

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 effects (θ and η) in equations 9, 10, and 11 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 remove 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 particular true in a case like this when we also have a binary endogenous regressor (e.g., Freedman and Sekhon, 2010). We will, however, provide probit results as a robustness check.

Data and Measures

We use data from different administrative registers maintained at Statistics Sweden to construct our sample and and acquire information on several socioeconomic and demographic variables. Our sample consists of all individuals born from 1970 to 1974. Since Swedish students normally finish compulsory schooling in the spring semester the year they turn 16 our sample includes nearly all individuals who started upper secondary school or dropped out of the school system after completing compulsory schooling between 1986 and 1990. We use the Multi-Generation Registry to match these individuals to their parents. The children and parents are matched to various administrative registers with information regarding educational attainment, income, occupational status, and some additional demographic and socioeconomic characteristics.⁸

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 census in 1985 together with information on available vocational tracks across municipalities.⁹ More precisely, the indicator measures the share of three-year

⁸See the Appendix for additional details on these registers and variables.

⁹We are grateful to Caroline Hall for sharing the code used to construct this indicator.

vocational tracks out of all vocational tracks.¹⁰

Family socio-economic status constitutes another key variable in our analysis. Broadly 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 different dimensions of SES, researchers have traditionally relied on composite measures including family income, parental educational attainment, and parental occupational status.¹¹ 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 the slight deviation that 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*) total parental labor income. All items are assigned the same weight in calculating the SES index and in case information on one of the indicators is missing the index is based on the two indicators for which data is available.¹² To adjust for the differences in scales between the variables, all sub-items were initially standardized to have a mean of 0 and a standard deviation of 1.¹³ 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

¹⁰Hall (2012) sets the reform indicator to zero for municipalities not offering any vocational tracks. However, students living in such municipalities could enroll in upper secondary schools in nearby municipalities. Therefore, for municipalities that lacked vocational tracks 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 reform cohort) attended a vocational track.

¹¹The 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)).

¹²Complete data on all three indicators are available for 94% of the cases.

¹³The scale reliability of this index is .77.

from the registers our measure of occupational status is based on census occupational codes. More precisely, we use the occupational codes to compute three well-known measures of occupational status: the *International Socio-Economic Index* (ISEI, Ganzeboom and Treiman, 1996)), the *Standard International Occupational Prestige Scale* (SIOPS, Treiman, 1977), and the *International Cambridge Scale* (ICAMS, Meraviglia et al., 2016; Prandy and Jones, 2001).¹⁴ 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.¹⁵

Turning to the dependent variable, the supply of data from Statistics Sweden is less satisfactory where electoral participation is concerned. The public registers do contain validated information on voter turnout from large samples in connection to each election from 1991 and onwards. However, our research design require 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 digitalizing the information in the publicly available election rolls (we provide a detailed description of the procedures we have used to scan and digitalize them in the Appendix). The resulting dataset is unique in both scope and quality. The reliability of the digitalized individual-level turnout data is very high. Quality checks suggest that the digitalized information on electoral participation conform with the manual coding of Statistics Sweden in 99.7% of the cases.

Table 2 presents summary statistics for individual background variables (Panel A) and some 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 are considered high intensity municipalities. Compar-

¹⁴See the Appendix for more detailed descriptions of these measures. The code to translate census occupational codes into ICAMS, ISEI, and SIOPS was downloaded from <http://www.harryganzeboom.nl/isco88/index.htm>.

¹⁵The 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 have been set to its sample minimum value.

ing across the columns it is evident that the two groups are very similar in terms of background characteristics.¹⁶ As expected a larger share of the students had attended a three-year track at age 20 in the group of above median municipalities. However, in their late thirties (in 2010) there is no clear differences in either total years of schooling or turnout participation across the two groups of individuals.¹⁷

