Can Political Inequalities Be Educated Away? Evidence from a Large-Scale Reform

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Abstract: Over the years, many suggestions have been made on how to reduce the importance of family background in political recruitment. In this study, we examine the effectiveness of one such proposal: the expansion of mass education. We utilize a difference-in-difference strategy to analyze how a large school reform launched in Sweden in the 1950s, which lengthened schooling and postponed tracking, affected the likelihood of individuals with different family backgrounds to run for public office. The data come from public registers and pertain to the entire Swedish population born between 1943 and 1955. The empirical analysis provides strong support for the view that improved educational opportunities for individuals from disadvantaged backgrounds can be an effective means to reduce the social bias of elected assemblies.

Replication Materials: The data, code, and any additional materials required to replicate all analyses in this article are available on the American Journal of Political Science Dataverse within the Harvard Dataverse Network, at: http://dx.doi.org/10.7910/DVN/STOQ1Q.

A century after the breakthrough of modern democracy, the political elite in most developed countries still continue to be dominated by individuals from privileged social backgrounds (Aberbach, Putnam, and Rockman 1981; Carnes 2013; Carnes and Lupu 2015; Cotta and Best 2007; Norris 1997). The fact that an individual’s chances of having a career in politics at least partly depends on who his or her parents are has been a constant source of concern for observers across the political spectrum. One reason for this, as Verba, Burns, and Schlozman (2003, 45) explain, is that this type of transmission of political influence across generations could be seen as constituting a “double infringement: transgressing not only the principle of equality of opportunity but also the principle of equality of outcome among citizens.”

Over the years, many suggestions have been made on how to reduce the importance of family background in political recruitment. One much-discussed solution is that of improved educational opportunities for the masses. Thomas Jefferson was an early proponent of this view, arguing for increased educational attainment as a way to replace what he referred to as an artificial aristocracy based on wealth and fortune with a natural aristocracy based on talent and virtue. Since Jeffersonian times, the idea that equality of educational opportunity is a necessary condition for political equality has gained considerable currency in liberal democratic thought (Dewey 1916; Gutmann 1987; Verba 2003). In a recent book on the politics of education, Ansell (2010, 2) even goes as far as describing universal education “as the sharpest edge of progressive distribution.”

The liberal view of education as “the great equalizer” of social conditions has not gone uncontested, however. For instance, there is an abundance of evidence from decades of social mobility research that speaks to the difficulty of reducing inequality through education reforms (e.g., Brown 2013). A problem with the optimistic liberal view of education, the argument goes, is that it tends to underestimate “the flexibility and effectiveness with which the more powerful and advantaged groupings in...
society can use the resources at their disposal to preserve their privileged position” (Goldthorpe 1987, 328).

Ultimately, the extent to which improved educational opportunities can help make political recruitment less socially biased is an empirical question. Despite the importance of this question, empirical research on the topic is scant. An important reason for this is the lack of adequate data. Given that political candidates constitute such a small fraction of the overall population, it is usually not possible to study political recruitment using traditional representative surveys.1 A second factor hampering research in this area is the well-known problem that educational choices are highly dependent on various types of preadult experiences and predispositions that are difficult to observe. In recent years, students of political behavior have therefore increasingly expressed a concern that the relationship between educational attainment and political participation is spurious rather than causal (e.g., Kam and Palmer 2008).

To examine whether the importance of family background in political recruitment depends on the design of the educational system, one would thus like to have access to population data and some kind of exogenous variation in education to disentangle the effect of education from the confounding effects of unobserved preadult characteristics. In this study, we utilize unique administrative data from Sweden that arguably meet both of these requirements. More precisely, we analyze how a large comprehensive school reform launched in Sweden in the 1950s, which lengthened compulsory schooling by up to 2 years, affected the likelihood of individuals with different family backgrounds to run for public office. The data used in the analysis pertain to the entire Swedish population born between 1943 and 1955 and contain complete records of all individuals who ran for public office in each of the six general elections held between 1991 and 2010.

The Swedish comprehensive school reform is of great interest methodologically as well as substantively. On the methodological side, the reform was implemented at different times in different parts of the country, which means that there is (arguably) conditionally exogenous variation in the education system across cohorts and regions that can be used to assess the equalizing potential of education (Hjalmarsson, Holmlund, and Lindquist 2015; Lager and Torssander 2012; Meghir and Palme 2005). More substantively, learning about the equalizing impact of this reform is valuable since it has been portrayed as a blueprint example “of progressive school ‘democratization’ through rational educational planning” (Lyon 2001, 513).

Overall, the empirical analysis provides rather strong support for the view that improved educational opportunities for individuals from disadvantaged backgrounds can be an effective means to reduce the social bias of elected assemblies. According to our estimates, the Swedish comprehensive school reform served to reduce the effect of parental social class on the likelihood of running for political office by as much as 40%.

The Importance of Social Origin

Alongside gender, social origin has historically been the most important attribute for reaching political leadership positions. Though the formal hereditary path to political power has long since been abolished in most democracies, surnames such as Bush, Gore, Ghandi, Mussolini, and Le Pen are reminders that family background continues to play an important role in politics. It is well known that individuals from lower-class backgrounds have always faced difficulties in reaching important decision-making positions (Carnes 2013). For instance, in their seminal study of political and bureaucratic elites in seven Western democracies, Aberbach Putnam, and Rockman concluded that “persons from working-class and lower-middle-class backgrounds are largely excluded from the elite” (1981, 81).

