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Can political inequalities be educated away?

Evidence from a Swedish school reform

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WORKING PAPER 2014:29

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ISSN 1651-1166

Can political inequalities be educated away? Evidence from a Swedish school reform[a]

by

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19th December, 2014

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. More precisely, we utilize a difference-in-difference strategy to analyze how a large school reform launched in Sweden in the 1950s, which lengthened compulsory schooling and postponed tracking, affected the likelihood of individuals with different family backgrounds to run for public office. The data comes from public registers and pertains to the entire Swedish population born between 1943 and 1955. Overall, 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 importance of family background in political recruitment. According to our estimates, the Swedish comprehensive school reform served to reduce the effect of family background on the likelihood of running for public office by up to 40 percent.

Keywords: Political inequality, political participation, political candidacy, inequality, education
JEL-codes: H7, I24

[a]We are grateful for comments from Olle Folke, James Fowler, Helena Holmlund, Tim Johnson, Martin Lundin, Mikael Persson, Marcus Österman, and seminar participants at IFAU and the Department of Government, Uppsala University. This project has been financed by the Swedish Research Council (VR).
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"The aim of every political constitution is, or ought to be, first to obtain for rulers men who possess most wisdom to discern, and most virtue to pursue, the common good of the society." (Madison, Federalist 57).

"The very term representative, implies, that the person or body chosen for this purpose, should *resemble* those who appoint them — a representation of the people of America, if it to be a true one, must be *like* the people" (Brutus, Essay III) .

1 Introduction

Equal access to public office for all individuals is on the UN's list of basic human rights. Yet, a brief look at the composition of the legislatures around the world suffices to show that this formal right does not necessarily translate into equal political representation for all segments of society. On the contrary, a consistent finding of the research on legislative recruitment is that the opportunities to hold political office are highly unevenly distributed in all societies (e.g., Aberbach et al., 1981; Matthews, 1984; Norris, 1997). Or, as two of the pioneers of this literature noted more than forty years ago, "Government officials and other political leaders in most societies come disproportionately from the more prestigious occupations, the better-educated or otherwise privileged members of the community" (Prewitt and Eulau 1971:301). Recent research in the field has done little to change this view (e.g., Cotta and Best, 2007; Carnes, 2013; Carnes and Lupu, 2014).

As indicated by the opening quotes, whether the social bias of elected assemblies constitutes a problem is an issue that has been debated since the inception of representative government. For meritocrats like Madison, the non-representativeness of legislatures only poses a problem insofar as the political representation of different groups do not stand in proportion to their political leadership skills. Whereas others, such as the anti-federalist writing under the name of Brutus, argue that because our ability to place ourselves in the shoes of others is so limited, true representation requires *social likeness* between representatives and the represented.

One thing that both sides in this debate agree on, however, is that no one should be entitled to public office by accident of birth. Consequently, the fact that political elites usually tend to be drawn from a privileged social background has been a source of concern for adherents of meritocracy and social representativeness alike (e.g., Aberbach et al., 1981;

Norris, 1997). Over the years, many suggestions have been made on how to reduce the importance of family background in political recruitment. This study focuses on one such proposal that has attracted widespread attention across the political spectrum, namely the expansion of mass education. Thomas Jefferson provided an early and vivid formulation of this view:

> [T]hose persons whom nature has endowed with genius and virtue should be rendered by liberal education worthy to receive and able to guard the sacred deposit of the rights and liberties of their fellow citizens; and ... they should be called to that charge without regard to wealth, birth or other accidental condition or circumstance. But the indigence of the greater number disabling them from so educating at their own expense those of their children whom nature has fitly formed and disposed to become useful instruments for the public, it is better that such should be sought for and educated at the common expense of all, than that the happiness of all should be confined to the weak or wicked (Thomas Jefferson: Diffusion of Knowledge Bill, 1779, Papers 2:527).

For Jefferson, increased educational attainment was thus 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, if not sufficient, condition for political equality has gained considerable currency in liberal democratic thought. Thinkers from Dewey (1916) and Mann (1960) to Gutmann (1987) and Verba (2003) have all pointed to the equalizing potential of education. The egalitarian effects of education are not uncontested, however. According to some scholars the educational system may even serve to reproduce existing inequalities rather than to mitigate them (e.g., Bourdieu, 1973; Bowles and Gintis, 1976).

Ultimately, it is an empirical question to what extent improved educational opportunities can help make political recruitment less socially biased. Despite the importance of this question, empirical research on the topic is scant. One 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 non-observed 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 and postponed tracking,[2] affected the likelihood of individuals with different family background 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 that 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 (Meghir and Palme, 2005; Holmlund, 2007). 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 im-

[1] The 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.

[2] More generally, tracking refers to the sorting of individuals into different classes or schools based on some criterion such as study motivation or academic performance.

proved educational opportunities for individuals from disadvantaged backgrounds can be an effective means to reduce the importance of family background in political recruitment. Before the reform the probability of standing as a candidate was almost 1.3 percentage points greater for individuals from non-working class homes compared to individuals from working class homes. The corresponding difference after the reform was less than 0.8 percentage points (the baseline probability of running for office in the entire sample was 3.5 percent). According to our estimates, the Swedish comprehensive school reform thus served to reduce the effect of social origin on the likelihood of running for political office by as much as 40 percent. Based on the current study we are unfortunately not in a position to pinpoint the exact causal mechanisms underlying this effect, but in the end of the paper we speculate that the reform helped reduce political inequalities by promoting political skills and interest in the the lower parts of the educational distribution.

The rest of this paper is organized as follows. We begin by reviewing the relevant literature on the subject. The next section provides a brief historical narrative of the school reform. We then describe the construction of the dataset, provide sample summary statistics and discuss the empirical framework and issues of identification. The following sections reports the results from the basic models and numerous robustness checks. We conclude with a discussion of the main findings.

2 It is a long way to the top

Who our elected representatives are is a question that has attracted scholarly attention for a long time. As mentioned in the introduction, the existence of a substantial status gap between citizens and their representatives is a frequently reported finding in this line of research. However, while the social bias of elected assemblies is a well-established fact, the sources of this bias are less well understood. An important reason for this is that empirical work on political elites traditionally "has been concerned with documenting trends rather than with explaining the composition of parliament" (Norris and Lovenduski, 1995, 9).

This state of affairs is slowly starting to change as a result of the growing research on the political representation of women and ethnic minorities. Studies in this area typically use aggregate data, at the local or national level, to examine how differences in political context affect the representation of politically marginalized groups (Wängnerud, 2009; Bloemraad and Schönwälder, 2013). Although these studies have yielded many valuable insights, they are, as Jennifer Lawless points out, "limited in the extent to which they aid our understanding of whether and why certain people pursue elective positions in the first place, whereas others recoil at the notion" (Lawless, 2012, 5). Making progress on this latter issue calls for individual level data and analysis.

Turning to the individual level, 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, names 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, in particular, have always faced difficulties in reaching important decision-making positions. For instance, in their seminal study of political and bureaucratic elites in seven Western democracies, Aberbach et al. concluded that "Persons from working-class and lower-middle-class backgrounds are largely excluded from the elite" (1981, 81).

Aberbach and his associates argued that the educational system is key in understanding the absence of working-class people in the top rungs of society. First, because education is such an important credential for individuals seeking public office, the fact that people from lower-class background, on average, tend to be less educated will be of direct relevance for their chance of holding office. Second, the cross-tabulation results presented by the authors show that the importance of family background weakens with educational attainment, suggesting that schooling may help compensate for some of the disadvantages coming with lower-class upbringing. Therefore, education can serve both to mediate and moderate the effect of family background on the likelihood of entering politics.

Based on their analysis, Aberbach et al. (1981) therefore concluded that educational reform might be an effective means to reduce the elite bias of elected assemblies, in particular in many European countries, which traditionally have had rather inegalitarian educational systems:

> [B]y 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 (1981, 62).

This view echoes Jefferson's vision of education as a way to establish a natural aristocracy in which political power is based on virtue and talent rather than on fame and fortune.

The pioneering study of Aberbach et al. (1981) represented a great achievement theoretically as well as empirically. Nonetheless, it also had some important shortcomings with respect to data and methodology. First, to keep the workload manageable, the sample of politicians was restricted to between 50 and 100 national parliamentarians in each country. Consequently, when the results are broken down by family background and education there are often rather few observations in each cell, which may negatively impact the reliability of the results.

Second, as forcefully argued in recent contributions on the education-participation nexus, it is very difficult to assess the importance of education in cross-section studies of the type conducted by Aberbach et al. (1981). The problem is that education is believed to operate as a proxy for important, but difficult to observe, preadult experiences and predispositions (e.g., Kam and Palmer, 2008; Sondheimer and Green, 2010; Persson, 2013). According to this 'education as a proxy view', the high levels of education in elected assemblies may, thus, simply reflect the fact that there are unobserved factors that affect both educational choices and the likelihood of pursuing a political career. Likewise, the finding that the importance of family background tend to decrease with educational attainment may be due to individuals with varying levels of education being intrinsically different. If correct, the education as a proxy view has far-reaching consequences since it implies that we have little reason to expect educational reforms of the type envisioned by Jefferson and Aberbach et al. to have any effect on the social composition of our elected assemblies.

The belief in the equalizing potential of education has also been questioned on theoretical grounds. For advocates of 'the cultural reproduction thesis' (developed in the field of educational sociology by scholars such as Pierre Bourdieau), the key function of modern educational systems is to confirm and maintain established social hierarchies (e.g., Bourdieu, 1973; Bowles and Gintis, 1976). Reforms aimed at expanding formal education, the argument goes, are unlikely to change this state of affairs since the dominant social groups will always find ways to influence and re-stratify the system in their favor.

The optimistic claim that reforming the education system could considerably reduce, if not completely eliminate, the importance of family background 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., Breen and Jonsson, 2005; Björklund and Salvanes, 2011).

In assessing the role of education in social mobility, sociologists and economists have focused on two aspects of the educational system in particular: overall educational attainment and the extent of educational tracking. According to the conventional view, increased educational attainment contributes to social mobility because the influence of family background on various social outcomes tends to be weaker at higher levels of education (cf., Breen and Jonsson, 2005, 234). With respect to educational tracking, it has been frequently argued that deciding later which educational track children will enter mitigates the influence of family background on children's educational and occupational careers (e.g., Brunello and Checchi, 2007).