Table 2: Summary Statistics

	Low level pilot intensity	High level pilot intensity
<i>Panel A. Background variables.</i>		
Female	0.49	0.49
Immigrant background	0.14	0.14
Student's birthyear	1972.01	1972.02
Mother's birthyear	1945.52	1945.53
Father's birthyear	1942.80	1942.76
Family SES	0.02	0.02
Share in first family background quartile	0.25	0.25
Share in second family background quartile	0.25	0.25
Share in third family background quartile	0.25	0.25
Share in fourth family background quartile	0.25	0.25
<i>Panel B. Outcomes.</i>		
Share completing three-year tracks (at age 20)	0.48	0.50
Years of schooling (2010)	12.77	12.82
Turnout	0.89	0.88
Number of individuals	229,876	258,494

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

Empirical Results

Table 3 reports how the reform of vocational upper-secondary education in the late 1980s 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—

¹⁶Immigrant background is a dummy equal to 1 for individuals who are not born in Sweden by two Swedish-born parents.

¹⁷The average turnout in the electorate in the 2010 election was 84.6%.

i.e., the share of three-year vocational tracks 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 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 belonging to 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 3.2 percentage points in this group, whereas we find no statistically significant effect in any of the other quartile groups. With respect to the differences in coefficients between groups we find that three out of six differences have p-values below .1. These are Q2 vs. Q1 ($p = .003$), Q3 vs. Q1 ($p = .043$), and Q4 vs. Q1 ($p = 0.074$).

To further clarify the meaning of these results for the socio-economic voting gap, the leftmost graph in Figure 1 displays how expected turnout in the four groups varies with reform intensity.¹⁸ There are, indeed, substantial inequalities in voting remaining also as the share of three-year tracks 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 tracks the expected voting

¹⁸Expected turnout is calculated on the basis of the results presented in Table 3 holding all other independent variables constant at their sample values.