Aberbach, Putnam, and Rockman (1981) argued that the educational system is key in understanding and also overcoming the absence of working-class people in the top rungs of society. Put simply, there are two ways in which educational initiatives, such as a compulsory school reform, can help to reduce inequality in political participation. A first possibility is that increased education leads to a general and identical increase in political participation among all individuals irrespective of social origin, and at the same time the educational reforms reduce the differences in education between individuals from different social groups. A second possibility is that education has a particularly positive effect on political participation among individuals of weaker social background. If education has such a compensatory effect on political participation, a higher level of education will lead to reduced political inequality even if the differences in education between individuals of differing social backgrounds remain unchanged. The two dynamics through which an educational reform can equalize political participation are closely related to what Mettler (2002) refers to as the critical effects hypothesis in her study on the policy

1The innovative work of Lawless and Fox (2010) constitutes a partial exception. However, the type of strategic sampling used by these authors is less suitable for investigating the relationship between family background and candidacy.
feedback effects of the G.I. Bill among World War II veterans (see also Campbell 2012).

However conceived, the idea that educational reforms may have an equalizing influence on political representation rests on the assumption that there is a real causal connection between education and political participation. Previous research on the relationship between education and political participation has highlighted two distinct ways by which education may spur political activity. The traditional understanding is that education increases political participation by enhancing civic skills and cognitive abilities in general, and political interest and political knowledge in particular (Lewis-Beck et al. 2008). More recently, it has been argued that it is not the absolute level of education but rather the relative level that affects political participation. Relatively better educated individuals, so the argument goes, typically enjoy higher social status and are tied to more politically active social networks, which in turn increase the likelihood of political participation (e.g., Campbell 2009; Nie, Junn, and Stehlik-Berry 1996). Often these two mechanisms are referred to as the cognitive and positional pathways, respectively.

The empirical evidence in support of the claim that educational reforms may moderate the effect of social origin on political activity is scarce. Using survey and interview data, Mettler (2002) found that the G.I. Bill's educational provisions for World War II veterans led to increased civic and political engagement, especially among veterans from less privileged social backgrounds. Aberbach, Putnam, and Rockman (1981), focusing on an outcome more closely related to the current study, showed that people from lower-class backgrounds tended to be less educated and therefore less likely to run for public office. According to the authors, educational reforms may therefore be an effective means to reduce the elite bias of elected assemblies:

By expanding educational opportunities for working-class children, for example, the social composition of European political elites, ceteris paribus, might become reasonably representative of the population as a whole, at least in terms of family background. (Aberbach, Putnam, and Rockman 1981, 62).

The pioneering studies of Aberbach, Putnam, and Rockman (1981) and Mettler (2002) represent important theoretical as well as empirical advancements. Nonetheless, they also have some important shortcomings with respect to data and methodology. Above all, as forcefully argued in recent contributions on the education-participation nexus, it is very difficult to assess the importance of education in cross-sectional data. The problem is that education is believed to operate as a proxy for consequential, but difficult to observe, preadult experiences and predispositions (e.g., Dinesen et al. 2016; Kam and Palmer 2008; Sondheimer and Green 2010). According to this “education as a proxy” view, the high levels of education in elected assemblies may simply reflect the fact that there are unobserved factors that affect both educational choices and the likelihood of pursuing a political career.

In addition, the equalizing potential of education has been questioned by many sociologists studying intergenerational social mobility. In a recent overview of this research, Brown, Reay, and Vincent (2013, 637) concluded that available empirical evidence “has cast doubt on this optimistic, if not utopian, claim that reform of the education system could eliminate the influence of class, gender, and ethnicity on academic performance and occupational destinations.” Theoretically, there are at least two reasons why school reforms may not have the desired effects on social mobility. First, lower educational attainment can only explain part of the social disadvantage associated with a lower-class upbringing. Children from lower-class homes also face many other types of difficulties related to things such as the home environment, the presence of successful role models, and access to important social networks that education alone cannot offset (cf. Brown 2013). Second, it has been argued that the educational advantage of higher social classes is likely to remain even as educational opportunities are expanded. For instance, if primary or lower-secondary education is expanded, privileged social groups are said to intensify their investments in upper-secondary or tertiary education to keep their relative advantage (e.g., Raftery and Hout 1993).

The optimistic claim that reforming the education system could considerably reduce the importance of social origin in politics is thus open to debate on both theoretical and methodological grounds. It is therefore somewhat surprising that this issue has received such scant attention in political science in recent decades, especially since the importance of education for various forms of intergenerational mobility has been extensively researched in the neighboring fields of sociology and economics (see, e.g., Björklund and Salvanes 2011; Breen and Jonsson 2005). The present study seeks to fill this gap in the literature by studying the large comprehensive school reform launched in Sweden in the 1950s.

The Type and Purpose of the Reform

In the pre-reform Swedish school system, pupils attended basic compulsory school (folkskolan) until the fourth or
sixth grade. Based on their marks, the more able students were selected for the 5-year (starting in fifth grade) or 4-year (starting in seventh grade) junior secondary school (realskolan). The junior secondary school was a requirement for the upper secondary school and further higher education at the university. Pupils not attending junior secondary school instead completed the 7-year compulsory education.²

In the first half of the 20th century, the elitist nature of the prevailing school system came increasingly under attack, and in 1946, an inclusive parliamentary committee was appointed with the task of proposing guiding principles for the future organization of the compulsory school system. In the final report released in 1948, the committee recommended replacement of the 7-year compulsory school and the selective junior secondary school with a 9-year compulsory school and the abolition of all parallel forms of school. The proposal thus included both a lengthening of compulsory schooling, by up to 2 years, and a postponement of educational tracking in that children with different levels of educational ambition would be kept in the same classroom for a longer period. The committee also proposed changes to the content of education. Above all, English was introduced in fifth grade, and particular focus was placed on the study of civics in the new comprehensive school. However, the changes with respect to educational tracking and the curriculum should not be exaggerated (Hjalmarsson, Holmlund, and Lindquist 2015, 1294). During the period under study, the pupils were separated into vocational, general, or theoretical tracks (and classes) in the ninth grade. Furthermore, in 1953, English was also introduced in the old basic compulsory school (folkskolan). That is, both treated and untreated individuals born after 1945 studied English from the fifth grade.