Against this background, the reforms of primary education undertaken in many Western countries during the 1950s and 1960s constitute attractive research objects, since they often implied an increase in the minimum school leaving age as well as postponed tracking (Brunello et al., 2009, 524). In addition, a methodological advantage of the compulsory schooling reforms is that they were subject to sequential implementation in many countries, which considerably increases our opportunities to evaluate their effects.

In this article, we focus on the comprehensive school reform launched in Sweden in the 1950s. More precisely, we examine whether the importance of family background for seeking public office decreased as a result of the educational reform. The current study sets out to fill the research gap identified above by investigating whether the claims made by Aberbach et al. and others—that educational reforms are an effective means to reduce the importance of family background in politics—hold up to closer scrutiny.

3 The type and purpose of the reform

This section briefly discusses the Swedish compulsory school reform that was gradually rolled out across the country's municipalities during the 1950s and 1960s. The discussion focuses on the features of the reform that are directly relevant for evaluating the plausibility of the identification strategy described in the next section. A comprehensive discussion of these questions is also provided by Marklund (1981), Meghir and Palme (2005), Holmlund (2007) and the references cited therein.

In the pre-reform 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 five (starting in fifth grade) or three (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 seven year compulsory education.[3]

The prevailing system, based on directing more and less able pupils into different tracks, was extensively debated among political parties and education experts throughout the interwar period. Especially within the ruling social democratic party, the earlier view of the school as a vehicle for nurturing law-abiding and dutiful subjects gave way to radically different ideas about the role of the school as the key to abolishing class-based society and promoting democratically minded citizens (Husén, 1986; Oftedal Telhaug et al., 2006).

Against this backdrop, an inclusive parliamentary committee was appointed in 1946 with the task of proposing guiding principles for the future organization of the compulsory school system. As part of the final report released in 1948, the committee recommended replacement of the compulsory and selective junior secondary school with a nine year

[3] In some municipalities, mainly the big cities, compulsory schooling was extended to eight years before the comprehensive school reform.

compulsory school and the abolition of all parallel forms of school. Furthermore, all pupils would attend the same schools. The committee also proposed important changes to the content and form of education. Above all, particular focus was placed on the study of civics.

These radical proposals rested on three main objectives, out of which the latter two were considered the primary aims of the reform (Lyon, 2001; Oftedal Telhaug et al., 2006). The first objective was economic in nature. Higher levels of education were argued to promote economic development. In this vein, advocates of the reform maintained that a comprehensive school system would be better at unearthing any hidden talent pools.[4]

The second objective of the reform was social: "The aims of the school reform are clearly formulated by the 'early' educational reformists: the abolition of injustice due to geographical, class, and gender factors" (Nilsson, 1989, 359). The expectations were high indeed. By giving all pupils the chance to develop in accordance with their goals and abilities, and by promoting social community in untracked classes in which the stronger aided the weaker, the ultimate goal was to pave the way for a society less stricken by class-based differences. In the end, the "goal of the reform was to change the traditional elitist recruitment not only to higher education, but thereby also to key positions in the state" (Rothstein, 1996, 66).

No less important was the political or democratic objective of the reform. Proponents of the reform argued that the new comprehensive school should serve to socialize the pupils into democratic citizens (Oftedal Telhaug et al., 2006, 253):

> This aspect of socialization was to be reinforced through specific measures such as an increase in the number of teaching hours devoted to the study of society, the establishment of pupils' councils, and opportunities for election which allowed the pupils to influence their own education. The comprehensive school was in itself seen as a stage in pupils' democratic socialization as it placed them within a community where all classes of society would meet. The comprehensive school was a democratic society in miniature.

The committee proposal led to a large-scale nationwide evaluation between 1949 and

[4] Closely related to this objective, the school reform was seen as a way to meet the increased demand for education that had been observed among the younger cohorts at the time.

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.[5] As a general rule, the comprehensive school was first implemented in grades 1 and 5. This meant that the first graders were immediately exposed to the reform, whereas those in the second, third, and fourth grades were exposed from the fifth grade and up. Consequently, all pupils who in the year the reform was implemented in a municipality attended grades one to five are considered exposed to the reform, whereas those in the sixth grade and up are not exposed. 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.

The selection of municipalities to take part in the evaluation program was not random. Under the auspices of the National Board of Education, municipalities that met certain criteria in terms of population growth, local demand for education, and availability of teachers and school premises were chosen for participation from a group of applicants. From a modest start, where only 14 municipalities were selected for the first year of assessment (1949/1950), the number of municipalities joining the evaluation program grew steadily. In 1962, the parliament decided to permanently introduce the nine year comprehensive school throughout the country. The municipalities then had until 1969 to implement the new system for all affected cohorts.

Previous studies have utilized the quasi-experimental nature of the Swedish school reform in order to estimate the causal effects of education. Based on a sample of pupils born in 1948 and 1953 Meghir and Palme (2005) evaluate the effects of the compulsory school reform on final educational attainment and earnings. They find that the reform had a positive impact on both total years of schooling and later life income levels, especially among individuals from less favoured socioeconomic backgrounds. Later, a number of studies have used the reform as an instrument for years of schooling. For example, Holmlund et al. (2011) study the reform-induced intergenerational education effects and report

[5] At the start of the evaluation period Sweden was divided into about 2,500 rural municipalities and city communities. In 1952 the number of rural municipalities was greatly reduced resulting in a total of 1,037 municipalities. The latter is the municipality division used in our empirical models.

a statistically significant parental education influence on children's schooling, and Lager and Torssander (2012) find that individuals assigned to the new comprehensive school system have a somewhat lower mortality rate from causes known to be related to education. More closely related to the current study, Fredriksson and Öckert (2013) show that the effect of school-starting age on educational attainment decreased as a result of the reform.

4 Data and method

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 (i.e., they started school in 1962 when the parliament decided to permanently introduce the nine-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.

To construct a reform status indicator for each individual in our sample, we follow Holmlund (2007) and use information on home municipality from the censuses in 1960.[6] In the analysis presented in the next section, we retain only those individuals born 1943 or later, because by 1960 large portions of the cohorts born 1938-1942 were likely to have moved from the municipality in which they attended compulsory school.[7]

In what follows, we mainly use *parental social class*—based on occupational information from the 1960 census—to measure family background. The class division used is based on the official Swedish occupational classification (SEI), which closely follows the popular EGP class schema (Erikson and Goldthorpe, 1992). In the original data, seven classes of employed persons are identified: (1) higher non-manual workers, (2) intermediate non-manual workers, (3) lower non-manual workers, (4) self-employed, (5) farmers,

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

[7]Holmlund (2007) shows that the share of individuals living together with their biological mother in 1960 decreases sharply for cohorts born before 1943.

(6) skilled manual workers, and (7) non-skilled manual workers.[8]

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 distinguish between individuals with working- and non-working class origin. However, we also present results for a more fine grained classification as a robustness check. Moreover, in what follows, class origin will be measured at the household level using the class coding of the parent with the dominant class position (see the Appendix for further details on this approach).

The reason for using social class as our measure of family background is mainly governed by data availability.[9] Other likely candidates to measure parental SES, such as income or education, are unfortunately not available in the 1960 census. Yet, social class is known to be highly correlated with other SES-variables, and we will also be checking the robustness of our findings by utilizing information on parental education from the 1970 census.[10]

To obtain information on our dependent variable, we matched the children in our sample to a register that contains information on all nominated and elected candidates in the six parliamentary, county council, and municipal elections in the period 1991–2010.[11] Finally, we have obtained information on a range of demographic and socioeconomic characteristics for the children as well as their parents from various administrative registers.[12]

Table 1 reports descriptive statistics separately for the whole sample of individuals

[8] The algorithm for coding occupational codes into these seven social classes was originally developed by Jan O. Jonsson (see Erikson and Jonsson, 1993), and we are grateful to Martin Hällsten for sharing his stata code with us.

[9] The coverage of this measure of family background is very good for all the studied cohorts, and ranges from a low of 89% for the 1943 cohort to a high of 96% for the 1955 cohort.

[10] A problem, however, is that educational attainment is only available for parents born after 1910. Using this variable results in a reduction of our sample by more than a quarter.

[11] In Sweden, the national and the two local (county- and municipal-level) elections are held simultaneously on the third Sunday in September every three (until 1994) or four (after 1994) years.

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

Table 1: Descriptive statistics

	All	Nominated	Elected
Birth year	1949.30	1949.21	1949.15
	(3.48)	(3.48)	(3.48)
Male	51.02	57.90	57.20
	(50.00)	(49.37)	(49.48)
Earnings	1488.85	1587.77	1774.71
	(1089.80)	(1064.48)	(1082.35)
Education	11.41	12.17	12.46
	(2.65)	(2.59)	(2.51)
Immigrant background	7.35	7.20	7.17
	(26.09)	(25.85)	(25.81)
Parental class	50.45	57.52	57.07
	(50.00)	(49.43)	(49.50)
Nominated (at least once)	3.53	100.00	100.00
	(1.85)	(0.00)	(0.00)
Elected (at least once)	1.06	30.01	100.00
	(1.02)	(45.88)	(0.00)
Nominated (times)	0.09	2.44	3.60
	(0.54)	(1.60)	(1.68)
Elected (times)	0.02	0.69	2.31
	(0.28)	(1.35)	(1.52)
N	786,790	27,778	8,336

included in the main analysis (column 1), the candidates (column 2) and those elected (column 3). The first five rows in *Table 1* provide information on the distribution of birth years, sex, earnings, education, and immigrant background.[13] The candidates and those elected from the 1943–1955 birth cohorts comprise a notedly selected group of individuals. They are better educated, better paid, and more likely male and of Swedish heritage compared to the average individual of the same age.

The mean value and standard deviation for the main independent variable is presented in the sixth row of the table. As described above, the parental class dummy indicates whether an individual is of working (=0) or non-working (=1) class origin. Individuals with non-working class origin constitutes just over 50 percent of the overall sample, whereas the corresponding figures are 57.5 and 57.1 percent for those nominated and

[13] The immigrant background dummy is equal to 1 if the individual or at least one parent is born abroad.

elected, respectively. On average, individuals from non-working class homes in the studied cohorts are thus over-represented politically.

