IFN Working Paper No. 1515, 2025

Does Education Foster Civic-Minded Citizens? Evidence from a Compulsory Schooling Reform

Daniel Almén, Mikael Elinder, Per Engström, Oscar Erixson, Erik Lundberg and Mårten Palme

Research Institute of Industrial Economics
P.O. Box 55665
SE-102 15 Stockholm, Sweden
info@ifn.se
www.ifn.se

Does Education Foster Civic-Minded Citizens? Evidence from a Compulsory Schooling Reform

Daniel Almén[1], Mikael Elinder[2], Per Engström[2], Oscar Erixson[1], Erik Lundberg[2], Mårten Palme[3]

[1]Institute for Housing and Urban Research, Uppsala University.
[2]Department of Economics, Uppsala University.
[3]Department of Economics, Stockholm University.

Abstract

We exploit education reforms in Sweden and other European countries to estimate the causal effects of longer and modernized compulsory education on civic engagement. In most countries, compulsory education was extended by 1–2 years, and the curricula were reformed to better foster democratic and socially responsible citizens. We use high-quality, population-wide register data from Sweden and survey data from the other countries on key measures of civic engagement: voting in elections, charitable giving, and blood donations. Our estimates are generally precise and close to zero, allowing us to rule out even comparatively small positive effects. These results suggest that the post-WWII education reforms in Europe under study were unsuccessful in fostering more civic-minded citizens, and the additional years of schooling attributed to these reforms did not contribute to the positive association between educational attainment and civic engagement also observed in the data.

Keywords: Education reforms, Civic engagement, Prosocial behaviour, Political participation, Voting, Charitable giving, Blood donation

JEL Classification: I21 D64 D76 N34

Version: 20 December 2024

1 Introduction

The idea that education fosters civic-mindedness and that a well-educated population is crucial for democracy has a rich historical lineage. It dates back to ancient Greek philosophers and permeates scholarly discourse throughout the ages, enduring to the present day.[1] Since the seminal works by Lipset (1959), Campbell et al. (1960) and Wolfinger and Rosenstone (1980), the literature documenting a positive association between education and civic engagement has grown enormously. A famous quote by Robert Putnam summarises the conventional wisdom: *"Education is one of the most important predictors – usually, in fact, the most important predictor – of many forms of social participation – from voting to associational membership, to chairing a local committee to hosting a dinner party to giving blood"* (Putnam, 2000, p. 186).[2]

The fact that many Western countries have seen rising levels of education and at the same time falling levels of civic engagement does, however, not square with the notion that there is a positive causal relation between educational attainments and civic engagement (see e.g. Acemoglu et al. (2005); Caplan (2018), and the reviews by Hillygus and Nie (2001); Campbell (2006); Persson (2015); Dee (2020); Willeck and Mendelberg (2022)). Instead, the strong association between education and civic engagement may reflect early exposure to social norms or personal characteristics influencing both educational attainment and civic engagement (Willeck and Mendelberg, 2022; Persson, 2015).[3] Reverse causality is also a potential issue since civic-minded young individuals may be more inclined to enrol in longer education.

More recently, studies that rely on experimental, quasi-experimental, or other credible econometric methods to identify causal effects have been published. Early attempts include Milligan et al. (2004) and Dee (2004). Both these studies used various instrumental variables for post-secondary educational attainment to estimate the effects on survey measures of civic engagement or attitudes. The results indicated positive effects on some measures of civic engagement, but contradictory results on voting between the countries. Later studies, also including data from other countries than UK and the US, have found mixed effects (positive or null) on various measures of civic engagement (e.g. Tenn (2007); Persson and Oscarsson (2010); Siedler (2010); Persson (2014); Pelkonen (2012); Kam and Palmer (2008); Lindgren et al. (2019); Marshall (2019)).

A limitation of this literature, as summarised by Willeck and Mendelberg (2022), is that most studies rely on subjective survey measures of civic engagement and suffer from low statistical power due to the limited size of survey datasets. An additional challenge is that educational variation, typically measured in years of schooling, differs in both content and timing (e.g., compulsory schooling versus college attainment). This raises an interesting and highly policy-relevant question: What types of education reforms lead to improved civic engagement in the population?

In this study, we leverage the experimental feature of the Swedish comprehensive schooling reform, rolled out between 1949 and 1962 across municipalities, to study the effect of education and education policy on civic engagement. This reform has, following Meghir and Palme (2005), been used to identify different effects of education on, for example, skills, income, crime, health, intergenerational

[1] Philosophers and scholars like Socrates, Plato, John Locke, Jean-Jacques Rousseau, Emile Durkheim, John Dewey, Jean Piaget, and Lawrence Kohlberg, all emphasised the role of education in fostering virtuous citizens (see e.g. Althof and Berkowitz (2006); Dill (2007)).

[2] The importance of civic engagement, often viewed as a proxy for social capital or trust, has been emphasised in the economics literature as well. It has, for example, been shown to impact economic performance and growth (Knack and Keefer, 1997; La Porta et al., 1997; Zak and Knack, 2001; Guiso et al., 2004; Algan and Cahuc, 2009, 2010), and collective action behaviour as most recently documented with compliance to Covid-19 restrictions (Barrios et al., 2021). In fact, it is argued that social capital might explain parts of the Solow-residual (see Guiso et al. (2011); Algan and Cahuc (2014) for overviews).

[3] See, e.g. Cesarini et al. (2014); Bratsberg et al. (2022); Oskarsson et al. (2018); Dawes et al. (2021) for studies on the influence of genes and family environment, and Elinder and Erixson (2024) for evidence of cognitive ability influencing both education and prosocial behaviour.

spillovers and political candidacy.[4] We focus on two main dimensions of civic-mindedness: voting in elections and generosity. We use data collected from various population-wide registers that include information on the treatment status of 1.2 million individuals, as well as data on civic behaviours from adolescence to retirement. To measure voting in elections, we have assembled data on the turnout of the entire Swedish electorate in the general elections of 1970, 1994, 2010 and 2018, the EU parliament election of 2009 and 2019, and the Swedish referendum to join the European Union in 1994. To measure generosity, we use third-party reported register data on charitable giving and blood donations. In addition, we complement this analysis with evaluations of 12 other post-WWII European reforms extending compulsory schooling, using the Regression Discontinuity approach proposed by Cavaille and Marshall (2019) and similar measures of civic engagement from three large-scale surveys: European Social Survey, European Election Study, and the Eurobarometer.