The committee proposal led to a large-scale, nationwide evaluation between 1949 and 1962 (Marklund 1981). During this assessment period, the new comprehensive school was introduced throughout entire municipalities or, in the bigger cities, in certain subparts of the municipalities. As a general rule, the comprehensive school was first implemented for those in the fifth grade and below. Thus, for an extended period of time, pupils belonging to the same age cohort but living in different municipalities and pupils living in the same municipality but from adjacent age cohorts were assigned to different school systems.

²In some municipalities, mainly the big cities, compulsory schooling was extended to 8 years before the comprehensive school reform. See Marklund (1981) and Holmlund (2007) for detailed descriptions of the Swedish school system before and after the reform.

From a modest start where only 14 out of about 1,000 municipalities were selected for the first year of assessment (1949/1950), the number of municipalities introducing the 9-year comprehensive school grew steadily. In 1962, the parliament decided to permanently introduce the new school system throughout the country. The municipalities then had until 1969 to implement the reform. Figure 1 provides a graphical illustration of the gradual implementation of the school reform. The solid line shows the share of individuals in different cohorts who went to the new school system, and the dashed line depicts the share of municipalities that had implemented the reform for a particular cohort.

The selection of municipalities taking part in the evaluation program was not completely random, however, since municipalities could announce their interest in taking part in the reform to the National Board of Education. When announcing their interest, the municipalities had to report on things such as population growth, the local demand for education, tax revenues, and the availability of teachers and school buildings. The National Board of Education then took these factors into account when deciding on participation in the evaluation program. Although the participating municipalities were not chosen randomly, the National Board of Education attempted to choose the municipalities so as to obtain a mix of different municipality types, and there were, in fact, a rather large number of municipalities that were not allowed to implement the reform as early as they would have liked (Hjalmarsson, Holmlund, and Lindquist 2015, 1294).

Since the implementation of the reform included elements of voluntarism, we could fear that the timing of the reform was correlated with municipality-specific factors that also affected political activity. More precisely, and explained in more detail below, our identification strategy hinges on the assumption that the exact timing of the reform was uncorrelated with changes in important municipality characteristics. Time-invariant differences between early and late reformers will, however, not jeopardize identification since these differences will be accounted for by the municipality fixed effects included in our analyses.

To provide a better sense of the plausibility of our identification strategy, Figure 2 plots changes in (log) population size, income per capita, and the agricultural land share against reform timing in the municipalities included in our main sample. The lines denoting the average within-group changes for these variables are very flat for both the shorter period from 1952 to 1955 and the longer period from 1952 to 1965, lending credibility to our estimation strategy. In the supporting information, we also present evidence from more systematic balance
Figure 1 Reform Implementation by Cohort and Municipality

Note: The solid line shows the share of reformed individuals within each cohort, and the dashed line depicts the share of municipalities that had implemented the reform for a particular cohort.

Tests that further corroborates the view that we can treat reform participation as (conditionally) exogenous in our sample.3

Data and Measures

During the reform period, Swedish children started school the year they turned seven. Thus, the oldest cohort that was exposed to the reform program was born in 1938 (started the fifth grade in 1949), and the youngest cohort in which some pupils still attended the old school system was born in 1955 (started school in 1962 when the parliament decided to permanently introduce the 9-year comprehensive school). Thus, the core of the sample consists of all individuals born between 1938 and 1955. We use the Multi-Generation Registry from Statistics Sweden to match these individuals to their parents, and we have obtained information on a range of demographic and socioeconomic characteristics for the children as well as their parents from various administrative registers.

To construct a reform status indicator for each individual in our sample, we follow Holmlund (2007) and use information on home municipality from the census in 1960.4 In the analysis presented in the next section, we retain only those individuals born in 1943 or later because by 1960, large portions of the cohorts born from 1938 to 1942 were likely to have moved from the municipality in which they attended compulsory school.5

In what follows, we use parental social class—based on occupational information from the 1960 census—to measure family background. The main reason for using parental occupational class to measure family background is theoretical. Occupation figures prominently in most theories of social stratification since many researchers share the view of Crompton (1993, 120) that “the work individuals do remains the most significant determinant of the life-fates of the majority of individuals and families in advanced industrial societies.”

The class division used is based on the official Swedish occupational classification, the so called Socio-Economic Index (SEI). This measure closely follows the influential Erikson-Goldthorp-Portocarero (EGP) class schema that seeks to differentiate various labor market positions

4We are grateful to Helena Holmlund for sharing the data and code used for creating this indicator.

5Holmlund (2007) shows that the share of individuals living together with their biological mother in 1960 decreases sharply for cohorts born before 1943. Unfortunately, it is not possible to use census data on home municipality prior to 1960 since these are not available in digitalized form.

3The detailed analysis presented in Hjalmarrson, Holmlund, and Lindquist (2015) provides additional evidence in favor of this conclusion.
in terms of the employment relations that they entail (Erikson and Goldthorpe 1993, 37). More precisely, the EGP measure of occupational class rests on two important distinctions. First, it distinguishes employees from the self-employed. Second, it differentiates between occupations regulated by service and worker contracts. In the former group, we find white-collar occupations offering well-defined career opportunities, employment security, and low degrees of supervisory monitoring. In the latter group, we find blue-collar jobs in which the exchange of wages for effort is very specific, and the worker is relatively closely supervised (Breen and Rottman 1995, 70).