The last rows report summary statistics for the main outcomes. On average, 3.53 percent of the individuals born between 1943 and 1955 ran for office at least once at the national, county or municipal level in the general elections held between 1991 and 2010, corresponding to an average of 0.09 out of a maximum of six nominations per individual. The baseline probability for attaining office at least once and the average number of terms served are 1.06 percent and 0.02 times, respectively.

The variation in our outcome measures is largely driven by candidates elected at the municipality level which makes up approximately 80 percent of the total number of cases. In light of this, it is important to note that Swedish municipalities play a crucial role in the provision of government goods and services, not the least in key areas, such as social assistance and education. Much like the national parliament and county level assemblies, the municipal councils are elected using a party-list proportional system. The municipalities are governed by a "quasi-parliamentary" system, where a majority party or coalition typically appoints committee leaders and determines local policy (Bäck, 2003). Municipalities have independent income taxation rights. In 2010, the average municipal income tax rate was approximately 21 percent. Municipalities also employ large shares of the labor force. In 2010, for instance, about 17 percent of the employed worked in the municipal sector. The fact that municipal politics are a crucial springboard to national politics in Sweden (see e.g., Lundqvist, 2011) further underscores the importance of studying local political assemblies.

For several reasons, our analyses will mainly focus on the indicator for candidacy. First, the fact that more than 96 percent of all the eligible individuals are eliminated in the nomination stage reflects the vital role of candidate selection. Put simply, the real hurdle to clear is not getting elected, but instead getting one's name on the party list in the first place. Second, failure to get elected does not necessarily preclude individuals at the lower end of the party lists from reaching different political positions. Above all, non-elected candidates are commonly used to populate the many municipal boards and committees.[14]

Figure 1 provides an initial illustration of the relationship between the school reform and the degree of social bias of elected assemblies. To obtain a measure of social bias, we have calculated the risk ratio of being nominated given parental socioeconomic status for each birth cohort from 1938 to 1958. The risk ratio measures the probability of a certain event (e.g. being nominated) in one group (e.g. children of workers) divided by the probability of the same event happening in another group (e.g. children of non-workers).

The dashed line (with the associated 95 percent confidence intervals) in *Figure 1* reveals that the social bias in political candidacy is significantly lower among the younger cohorts in our sample. The likelihood of being nominated to a political assembly for a person born in 1938 from a working-class home is only about 67 percent of the same likelihood for someone of non-working class origin. For the 1958 cohort, the corresponding risk ratio is above 85 percent. We have also superimposed the share of pupils in each birth cohort that was exposed to the school reform (solid line).

The upward trends of the two lines depicted in *Figure 1* are in line with an equalizing influence of the comprehensive school reform. However, simple bivariate relationships of this type may be deceptive for a number of reasons. For instance, the fact that the share of individuals exposed to the reform goes hand in hand with less social bias in candidate selection may be driven by a general tendency towards egalitarianism during the time period under study. To rule out such possible confounders, a more stringent identification strategy is needed. The fact that the Swedish comprehensive school reform was implemented at different times in different municipalities enables us to employ a difference-in-difference approach that controls for regional differences and time trends in the relationship between parental background and the probability of running for office.

The departure point for the empirical framework is a simple specification relating the indicator for running for office to family background:[15]

[14] In 2007 and 2011, Statistics Sweden conducted population surveys on all elected and non-elected representatives in Sweden's municipality and county councils. About 60 percent of the non-elected candidates in the 2006 and 2010 municipal and county elections were members of one or more board or committee in 2007 and 2011.

[15] For a similar empirical framework, see Pekkarinen et al. (2009). A more detailed discussion of the difference-in-difference framework is provided in the Appendix.

Figure 1: Stratification of candidacy

Note: The dashed line depicts the risk ratio for individuals born 1938–1958, calculated as a three year moving average, and the shaded area denotes 95 percent confidence intervals for this risk ratio. The solid line shows the share of reformed individuals within each cohort.

$$y_{icm} = \beta_0 + \beta_1 R_{cm} + \beta_2 P_{icm} + \theta_c + \eta_m + \varepsilon_{icm}, \tag{1}$$

where y is a dichotomous indicator taking on the value of 1 for individuals nominated for political office, R_{cm} is a dummy indicating whether the individual was exposed to the reform, P_{icm} indicates parental class position, and θ_c and η_m are cohort and municipality fixed effects, respectively. The coefficient β_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 if the school reform influenced this relationship, we allow this regression coefficient to vary across cohorts (γ_c), municipalities (λ_m) and reform status:

$$\beta_2 = \delta_1 + \delta_2 R_{cm} + \gamma_c + \lambda_m \tag{2}$$

Inserting equation (2) into equation (1) yields:

$$\begin{aligned} y_{icm} = &\beta_0 + \beta_1 R_{cm} + \theta_c + \eta_m + \delta_1 P_{icm} + \\ &\delta_2 R_{cm} \times P_{icm} + \gamma_c \times P_{icm} + \lambda_m \times P_{icm} + \varepsilon_{icm} \end{aligned} \tag{3}$$

The parameters of primary interest are δ_1 and δ_2. The former provides a measure of the strength of the relation between parental class and the probability of running for office for individuals not exposed to the reform. δ_2 measures the influence of the school reform on this relationship. A negative estimate of δ_2 would suggest that the reform had an equalizing effect on the social bias of political representation. By including a full set of cohort 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.

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 et al. (2009) and Borgonovi et al. (2010), we limit our sample to those individuals born at most seven years before or six years after the first cohort affected by the reform (i.e., the maximum observation is seven years around 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 one year too old to be affected by the reform (Holmlund, 2007;

Hjalmarsson et al., 2014). 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). After presenting our main results, we also provide sensitivity analyses of our results to both the common trend assumption and these two sample restrictions.

5 Empirical results

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 for two different reasons. First, and most importantly, when applying non-linear models such as logit or probit in a difference-in-difference framework, additional and restrictive assumptions are required 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 constant, the nonlinear specification requires such differences to be absent (Lechner, 2011). Second, the interactive relationship between reform status and parental class is of central interest in this study and the interpretation of interaction effects is considerably more complicated in the non-linear case (see e.g., Ai and Norton, 2003). This being said, in the Appendix 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 by Swedish parents), and a set of dummies for both father's and mother's birth year. For expositional clarity, the tables do not report coefficients for these baseline covariates. In line with current practice, 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 3.35

percent, 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 (not shown here) is about 0.96 percentage points. Therefore, according to these results, the importance of family background for having a career in politics is on par with that of gender.

According to model 1, the average effect of the educational reform is, if anything, negative 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 include 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 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. For instance, Meghir and Palme (2005) found that the earnings of individuals with skilled fathers fell by almost 6 percent as a result of the compulsory school reform.[16] According to Meghir

[16] Using a similar reform in Finland, Pekkarinen et al. (2009) also observed a negative reform effect for sons of high-earnings fathers.

and Palme, a potential explanation for this result is that the abolishment of the old differentiated, and more selective, school system “reduced the quality of education and ultimately the earnings of this group” (2005, 419).

This could be the case, however our result could also be due to the competitive nature of the electoral process. Given that there is a fixed number of seats to be filled (and that there is usually a rather 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, which could help explain why we observe a negative reform coefficient in the latter group.

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 (apart from the differences caused by the implementation of the reform). 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 zero. In columns 3–6 we report results from models in which we successively relax these restrictions.

In column 3 we include a full set of controls for the interaction between the thirteen cohort dummies and the parental class indicator. In column 4 we instead enter 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 model specification in column 6 is equivalent to estimating Equation 1 in two separate samples for individuals with working- and non-working class parents, respectively.[17] The overall pattern of results in columns 3-6 are very similar to the ones 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 compre-

[17] The first row of columns 3–6 reports the average effect of parental class across the sample for individuals not affected by the reform. That is, these estimates are directly comparable to the corresponding estimate in column 2.

hensive school system. Based on least the restrictive models presented in columns 5 and 6, the effect of family background was reduced by as much as 40 percent as a result of the reform. Since the estimates from Models 5 and 6 are close to identical, we will treat the simpler Model 5 as our preferred specification in what follows.

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 background 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 percent 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 reform decreased the probability of running for office by 0.19 percentage points among these individuals (which constitutes a 5 percent reduction compared to their pre-reform probability). The substantive magnitude of these effects is comparable to those found for other outcomes in previous research. For instance, Meghir and Palme (2005, 420) reported that the reform lead to an increase in earnings of between 3 and 7 percent for individuals with unskilled fathers, whereas it implied a reduction in earnings of between 5 and 7 percent for those with skilled fathers.

In *Table 3*, we test whether the results in *Table 2* are driven by (i) the outcome used in the models or (ii) the level of analysis.[18] The models in *Table 2* employ an indicator of running for office at least once in the six elections between 1991 and 2010. The first column of *Table 3* instead reports results from a model using a count variable — the number of times the individuals ran for office during the time-period. The pattern of results is very similar to the one displayed in *Table 2*. For the pre-reform individuals, an advantaged family background increased the expected number of nominations by 0.035 (which can be compared to the baseline rate of 0.09 nominations). The substantially strong and statistically significant interaction effect implies that this social bias is reduced

[18] All models presented in *Table 3* include the same set of control variables and fixed effects as specified in Model 5 in *Table 2*.

Table 2: The reform and the social stratification of candidacy

	(1)	(2)	(3)	(4)	(5)	(6)
P	1.035***	1.199***	1.244***	1.189***	1.273***	1.275***
	(0.053)	(0.065)	(0.082)	(0.041)	(0.082)	(0.082)
R	0.067	0.244***	0.293***	0.234***	0.324***	0.324***
	(0.099)	(0.101)	(0.113)	(0.098)	(0.115)	(0.115)
P × R		−0.355***	−0.454***	−0.335***	−0.515***	−0.516***
		(0.089)	(0.127)	(0.063)	(0.176)	(0.176)
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mun. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × P FEs	No	No	Yes	No	Yes	Yes
Mun. × P FEs	No	No	No	Yes	Yes	Yes
Controls × P	No	No	No	No	No	Yes
Observations	786,790	786,790	786,790	786,790	786,790	786,790

Notes: All models include controls for sex, immigrant background, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

to 0.02 among individuals attending the reformed school system. Likewise, we can see that the reform had a positive effect (0.006, p = 0.10) on being nominated for election among low status individuals, whereas the reform effect is negative (−0.008, p = 0.07) for those raised by high SES parents.