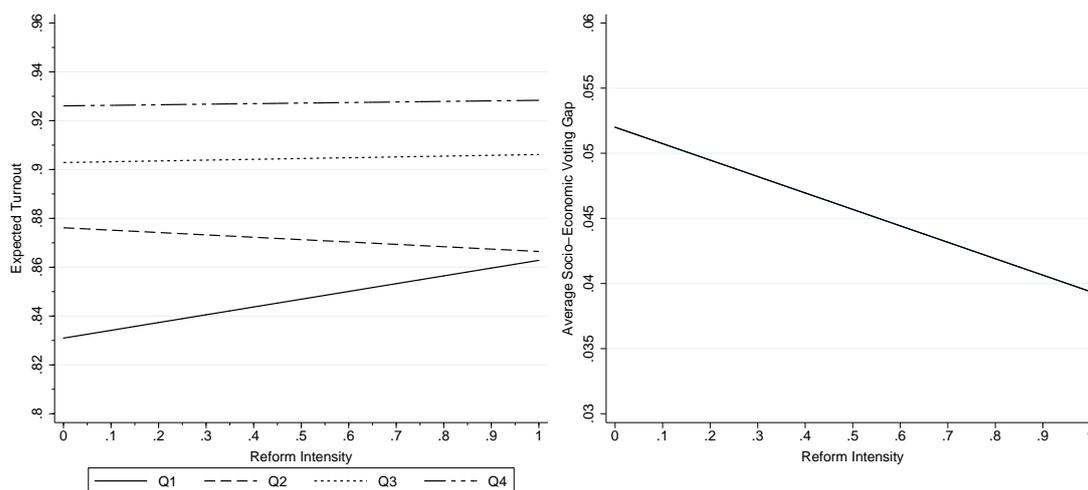
Table 3: Reform Effect on Voter Turnout

	All	Q1	Q2	Q3	Q4
<i>Panel A. Dependent variable: Voting.</i>					
<i>(Reduced form)</i>					
Reform intensity	0.76 (0.52)	3.19*** (1.09)	-0.97 (0.89)	0.33 (0.95)	0.22 (1.02)
Gender	2.37*** (0.11)	3.81*** (0.21)	3.37*** (0.21)	1.65*** (0.17)	0.57*** (0.13)
Immigrant background	-6.09*** (0.27)	-8.13*** (0.43)	-5.83*** (0.34)	-4.71*** (0.36)	-4.14*** (0.33)
Family SES	3.78*** (0.09)				
<i>Panel B. Dependent variable: ≥ 3 years of post-primary educ. at age 20</i>					
<i>(First-stage 2SLS)</i>					
Reform intensity	17.56*** (1.85)	21.86*** (2.64)	19.88*** (2.45)	17.82*** (1.92)	6.14*** (1.91)
Gender	3.07*** (0.30)	3.91*** (0.42)	3.72*** (0.44)	3.65*** (0.44)	1.05*** (0.32)
Immigrant background	1.30*** (0.44)	2.51*** (0.61)	2.05*** (0.62)	0.08 (0.66)	-1.92** (0.42)
Family SES	19.83*** (0.16)				
<i>Panel C. Dependent variable: Voting.</i>					
<i>(Second-stage 2SLS)</i>					
Completed 3-year track	4.35 (3.00)	14.57*** (5.15)	-4.88 (4.39)	1.84 (5.30)	3.71 (16.46)
Gender	2.23*** (0.14)	3.24*** (0.30)	3.55*** (0.26)	1.59*** (0.26)	0.53*** (0.22)
Immigrant background	-6.14*** (0.28)	-8.50*** (0.48)	-5.73*** (0.34)	-4.71*** (0.36)	-4.07*** (0.44)
Family SES	2.92*** (0.59)				
Observations	488,370	122,091	122,094	122,093	122,092

Notes: Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

gaps between individuals in the lowest quartile and those in the other three quartiles would be 4.5 (Q2 vs Q1), 7.2 (Q3 vs Q1), and 9.5 (Q4 vs Q1) percentage points, whereas the corresponding figures would be 0.4, 4.3, and 6.6 percentage points when all vocational tracks are three years long.

Figure 1: Voting Gaps by Reform Intensity



In the rightmost graph of Figure 1 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.¹⁹ More substantively, we can think of these differences as the expected (absolute) difference in turnout between two randomly selected individuals representing two different quartile groups. To judge from our results the average voting gap decreases from 5.2 to 3.9 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

¹⁹Q1 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.

panels of Table 3 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 at age 20.

The first-stage results presented in Panel B provides 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 almost 22 percentage points as all vocational tracks in a municipality are lengthened from two to three years. The corresponding figure for children in the highest quartile is only 6.1 percentage points, i.e., almost 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.

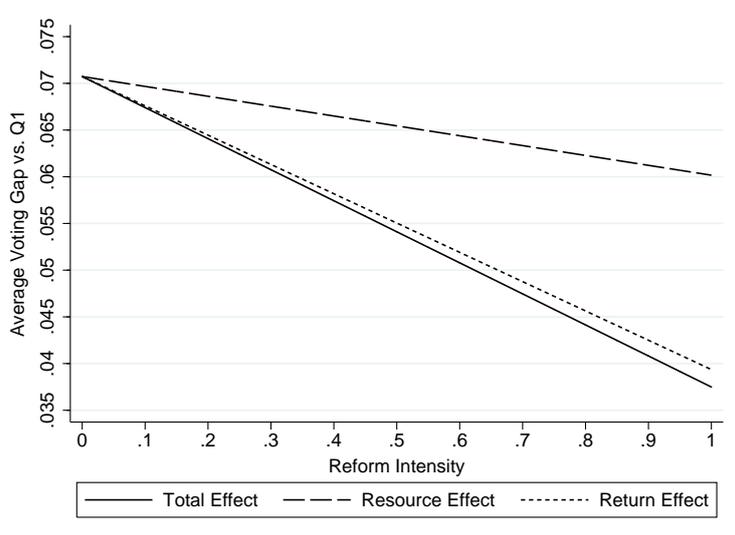
Turning instead to the return effects these are given by the second-stage results presented in Panel C of Table 3.²⁰ In this setting, the coefficients give us the marginal change in the propensity to vote associated with completing at least three-years of post-primary education (rather than *less* than 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 track is estimated to increase the probability of voting by almost 15 percentage points. For the other three quartile groups the IV-estimates are considerably lower 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 ($p = .002$) and Q3 vs. Q1 ($p = .08$) are statistically significant at the .1 level, whereas the difference between Q4 and Q1 ($p = .50$)

²⁰Alternatively, we could have obtained these coefficients by dividing the reduced form coefficients in Panel A by the first-stage coefficients in Panel B.

despite being large in magnitude does not reach conventional levels of statistical significance.²¹ Overall, our results thus appear consistent with the *compensation hypothesis*. That is, 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 (cf., Campbell, 2008).