In the class measure at our disposal, these two distinctions have been used to identify seven occupational classes: (1) higher nonmanual workers, (2) intermediate nonmanual workers, (3) lower nonmanual workers, (4) self-employed workers, (5) (self-employed) farmers, (6) skilled manual workers, and (7) nonskilled manual workers.6

However, given that we mainly use the variation between cohorts within municipalities to identify the effects of interest, such a detailed classification may lead to noisy parameter estimates since there will simply not be a sufficient number of individuals in the small classes to obtain adequate statistical precision. Therefore, our primary analysis utilizes a simple dichotomy distinguishing between classes 6–7 on the one hand, and classes 1–5 on the other. In essence, this means that we will differentiate between individuals with working- and non-working-class backgrounds. Based on the logic of the EGP schema, this grouping can be justified based on the fact that that skilled and nonskilled manual workers are the only two groups with a pure labor contract relationship with their employer (Breen and Rottman 1995, 71). Moreover, as is typically done, class origin will be measured at the household level using the class coding of the parent with the dominant class position. We provide an extended discussion of our class measure in the supporting information together with a range of checks showing that our main results are highly robust with respect to different operationalizations of parental social class.

To obtain information on our dependent variable, we matched the children in our sample to a register that

\[6\] The algorithm for coding occupational codes into these seven social classes was originally developed by Jan O. Jonsson (see Erikson and Jonsson 1992), and we are grateful to Martin Hallsten for sharing his Stata code with us.
As can be seen, about 3.5% of our sample (1) indicates parental class position, and \( y \) is a dichotomous indicator taking on the value of 1 for individuals nominated for political office, \( R_{c1m} \) is a dummy indicating whether the individual was exposed to the reform, \( P_{c1m} \) indicates parental class position, and \( \gamma_c \) and \( \eta_m \) are cohort and municipality fixed effects, respectively. The coefficient \( \beta_2 \) is a direct measure of the degree of social bias in political representation—the strength of the relationship between parental background and the probability of standing as a candidate. To examine whether the school reform influenced this relationship, we allow this regression coefficient to vary across cohorts (\( \gamma_c \)), municipalities (\( \lambda_m \)), and reform status:

\[
\beta_2 = \delta_1 + \delta_2 R_{c1m} + \gamma_c + \lambda_m. \tag{2}
\]

Inserting Equation (2) into Equation (1) yields

\[
y_{icm} = \beta_0 + \beta_1 R_{c1m} + \theta_c + \eta_m + \delta_1 P_{icm} + \delta_2 R_{cm} \\
\times P_{icm} + \gamma_c \times P_{icm} + \lambda_m \times P_{icm} + \epsilon_{icm}. \tag{3}
\]

The parameters of primary interest are \( \delta_1 \) and \( \delta_2 \). The former provides a measure of the strength of the relationship between parental class and the probability of running for office for individuals not exposed to the reform. \( \delta_2 \) measures the influence of the school reform on this relationship. A negative estimate of \( \delta_2 \) would suggest that the reform had an equalizing effect on the social bias of political representation. By including a full set of cohort effects, respectively. The coefficient \( \beta_2 \) is a direct measure of the degree of social bias in political representation—the strength of the relationship between parental background and the probability of standing as a candidate. To examine whether the school reform influenced this relationship, we allow this regression coefficient to vary across cohorts (\( \gamma_c \)), municipalities (\( \lambda_m \)), and reform status:

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and municipality dummies, and their interactions with parental class, we control for time trends and local differences. This allows us to estimate the effect of the school reform net of these potentially confounding factors.\footnote{In some previous studies (e.g., Hjalmarsson, Holmlund, and Lindquist 2015), the school reform has been used as an instrument for years of schooling. However, due to the zero-sum nature of the outcomes examined here, the school reform can be expected to have negative externalities on those not affected by the reform (by decreasing their relative level of education), in which case the exclusion restriction underlying the Instrumental Variables (IV) approach fails to hold. For this reason, we will not employ the IV-strategy in this article but instead focus on the total reform effect.}

The key identifying assumption within this difference-in-difference framework is that of parallel trends: In the absence of the reform, the outcome of interest—in our case, the relationship between parental background and the probability of standing as a candidate—would have followed the same time trend among those exposed as among those not exposed to the reform.

Before turning to the empirical analysis, two important sample restrictions need to be mentioned. First, following Brunello, Fort, and Weber (2009) and Borgonovi, d’Hombres, and Hoskins (2010), in each municipality, we limit our sample to those individuals born at most 7 years before or 6 years after the first cohort affected by the reform. The choice of the window width is dictated by a trade-off between obtaining a large enough sample size to allow for precise estimates and a small enough window size to exclude other policy changes that may bias the results. Second, in each municipality, we exclude the birth cohort preceding the first cohort affected by the reform. This restriction reflects the fact that previous research has identified a significant reform effect for the cohort that was 1 year too old to be affected by the reform (Hjalmarsson, Holmlund, and Lindquist 2015). This could either be due to measurement error in the exact timing of the reform in particular municipalities or due to the fact that a non-negligible share of the pupils started school a year later than they were supposed to (Fredriksson and Öckert 2013).