Columns 2 and 3 display estimates from models using a dummy indicator for being elected (2) and the number of times an individual was elected (3) as outcomes. Since only about 30 percent of those running for office are elected, the baseline rates are lower than in the corresponding models for being nominated. The overall probability of being elected at least once between 1991 and 2010 is 1.06 percent whereas the average number of terms served is 0.02. The models in columns 4 and 5 are instead based on multiple observations for each individual across the six elections between 1991 and 2010 — in total 4,579,299 election-individual observations. The binary outcome is set to one for individuals who ran for office (4) or was elected (5) in a particular election.

The estimates in columns 2 to 5 are consistent with the results presented in *Table 2* and in column 1 of *Table 3* and reveal sizeable equalizing effects. Irrespective of the outcome used (Models 2 and 3) or the level of analysis (Models 4 and 5), the influence of family background on the chances of nomination or election is reduced by up to a half as a consequence of the school reform.

Table 3: Reform effect on alternative outcomes

	(1)	(2)	(3)	(4)	(5)
P	0.035***	0.415***	0.010***	0.595***	0.172***
	(0.002)	(0.047)	(0.001)	(0.043)	(0.022)
R	0.006*	0.079	0.002	0.094	0.033
	(0.003)	(0.070)	(0.002)	(0.058)	(0.031)
P × R	−0.014**	−0.224**	−0.006**	−0.236**	−0.096**
	(0.005)	(0.100)	(0.003)	(0.092)	(0.048)
Cohort FEs	Yes	Yes	Yes	Yes	Yes
Mun. FEs	Yes	Yes	Yes	Yes	Yes
Cohort × P FEs	Yes	Yes	Yes	Yes	Yes
Mun. × P FEs	Yes	Yes	Yes	Yes	Yes
Observations	786,790	786,790	786,790	4,579,299	4,579,299

Notes: The dependent variable is defined as follows: (1) Total number of nominations, (2) Elected at least once, (3) Total number of elections, (4) Nominated in a particular election, (5) Elected in a particular election. All models include controls for sex, immigrant background, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

In summary, there are two important lessons that can be drawn from the results presented in Tables 2 and 3. First, there is a strong social bias in political representativeness. Consistent with previous studies, we find that Swedish political candidates tend to come from privileged social backgrounds (Prewitt and Eulau, 1971; Matthews, 1984; Cotta and Best, 2007).

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 percent. Although sizeable, we do not think this effect is unrealistically large. Effects of comparable magnitude have been reported for other outcomes. In a study of the effect of school-starting age on educational attainment, Fredriksson and Öckert (2013) found that the introduction of the comprehensive school reform reduced the effect of school-starting age by almost 50 percent. Also, Pekkarinen et al. (2009) demonstrated that a similar school reform in Finland led to a reduction in the intergenerational income-elasticity by almost a quarter.

5.1 Sensitivity assessment

The results presented above support the idea embraced by democratic thinkers from Jefferson and onward that an expansion of mass education can help mitigate the political underrepresentation of individuals from lower rungs of the socioeconomic ladder. However, before drawing any firm conclusions, we conduct an extensive sensitivity analysis.

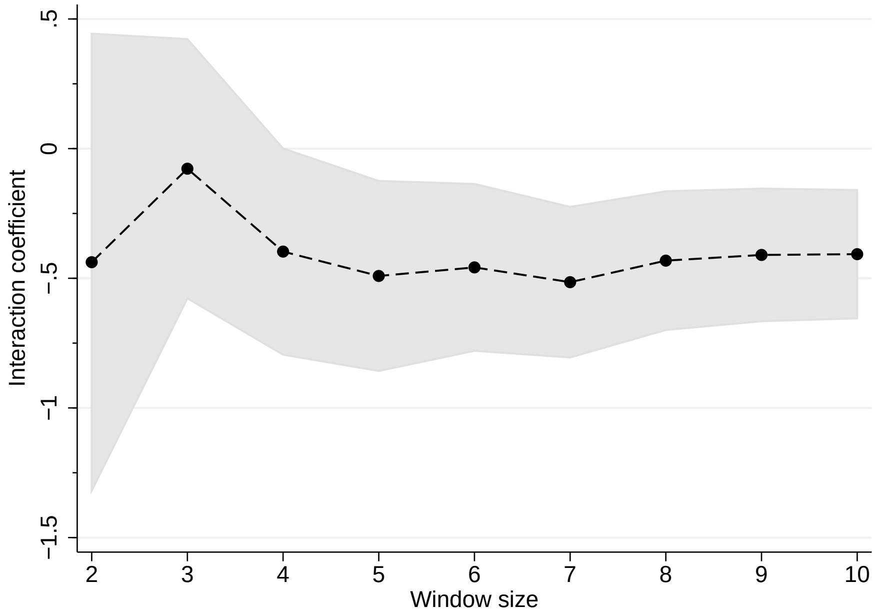
Although the type of difference-in-difference estimator employed here rests on considerably less stringent modeling assumptions than those invoked in previous research on the topic, the chosen research design is not assumption free. Most importantly, the key identifying assumption underlying our empirical 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, in the Appendix we report the results from two different tests for the presence of pre-reform trends in the data. We could not find evidence for pre-reform trends based on either of these tests. Although the outcome of these tests do not prove that our key identifying assumption is correct, it serves to considerably strengthen the credibility of this assumption.

Another potentially important modeling choice is that of restricting the analysis to a seven years window around the reform. *Figure 2* displays how the coefficient of the interaction effect between reform status and parental background, from our preferred specification based on model 5 in *Table 2*, varies with the size of the reform window.

It is clear from the figure that our main findings are not very sensitive to the choice of the window size. Except for the case when the window is set to three years around the reform (which seems to be an outlier case), the interaction effect remain around -0.4 to

Figure 2: Effects for different reform windows



Note: The dashed line denotes the interaction effect between reform status and parental background for different choices of window size. The shaded area represents 90 percent confidence intervals for this effect.

-0.5 irrespective of the window size. As expected, however, the estimate becomes much more noisy and imprecisely estimated when only a few years around the reform is used in the estimation.[19] It is thus comforting to note that our main findings do not appear to hinge on this modeling choice.

Furthermore, we have rerun the baseline models displayed in *Table 2* also including the birth cohort preceding the first cohort affected by the reform. Results from these models are presented in the Appendix. Although the main pattern of results are very similar to the ones presented in *Table 2*, the coefficients for the interaction term between reform status and family background are somewhat weaker in magnitude. The most likely explanation for this is the fact that a significant share of the pupils born in the last months of the year started school a year later than their peers and consequently are defined as non-treated despite the fact that they were affected by the reform.

Finally, a potential objection to our estimation strategy is that families may have

[19] Moreover, although not shown here, we also obtain similar estimates for the main effect of parental background when changing the size of the reform window.

moved residence as a consequence of the reform. At the time of the evaluation of the reform, concerns were raised that parents in municipalities where the new comprehensive school were introduced would opt out of the reform by moving to nearby non-reformed municipalities (Marklund, 1981). Such systematic mobility patterns would imply that reform status should be treated as endogenous even conditional on the fixed effects for municipality at school-starting age. Meghir and Palme (2005) test this assertion in their sample of pupils from the 1948 and 1953 cohorts. First, they show that 9.9 percent of the pupils in their sample changed reform status between birth and the census in 1960. Of these, approximately equally large shares moved from a reform to a non-reform municipality (5.3 percent) as in the opposite direction (4.6 percent), suggesting that the mobility patterns were not systematically related to the school reform. Second, they reestimate their main specifications using only those pupils who did not change their reform status as a consequence of moving from their birth municipality. They also reestimate their main models using birth municipality as an instrument for reform status. Their results were robust both to the sample restriction and the instrumental variable approach. As a result, we see no strong reason to suspect that mobility is a source of bias for our results.

5.2 Details and mechanisms

Based on the results presented above, we conclude that the Swedish comprehensive school reform helped mitigate the importance of family background in political recruitment. Having established that, the next and natural question is what drives this effect. A more systematic analysis of the causal mechanisms at work is, unfortunately, beyond the scope of this paper, since it would require both additional data and a different research design. However, a more detailed analysis of the observed relationship is useful in generating hypotheses about likely mechanisms driving the results.

Towards this end, we have re-estimated our main model for various sub-groups of interest. In *Table 4*, we report results by gender, municipality size, and late and early reformers. The reason for focusing on these particular subgroups is that previous research have found the effect of the reform to differ between these groups (e.g., Meghir and Palme, 2005; Hjalmarsson et al., 2014).