Figure 2: Decomposing the Reduction in the Voting Gap



According to the results presented in Table 3 both the resource effect and the return effect may have been of some importance in explaining the reduction in the socio-economic voting gap pictured in Figure 1. We might, however, want to characterize the relative importance of these two factors in accounting for the reduction in the overall voting gap. Towards this end, Figure 2 presents the results from an empirically informed thought experiments. Because the reduction in the overall voting gap is almost completely driven by the relative improvement for those from the most disadvantageous homes we now focus solely on the voting gaps in relation to quartile group 1, and disregard the differences found between the upper three quartiles.

As can be seen from the solid line in Figure 2 the expected voting gap between Q1 and the upper three quartile groups reduces from about 7.1 percentage points to 3.7 percentage points as reform intensity increases from 0 to 1. In order to obtain a sense for the relative importance of differences in the resource effects in accounting for this

²¹However the IV-estimate for Q4 is very imprecisely estimated due to a weak first stage.

reduction we can imagine a situation in which all socio-economic groups would have had the same returns to education as quartile group 1, but where the effects of the reform on educational attainment (the resource effects) were the ones previously estimated from the data. The development of the voting gap under 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 resource effect of the first quartile group to all other groups as well.

In line with what is expected based on the results in Table 3, the differences in return effects across groups are much more important than differences in resource effects in explaining the reduction in the voting gap between Q1 on the one hand and the other three quartile groups on the other. Under the assumption of equal return effects in all groups an increase in reform intensity from 0 to 1 would only have reduced the voting gap by 1 percentage point, whereas the corresponding decrease under the assumption of equal resource effects is more than 3 percentage points.

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 tracks in upper secondary school. The main advantage with this is that we need to be less worried that our results are driven by compositional changes between different types of tracks. Yet, given that students in vocational tracks were those primarily affected by the reform we should expect the effect on voting to be most pronounced in this group. In panels A and B of Table 4 we therefore present separate results for individuals who had not completed an academic upper-secondary track by the age of 20 (Panel A) and those who had (Panel B).²²

A first thing to note is that the results for the sample excluding individuals with

²²Admittedly, if the introduction of the reform affected the probability to complete an academic track this means that we will condition on an outcome of the reform, which could bias the results. But we still believe these results are worth reporting.

Table 4: Sensitivity Analyses

	All	Q1	Q2	Q3	Q4
<i>Panel A. Excluding individuals with theoretical degree.</i>					
Reform intensity	0.97 (0.81)	4.31*** (1.41)	-1.40 (1.06)	-0.29 (1.56)	-0.44 (1.99)
Gender	3.00*** (0.15)	3.66*** (0.26)	3.61*** (0.28)	2.07*** (0.28)	1.29*** (0.31)
Immigrant background	-7.01*** (0.31)	-8.62*** (0.50)	-6.52*** (0.40)	-5.68*** (0.49)	-4.50*** (0.60)
Family SES	4.29*** (0.13)				
Observations	267,671	92,257	81,240	60,605	33,569
<i>Panel B. Excluding individuals with vocational degree.</i>					
Reform intensity	0.89 (0.68)	0.73 (1.70)	1.36 (1.53)	1.54 (1.13)	0.60 (0.99)
Gender	0.49*** (0.11)	0.90 (0.33)	1.03*** (0.27)	0.43*** (0.20)	0.13 (0.14)
Immigrant background	-5.11** (0.32)	-8.12*** (0.62)	-5.22*** (0.57)	-3.81*** (0.40)	-3.92*** (0.35)
Family SES	1.01*** (0.12)				
Observations	220,699	29,834	40,854	61,488	88,523
<i>Panel C. Pre-dating reform intensity with 5 years</i>					
Reform Intensity	-0.44 (0.43)	-0.26 (0.97)	-0.98 (1.08)	-1.22 (0.87)	0.89 (0.82)
Gender	3.79*** (0.11)	4.75*** (0.21)	3.92*** (0.19)	2.78*** (0.16)	1.29*** (0.13)
Immigrant background	-4.57*** (0.20)	-6.25*** (0.41)	-4.77*** (0.34)	-3.45*** (0.28)	-2.92*** (0.26)
Family SES	3.79*** (0.11)				
Observations	497,447	124,361	124,362	124,362	124,362