### Results and Analyses

Table 2 reports estimates of the influence of the educational reform on social stratification of political candidacy using a linear probability model. We prefer to work with a linear specification since nonlinear models such as logit or probit in a difference-in-difference framework require additional and restrictive assumptions to identify the causal effect of a treatment on an outcome. Above all, whereas the linear specification requires differences between the treatment and control groups to be time-invariant, the nonlinear specification requires such differences to be absent (Lechner 2011).\footnote{This being said, in the supporting information, we show that the results from a logit specification correspond very closely with those reported in Table 2.}

Throughout, we report estimates from models with a set of baseline covariates. These covariates are gender, a dummy for immigrant status (where zero denotes individuals born in Sweden to Swedish parents), and a set of dummies for both father’s and mother’s birth year. We use cluster-robust standard errors at the municipality level to take the grouping structure of the data into account.

Column 1 reports the results from a model with parental class and reform status entered additively in accordance with Equation (1). The results show that the effect of family background is strongly related to the probability of running for office. The probability of standing as a candidate is 1.04 percentage points greater if an individual comes from a non-working-class home. Given a baseline probability of running for office of 3.53%, the size of this effect should be considered substantial. To put this figure in further perspective, we note that the estimated difference between males and females is 0.97 percentage points. Therefore, according to these results, the importance of family background for running for office is on par with that of gender.

According to Model 1, the average effect of the educational reform is positive but modest in size and far from statistically significant. Thus, we find no general positive effect of the reform on the probability of standing as a candidate. However, as described in the previous section, the hypothesis that the school reform had an equalizing influence on social stratification implies that reform status should be interacted with the parental class indicator.

The estimates reported in column 2 clearly support this notion. Since the model includes an interaction term, the coefficient for parental class now refers to the effect among individuals not exposed to the educational reform. This effect is slightly larger compared to the corresponding additive estimate in Model 1. Most importantly, the estimated interaction effect is negative and significant. According to the estimates, the reform served to reduce the effect of family background on the likelihood of running for public office by almost a third—from 1.20 to 0.84.

The conditional effects of reform status provide further insight into the equalizing influence of the comprehensive school reform. The positive main effect of the reform indicator implies that the reform increased
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</tr>
<tr>
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<td>Yes</td>
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<tr>
<td><strong>Municipality FEs</strong></td>
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<td>Yes</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Cohort × P FEs</strong></td>
<td>No</td>
<td>No</td>
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<td>No</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Municipality × P FEs</strong></td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
</tr>
<tr>
<td><strong>Controls × P</strong></td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
<td>No</td>
</tr>
<tr>
<td><strong>Observations</strong></td>
<td>786,790</td>
<td>786,790</td>
<td>786,790</td>
<td>786,790</td>
<td>786,790</td>
</tr>
</tbody>
</table>

Note: All models control for father’s and mother’s birth years. All estimates have been multiplied by 100 to simplify interpretation. Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/*** indicates significance at the 1/5% level.

the probability of running for office among individuals with lower-class parents by 0.24 percentage points. The corresponding effect among individuals raised by high-status parents is instead negative (−0.11), although not statistically significant (p = 0.33).

Initially, the negative sign of the reform coefficient in the latter group may seem surprising; however, similar findings have been reported in previous studies on education and income (Meghir and Palme 2005). In this particular case, the result could be driven by the competitive nature of the electoral process. Given that there is a fixed number of seats to be filled (and that there is a strong correlation between the number of seats and the length of the party lists), the type of political participation studied here will, at least to some extent, have the character of a zero-sum game. What one group gains, another group necessarily loses. Thus, if children of low-status parents gained from the reform, they were likely to have done so at the expense of children of high-status parents.

We should note, however, that the causal interpretation of the reform effect reported in column 2 hinges on the assumption that the effect of parental class on the likelihood of running for office does not vary across municipalities or cohorts. In other words, if we are to believe in the estimates presented in column 2, we must assume that both the municipality and cohort effects specified in Equation (2) are equal to 0. In columns 3–6, we report results from models in which we successively relax these restrictions.

The results reported in column 3 are based on a model that includes a full set of controls for the interaction between the 13 cohort dummies and the parental class indicator. The estimates in column 4 are from a model that includes almost 900 municipalities by parental class interaction terms. The model specification in column 5 includes both cohort and municipality interactions with family background. Finally, column 6 reports estimates from a model where the control variables are also interacted with parental class. The overall pattern of results in columns 3–6 is very similar to the one reported in column 2. The influence of family background on the likelihood of standing as a candidate is significantly lower.
among individuals who enrolled in the new comprehensive school system. Based on the least restrictive models presented in columns 5 and 6, the effect of family background was reduced by as much as 40% as a result of the reform. Since the estimates from Models 5 and 6 are close to identical, we will focus on the results from Model 5.

As explained above, there are two distinct effects contributing to the reduced importance of family background. On the one hand, the reform made it more likely for individuals of working-class backgrounds to enter politics. According to Model 5, the probability of running for office increased by 0.32 percentage points in this group as a result of the reform. Compared to the pre-reform average in the working-class group, this effect amounts to a 9% increase. On the other hand, there is evidence of a negative effect of the reform on individuals from non-working-class homes. Although not statistically significant (p = 0.19), the coefficient estimate for this group suggests that the reform decreased the probability of running for office by .19 percentage points among these individuals (which constitutes a 5% reduction compared to their pre-reform probability).

In summary, there are two important lessons that can be drawn from the results presented in Table 2. First, there is a strong social bias in political recruitment. Consistent with previous studies, we find that Swedish political candidates tend to come from privileged social backgrounds (Cotta and Best, 2007; Prewitt and Eulau, 1971). Second, across all model specifications, we find that the comprehensive Swedish school reform mitigated the importance of family background in political recruitment. The magnitude of this effect is substantial. In our preferred model specification, the strength of the relationship between family background and the probability of running for office was reduced by as much as 40%.