The Swedish schooling reform provides both exogenous variation in educational attainment and an interesting context for studying the effect of education on civic engagement. The reform increased the number of years of compulsory schooling from seven or eight years to nine years, abolished the tracking of more able students into separate schools, and introduced a common national curriculum. The new curriculum explicitly emphasised democratic values and a change to more progressive teaching methods inspired by the high school system in the US - from a focus on facts and skills to a more academic approach based on critical thinking and own initiatives (Dewey, 1916). Following the fall of authoritarian regimes leading up to WWII, new values, such as objectivity, critical inquiry, solidarity, and an emphasis on learning about democracy, were introduced into civic education. Hence, an explicit aim of the reform was to shape democratic civic-minded citizens. Moreover, the reform pre-dates several similar reforms in Western Europe, e.g. France and the United Kingdom.

The main theoretical argument for why education increases civic engagement is that education improves civic skills, political knowledge, and/or political efficacy, all lowering the cost or increasing the benefits of civic engagement (see reviews by Persson (2015); Willeck and Mendelberg (2022).[5] However, the theoretical link between education and civic engagement is not limited to mere quantity; content, such as civic-oriented curricula, might be equally important. Hence, these arguments are highly relevant in the context of the multi-faceted post-WWII European compulsory schooling reforms, suggesting that there is fair theoretical support for the hypothesis that these reforms would increase civic engagement.

In the analysis of the Swedish comprehensive school reform, we first document a positive associations between years of schooling and all outcomes. However, we find no effects of the reform on any of our measures of civic engagement. In general, the estimated effects are very precise and close to zero. The statistically significant estimates point towards small negative effects. The results are robust to several sensitivity checks, including applying the recently developed estimators for causal inference of effects of policies implemented in a staggered way (Borusyak et al., 2021; Callaway and Sant'Anna, 2021).

Like Sweden, many countries in Western Europe reformed their education systems after WWII to extend the duration of compulsory schooling and modernise the curricula to emphasise democratic values, social responsibility and tolerance towards all members of society (Wagner and Kössler, 2022).

[4] See Lager et al. (2017) on skills, Meghir and Palme (2005); Fischer et al. (2022) on income, Meghir et al. (2012); Hjalmarsson et al. (2015) on crime, Lager and Torssander (2012); Palme and Simeonova (2015); Meghir et al. (2018); Fischer et al. (2021) on health, Holmlund et al. (2011); Lundborg et al. (2014) on intergenerational spillovers and Lindgren et al. (2017) on political candidacy. The latter is more closely related to the present study and finds that the reform increased the likelihood of running for political office for individuals with disadvantaged backgrounds.

[5] Another model suggests that education, increases social status, which in turn increases civic engagement because individuals with high social status are more exposed to networks and norms encouraging civic engagement. A third argument suggests that the positive relationship between education and civic engagement is due to selection and not causality. This model is based on the idea that early socialisation in the home environment or genetic factors influence both education and civic engagement.

The focus on the individual's responsibility extending beyond the family, own ethnic group or class became a central feature of modern curricula. In line with the results of the Swedish reform, we do not find any positive effects of the other European reforms either. While the effects of these reforms are less precisely estimated, the point estimates are generally small because of comparatively small samples.

Like the present paper, a few studies have focused on the effects of post-WWII schooling reforms in Europe, extending compulsory schooling and modernising the curricula. Siedler (2010) estimates the effects of a one-year extension of compulsory education in Germany on various survey measures of civic engagement. He uses the gradual roll-out of the reform as an instrument for educational attainment and generally finds zero effects, with the exception of a negative effect on interest in politics. Pelkonen (2012) estimates the effect of a Norwegian reform, extending compulsory education from seven to nine years, on voting in several elections. The analyses are based on rather small samples and survey data, and he finds no statistically significant effects. Cavaille and Marshall (2019) pools several compulsory schooling reforms across Europe and uses a Regression Discontinuity approach, comparing cohorts just affected by the reforms with adjacent cohorts, to study the effects on anti-immigration attitudes. The results indicate that more education reduced anti-immigration sentiments. Finally, Lindgren et al. (2017) exploit the same Swedish reform as we do and find that it reduced the influence of social background on participation in elections as a candidate for public office.[6]

Our analysis of the Swedish school reform makes three main contributions to the existing literature. First, as opposed to previous studies using education reforms that applied to entire birth cohorts, the identification in our study utilises the staggered implementation of the comprehensive schooling reform in Sweden's, at the time of the reform, more than 1,000 municipalities. Hence, we compare the differences in the outcomes of students (in the same municipality) who were just affected by the reform or just too old to be affected, with the differences (in the same birth cohorts) among students in municipalities that stayed out of the reform or where in the post-reform system in both cohorts. Thus, we evaluate the effects of the reform without imposing any strong functional form assumptions, as has been common in previous studies in this area. Second, this is the first study to use outcome measures based on population-wide register data rather than outcomes measured from survey responses.[7] This allows us to avoid measurement errors such as misreporting and non-response bias, which is of particular concern in research on prosocial behaviour (Paxton and Ressler, 2018). Hence, the precision and reliability of this study are much improved compared to the previous ones. Third, while most previous studies focus on voting, we extend the analysis to two other core measures of civic engagement: charitable giving and blood donations. In addition to these contributions, our cross-country analysis is the first to jointly evaluate the numerous post-WWII European comprehensive schooling reforms on a similar set of indicators of civic engagement.

More broadly, our paper is also related to two other strands of literature. First, the small literature on how social preferences and prosocial behaviour are shaped (e.g. Schroeder and Graziano (2014), Van Dijk (2014), Cappelen et al. (2020), Kosse et al. (2020), Elinder and Erixson (2024)). Second, because voting and blood donation are argued to be excellent measures of social capital (Guiso et al., 2011), our article also relates to the literature on the sources of social capital, and in particular to

[6]The literature on education and social- or political outcomes has also grown in other directions, and estimated e.g. effects of preschool education (Sondheimer and Green, 2010), curricula changes (Cantoni et al., 2017), paired with certain teaching styles (Pasek et al., 2008; Gill et al., 2020), cooperative teaching styles alone (Algan et al., 2013), class composition (Rao, 2019), and used twin comparisons (Dinesen et al., 2016; Weinschenk et al., 2021).

[7]To the best of our knowledge, Lindgren et al. (2017, 2019) are the only other studies that use credible identification strategies and register-based measures of civic engagement. The former uses the same reform as we do, but political candidacy as an outcome, while the latter uses a reform of post-compulsory education and studies its effect on turnout in the 2010 election.

the role of education (see, e.g. Helliwell and Putnam (2007); Algan et al. (2013); Algan and Cahuc (2014); Yang (2019); Österman (2021)).[8]

Taken together, our precisely estimated effects allow us to rule out meaningful positive effects on a broad set of indicators of civic-mindedness. Our study thus suggests that the modernisation of the school systems in post-WWII Europe did not play a significant role in shaping civic-minded citizens.

The remainder of the paper is structured as follows. In the next section, we describe the Swedish education reform in the context of post-WWII compulsory schooling reforms across Europe. We discuss the empirical strategy in Section 3 and describe the data and how we measure civic-mindedness with register data in Section 4. Sections 5 and 6 present the results of the analysis of the Swedish school reform and the analysis of the European reforms, respectively. Finally, Section 7 concludes.