Notes: Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

academic upper-secondary 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 3.2 to 4.3, which is to be expected since it was the vocational tracks that were affected by the reform. Further, 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 tracks in upper-secondary school. For the other quartile groups we find no statistically significant effects of the reform either among vocational or theoretical students.

These results notwithstanding, some readers might still fear that reform intensity was correlated with *changes* in (unobserved) municipality-specific factors that also affected voter turnout, which would invalidate our identification strategy. One often used method 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 evidence of an effect also in the pre-reform period. In Panel C of Table 4 we therefore artificially change the date of the pilot scheme and assume that it was implemented five years earlier than what was actually the case.²³ In practice, this means that we examine how reform intensity affected turnout of individuals born between 1965 and 1969 and who 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. the results can be 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.

We have also performed a number of additional robustness checks. For reasons 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 tracks were offered. To examine how sensitive the

²³The reason why we pre-date the reform by five years is that we want to make sure that all cohorts included in this analysis were unaffected by the actual reform.

results are to our choice of method to handle this problem we have re-estimated the main specification including only the 193 (out of 284) municipalities offering vocational tracks at the upper-secondary level. This does not affect the results (see Table 6 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 probit regression, and in terms of average marginal effects the results are very similar to those of the linear probability model (see Table 7 in the Appendix).

Finally, we have also examined the sensitivity of our results with respect to the composite measure of family background that we have used for the main analyses. More precisely, we have estimated separate models for each of the three sub-items making up our family SES measure. Although the reform effect shrinks in magnitude (to about 2 percentage points) when considering the different indicators in isolation, the overall results of this disaggregated analysis closely mimics the results obtain with the composite index (see Table 8 in the Appendix).

What Accounts for the Effect?

Given the apparent robustness of the reform effect among individuals of low socio-economic background a natural question to ask is why the introduction of three year vocational tracks increased turnout in this group. Due to data constraints, a more in-depth analysis of the causal mechanisms underlying the observed relationship 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.

In particular, we can use the data at our disposal to examine to what extent the educational effect is mediated by social position. This is interesting, since it is commonly argued that one important reason for why highly educated individuals are more likely

to be political active is that they more often to occupy a privileged social position. For instance, in their seminal contribution Verba and Nie (1972) suggested a number of reasons why social status should be linked to political participation:

Some depend on the social environment of upper-status citizens: They are more likely to be members of organizations, and they are more likely to be surrounded by other who are participating. Some connecting links depend on the availability of resources and skills: Upper-status citizens have the time, the money, and knowledge to be effective in politics. Other connecting links depend on the psychological characteristics of upper-status citizens: They are more likely to be concerned with general political problems, and they are more likely to feel efficacious (1972, 133).

As can be seen, some of these purported mechanisms are more closely related to education per se such as political knowledge, interest, and efficacy, whereas other posit a more indirect link of education on political participation through things such as income, occupation, and social networks. Although our administrative data do not contain any information on the former set of factors, we can, however, examine the degree to which educational effect on voting is mediated by factors of the latter type.

In Table 5 we therefore present results from a simple mediation analysis in which we sequentially control for a number of potential mechanisms such as income, occupation, family status, and political activity of surrounding social networks. 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 2SLS model for individuals of low social background (Q1) with complete data records. In this restricted sample completing a three year track is estimated to increase the probability of voting by about 13 percentage points, which is fairly close to effect previously found for the larger sample (14.5).