**Internal Validity**

In order to examine the robustness of our findings, we have performed a number of additional analyses and sensitivity checks. For reasons of space, we only provide a brief summary of the most important results from these robustness tests. However, the full details are given in the supporting information.

Most importantly, the key identifying assumption underlying our empirical analysis is that the trend in the effect of parental background on political candidacy would have been similar in all municipalities in the absence of the school reform. Since the common trend assumption concerns a counterfactual scenario, it is not directly testable. However, in the methodological literature, it is frequently suggested that the tenability of various common trend assumptions should be investigated by testing for different trends in the pre-reform period. Therefore, we have performed two different tests for the presence of pre-reform trends in the data.

The first test rests on placebo estimates in which we move the date of the reforms back in time. If we were to find significant effects of these placebo reforms, this would serve as an indication that the reform effects could be driven by other time-variant, municipality-specific characteristics. The second test uses an interaction model to check whether the relationship between family background and the probability of running for office among individuals not affected by the reform is conditional on the timing of the school reform. In neither of the tests did we find any clear evidence for pre-reform trends in the data.

We have also examined how sensitive our results are with respect to various sample restrictions. Neither the choice to focus on individuals born 7 years around the first affected cohort nor the decision to leave out the cohort that was 1 year too old to be affected by the reform seems to be of importance for the substantive findings.

In addition, we have reestimated our model using alternative outcome measures. The substantive results remain intact both if we treat the number of times an individual has been nominated to office as the dependent variable or if we use individual-year observations as the unit of analysis. Moreover, although less precisely estimated, the overall pattern of coefficients is very similar if we study the probability of becoming elected rather than nominated.

We have also examined how the results change if we use alternative and more fine-grained measures of social origin. For instance, we have reestimated the models using both a 5-point scale of social class and a combined measure of parental social class and parental education. Whereas these changes do not affect our main findings, they provide additional insights into the relationship under study. Perhaps most interestingly, we find that the equalizing effect of the reform is strongest in the lower regions of the class distribution. A possible interpretation of this is that the reform mainly served to strengthen the position of the working class vis-à-vis the middle class, whereas the advantage of children from the upper class was too large to be overcome by reforming the compulsory school system.

Finally, we have reestimated all models using a logit specification and obtain virtually identical results. We have also performed heterogeneity analyses showing that the reform effect is most pronounced for boys living in large municipalities that implemented the reform earlier.
External Validity

In the previous section, we argued for the internal validity of our research design. Of equal importance is the question about the external validity of the study. To what extent, if at all, can the results reported here be translated to other countries and political contexts? The simple but unfortunate answer to this question is that we do not really know. Due to the uniqueness of our data, we cannot replicate our findings on data from other countries.

Nonetheless, we will attempt to gain some leverage on the issue of external validity by utilizing data on related outcomes from the European Social Survey (ESS; Rounds 1–5). More precisely, in the first column of Table 3, we regress a survey item asking the respondents whether they had worked in “a political party or action group” during the last 12 months on parental class (0 = working class, 1 = non-working class) for a sample of individuals born between 1943 and 1955 from 31 ESS countries (excluding Sweden). The second column reports the results from the same analysis on the Swedish subset of the ESS sample.

We see that the size of the parental class coefficient is fairly similar in the two samples. In the ESS-31 sample, the probability of party work is, on average, 1.4 percentage points higher for respondents from non-working-class homes, whereas the corresponding figure for the Swedish sample is 1.6 percentage points.\(^\text{13}\) Second, we note that although the coefficient of parental class is substantively large in both samples (more than 25% of the baseline probability), the coefficient is not statistically significant when restricting the analysis to the Swedish subsample. This fact indicates that even large surveys such as the ESS are too small to allow for sufficient precision in the analysis of rare outcomes such as working on behalf of a political party.

In columns 3 and 4 of Table 3, we perform a similar analysis, but this time with party membership as the dependent variable. The point estimate of parental class is somewhat higher in the Swedish case, but so is average party membership, and we cannot reject the null hypothesis that the coefficient is the same in both groups (\(p = 0.67\)). Judging from these results, the effect of parental class on the probability of becoming a party member or working on behalf of a party decreased by about 50% as a result of the various school reforms implemented in the different countries.

Potential Mechanisms

Having argued for the internal and external validity of our study, a natural question is what mechanisms underlie the observed reform effect. As discussed above, previous research has focused on two potential mechanisms mediating the relationship between education and political activity: the cognitive and positional pathways, respectively. Unfortunately, a more systematic analysis of the causal mechanisms at work is beyond the scope of this article since the data required for such an analysis are largely lacking in the administrative registers at our disposal. However, by utilizing additional data from the Swedish Election Survey for the period 1973–2010, we attempt to shed some light on this important issue.\(^\text{14}\)

Whereas the data from the Swedish Election Survey contain information on variables related to both the cognitive and the positional pathway, it is unfortunately not

---

\(^\text{13}\)The difference between the coefficients is not statistically significant (\(p = 0.85\)).