2 Compulsory schooling reforms, civic engagement, and the Swedish experiment

With the horrors of WWII in fresh memory, most democratic countries in Europe became self-critical regarding their education systems. It was widely believed that citizens needed to be better equipped to uphold democratic and humanistic values (Leschinsky and Mayer, 1999). In the 1950s, 60s, and early 70s, many European countries reformed their education systems by extending compulsory education, abolishing tracking, and reforming the curricula (Cavaille and Marshall, 2019). In particular, the education reforms were considered key instruments to make citizens more democratic and civic-minded (Wagner and Kössler, 2022). Like other European countries in the post-WWII era, the Swedish government also sought to raise the population's education level and modernize the curricula to better foster civic-minded citizens.

2.1 The Swedish compulsory schooling experiment

In 1948, the committee released its proposals, recommending that Sweden introduce a new type of compulsory school (*grundskola*). The Parliament followed the proposal and implemented the new compulsory school system, first as an experiment and then made it mandatory. This comprehensive school reform consisted of three main components:

Extension of compulsory education: The reform extended mandatory education to nine years, adding one or two years for all students.

A new curriculum: A new common nationwide curriculum was introduced, previously formulated and decided locally. The new curriculum stressed the importance of virtuous, free-thinking, democratic, and civic-minded citizens. In line with this, new values such as objectivity, critical inquiry, and solidarity were emphasized, with solidarity extending beyond the family and the national borders. A new subject, Social Science, was introduced in which the ambitions to foster civic-minded citizens were concentrated. Previously, good citizenship was taught with an emphasis on Christian ethics, patriotism, virtue, and duty in subjects like Christianity, History, and in citizenship education directed to those not pursuing secondary education.[9] However, policymakers increasingly questioned whether conformity to Christianity and nationalism was still the basis for a strong democratic society. Alongside content changes, pedagogy also shifted toward progressive teaching inspired by Dewey (1916), focusing on active learning and cooperation to prepare students for active and democratic citizenship.

[8] Similar outcomes have previously been used to proxy trust in works by e.g. Putnam (1993, 2000); Nannicini et al. (2013). However, aside from earlier work by one of the present authors (Almén, 2020), population-wide individual-level register data has not been used before.

[9] For example, the first edition (1923) of an influential civic education textbook ended with the exhortation: "Freedom obliges!". This final remark remained nearly unchanged until the fifteenth and final edition in 1958 (Larsson, 2001).

Abolishment of tracking: The reform eliminated tracking, increasing classroom diversity regarding socioeconomic background and ability. This aimed to enhance societal cohesion along class lines and improve equality of opportunity.

There is both theoretical and empirical support for each component of the reform increasing civic engagement.[10] Hence, a priori, there are good reasons to believe the reform had effects consistent with policymakers' intentions.

2.1.1 Implementation

The implementation of the reform began in the 1949/50 school year, with a subsequent gradual roll-out across municipalities; typically, it was first introduced in the fifth grade. In the first school year, 14 municipalities implemented the new nine-year compulsory school, and the number of municipalities adopting the reform steadily increased during the trial period. In 1962, the Swedish parliament finally decided to make the reform universal, requiring all municipalities to adopt the new system by 1969.

Figure 1 illustrates the implementation of the reform, showing the share of individuals (and municipalities) treated by the reform in different birth cohorts. The proportion of treated individuals (and municipalities) increased steadily over cohorts. For the 1943 birth cohort, around 7% (6%) were treated, while for the last cohort in our sample, about 85% (74%) were treated. In total, we assign treatment status to approximately 1.26 million individuals, born between 1943 and 1954. This allows us to exploit most of the variation generated by the reform.[11]

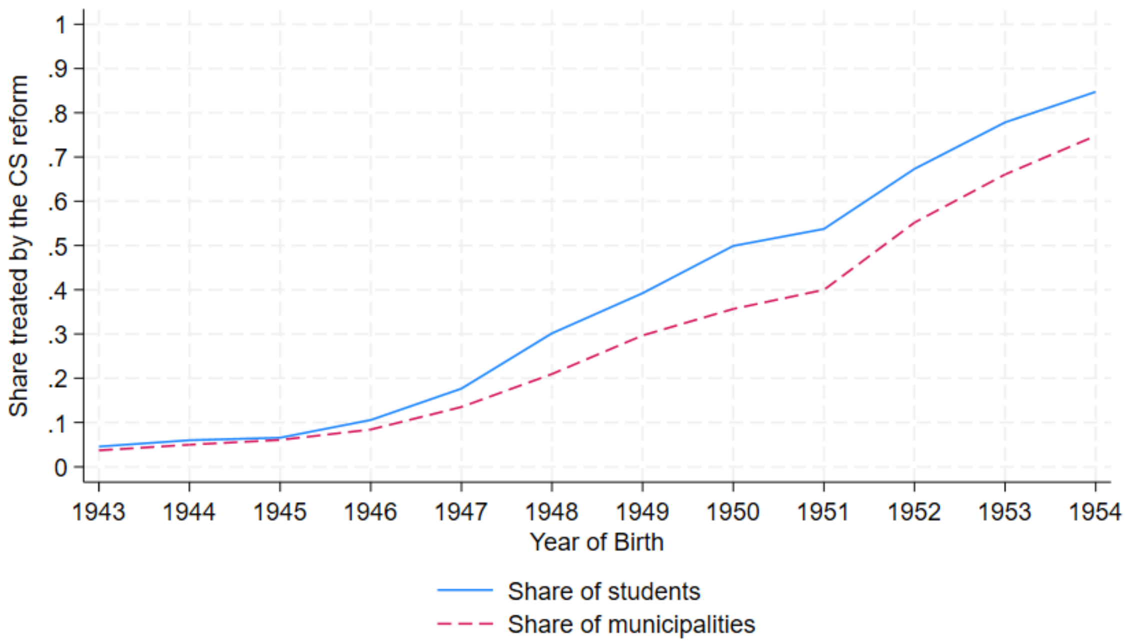

Figure 1: Share of individuals and municipalities that are treated by the reform by birth year
Note: This figure shows the share of individuals and municipalities in our main sample that are treated by the reform by each cohort.

[10] While Persson (2015); Willeck and Mendelberg (2022) discuss the quantity- and content of education in terms of curricula, they do not discuss the potential role of class composition. Theoretical support for class composition effects in the present context is found in the contact- and the common ingroup identity theories (Allport, 1954; Gaertner and Dovidio, 2000). Empirical support can be found in other contexts (see review by Pettigrew et al. (2011) and Paluck et al. (2019)), but also in education (Rao, 2019). See e.g. Cantoni et al. (2017) for evidence on the effects of curricula changes, and Algan et al. (2013) for effects of cooperative teaching styles.