Table 4: Heterogeneity analysis

	Gender		Size		Timing	
	Male	Female	Large	Small	Early	Late
P	1.632***	0.895***	1.156***	1.388***	1.756***	1.254***
	(0.131)	(0.108)	(0.082)	(0.111)	(0.232)	(0.093)
R	0.513***	0.123	0.387**	0.261	0.618**	0.294*
	(0.168)	(0.171)	(0.153)	(0.164)	(0.281)	(0.169)
P × R	−0.688***	−0.313	−0.691***	−0.367	−1.289***	−0.405
	(0.282)	(0.232)	(0.253)	(0.261)	(0.364)	(0.258)
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mun. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × P FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mun. × P FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	401,840	385,733	342,446	445,127	316,354	471,219

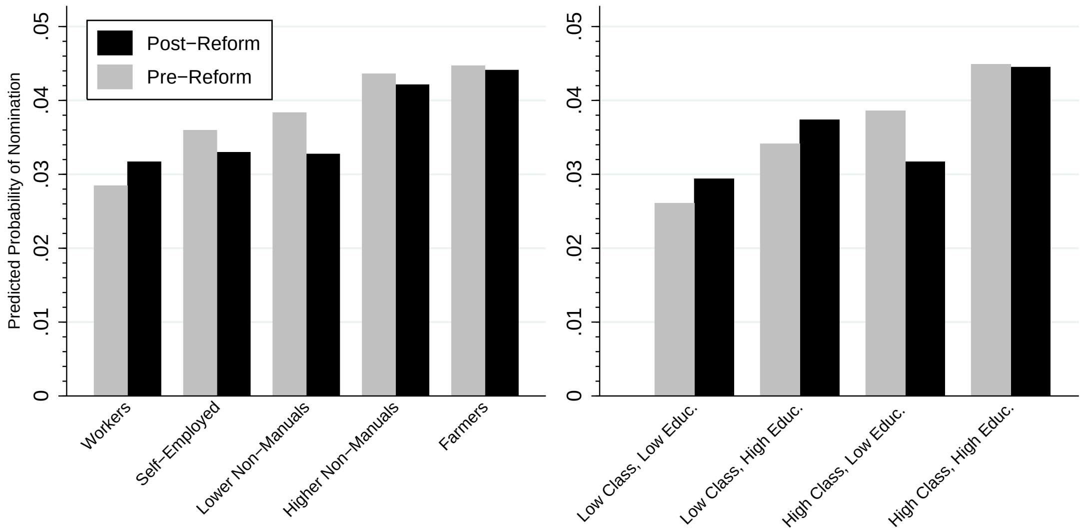
Notes: All models include controls for sex, immigrant background, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

By comparing the first two columns of the table, we see that the reform appears to have affected boys more than girls. In particular, the positive effect of the reform on the likelihood of running for office is entirely driven by the male part of the sample. Even though we find a substantial equalizing effect of the reform also among females—the coefficient of class origin drops by almost a third—this effect appears mainly be due to the fact that women from non-working class homes were less likely to become a candidate after the reform (although statistical precision is unfortunately too low to warrant any firm conclusions). The result that the compulsory school reform primarily impacted boys is also consistent with the findings of previous research on various non-political outcomes (Meghir and Palme, 2005; Holmlund, 2007; Meghir et al., 2011).

In the remaining columns of *Table 4*, we examine whether the effects of the reform differ between small (less than 10,000 inhabitants) and large municipalities, and between early (the first affected cohort was born prior to 1950) and late reformers. Although the patterns of coefficient are similar for all groups, the equalizing effect of the reform is more pronounced in large and early reforming municipalities, respectively. A likely explanation for the latter result is that due to the general increase in the demand for education among the studied cohorts, the share of individuals with only compulsory education dropped rather rapidly over time, which should have reduced the importance of increased mandatory schooling.

The question is then how to interpret the fact that the reform seems to have been more important for individuals growing up in larger municipalities. One possibility is that the composition of the class groups differ between urban and rural areas, and that our dichotomous class measure is too coarse to sufficiently capture these nuances. To examine if this is the case, we have reestimated the model using more fine-grained measures of family background. *Figure 3* provides a graphical summary of the most noteworthy findings from these analyses (see the Appendix for coefficient estimates and additional details).

Figure 3: Predicted probabilities by social origin and reform status



The leftmost graph shows the predicted probabilities of being nominated to political office by social origin and reform status.[20] The fact that the reform served to reduce the political underrepresentation of individuals of working class origin is clearly visible from the graph. The height of the bar labelled *Workers* is much more similar to that of the other bars in the post-reform state. More interestingly, however, we see that working-class children mainly seem to have benefited at the expense of children whose parents

[20] In this analysis, the original seven category class indicator has been transformed into five categories by lumping together unskilled and skilled workers, and intermediate and higher non-manual workers, respectively.

were either self-employed or held lower non-manual jobs. If we instead compare children of workers with those of the other two groups, we see a much smaller reduction in the representation gap.

The finding that the school reform was relatively less important for equalizing the political representation between workers and farmers is unsurprising given research showing that the educational attainment profile of children of farmers was the one most similar to that of children of workers both before and after the reform (Erikson and Jonsson, 1993).[21] The fact that the reform appears to have done more to close the representation gap between non-skilled manual workers and lower non-manual workers and self-employed than between non-skilled manual workers and intermediate and higher non-manual workers is somewhat more surprising. A possible interpretation of this is that the reform mainly served to strengthen the position of the working class vis-a-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.

The results presented in the rightmost graph of *Figure 3* further supports this interpretation. In this analysis, we have used the information regarding parental education from the 1970 census to code educational status at the household level (a household is coded as highly educated if either the father or the mother have completed junior secondary school or higher). In the next step, we have cross-tabulated parental education with our dichotomous class measure to differentiate between four types of households: low class—low education, low class—high education, high class—low education, and high class—high education.

Since information on parental education is only available for parents born after 1910, the sample is reduced by almost thirty percent when including parental education in the specification. Nonetheless, the overall pattern of the results is similar to those reported above. We see that the reform increases the probability of running for office among children from working-class homes by the same amount irrespective of parental education. For children from non-working class homes, however, the situation looks a bit different.

[21] The reason why children of farmers are so likely to run for office is by and large an effect of the presence of the Agrarian Party in Swedish politics.

Most importantly, the negative effect of the reform is only visible for the high-class—low education group, whereas we find a null effect of the reform in the high-class—high education group. Given that the latter type of households could be expected to be more firmly rooted in the upper echelons of society, this result corroborates the idea that the equalizing effect of the compulsory school reform was most marked in the lower regions of the class distribution.

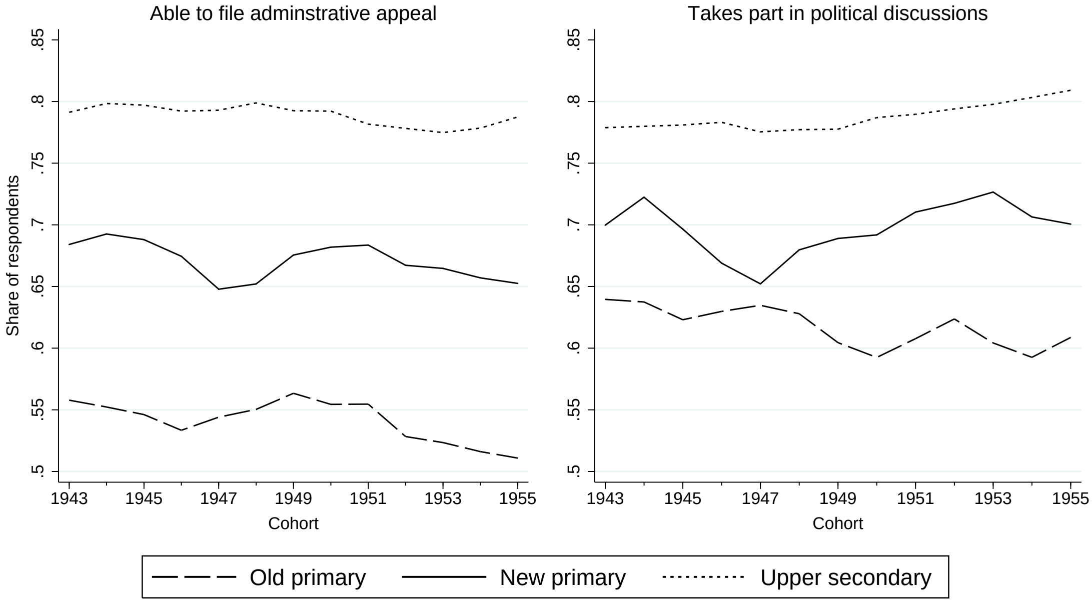
The type of heterogeneity analyses presented above provides a first modest attempt to unpack the causal processes underlying our main findings by studying how the reform affected specific sub-groups. In order to shed additional light on the causal mechanisms at work, we have also performed some more traditional mediation analyses. In particular, we have examined to what extent the effect of the reform was channelled through increased social or geographical mobility. However, as shown by the results presented in the Appendix, changed social or geographic mobility could, at best, help explain a small share of the overall reform effect.

An alternative possibility is that the reform helped reduce political inequalities by promoting political skills and interest in the lower parts of the educational distribution. In *Figure 4*, we present descriptive evidence, based on a large repeated survey, that is consistent with this hypothesis.[22] The leftmost graph shows the share of respondents, in different cohort and educational groups, who say they are able to file an administrative appeal. This question serves as an admittedly imperfect measure of political skills. The rightmost graph instead displays the share of respondents saying that they sometimes engage in political discussions, which should be related to their levels of political interest. For both measures, the average scores for the individuals whose highest education correspond to the new, nine year, primary school are situated between those of the individuals whose highest education is the old primary and the upper secondary school, respectively.

Thus, there are at least some indications that the lengthening of compulsory schooling could have been beneficial for the political capacity and efficacy of low-educated individuals. Yet, even if true, this fact alone does not suffice to explain why the effect of the

[22] The data come from the ULF survey conducted by Statistics Sweden and cover the years 1980-2010. The analysis is based on approximately 26000 observations for which about 18 percent has old primary education, 16 percent new primary education, and 66 percent upper secondary education.

Figure 4: Eduction and political skills and interest



Note: The lines in the figure are calculated as three-year moving averages.

reform is negative for individuals with a non-working class background. However, compared to other types of political participation, such as voting or campaign volunteering, the type of political activity studied here is considerably more competitive in nature. This is important because it has been argued that relative educational attainment may be more important than absolute educational attainment for engagement in competitive political activities (e.g., Nie et al., 1996; Campbell, 2009). In the Appendix, we show that the reform had a considerably bigger effect on the educational attainment of children with a working-class background than it had on those with a non-working class background. According to our preferred specification, the reform served to increase average education by 0.35 years in the former group, and 0.20 years in the latter group. An implication of this is that children with working-class backgrounds experienced an increase in relative educational attainment, vis-a-vis those with non-working class backgrounds, which may help explain why the reform coefficient is differently signed in the two groups. Moreover, if individuals of working-class origin have larger returns to education—perhaps because class background and education are substitutes in the production of political skills—this would further reinforce this equalizing effect.[23]

While our analysis concerning potential causal mechanisms underlying our main findings can be considered suggestive, we strongly acknowledge that it is purely speculative in nature. Whereas the natural experiment utilized in this study is well suited to answer the question of whether educational reforms of the type examined here can help reduce political inequalities, it has obvious limitations when it comes to pinpoint the relevance of different causal mechanisms.

6 Conclusion

A century after the breakthrough of modern democracy, the political elite in most developed countries still continues to be dominated by individuals from privileged social backgrounds. 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 et al. (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."