\(^\text{14}\)We are grateful to Mikael Persson for his help with these analyses.
Table 3  Analysis of the European Social Survey

<table>
<thead>
<tr>
<th></th>
<th>Party Work</th>
<th></th>
<th>Party Member</th>
<th></th>
<th>Party Work</th>
<th></th>
<th>Party Member</th>
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<tbody>
<tr>
<td></td>
<td>ESS-31</td>
<td>Sweden</td>
<td>ESS-31</td>
<td>Sweden</td>
<td>ESS-12</td>
<td>Sweden</td>
<td>ESS-12</td>
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<td>Parental class (P)</td>
<td>1.383**</td>
<td>1.598</td>
<td>1.390**</td>
<td>1.934</td>
<td>2.167**</td>
<td>1.226</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.229)</td>
<td>(1.113)</td>
<td>(0.252)</td>
<td>(1.285)</td>
<td>(0.500)</td>
<td>(0.630)</td>
<td></td>
</tr>
<tr>
<td>Reform status (R)</td>
<td></td>
<td></td>
<td>1.187</td>
<td>2.560*</td>
<td></td>
<td></td>
<td>(1.041)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(1.041)</td>
<td></td>
<td></td>
<td></td>
<td>(0.871)</td>
</tr>
<tr>
<td>P × R</td>
<td></td>
<td></td>
<td>−1.009</td>
<td>−0.702</td>
<td></td>
<td></td>
<td>(0.955)</td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td>(0.955)</td>
<td></td>
<td></td>
<td></td>
<td>(1.216)</td>
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<td>Male</td>
<td>2.868**</td>
<td>2.017</td>
<td>3.379**</td>
<td>3.023*</td>
<td>2.638**</td>
<td>2.877**</td>
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</tr>
<tr>
<td></td>
<td>(0.225)</td>
<td>(1.090)</td>
<td>(0.247)</td>
<td>(1.258)</td>
<td>(0.584)</td>
<td>(0.795)</td>
<td></td>
</tr>
<tr>
<td>Country FEs</td>
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<td>No</td>
<td>Yes</td>
<td>No</td>
<td>Yes</td>
<td>Yes</td>
<td></td>
</tr>
<tr>
<td></td>
<td>Yes</td>
<td></td>
<td>Yes</td>
<td></td>
<td>Yes</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Country × P FEs</td>
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<td>No</td>
<td>No</td>
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<td>No</td>
<td>No</td>
<td>Yes</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Mean outcome</td>
<td>5.243</td>
<td>5.733</td>
<td>6.502</td>
<td>7.731</td>
<td>4.860</td>
<td>4.647</td>
<td></td>
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<tr>
<td>Observations</td>
<td>39,195</td>
<td>1814</td>
<td>39,234</td>
<td>1811</td>
<td>18,458</td>
<td>18,465</td>
<td></td>
</tr>
</tbody>
</table>

Note: Party work is equal to 1 for respondents who had worked in a political party or an action group during the last 12 months. Party work is equal to 1 for respondents who were members in a political party. All estimates have been multiplied by 100 to ease interpretation. All models include controls for survey year. All models use data from ESS Rounds 1–5. In columns 1–4, the sample includes individuals born from 1943 to 1955, whereas in columns 5–6, the sample is restricted to cohorts born within 7 years from the first affected cohort. Standard errors are in parentheses (in columns 5–6, the standard errors are clustered at the country level). ***/∗ indicates significance at the 1/5% level.

possible to employ the empirical strategy used in the main analysis of the article since the necessary information on social origin and place of birth is missing. Instead, we make use of the information on highest attained education to see whether there are any important differences between respondents whose highest education corresponds to the new 9-year primary school and those who left school after 7 years in the pre-reform system. In Table 4, we regress various outcomes related to the cognitive and positional pathways on a trichotomous measure of education that distinguishes between old primary school (the baseline category), new primary school, and secondary or tertiary schooling (see the supporting information for details on the data). To simplify interpretation, all outcome measures have been scaled between 0 and 1.

Looking at the first two columns, we see that survey respondents who have completed the new primary school are more likely to answer that they are interested in politics than those who went to the old primary school. In contrast, we find no statistically significant difference between these two groups in the ability to correctly answer a number of political knowledge questions posed in the Swedish Election Survey.

In the third column of the table, we examine the differences between the educational groups with respect to a more general measure of cognitive ability. This indicator is based on four subtests measuring logical, verbal, spatial, and technical skills taken during military conscription, which used to be mandatory for all Swedish men. We have obtained from the Military Archives of Sweden access to the test scores for all individuals (almost exclusively men) who were born between 1951 and 1955 and who went through military conscription at around age 18. We find a statistically significant difference in the cognitive test scores between those completing the new and old primary school.

For two of the three outcome measures related to the cognitive pathway, we find evidence suggesting that the reform may have helped reduce political inequalities by promoting political skills and interest in the lower parts of the educational distribution. Based on these results, the gap in political interest and general cognitive ability between those with primary schooling and those with secondary or tertiary schooling dropped by more than a quarter as a result of the reform.

In the last three columns of Table 4, we focus on outcomes associated with the positional pathway. The first of these measures taps into the political activity of individuals’ social networks by utilizing a survey item that asks how often political discussions occur in the “nearest
Table 4 Analysis of Potential Mechanisms

<table>
<thead>
<tr>
<th></th>
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<th></th>
<th></th>
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</thead>
<tbody>
<tr>
<td>New primary school</td>
<td>0.031**</td>
<td>0.020</td>
<td>0.086**</td>
<td>0.024</td>
<td>0.020**</td>
<td>0.036**</td>
</tr>
<tr>
<td></td>
<td>(0.008)</td>
<td>(0.016)</td>
<td>(0.003)</td>
<td>(0.014)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Secondary/tertiary</td>
<td>0.123**</td>
<td>0.155**</td>
<td>0.269**</td>
<td>0.109**</td>
<td>0.131**</td>
<td>0.158**</td>
</tr>
<tr>
<td></td>
<td>(0.007)</td>
<td>(0.013)</td>
<td>(0.002)</td>
<td>(0.011)</td>
<td>(0.000)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Male</td>
<td>0.071**</td>
<td>0.161**</td>
<td>−0.130**</td>
<td>−0.003</td>
<td>−0.008**</td>
<td>0.231**</td>
</tr>
<tr>
<td></td>
<td>(0.005)</td>
<td>(0.007)</td>
<td>(0.051)</td>
<td>(0.007)</td>
<td>(0.000)</td>
<td>(0.001)</td>
</tr>
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<td></td>
<td></td>
<td>−0.014**</td>
<td>−0.006**</td>
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<tr>
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<td>(0.006)</td>
<td>(0.001)</td>
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<td>Yes</td>
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<td>Yes</td>
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<tr>
<td>Municipality FE $\ddagger$</td>
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<td>No</td>
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<td>No</td>
<td>Yes</td>
<td>Yes</td>
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<tr>
<td>Mean outcome</td>
<td>0.657</td>
<td>0.617</td>
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<td>0.491</td>
<td>0.497</td>
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<td>836,507</td>
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<td>Survey</td>
<td>Admin.</td>
<td>Survey</td>
<td>Admin.</td>
<td>Admin.</td>
</tr>
</tbody>
</table>