In Column 2 we add controls for marital status (1=Married), the number of children below 19 years old, disposable income, and occupational dummies (based on 4-digit occu-

Table 5: Mediation Analysis

	(1)	(2)	(3)	(4)	(5)	(6)
Completed 3-year track	13.12*** (4.69)	12.07*** (4.64)	12.29*** (4.64)	11.84** (4.66)	10.25** (4.52)	10.13** (4.52)
<i>Controls</i>						
Gender	3.67*** (0.40)	3.13*** (0.39)	1.38*** (0.27)	1.37*** (0.35)	2.26*** (0.27)	2.08*** (0.27)
Immigrant background	-6.34*** (0.47)	-6.17*** (0.42)	-5.76*** (0.39)	-6.04*** (0.40)	-4.32*** (0.35)	-4.17*** (0.35)
Disposable income		0.06*** (0.02)	0.06*** (0.02)	0.06*** (0.02)	0.12*** (0.03)	0.12*** (0.03)
Marital status		4.60*** (0.42)	3.73*** (0.30)	3.94*** (0.30)	-3.34*** (0.29)	-3.35*** (0.29)
Number of children		2.57*** (0.14)	2.25*** (0.12)	2.39*** (0.12)	-2.04*** (0.35)	-2.07*** (0.14)
Turnout neighbors			31.93*** (3.32)			7.99*** (2.48)
Turnout colleagues				5.90*** (0.55)		4.82*** (0.51)
Turnout family members					26.86*** (4.52)	26.70*** (0.39)
First stage coef.	22.31	22.31	22.35	22.29	22.26	22.30
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: Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

pational codes). Although the results suggest that all these variables are related to voting the effect of completing a 3-year track 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 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 about one fifth of the overall educational effect.

Unfortunately, we can only speculate on what explains the remaining effect, but a likely possibility is that the effect is driven by factors more directly related to the nature and content of education. For instance, it can be hypothesized that the lengthening of vocational tracks from two to three years—and the increased focus on civic education—spurred greater interest in politics, helped increase internal as well as external political efficacy, and bolstered support for the norm of voting.

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 direct 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 changes the allocation of education between socio-economic groups and return effects which refer to size of the effect of education in different groups. We find that the Swedish reform decreased the voting gap mainly by a return effect indicating a stronger effect of education on electoral participation among those from families with low socio-economic status.

This finding is important since it suggests that education can have a compensatory effect for students from families with lower socio-economic status. It seems as what these student 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 the a larger return effect among the most advantaged. Our data show little support for such worries. Quite 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 mean to reduce socioeconomic inequalities in voting.

Although we cannot for sure say which aspect of the reform—the new educational content or the extended length—that triggered the effect, the policy recommendations are quite clear. If policy makers want to use reforms of the upper-secondary school system to reduce socio-economic inequalities in political participation they should make sure that students on vocational tracks have a high quality education that is not exclusively focusing on training for work life but also providing a solid ground in core subjects such as social science.

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Appendix

Details on Data and Measurements

[Once we find the time, a more complete description of the data will appear here.]

Additional Analyses

[Once we find the time, interpretations of the various analyses will be added here.]

Table 6: Restricting the analyses to municipalities with vocational tracks.

	All	Q1	Q2	Q3	Q4
<i>Panel A. Dependent variable: Voting.</i>					
Reform intensity	0.71 (0.55)	3.46*** (1.18)	-1.39 (0.99)	0.95 (1.07)	-0.40 (1.07)
Gender	2.35*** (0.12)	3.87*** (0.24)	3.42*** (0.23)	1.71*** (0.18)	0.43*** (0.13)
Immigrant background	-6.08*** (0.30)	-8.11*** (0.48)	-5.90*** (0.37)	-4.58*** (0.39)	-4.17*** (0.34)
Family SES	3.84*** (0.10)				
<i>Panel B. Dependent variable: At least 3 years of post-primary education at age 20.</i>					
Reform intensity	17.68*** (2.15)	21.99*** (3.14)	20.26*** (2.85)	18.48*** (2.12)	5.23*** (2.00)
Gender	2.85*** (0.34)	3.63*** (0.48)	3.44*** (0.49)	3.36*** (0.50)	1.01*** (0.34)
Immigrant background	1.59*** (0.46)	2.87*** (0.65)	2.58*** (0.66)	0.03 (0.72)	-1.85*** (0.44)
Family SES	19.83*** (2.15)				
<i>Panel C. Dependent variable: Voting.</i>					
Completed 3-year track	4.04 (3.18)	15.73*** (5.62)	-6.86 (4.84)	5.14 (5.81)	-7.64 (20.90)
Gender	2.23*** (0.15)	3.29*** (0.32)	3.66*** (0.28)	1.54*** (0.27)	0.51*** (0.26)
Immigrant background	-6.14*** (0.31)	-8.57*** (0.54)	-5.73*** (0.37)	-4.59*** (0.39)	-4.31*** (0.50)
Family SES	3.04*** (0.62)				
Observations	419,748	101,437	103,887	106,040	108,384