It is therefore no surprise that there is an abundance of suggestions on how to mitigate the importance of family background in politics. For liberal democratic thinkers, improved educational opportunities for the masses have long been the preferred means to fight class differences in society. Yet critics of this view have maintained that this is a vain hope since the educational system will mainly serve to legitimize and uphold existing social inequalities.

This study has examined the veracity of these positions. 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 percent. Our more de-

[23] Indeed, there are some signs in our data that the returns to education might differ across the two groups.

tailed analysis demonstrates that the equalizing effect of the reform was most marked in the lower regions of the class distribution. Above all, the school reform appears to have strengthened the position of the working class vis-a-vis the middle class.

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 question remaining for future research.

One issue concerns whether our findings can be generalized to countries other than Sweden. Although we see no obvious reason why Sweden should be special in this regard, this question cannot be answered solely on theoretical grounds, but needs to be studied empirically. Given that similar reforms were undertaken in many other European countries in the decades following the Second World War, it should be possible to conduct studies like the present one in those countries as well.

A second unresolved issue relates to the precise mechanisms driving our results. Whereas the design of the Swedish comprehensive school reform provides us with an excellent opportunity to credibly estimate the total effect of the reform on the social stratification of political recruitment, we are not currently in a position to say much about the causal pathway connecting the changes in the educational system and the reduction in stratification. We therefore consider the study of the mechanisms driving the observed relationship to be an important avenue for future research. For instance, by collecting additional large-scale survey data one could attempt to test the hypothesis that the reform helped reduce political inequalities by promoting political skills and interest in the the lower parts of the educational distribution.

The above limitations notwithstanding, we believe that our study has shown that Thomas Jefferson and others have been correct in pointing to improved educational standards for the masses as 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 (Hannum and Buchmann, 2005). 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."

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Appendix A: Details on data and measures

Nominated — Equal to 1 each time the individual ran for office at the national, county or municipal level in the six general elections held 1991, 1994, 1998, 2002, 2006, and 2010. Information is retrieved from the Register of Nominated and Elected Candidates.

Elected — Equal to 1 each time the individual was elected to office at the national, county or municipal level in the six general elections held 1991, 1994, 1998, 2002, 2006, and 2010. Information is retrieved from the Register of Nominated and Elected Candidates.

Board/committee member — Equal to 1 if the individual was a member of one or more board or committee at the municipality or county level. Data are available from 2007 and 2011. Information is retrieved from the Survey of Elected Municipal and County Council Representatives.

Sex — Equal to 1 if male. Information is retrieved from the Swedish Population Register.

Birth year — Information is retrieved from the Swedish Population Register.

Municipality of residence — Municipality of residence in 1960. Information is retrieved from the 1960 census.

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

Years of schooling — Educational attainment according to the three-digit Swedish standard classification of education (SUN 2000). Following the manual for classifying educational programmes in OECD countries (ISCED-97), we assigned the following years of schooling to each category: (old) primary school (7); (new) compulsory school (9); (old) junior secondary education (9.5); high school (10-12 depending on the program); short university (13); longer university (14-16 depending on the program); short post-graduate (17); long post-graduate (19). The information on educational attainment is retrieved from the Longitudinal integration database for health insurance and labour market studies (LISA by Swedish acronym).

Parental education — Equal to 1 if the individual's parents have completed some form of post-mandatory education. Information is retrieved from the 1970 census.

Parental class — As described in the main text, the original class measure differentiates between seven class categories, and is based on occupational codes from the 1960 census. In households where both parents are working, the class position of the household is based on the parent with the most dominant class position using the following dominance order: 1) higher non-manual workers, 2) farmers, 3) self-employed, 4) intermediate manual workers 5) lower manual workers, 6) skilled manual workers, 7) Unskilled non-manual workers. Lower numbers dominate higher ones (Erikson, 1984).

Able to file an administrative appeal — Equal to 1 for all survey respondents who answered yes to the question of whether they would be able to write an administrative appeal. The information is retrieved from the annual Living Conditions Surveys 1980-2010 (ULF by Swedish acronym).

Partake in political discussions — Equal to 1 if the respondent engages in political discussions. This measure is based on the following survey item:

> How do you behave when you are in a group and political questions are discussed?
> (1) I don't listen when people talk politics
> (2) I usually listen, but I never participate in the discussion
> (3) I sometimes express my opinions
> (4) I usually participate in the discussion and voice my opinions

Respondents whose answered either (3) or (4) are said to partake in political discussions. The information is retrieved from the annual Living Conditions Surveys 1980-2010 (ULF by Swedish acronym).

Appendix B: The DD approach

The purpose of this section is to explain our difference-in-difference approach in greater detail. As described in the main text, our estimating equation is obtained by combining the following two equations:

$$y_{icm} = \beta_0 + \beta_1 R_{cm} + \beta_2 P_{icm} + \theta_c + \eta_m + \varepsilon_{icm} \tag{4}$$

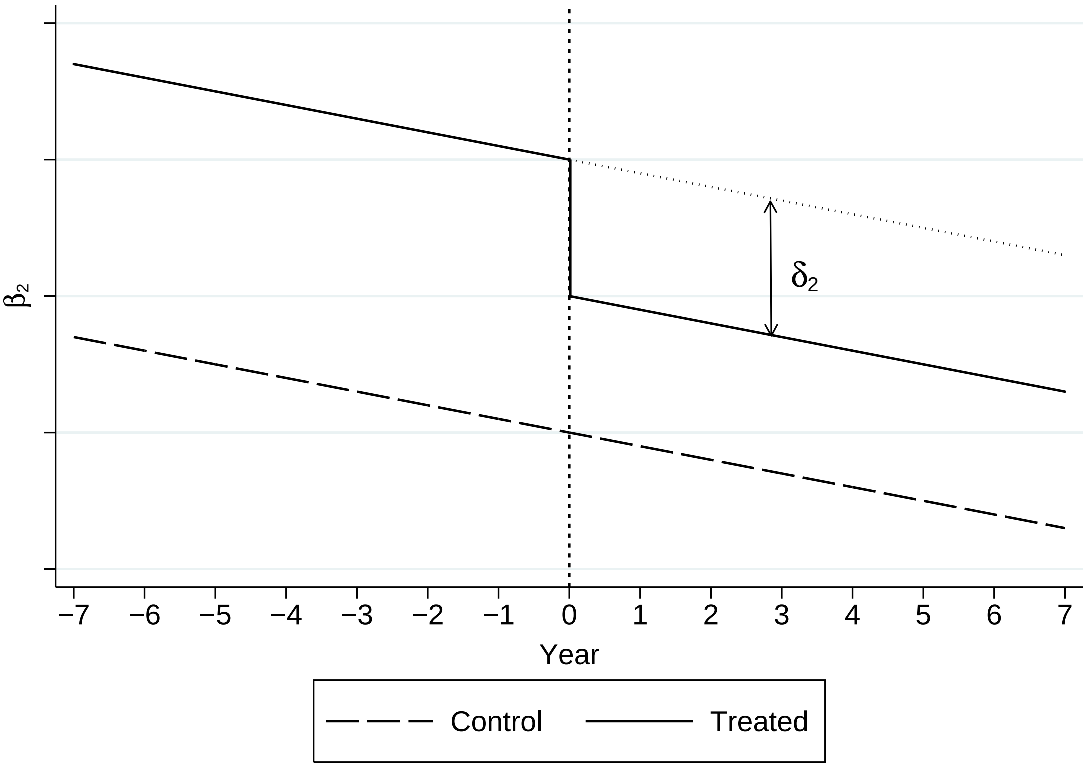
$$\beta_2 = \delta_1 + \delta_2 R_{cm} + \gamma_c + \lambda_m \tag{5}$$

where the parameters of primary interest are δ_1 and δ_2. The former provides a measure of the strength of the relation between parental class and the probability of running for office for individuals not exposed to the reform. δ_2 measures the influence of the school reform on this relationship. The key identifying assumption within this difference-in-difference framework is that of parallel trends: In the absence of the reform, the outcome in focus — 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.

The logic underlying this approach is illustrated in *Figure A1*. Somewhat simplified, the municipalities in our sample can be divided into two groups: i) treated municipalities in which younger cohorts were assigned to the reform at year 0 whereas older cohorts were not (the solid line); ii) control municipalities in which none of the cohorts under consideration were assigned to the reform (the dashed line). In our setup we will use a seven years window and the outcome of interest is the effect of family background on the probability of standing as a candidate (β_2).

The treatment effect (δ_2) is given by the difference between the observed value of β_2 among treated individuals (the solid line in the post treatment period) and what the value of β_2 would have been with parallel trends, had there been no treatment (the dotted line in the post treatment period). The latter is of course an unobserved counterfactual scenario. However, under the assumption of parallel trends, in the absence of the treatment we can employ the difference in difference (DD) approach to obtain an unbiased estimate of the

Figure A1: Illustration of the DD approach



treatment effect.[24] Our DD estimator is then defined as the difference in average outcome between younger and older cohorts in the treated municipalities minus the difference in average outcome between younger and older cohorts in the control municipalities. That is, the estimated treatment effect is literally a difference in differences.

More formally, using the notation from Equation 5 and adding superscripts T (treated municipality) and C (control municipality) and subscripts Y (younger cohorts) and O (older cohorts), the following simple algebraic manipulations show that the DD estimator

[24] In our application, it is not necessary for the trends to be linear as depicted in the figure, they only need to be parallel.

provides an estimate of the effect of interest:

$$
\begin{aligned}
DD &= \left[\beta_{2Y}^{T} - \beta_{2O}^{T}\right] - \left[\beta_{2Y}^{C} - \beta_{2O}^{C}\right] \\
&= \delta_1 + \delta_2 + \gamma_Y + \lambda^{T} - \delta_1 - \gamma_O - \lambda^{T} \\
&- \left[\left(\delta_1 + \gamma_Y + \lambda^{C}\right) - \left(\delta_1 + \gamma_O + \lambda^{C}\right)\right] \\
&= \delta_2
\end{aligned}
$$

Appendix C: Supplementary analyses

Testing the common trend assumption

In this subsection, we describe the two tests of the common trend assumption that we refer to in the main text. Both tests build on the fact that if the school reform is (conditionally) exogenous, it should not have any effect on cohorts that were too old to be affected by the school reform. One way to check this assumption is to perform placebo regressions, of the type conducted by Hjalmarsson et al. (2014), in which individuals that were t years too old to be affected by the reform are defined as treated. To the extent that we find reform effects for the placebo groups that are of similar sign and magnitude as those for the cohorts actually treated by the reform, it would signal that the timing of the reform is correlated with some unobserved factors not captured by the difference-in-difference model. In particular, negative and significant interaction effects between parental socioeconomic background and the placebo treatment would call into doubt our conclusion that the school reform reduced the social bias in political representation. If, instead, the common trend assumption is valid, the placebo treatment of older cohorts should not be systematically related to the outcome.