[11] Please refer to Section 4.1 for information about the study population and how we assign treatment status.

The Swedish comprehensive school reform has been extensively used in the empirical literature on the causal effects of education. Conditional on municipality and birth year fixed effects, whether a child was assigned to the new or old system was essentially random. Meghir and Palme (2005) provided the first quasi-experimental evaluation, finding that the new system increased schooling by an average of 0.3 years and raised labour earnings around age 40 by 1.4%. Students with fathers who had low education (LSES) increased their schooling by 0.4 years, while no change in educational attainment was observed for students with fathers who had high education (HSES). Consistent with the positive effects on schooling, LSES individuals also experienced higher earnings later in life.

3 Empirical strategy

Since the Swedish schooling reform not only affected years of schooling but also influenced curriculum and classroom composition, we estimate the reduced form effect of the reform on our outcomes using a differences-in-differences (DiD) approach. The model is specified as follows:

$$Y_{icm} = \beta_0 + \beta_1 Reform_{cm} + \eta_c + \mu_m + \epsilon_{icm} \tag{1}$$

where Y_{icm} is the outcome for individual i, in birth cohort c, and municipality m. $Reform_{cm}$ is a dummy equal to one for individuals who were treated by this reform. Birth cohort- and municipality fixed effects are represented by η_c and μ_m, respectively. The error term, ϵ_{icm}, is clustered at the municipality level.

For β_1 to capture the causal effect of the reform, the standard DiD identifying assumptions apply, i.e. that individuals in early and late-treated municipalities would share a common trend in the absence of treatment.

Because the reform has been exploited and validated extensively in previous studies, we refrain from detailed checks of the validity of these assumptions in the main text. Nevertheless, in Table A.1 we show that the conditional exogeneity assumption of reform status seems valid; the reform status cannot be predicted by pre-determined household characteristics. Moreover, in Table A.2 we show that the effects of the reform on years of schooling and educational attainment align with previous literature using this reform. Finally, in contrast to previous studies using the reform, in Table A.3 we show that our Two-Way Fixed Effect estimates using Equation (1) are robust to the estimators addressing heterogeneous dynamic treatment effects. Appendices A.1, A.2, and A.3 provide a more extensive discussion of the tests.

4 Data

Table 1: Descriptive statistics

	Obs. (in 1,000s)	Mean	St. dev
Panel A: Data set 1			
Reform variable:			
Reform Status[a] (%)	1,255	35.3	47.8
Individual Characteristics:			
High SES[a] (%)	807	34.0	47.4
Female[a] (%)	1,255	48.7	50.0
Birth Year[a]	1,255	1948	3.4
Outcomes:			
Years of Schooling[a]	1,197	11.4	2.8
YoS (Fischer et al. 2022)	1,151	11.2	2.8
Charitable Giving: Any (%)	1,102	32.7	46.9
Ch. Giving: Foreign Aid (%)	1,102	16.5	37.1
Ch. Giving: Religion (%)	1,102	2.0	13.9
Ch. Giving: Social Work (%)	1,102	3.2	17.6
Ch. Giving: Research (%)	1,102	21.6	41.2
Panel B: Data set 2			
Reform variable:			
Reform Status[a] (%)	1,257	35.3	47.8
Individual Characteristics:			
High SES[a] (%)	814	34.0	47.4
Female[a] (%)	1,257	48.7	50.0
Birth Year[a]	1,257	1948	3.4
Outcomes:			
Years of Schooling[a]	1,199	11.4	2.8
Share Voted Elections (%)	1,222	80.3	25.2
Voted, 1970 (%)	639	83.9	36.8
Voted, 1994 (%)	1,068	90.9	28.7
Voted, 2010 (%)	1,083	91.4	28.1
Voted, 2018 (%)	1,038	92.4	26.6
Voted EU, 2009 (%)	1,086	53.3	49.9
Voted EU, 2019 (%)	1,020	68.9	46.3
Voted EU ref., 1994 (%)	1,065	89.9	30.2
Blood Donor, age 47-62 (%)	1,216	8.4	27.7
Blood Donor, age 57-62 (%)	1,179	4.7	21.2

Notes: [a]Variable is available in both data sets

Several dimensions of civic engagement can be identified at the individual level, using population-wide and third-party reported measures of civic engagement. It allows us to provide a broad picture of civic-mindedness at different stages of life, from adolescence to retirement. Below, we describe our data sources, the study population, and the treatment status assignment.

4.1 Study population and assignment of treatment status

Our study population consists of all individuals born in Sweden between 1943 and 1954. We assign treatment status to 1.26 million individuals, of whom approximately one-third have experienced the new compulsory school. We identify individuals affected by the reform by using information on the municipality of residence from the 1960 Census, combined with historical records to identify the first treated birth cohort in each municipality.[12] Hence, we exclude individuals we do not observe in that Census. We also exclude municipalities (and some parishes in Stockholm, Gothenburg and Malmö) where the implementation year is unclear, according to Holmlund (2020). This sample will serve as our main sample.[13] Socio-economic status is defined following the classification proposed by Meghir

[12]In 1960, these individuals were between 6 and 17 years old. Treatment assignment assumes individuals were in the grade appropriate for their age. We are grateful to Helena Holmlund for sharing her implementation code.

[13]See Appendix B.1 for more details about the sample selection.

and Palme (2005), wherein individuals are categorised as either low or high socio-economic status (LSES or HSES) based on whether the father completed only primary education or attained education beyond primary level by 1970.[14] About one-third of the individuals in our sample have an HSES background. The rationale behind this stratification is that the reform has a more pronounced effect in terms of schooling years and educational attainment among individuals with an LSES background (see Table A.2 in Appendix A.2).

Table 1 presents summary statistics of all variables used in the main analyses.[15] The data has been retrieved from various population-wide registers held by Statistics Sweden (SCB). The outcome variables are available in two distinct data sets, which we refer to as Data Sets 1 and 2. Data Set 1 contains information on charitable giving, while Data Set 2 contains information on voting and blood donations. However, both data sets contain the information needed to identify reform status and relevant demographic and socio-economic variables. Since both datasets stem from population-wide registers, they are initially identical, but since they are created with respect to the measurement of the outcomes, they differ slightly regarding the number of observations.[16]

4.2 Voting

We measure voting with individual-level turnout data from seven elections spanning from 1970 to 2019, covering four general elections, one referendum, and two elections to the European Parliament. The data stem from election rolls digitized by researchers at the Department of Government at Uppsala University and Statistics Sweden (SCB) (see Appendix B.2 for further details). The extended coverage period and different types of elections enable us to capture several dimensions of voting behaviour.