Notes: Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

Table 7: Probit Results, Average Marginal Effects

	All	Q1	Q2	Q3	Q4
<i>Panel A. Dependent variable: Voting.</i>					
Reform intensity	0.69 (0.50)	3.16*** (1.09)	-0.92 (0.88)	0.33 (5.78)	0.24 (1.02)
Gender	2.32*** (0.10)	3.82*** (0.22)	3.38*** (0.20)	1.64*** (0.17)	5.76*** (0.13)
Immigrant background	-5.15*** (0.20)	-7.37*** (0.35)	-5.23*** (0.28)	-4.18*** (0.28)	-3.61*** (0.25)
Family SES	3.97*** (0.09)				
Observations	488,348	122,072	122,059	122,051	122,022
<i>Panel B. Dependent variable: ≥ 3 years of post-primary educ. at age 20.</i>					
Reform intensity	17.41*** (1.89)	19.22*** (2.37)	19.00*** (2.46)	19.35*** (2.21)	7.04*** (1.98)
Gender	3.08*** (0.30)	4.00*** (0.41)	3.75*** (0.44)	3.63*** (0.44)	1.02*** (0.32)
Immigrant background	1.41 (0.44)	2.59*** (0.60)	2.07*** (0.63)	-0.09 (0.66)	-1.91*** (0.41)
Family SES	19.94*** (0.12)				
Observations	488,354	122,075	122,086	122,088	122,082

Notes: Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

Table 8: Alternative Measures of Family Background

	All	Q1	Q2	Q3	Q4
<i>Panel A. Quartiles based on parental education.</i>					
Reform intensity	0.78 (0.52)	1.80* (1.06)	-0.08 (1.00)	0.73 (1.08)	0.99 (1.06)
Gender	2.39*** (0.11)	3.96*** (0.21)	2.87*** (0.19)	1.81*** (0.17)	0.66*** (0.15)
Immigrant background	-6.58*** (0.29)	-8.44*** (0.50)	-6.56*** (0.32)	-5.67*** (0.34)	-5.37*** (0.51)
Family Educ.	0.93*** (0.05)				
Observations	487,681	113,026	138,913	141,584	94,158
<i>Panel B. Quartiles based on parental occupational status.</i>					
Reform intensity	0.78 (0.52)	2.13* (1.13)	-0.49 (1.02)	0.40 (0.97)	1.04 (1.02)
Gender	2.33*** (0.10)	3.68*** (0.19)	3.26*** (0.21)	1.67*** (0.18)	0.56*** (0.16)
Immigrant background	-6.09** (0.28)	-8.33*** (0.50)	-6.02*** (0.36)	-3.84*** (0.36)	-5.00*** (0.39)
Family occ. status	0.18*** (0.01)				
Observations	457,233	120,169	108,827	113,379	103,757
<i>Panel C. Quartiles based on parental income.</i>					
Reform intensity	0.84 (0.52)	2.29** (1.04)	0.77 (1.03)	-0.04 (0.77)	0.20 (1.18)
Gender	2.30*** (0.11)	3.43*** (0.19)	2.55*** (0.21)	2.42*** (0.18)	0.98** (0.17)
Immigrant background	-5.76** (0.25)	-8.04*** (0.49)	-6.00*** (0.37)	-5.72*** (0.32)	-4.75*** (0.31)
Family earnings	0.55*** (0.02)				
Observations	488,370	122,104	122,317	122,044	121,897

Notes: Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

Figure 3: Reform Effect by Family SES Decile