In line with this logic, we reestimated our preferred model specification also including a placebo reform (R_p) indicator. We coded this indicator as if the reform had affected individuals who were between two and six years older than the individuals actually affected by the reform.[25] The results of this tests are presented in *Table A1*. Reassuringly, we do not find any systematic effects in any of the placebo groups. The estimates of the interaction effects between parental class and the placebo reform indicator tend to be both highly volatile and statistically insignificant (the only two coefficients that are statistically significant are incorrectly signed).

It may be argued that the sequential nature of this test is a weakness. Despite the fact that we do not observe any effects for the pre-reform cohorts when examined individually, it is nevertheless possible that we could find evidence for pre-reform trends if all pre-reform cohorts were analyzed jointly. To check for this possibility, we have extended an

[25] We did not test for any placebo effect among individuals who are just one year older than the reform individuals. The reason, as discussed in the main text, is that a quite large share of each cohort started school a year later and these individuals were in fact exposed to the reform.

Table A1: Placebo reforms

	(1)	(2)	(3)	(4)	(5)
P	1.267***	1.275***	1.278***	1.213***	1.238***
	(0.082)	(0.082)	(0.085)	(0.087)	(0.093)
R	0.319***	0.327***	0.285**	0.265**	0.350***
	(0.116)	(0.114)	(0.117)	(0.126)	(0.127)
P × R	−0.501***	−0.518***	−0.524***	−0.384**	−0.438**
	(0.177)	(0.176)	(0.182)	(0.187)	(0.199)
R_p	0.033	0.130	−0.169*	−0.129	0.039
	(0.117)	(0.106)	(0.100)	(0.092)	(0.103)
P × R_p	−0.099	−0.111	−0.031	0.287*	0.113
	(0.193)	(0.106)	(0.151)	(0.150)	(0.168)
Placebo	t-6	t-5	t-4	t-3	t-2
Mun. FEs	Yes	Yes	Yes	Yes	Yes
Cohort × P FEs	Yes	Yes	Yes	Yes	Yes
Mun. × P FEs	Yes	Yes	Yes	Yes	Yes
Observations	786,790	786,790	786,790	786,790	786,790

Notes: All models include controls for sex, immigrant background, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

approach suggested by Meghir et al. (2011). In order to explain the logic of this test, we can rewrite Equation 5 as follows:

$$\tilde{\beta_2} = \alpha_1 + \alpha_2 T + \sum_{j=1945}^{1955} \gamma_j G_j + \sum_{j=1945}^{1955} \theta_j (G_j \times T) \tag{6}$$

where T indicates a linear time trend that represents the cohorts born 1943-1953 and G is a dummy variable taking on the value one for all municipalities that implemented the reform starting with cohort j. The logic underlying this setup is that it allows us to test whether the effect of time (α_2) on the relationship between family background and the probability of running for office ($\tilde{\beta_2}$) among individuals *not* affected by the reform is conditional on the timing of the reform (θ_j). If we insert this expression into Equation 3 in the main text and estimate the resulting model for all individuals not affected by the reform, the presence of significant interaction effects between T and G would indicate a violation of the common trend assumption. This is due to the fact that such interaction effects imply that the changes in the importance of parental background in the pre-reform period are systematically related to the timing of the reform.

If we restrict the sample to include individuals who were between 2 and 7 years too old to be subject to the reform, as we do in the main text, a joint test of $\theta_{1945} = \theta_{1946} = ... = \theta_{1955} = 0$ results in a borderline statistically significant F-statistic of $F(10, 879)=1.65$ (p =0.09). This test result could be taken to indicate problems with the common trend assumption.[26] However, a closer inspection reveals that this result is entirely driven by the small group of 622 individuals that lived in municipalities that implemented the reform in 1945. There are two reasons why this group is so small (the next smallest group consists of about 7000 individuals). First, in only a handful (11) of the municipalities the first affected cohort was born in 1945. Second, in these municipalities the only individuals that can be used in this analysis are those born in 1943. This is due to the fact that our sample restriction requires both that individuals were at least 2 years too old to be affected by the reform and that they were not born prior to 1943.

One way to mitigate the problem of a low cell count in the 1945 category is to also include individuals born before 1943 in the sample. For instance, if we allow those born in 1942 to be included in the sample, the number of individuals in the 1945 group almost doubles and a joint test of $\theta_{1945} = \theta_{1946} = ... = \theta_{1955} = 0$ yields an F-statistic of $F(10, 879)=0.40$ (p =0.95), meaning we *cannot* reject the null hypothesis of common trends in the pre-treatment period. Alternatively, the 1945 and the 1946 categories could be combined into a common group and the model above reestimated with 10 municipality groups. A joint test of $\theta_{1945/1946} = \theta_{1947} = ... = \theta_{1955} = 0$ yields an F-statistic of $F(9, 879)=0.74$ (p =0.67) that again is highly non-significant.

Based on these test, we therefore believe it is fair to conclude that there are no clear signs of pre-reform trends in the data, which considerably strengthens our argument for the (conditional) exogeneity of the reform. Admittedly, tests such as these can never prove the accuracy of the common trend assumption. However, to the extent that there may be such trends, they appear to be sufficiently subtle and therefore are unlikely to influence

[26]The sample used for this analysis include 420,611 individuals, but because clustered standard errors are used the denominator degrees of freedom is one less than the number of clusters.

our results in a meaningful way.

Additional sensitivity checks

In the main text we argue for the use of linear regression, even though our main dependent variable is dichotomous. Nonetheless, in *Table A2* we replicate the results in Table 2 of the main text using a logit model specification.

Table A2: Results using a logit model

	(1)	(2)	(3)	(4)	(5)	(6)
P	0.305***	0.338***	0.345***	0.347***	0.391***	0.384***
	(0.013)	(0.018)	(0.022)	(0.022)	(0.024)	(0.024)
R	0.021	0.064**	0.073**	0.067**	0.116***	0.117***
	(0.030)	(0.032)	(0.036)	(0.031)	(0.040)	(0.040)
P × R		−0.076***	−0.091***	−0.081***	−0.167***	−0.168***
		(0.026)	(0.035)	(0.027)	(0.051)	(0.051)
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mun. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × P FEs	No	No	Yes	No	Yes	Yes
Mun. × P FEs	No	No	No	Yes	Yes	Yes
Controls × P	No	No	No	No	No	Yes
Observations	784,700	784,700	784,700	784,700	784,700	784,700

Notes: All models include controls for sex, immigrant background, and a second order polynomial for the age of mother and fathers in 1960. Standard errors, shown in parentheses, allow for clustering (879 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

Three key points about this analysis must be highlighted. First, since including the a full set of dummy variables for the birth year of mothers and fathers causes convergence problems due to empty cells, we instead control for parental age through a second order polynomial for the age of fathers and mothers in 1960 (in the linear case these two approaches provide very similar results). Second, the pattern of interaction effects is the same for the logit model as for the linear probability model. The school reform led to a reduction in the strength of the relationship between family background and the likelihood of running for office by between thirty and forty percent. Third, using the logit coefficients to estimate predicted probabilities of running for office, averaged across the sample, yields point estimates that are very similar to the ones reported in Table 2 in the main text. Among individuals attending the old school system, the (average) probability of standing as a candidate is about 1.32 percentage points greater if an individual has non-woking class origin. The corresponding effect among individuals that were subject to the school reform is about 0.76 percentage points.

In addition, for reasons discussed in the main text, we decided to exclude all individuals that were one year too old to be affected by the reform from the sample. However, in *Table A3* we replicate the analyses in in Table 2 of the main text also including the individuals that were one year too old to be affected by the reform.

Table A3: Results including the year before the reform

	(1)	(2)	(3)	(4)	(5)	(6)
P	1.029***	1.167***	1.201***	1.154***	1.176***	1.178***
	(0.054)	(0.064)	(0.075)	(0.035)	(0.064)	(0.064)
R	0.075	0.238***	0.279***	0.223***	0.250**	0.249**
	(0.086)	(0.086)	(0.094)	(0.084)	(0.097)	(0.097)
P × R		−0.328***	−0.410***	−0.299***	−0.352**	−0.354**
		(0.084)	(0.111)	(0.086)	(0.153)	(0.152)
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mun. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × P FEs	No	No	Yes	No	Yes	Yes
Mun. × P FEs	No	No	No	Yes	Yes	Yes
Controls × P	No	No	No	No	No	Yes
Observations	865,551	865,551	865,551	865,551	865,551	865,551

Notes: All models include controls for sex, immigrant background, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

Including the individuals born one year prior to the reform does not affect our substantive findings. There is still clear evidence that the reform helped reduce the political under-representation of individuals of working class origin. However, as is to be expected if there is a also treatment effect for those born just prior to the reform, the magnitude of the reform effect is somewhat reduced when including this group in the analysis. Based on the estimates from our preferred specification, the probability of running for office among individuals from working class homes increased by about .25 percentage points as a result of the reform (the corresponding figure reported in the main text was .32 percentage points). Likewise, the reform-induced reduction in the importance of family background is now 30 rather than 40 percent.

Alternative measures of family background

In *Table A4*, we check how sensitive our results are to changes in the operationalization of family background.

In the first column of the table, we exclude individuals with divorced or separated parents; in the second column we code the class origin of individuals for which we lack information on the occupation of both parents as working class (rather than excluding them from the analysis); and in the third column we exclude children of farmers from the analysis due to the large heterogeneity of this class category (it includes everything from small-holders and self-employed fishermen to owners of large industrial farms). The first two changes have no discernible effect on the results, whereas the exclusion of farmers leads to an increase in the interaction effect between class origin and reform status.