The general elections in 1970, 1994, 2010, and 2018 consist of three separate elections: the municipality council, the county council, and the national parliament, all held on the same day and each vote is cast at the same polling station. For the 1970 elections, Swedish citizens who turned 19 the year before the election were eligible to vote. After 1975, however, Swedish citizens aged 18 or older, on election day, were eligible to vote. Table 1 shows that the turnout in the general elections is high, ranging from about 84% (1970 election) to 92% (2018 election). The advisory referendum on Sweden's EU membership in 1994 reached a turnout of 89.9%. The turnouts in the 2009 and 2019 EU Parliament elections are considerably lower than in the other elections, with rates of 53% and 69%.

For each election, we create a binary variable that obtains the value one if the individual has voted (given eligibility) in that election. For the general elections, the variable obtains the value of one if the individual has voted in at least one of the three elections, i.e. showed up at the polling station and voted. Our main outcome of interest is the share of these seven elections in which an individual has voted.

4.3 Generosity

The data on blood donation are collected from the 30 regional organizational bodies that manage blood donations, which are part of the 21 regional councils responsible for public health care in Sweden. In contrast to many other countries, where NGOs and other private initiatives handle blood donations, blood donation in Sweden is managed solely by these public health care centres, ensuring universal data and no attrition. Donations can be made nationwide at numerous local donor

[14] Approximately 70% of our sample can be accurately assigned socio-economic status using this criterion.

[15] Descriptive statistics with respect to socio-economic status groups are reported in Table C.1.

[16] Individuals who have died or migrated before the initial measurement of a particular outcome and year have been excluded from the analysis of that particular outcome. Also, regarding voting outcomes, only eligible voters are considered; hence, for the 1970 election, only individuals born in 1950 or earlier are included.

centres or on mobile units, like buses. The visit lasts about 30–45 minutes, and there is no monetary compensation for the donation, but the donor is sometimes given a small gift.

The blood donation data covers the years 1990–2016 and contains yearly information on whether an individual has donated blood at least once. Unfortunately, data is lacking for Skåne County before the year 2000. Hence, the data covers the universe of Swedish blood donors between 2000 and 2016 (see Appendix B.3 for further details on the data collection). Our preferred measure, which ensures balance across counties and cohorts, is a binary indicator of whether an individual has donated blood at least once between the ages 57–62.[17] Table 1 shows that about 5% of the sample have donated blood at least once between the ages 57–62.

Data on charitable donations are collected from the Swedish Tax Agency's Income and Tax Register, focusing on individual-level contributions to major charities in Sweden between 2012 and 2015. These data are available due to tax reductions granted for donations to approved tax-exempt foundations or nonprofit organizations engaged in charitable activities or scientific research, as sanctioned by the Tax Agency.

To qualify for the tax reduction, donors needed to make one or more gifts, each totalling at least SEK 200 (approximately USD 20), to a specific organization annually, with a minimum total annual amount of SEK 2,000. The tax reduction amounted to 25% of the gift value, capped at a maximum of SEK 1,500 per year, corresponding to a total annual gift of SEK 6,000. The tax register includes information on all gifts of at least SEK 200, including those not resulting in a tax reduction and those exceeding the maximum allowable amount. Elinder and Erixson (2024) report that the gifts in the register make up almost 50% of the total amount donated to charities in Sweden.

Importantly, donors provide their personal identity numbers, and the charity reports the gift to the tax agency, facilitating automatic calculation of the tax reduction without active donor intervention. This process contrasts with systems like that in the United States, minimizing the potential influence of cognitive ability on charitable giving. Our main variable, Charitable giving, is an indicator, equaling one if the individual made a gift of at least SEK 200 during the years 2012–2015, and zero otherwise. Approximately 33% of the individuals in our study population contributed at least once to charities during this period (see Table 1).

In addition, we have information about what types of charities individuals donated to. To investigate whether the reform had any heterogeneous effects on which type of organization the treated individuals chose to give to, we created four mutually exclusive groups of charity organizations (see Appendix B.4). Charities focusing on foreign aid are categorized as *Foreign Aid*, charities with strong association with a religious group are categorized as *Religion*, charities focusing on helping families and other entities within the country are categorized as *Social Work*, and charities which gather money to finance research are categorized as *Research*.

[17] As a robustness check, we also consider donation between the ages 47–62, where we include the entire period 1990–2016.

5 Results

In this section, we present the empirical results of the effects of the Swedish compulsory schooling reform on the various register-based outcomes related to civic engagement. To put the results in context, we also present the associations between years of schooling and civic engagement. In Subsection 5.1, we present the results for voting, measured as voting in the national and European elections between 1970 and 2019. Subsection 5.2 presents the results for generosity, measured as donating blood and giving to charity. Finally, in Subsection 5.3, we compare our estimate on voting with previous studies' effect sizes.

Table 2: Association between years of schooling and civic engagement

	(1) Share Voted Elections	(2) Blood Donor, 57-62	(3) Charitable Giving
Panel A: Full Sample			
YoS	2.47***	0.20***	3.52***
	(0.016)	(0.009)	(0.029)
Obs.	1,191,086	1,154,596	1,098,809
Mean (%)	80.9	4.8	32.8
Panel B: Low SES			
YoS	2.46***	0.27***	3.26***
	(0.022)	(0.013)	(0.030)
Obs.	521,194	505,225	482,462
Mean (%)	79.2	4.9	30.1
Panel C: High SES			
YoS	2.34***	0.10***	3.72***
	(0.021)	(0.015)	(0.053)
Obs.	264,851	257,397	246,624
Mean (%)	84.7	5.1	37.6
Municipality FE	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes

Notes: Standard errors in parentheses. Clustered standard errors at the municipality level. * p<0.10, ** p<0.05, *** p<0.01. Coefficients and standard errors are scaled to represent percentage points. Each column in each separate panel presents separate regressions using Equation (1), except that we replace the reform dummy with years of schooling (using the conventional way of assigning years of schooling, see A.2 for a discussion of this measure). In Panel A, the estimates are obtained using the full sample while Panel B and C show the estimates for individuals from low and high socioeconomic backgrounds, respectively.

Table 3: Effects of the compulsory school reform on civic engagement

	(1) Share Voted Elections	(2) Blood Donor, 57-62	(3) Charitable Giving
Panel A: Full Sample			
School Reform	0.070	-0.120*	-0.535**
	(0.083)	(0.073)	(0.228)
Obs.	1,221,523	1,179,492	1,101,599
Mean (%)	80.3	4.7	32.7
Panel B: Low SES			
School Reform	-0.051	-0.181	-0.169
	(0.125)	(0.113)	(0.254)
Obs.	527,484	510,363	483,255
Mean (%)	78.9	4.9	30.1
Panel C: High SES			
School Reform	0.208	0.247*	-0.873***
	(0.156)	(0.146)	(0.331)
Obs.	271,246	264,330	247,379
Mean (%)	84.0	5.0	37.5
Municipality FE	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes

Notes: Standard errors in parentheses. Clustered standard errors at the municipality level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Coefficients and standard errors are scaled to represent percentage points. Each column in each separate panel presents separate regressions using Equation (1). In Panel A, the estimates are obtained using the full sample while Panel B and C show the estimates for individuals from low and high socio-economic backgrounds, respectively.