Note: All dependent variables are rescaled to the 0–1 range. The models based on administrative data (labeled “Admin.”) include controls for father’s and mother’s birth years. The models based on survey data (labeled “Survey”) control for election year. The survey data are from the general elections held between 1973 and 2010 (see the supporting information for further details). Robust standard errors are in parentheses. ***/** indicates significance at the 1/5% level.

Closely related to this, advocates of the positional pathway often stress the importance of individual social status for political participation. Relatively better-educated individuals are said to be more politically active because they tend to be in more prestigious occupations and earn higher incomes (e.g., Nie, Junn, and Stehlik-Berry 1996). In the last two columns of Table 4, we use administrative data to examine how education relates to a popular measure of occupational prestige (column 5) and relative earnings (column 6). Again, we find relationships in the expected direction. Those with 9 years of primary schooling enjoy somewhat higher occupational prestige and have higher earnings than those with 7 years of primary schooling.

The overall pattern is strikingly consistent across the different outcome measures. The average scores for the individuals whose highest education corresponds to the new 9-year primary school are situated between individuals whose highest education is the old primary school or secondary/tertiary education, respectively. These results may thus be taken to suggest that the Swedish school reform had an effect on political candidacy both through the cognitive and the positional pathways.

However, it should be noted that these results, taken by themselves, do not suffice to explain why the reform helped reduce the inequalities in political activity between different class groups. To better understand this, we must couple our findings with the two potential dynamics highlighted by Mettler (2002) in her study of the G.I. Bill: (1) that the reform had a larger impact on the education level (resources) of individuals from working-class homes, and/or (2) that education (resources) may be of greater value to children with working-class backgrounds since the “resources and skills that promote civic involvement” are typically already bestowed on those who grow up in more privileged families (Mettler 2002, 361). We provide tentative evidence in favor of both of these propositions in the supporting information. First, we demonstrate that the reform had a larger impact on the educational attainment of working-class children. Second, we show that the correlation between years of

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*The measure of occupational status we use is the so-called CAMSIS measure, which is constructed to represent an occupational unit’s relative position within the national order of social interaction and stratification (Prandy and Jones 2001). Empirically, this measure is related to measures of occupational class such as the EGP measure, but whereas most class measures focus on employment relationships, the CAMSIS scale is based on social (marriage) relationships (for reasons discussed in the supporting information, we currently lack information on this measure for the parental generation). Relative earnings refers to the within-cohort percentile rank of labor income. In both cases, the measurement year is 1990.
education and political candidacy is stronger for individuals with working-class backgrounds. Taken together, these different results suggest that the reform served to reduce the relative disadvantage of working-class children in the competitive struggle for political influence.

Conclusion

This study has examined whether improved educational opportunities for the masses can help reduce the importance of family background in politics. More precisely, we have studied whether the comprehensive school reform undertaken in Sweden in the 1950s and 1960s helped reduce the social bias in political recruitment. Overall, we find rather strong support for the view that educational expansion can further political equality. According to our difference-in-difference estimates, the reform reduced the effect of family background on the likelihood of seeking public office by up to 40%.

In comparison with previous research in the field, the present study benefits from access to considerably better data and a more convincing identification strategy. We therefore consider our findings to be an important contribution to the literature. However, as always, there are some lingering questions remaining for future research. In particular, we believe that more work remains to be done on examining the causal mechanisms at work. While our analysis concerning potential causal mechanisms underlying our main findings can be considered suggestive, we strongly acknowledge that it is somewhat speculative in nature. Whereas the administrative data utilized in this study are well suited to answer the question of whether educational reforms of the type examined here can help reduce political inequalities, they have some limitations when it comes to pinpointing the relevance of different causal mechanisms.

These limitations notwithstanding, we believe that our study has shown that improved educational standards for the masses can be an effective means to increase the social representativeness of elected assemblies. As such, the evidence presented here is of wider relevance for current debates on the effects of reforming primary education in the developing world. Development agencies ranging from UNICEF and UNESCO to the World Bank have long pleaded for educational expansion throughout the world in order to promote economic growth, reductions in poverty, better nutrition, and lower infant and child mortality. Our findings add an important component to this list because, as Gutmann (1987, 289) reminds us, the “most devastating criticism we can level at primary schools [. . .] is not that they fail to give equally talented children an equal chance to earn the same income or to pursue professional occupations, but that they fail to give all (educable) children an education adequate to take advantage of their political status as citizens.”

References


Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher’s website:
A1 Details on Data and Measures
A2 The DD Approach
A3 Analyzing the Exogeneity of the Reform
A4 Testing the Common Trend Assumption
A5 The Importance of Sample Restrictions
A6 Logit Results
A7 Heterogeneity Analyses
A8 Alternative Outcomes
A9 Other Measures of Family Background
A10 Educational Attainment and Returns