The results presented in column 4 shed additional light on why the equalizing effect of the reform strengthen once children of farmers are excluded. We now distinguish between five different class categories: *non-skilled manual workers* (the reference category), *farmers* (P_f), *self-employed* (P_{se}), *lower non-manual workers* (P_{ln}), and *intermediate- and higher non-manual workers* (P_{hn}). Using such a detailed categorization obviously leads to a substantial loss in statistical precision, however the analysis offers additional nuance to our previous findings. First, we see that heirs of farmers is the group with the highest likelihood of running for office. This can likely be attributed to the presence of the Agrarian Party in Swedish politics. According to our estimates, the difference in the probability of running for office between children of non-skilled manual workers and farmers was as high as 1.7 percentage points before the reform. Although not statistically significant, the negative interaction effect indicates that this difference was reduced by about a quarter as a result of the school reform. This equalizing effect is comparable in magnitude to that found between non-skilled manual workers and intermediate and higher non-manual workers, but it is considerably smaller in magnitude, both in absolute and relative terms, than that observed for the other two class categories. In particular, our results indicate that the pre-reform difference between children of non-skilled manual workers and lower non-manual workers vanishes almost completely as a result of the reform. In the main text, we

Table A4: Alternative measures of family background

	(1)	(2)	(3)	(4)	(5)
P	1.298***	1.271***	1.089***		
	(0.086)	(0.081)	(0.089)		
R	0.384***	0.313**	0.323***	0.324***	0.330**
	(0.118)	(0.114)	(0.115)	(0.115)	(0.156)
P × R	−0.510***	−0.505***	−0.582***		
	(0.185)	(0.174)	(0.186)		
P_f				1.665***	
				(0.168)	
P_{se}				0.751***	
				(0.149)	
P_{ln}				0.999***	
				(0.171)	
P_{hn}				1.514***	
				(0.111)	
$P_f \times R$				−0.384	
				(0.353)	
$P_{se} \times R$				−0.623*	
				(0.322)	
$P_{ln} \times R$				−0.884***	
				(0.357)	
$P_{hn} \times R$				−0.470**	
				(0.230)	
P_{lh}					0.803***
					(0.314)
P_{hl}					1.250***
					(0.286)
P_{hh}					1.879***
					(0.276)
$P_{lh} \times R$					−0.005
					(0.317)
$P_{hl} \times R$					−1.020***
					(0.282)
$P_{hh} \times R$					−0.367
					(0.253)
Observations	725,780	799,012	715,827	786,790	567,151

Notes: All models include controls for sex, immigrant background, and father's and mother's birth years. In addition, all models include municipality and birth year fixed effects, as well as municipality by class and birth year by class fixed effects. Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

argue that these results suggest that the equalizing effect of the compulsory school reform was most marked in the lower regions of the class distribution.

The results presented in the last column of *Table A4* further supports this interpretation. Here, we have used the information on parental education from the 1970 census to code educational status at the household level. More precisely, a household is coded as highly educated if either the father or the mother have completed junior secondary school or higher (which is the case for about a third of the households). In the next step, parental education is cross-tabulated with our dichotomous class measure to differentiate between four types of households: low class—low education (the reference category), low class—high education (P_{lh}), high class—low education (P_{hl}), and high class—high education (P_{hh}).

When interpreting the results in column 5, one should bear in mind that since information on parental education is only available for parents born after 1910, the sample drops by almost thirty percent when including this variable in the specification. Nonetheless, the overall pattern of the results is similar to those reported earlier. We see that the reform increases the probability of running for office for children from working-class homes by the same amount irrespective of parental education (the coefficient is about 0.33 and statistically significant in both groups). For children from non-working class homes, however, the situation looks a bit different. In particular, the negative interaction effect between household type and reform status is much more pronounced for the P_{lh} category than it is for the P_{hh} category. Given that the latter type of households could be expected to be more firmly rooted in the upper echelons of society, this result corresponds well with the results in column 4.

Mediation analysis

The analyses presented in Tables *Table 4* and *Table A4* can be seen as a modest first attempt to unpack the causal processes underlying our previous findings by studying how various sub-groups were affected by the reform. Another, more direct, way to shed light on the causal mechanisms at work is to examine to what extent the observed effect of the reform can be attributed to different mediating factors.

Therefore, in *Table A5* we show how controlling for some potentially mediating variables impact the reform estimate for individuals with working and non-working class background, respectively. Before commenting on the results, however, it must be acknowledged that a more systematic mediation analysis is beyond the scope of this paper for, at least, two different reasons. First, such an analysis would require additional data. Second, as explained in detail by Imai et al. (2011), even if we had access to the necessary data, the type of "single experiment design" utilized by our study is often insufficient to credibly identify the causal mechanisms of interest. The analysis presented below is thus mainly descriptive in nature and should be viewed as a means to generate hypotheses for future research.

For ease of interpretation, we have conducted separate regressions for working- and non-working class individuals. To lessen the risk of reverse causation, all intermediating variables are measured in 1990. The first column of *Table A5* simply replicates our main results for the slightly restricted sample used in this part of the analysis. The point estimates of the reform effect for the two groups correspond closely to those previously reported.

In the second column of the table, we include controls for the socio-economic position of the individuals, measured by their labor earnings and class belonging (see the table note for details). To the extent that the reform effect is due to increased social mobility, we should expect to see a substantial drop in the reform coefficients as we move from the first to the second column. Indeed, the effect of the reform among individuals from working class homes decreases somewhat when controlling for SES. The magnitude of the change is, however, rather modest. Based on the coefficient estimates, only about 8 percent of the reform effect for individuals of working-class origin could be attributed to increased social mobility, whereas among individuals from non-working class homes, controlling for SES hardly affects the reform coefficient at all.

Another possibility is that the effect of the reform is driven by changes in geographic mobility. For instance, because the size of municipal legislatures in Sweden is not directly proportional to population, "the candidates to voters ratio" is inversely related to municipality size. In the Swedish context, place of residence is thus an important factor affecting the likelihood of running for office. Consequently, to the extent that the introduction of

Table A5: Mediation analysis

	(1)	(2)	(3)	(4)
Panel A. Working-Class				
Reform effect	0.324**	0.299**	0.342***	0.160
	(0.128)	(0.128)	(0.127)	(0.127)
Observations	341,925	341,925	341,925	341,925
Panel B. Non-Working Class				
Reform effect	−0.178	−0.178	−0.145	−0.264
	(0.162)	(0.166)	(0.164)	(0.163)
Observations	348,681	348,681	348,681	348,681
Mediating factors				
SES	No	Yes	No	No
Place of residence	No	No	Yes	No
Years of education	No	No	No	Yes

Notes: All models include controls for sex, immigrant background, and father's and mother's birth years. In addition, all models include municipality and birth year fixed effects, as well as municipality by class and birth year by class fixed effects. The SES controls include a second-order polynomial of yearly labor earnings and a categorical indicator distinguishing between seven social classes (the same seven categories that form the basis of our measure of parental class). Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

the compulsory school reform affected the mobility patterns of different class groups, this could potentially help explain our findings. In the third column of *Table A5*, we examine this possibility by including municipality-of-residence fixed effects (for the year 1990). Controlling for place of residence affects the reform coefficient in the two groups differently. It increases the positive effect for individuals from working-class homes somewhat, but decreases the negative effect among non-working class individuals. A potential explanation for these results is that the reform increased the probability of moving to more populous municipalities in both groups, which, all else equal, should have made it less likely for these individuals to become political candidates.

According to these results, changed social or geographic mobility could therefore, at best, help explain a small share of the overall effect of the reform. This failure to account for the mechanisms underlying the effect of the reform is most likely due to the fact that data on many potentially mediating variables is simply lacking in the public registers at our disposal. In particular, conventional wisdom holds that education may help foster both political skills and interest. To the extent this is the case, it is conceivable that educational reforms can be instrumental in reducing political inequalities even if they only have marginal impact on social or economic inequalities.

This reasoning suggest that the most important effect of the compulsory school reform may have been to increase the political knowledge and interest of individuals in the lower parts of the educational distribution (i.e., those who received longer education as a result of the reform). The model reported in the fourth column of *Table A5* provides a blunt, and admittedly highly imperfect, test of this mechanism. In the absence of suitable indicators of political skills or interest in our data, we have simply chosen to control for years of education to see how much of the total reform effect that is channeled through longer schooling. Among individuals with working-class origin, the reform effect is more than halved, and no longer statistically significant, when introducing a linear control for years of schooling in the model. Obviously, this analysis does not prove that the lengthening of compulsory schooling helped reduce the representation gap by increasing the political skills and interest among working-class children, however the result is at least consistent with such an interpretation.

Finally, in our discussion in the main text of potential mechanisms underlying our findings, we make the argument that the reform served to increase the relative level of education for individuals with a working-class background. This assertion rests on the results reported in *Table A6*, which show the effect of the reform on educational attainment conditional on working-class background.

Table A6: The effect of the reform on educational attainment

	(1)	(2)	(3)	(4)	(5)	(6)
P	1.208***	1.312***	1.215***	1.345***	1.278***	1.275***
	(0.038)	(0.046)	(0.029)	(0.013)	(0.012)	(0.012)
R	0.276***	0.389***	0.287***	0.426***	0.353***	0.354***
	(0.019)	(0.018)	(0.050)	(0.018)	(0.021)	(0.021)
P × R		−0.225***	−0.019	−0.299***	−0.150***	−0.151***
		(0.025)	(0.050)	(0.032)	(0.027)	(0.027)
Cohort FEs	Yes	Yes	Yes	Yes	Yes	Yes
Mun. FEs	Yes	Yes	Yes	Yes	Yes	Yes
Cohort × P FEs	No	No	Yes	No	Yes	Yes
Mun. × P FEs	No	No	No	Yes	Yes	Yes
Controls × P	No	No	No	No	No	Yes
Observations	786,790	786,790	786,790	786,790	786,790	786,790

Notes: All models include controls for sex, immigrant background, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering (880 clusters) at the municipality level. ***/**/*, indicates significance at the 1/5/10% level.

Judging from our preferred specification presented in column 5, the implementation of the reform, on average, meant 0.35 extra years of schooling for individuals of working-class origin, and 0.20 extra years of schooling for individuals from non-working class homes. Moreover, the importance of family background for educational attainment was reduced by about 12 percent as a result of the reform.

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