5.1 Voting

Before presenting our causal estimates, we examine the gradient i.e. the association between years of schooling and voting. Employing Equation (1) while substituting the reform dummy with years of schooling, we observe in column 1 in Table 2 a positive association between years of schooling and the share of elections voted. The association holds independently of an individual's socio-economic background. For each additional year of schooling, the probability of voting increases by around 2.5 percentage points.

Looking at the elections separately, we see in columns 1-7 of Table C.2 that the positive association between years of schooling and voting exist for all elections. Notably, the gradient appears to be more pronounced in elections to the EU Parliament than in national elections and the EU referendum. The higher point estimate, together with the lower turnout rate in the elections to the EU parliament compared to the national elections, suggest that voters in the EU parliament are, on average, from a higher segment of the socio-economic distribution compared to the average eligible voter.

Table 3, column 1, presents the causal effect of the reform on the share of elections voted for, in the full sample and by SES background. The table also provides the number of observations and the outcome means. The estimated effect for the full sample is close to zero and statistically insignificant. We can rule out an effect size larger (in absolute terms) of +/- 0.25 percentage points at the 5% significance level. Comparing the effect of the scaled reduced form estimate with the gradient, we can rule out a causal effect larger than 1/3 of the gradient.[18]

When analysing the results by socio-economic background, we observe small differences. If anything, the reform had a small negative effect on individuals with LSES background and a positive effect on individuals with HSES background. However, both point estimates are statistically insignificant.

The results for the separate elections are presented in columns 1-7 of Table C.3 in Appendix C. None of the estimates are statistically significant, and the point estimates are generally precisely estimated and close to zero. However, the estimates for the 1970 general election and elections to the European Parliament are less precise. The former is because of a smaller sample size and the latter because of lower turnout and, thus, higher variance.[19] We conclude that the reform had a negligible role in inducing voter turnout.

5.2 Generosity

As above, before presenting causal estimates, we examine gradients found in columns 2 and 3 of Table 2. As anticipated, a positive association exists between years of schooling and blood and charity donations independently of socio-economic background. For each additional year of schooling, the probability of donating blood and giving to charity increases by 0.2 and 3.5 percentage points, respectively. The association is, thus, more pronounced for charitable giving, both in absolute and relative terms.

Columns 2 and 3 in Table 3 provide the causal estimate for the respective outcome using Equation (1), including the number of observations and outcome means. Our findings indicate that the compulsory schooling reform had a negative impact on individuals' generosity. However, it is important to note that the estimated effects are relatively small.

[18] The upper limit of the scaled reduced form at the 5% level of significance is $\frac{\hat{\beta}+1.96\cdot\hat{\sigma}}{\hat{\beta}_{\mathrm{YoS}}} = \frac{0.070+1.96\cdot 0.083}{0.28} = 0.831$, while the lower limit of the gradient is $(2.47 - 1.96 \cdot 0.016 =)2.439$.

[19] Cohorts later than 1950 are too young to be eligible to vote in the 1970 general election and are thus not included in the regressions.

Specifically, for blood donations, the estimated effect is a decrease of around 0.1 percentage points (approximately a 2.4% decline) and is statistically significant only at the 10% level. Similarly, for charitable giving, the estimated effect is a decrease of 0.5 percentage points (approximately a 1.6% decline) and is significantly different from zero at the 5% level.

When analysing the results by socio-economic background, we observe that the negative effect on blood donation is primarily observed among individuals from a low socio-economic background (LSES). In contrast, the point estimate for individuals from a high socio-economic background (HSES) is positive. Conversely, for charitable giving, the negative effect appears to be driven by individuals from an HSES background, while the estimate is not significantly different from zero for individuals from an LSES background.

To assess whether the reform shaped the preferences for different causes of charitable giving, we look at whether there is any heterogeneity in giving between different types of charity organisations. Columns 9 to 12 in Table C.3 provide the estimates for each group of charity organisations. All estimates are negative, and the estimates are significant at conventional levels for foreign aid and social work. When analysing these effects by socio-economic background, the same pattern as for overall giving appears for all outcomes except for religious giving, i.e., HSES individuals appear to drive the negative effects. In the same table, column 8, we also assess blood donation between the ages 47–62. The point estimate is still negative and hence does not change our conclusion of a non-positive effect of the reform on blood donation. [20]

To conclude, our analyses indicate that the compulsory schooling reform did not lead to an increased inclination among individuals to support distant others, which contradicts the intended goal of the reform. If anything, our findings suggest that the reform had a negative (but small) impact on individuals' generosity.[21]

5.3 Comparing the effects on voting with previous studies

So far, we have found that the reform had negligible effects on civic engagement. To contextualize these findings further, we compare our estimates on voting with those of previous studies. We focus on voting since it is the most common form of civic engagement studied in the literature, and it is relatively straightforward to compare studies.

We begin by scaling our causal estimate so that it represents the effect of one more year of schooling.[22] The scaled estimates are presented in Figure 2 (black dots) with corresponding 95% confidence intervals (black lines).

The figure shows that the point estimate is exceptionally close to zero, and we can rule out effect sizes outside the range of -1 and 1 percentage point per extra year of schooling.

For comparison, we also present the estimate of the gradient reported in Table 2 column 1. The estimate is obtained using Equation (1), where the independent variable is years of schooling. This comparison is not perfect, as the reduced form effect captures more than a longer education (such as curriculum changes and changes in class composition). Rather, this comparison aims to get a better understanding of the relative effect size of the causal estimate and how it relates to the gradient size, which serves as our prior for the causal effect (Abadie, 2020). The gradient, along with a 95% confidence interval, is presented in red together with our causal effects in the same figure. Focusing on the gradient (the red dot along almost non-visible 95% confidence intervals), we see that the

[20]The gradient for all these outcomes are showed in Table C.2 columns 8 to 12

[21]We also explore the effects of the reform on the probability to give each year (2012-2015), the sum of giving (using hyperbolic sine transformation) and the probability of giving at least one large gift (at least SEK 6,000). These alternative specifications provide similar qualitative results as the main outcome, and results are available upon request.

[22]See Panel A Table A.2 for the reform effect on years of schooling.

estimate is around 3 percentage points from one more year of schooling, i.e. both economically and statistically higher than the causal estimate.

We now turn to how our estimate compares with previous studies on voting. To address this, we focus on previous papers that employ credible empirical designs and examine the effect of schooling on voting behaviour. To ensure comparability with our estimates, we adjust each point estimate and standard error to reflect the effect of one more year of schooling. The papers included in this comparison are Milligan et al. (2004), Dee (2004), Siedler (2010), Berinsky and Lenz (2011), Pelkonen (2012), Persson (2014), and Doyle and Skinner (2017). The results, as shown in Figure 2, indicate that our voting estimate is smaller and more precise than most findings in the previous literature. A notable exception, however, is the work by Siedler (2010), who utilized a large-scale German survey (ForsaBus) and the phased introduction of a German schooling reform to estimate the impact of additional years of schooling on voting behaviour and other civic engagement metrics. Despite relying on self-reported data on voting, Siedler (2010) found no statistically significant effect from the additional schooling induced by the reform. His point estimate is almost exactly zero, and the 95% confidence interval is even slightly narrower than ours.

In conclusion, our precise estimate based on register data from the Swedish compulsory schooling reform suggests that even very small positive effects on voting can be ruled out. This finding starkly contrasts with much of the earlier literature that relies on survey-based measures of civic engagement.

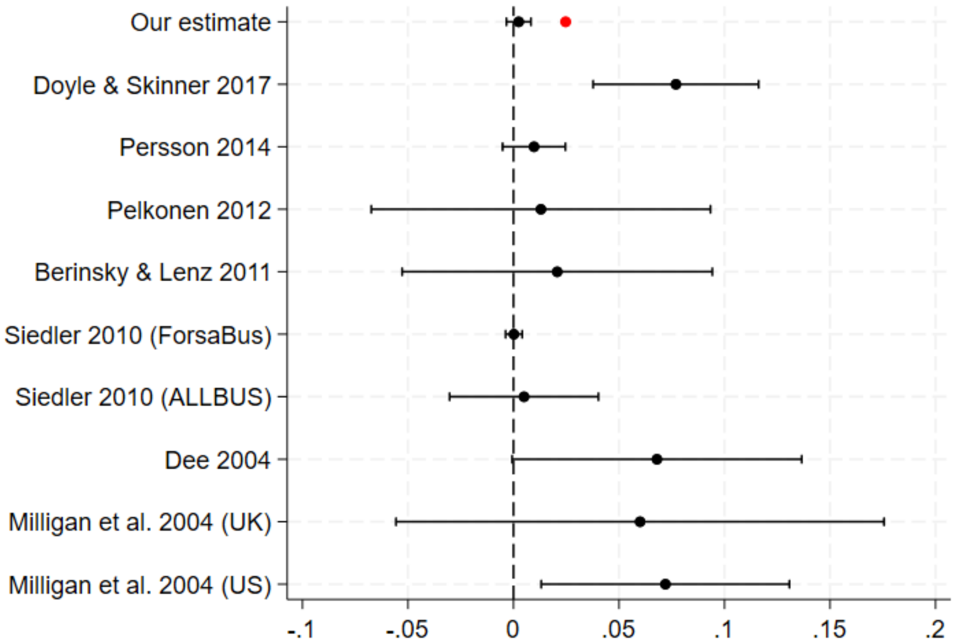

Figure 2: Comparison between our estimate on voting with previous papers
Notes: The estimate from each paper is obtained by scaling the point estimate and standard error to represent the effect of one year of extra schooling on the probability of voting. Please refer to Appendix A.4 for more details.

6 External validity: Evidence from other European reforms

In the previous section, we demonstrated that the compulsory schooling reform in Sweden had a negligible impact on several register-based measures of civic-mindedness. The question remains, however, whether this result is specific to the Swedish context or if similar compulsory schooling reforms elsewhere also failed to induce civic-mindedness.

As noted previously, many other countries have also reformed their school systems during the second half of the 20th century to increase compulsory schooling, with one or more years, and modernise the curricula. The European post-war era faced the challenge of moulding individuals into democratic citizens. The schooling system in general, and the post-war compulsory schooling reforms in particular, were considered key instruments in this process (Wagner and Kössler, 2022). We will, therefore, investigate whether these reforms have had effects similar to the Swedish reform.

To explore this, we follow a growing literature that studies the effects of compulsory schooling reforms in post-war Europe on various outcomes through pooled analyses (Brunello et al., 2009; Cavaille and Marshall, 2019; Angrist et al., 2024). In particular, we adopt the approach of Cavaille and Marshall (2019), who analyse 14 such reforms across 13 European countries between 1947 and 1975 to estimate the impact of education on anti-immigration attitudes.

Their analysis is based on pooled survey data from the European Social Survey (ESS) for the reformed countries and a regression discontinuity approach that compares cohorts just young enough to be affected by the reforms to cohorts just too old to have been affected. This strategy is suitable for estimating the average causal effect of the reforms among the first treated cohorts, under the assumption that there is no sorting cohort eligibility threshold and no confounding contemporary reforms. The authors provide compelling evidence in the form of balancing and continuity tests, suggesting that the identifying assumptions hold. While the initial sample comprised 14 reforms, Cavaille and Marshall (2019) limited their analysis to the six reforms that had a statistically significant positive impact on educational attainment. The specific countries are Denmark, France, Great Britain (two reforms), the Netherlands, and Sweden. Their results show that raising secondary school with at least an additional year decreases anti-immigration attitudes later in life (e.g., opposing immigration, believing that immigration erodes a country's quality of life, and feeling close to far-right anti-immigration parties).

6.1 Empirical model

We follow the empirical strategy in Cavaille and Marshall (2019) to investigate the impact of the reforms on survey-based measures of voting, donations of money and blood. We exclude Sweden from the main analysis to avoid confounding the comparison. However, in contrast to Cavaille and Marshall (2019), we analyze all reforms, not only those that had a statistically significant positive effect on educational attainment. The motivation is that all reforms had curriculum changes that may impact the outcomes of interest. We use the reforms in Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Netherlands, and Great Britain.[23] We refer to all reforms as the "full sample" and the reforms with a significant impact on years of schooling as the "restricted sample" (i.e. Denmark, France, Great Britain, and the Netherlands).

[23] Cavaille and Marshall (2019) also included Portugal and Spain in their full sample. However, we drop those reforms since Portugal and Spain were not democratic when the reforms were implemented.

6.2 Data

We combine individual-level data from four cross-sectional surveys that contain relevant outcomes that match the register-based ones analysed in Section 5.[24]

From the ESS, we obtain data on voting in national elections for seven waves for the relevant countries.[25] The voting question is "Did you vote in the last national election" with the possible answers "Yes", "No", and "Not eligible to vote". We construct an indicator variable that takes value one if the individual answered yes and zero if no. Not eligible individuals (2.2%) are omitted from the analysis.

From the European Election Study (EES), a cross-national survey of a representative sample of the electorate in EU member states, we obtain data on turnout in the European Parliamentary elections in 2004 and 2009 (see e.g. Schmitt et al. (2009), Egmond et al. (2017)).[26] The question regarding voting reads similarly in both surveys: "A lot of people abstained in the European Parliament elections of June X, while others voted. Did you cast your vote" with possible answers Yes/No/do not know. We create an indicator variable taking the value one if the individual gives an affirmative answer, and zero if no.[27]

Data on charitable giving are obtained from the first wave of the ESS in 2002 (ESS ERIC, 2002).[28] The question in focus asks the respondent about any donations made to any organization within a large set of different organizations (e.g. humanitarian, religious, or science organizations) during the last 12 months.[29] We create an indicator variable taking the value one if the individual has indicated donation to any of the groups and zero otherwise. While this measure matches the register-based counterpart in that it is restricted to a certain time, it covers a larger set of organizations (e.g. sports clubs and political parties) and does not specify donations exceeding a certain amount.

Similarly, information on blood donors is obtained from the 2014 wave of the Eurobarometer, which focused particularly on peoples' opinions about and experiences of donations of blood, cells and tissue (Eurobarometer, 2015).[30] The question capturing blood donations is "During the lifetime of a person it is possible to donate different body substances (blood or cells) to help other people. Could you please indicate which ones you have or would be prepared to donate yourself". We focus mainly on blood (and leave out plasma, bone marrow, umbilical cord blood after birth, sperm, eggs) and treat the answers "Yes, you have donated in the past and you would be prepared to donate in the future" and "Yes, you have donated in the past but you would not be prepared to donate in the future" as indications of the individual being a blood donor.[31] In contrast to the register-based measure used in the main analysis, which considers any donations during the ages 57–62, the survey measure considers any blood donation during the respondent's lifetime.

---

[24] The surveys also contain information that enables us to calculate school starting age and years of completed schooling, which is required for the implementation of the RD estimator.

[25] ESS is a cross-national bi-annual survey conducted since 2002 in more than thirty European countries to measure political attitudes, social trust, family dynamics, education, and well-being (ESS ERIC, 2010). The survey was conducted through face-to-face interviews with representative samples of individuals aged 15 and above in the participating countries.

[26] Both surveys were conducted immediately following the elections that took place in early June in the respective years. The 2004 election took place between June 10 and 13 and the 2009 election took place between June 4 and 7. While the 2004 survey covered 25 of the 27 member states, the 2009 covered all. The sampling is done in a two-step way, with an initial selection of households by random digit dial and then a selection of respondents by last-birthday method. The data are available at: https://search.gesis.org/research_data/ZA4566, and https://search.gesis.org/research_data/ZA5055

[27] Individuals answering that they do not know are excluded.

[28] The dataset, which is titled ESS1 (ESS ERIC, 2023) has been downloaded from http://www.europeansocialsurvey.org/downloadwizard/ and merged to the analysis dataset in Cavaille and Marshall (2019), which does not contain the donation information.

[29] The groups cover sports (sptcdm), social (sclcdm) or cultural clubs (cltodm), trade unions (trudm), business (prfodm) or consumer (cnsodm) organizations, humanitarian (hmnodm), or environmental and peace (epaodm) organizations, religious (relgodm) or political (Dntmny, prtydm) organizations, science or education (setodm) organizations, as well as other form of voluntary organizations (othvdm).

[30] The survey was carried out in the 28 EU member states and about 1,000 respondents from different social and demographic groups in each country were interviewed in their homes in their mother tongue (Eurobarometer, 2015). The data has been obtained from https://search.gesis.org/research_data/ZA5931.

[31] The other two possible answers are: "No, you have not donated in the past but you would be prepared to donate in the future" and "No, you have not donated in the past and you would not be prepared to donate in the future".

To improve the precision of the estimates, we use pooled data from all the countries and available survey waves. Since we have data on the survey outcomes for Sweden, we can directly compare Sweden to the other countries. Reassuringly, the sample means for Sweden are on par with those for the other countries, suggesting that any difference in results is unlikely to stem from different incidences of the outcomes.

6.3 Results

Figure 4 shows the results of this validation analysis. Turning first to the results that compare voting outcomes between pre- and post-reform cohorts, we find that the affected cohorts are, on average, not more likely to have cast votes in the national elections later in life. The point estimate in the full reform sample is small, 1.2 percentage points, and statistically insignificant at conventional levels. The point estimate for the restricted sample is more imprecisely estimated but of the same order of magnitude.

Regarding voting in the European Parliamentary elections, we find that the point estimates for both samples are negative, small, and statistically insignificant.

Turning to charitable giving, we find that the point estimates are positive but imprecise. The estimate of the school reform on blood donation is positive and statistically significant at the 10% level of significance for the full sample. Taken at face value, it implies that affected individuals are 7 percentage points, or 17% relative to the sample mean, more likely to have donated blood. However, the estimate for the restricted sample is negative and statistically insignificant.

Thus, the above analysis suggests that the Swedish reform is not unique in that it did not increase civic-mindedness, but rather that similar reforms in a large set of other countries yield similar results. Yet, it should be noted that the analysis is based on self-reported outcomes, which thus may be susceptible to response biases. This may be particularly severe for questions regarding civic engagement if people desire to appear socially responsible to the interviewer. See DeBell et al. (2018) for a recent discussion about the over-reporting of voter turnout in surveys.

Table 4: Effects of European school reforms on voting and generosity

	Voting		Voting EU		Charity		Blood	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
RD Estimate	1.23	1.20	-1.67	-6.62	3.44	4.17	7.38**	-1.55
	(0.76)	(1.02)	(2.36)	(4.18)	(2.44)	(4.19)	(3.67)	(7.17)
Obs.	100,437	55,134	18,913	6,338	18,485	7,706	9,490	2,728
Mean (%)	83.63	81.20	72.01	71.02	34.87	41.57	41.78	44.87
Survey	ESS	ESS	EES	EES	ESS	ESS	Eurobaro.	Eurobaro.
Austria	X		X		X		X	
Belgium	X		X		X		X	
Denmark	X	X	X	X	X	X	X	X
Finland	X				X			
France	X	X	X	X	X	X	X	X
Germany	X		X		X		X	
Great Britain	X	X	X	X	X	X	X	X
Greece	X		X		X		X	
Ireland	X		X		X		X	
Italy	X		X		X		X	
Netherlands	X	X	X	X	X	X	X	X

Notes: Clustered standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Coefficients and standard errors are scaled to represent percentage points. Each column presents separate regressions using Model 2 in Cavaille and Marshall (2019).

7 Concluding remarks

The comprehensive school reform in Sweden resembles many similar schooling reforms in other European countries in the post-World War II era. These reforms, typically, included extensions of compulsory schooling, abolishing of tracking and introduction of a more academic curriculum to make entry to secondary and higher education more accessible for a larger share of the population. An important goal of the new and longer compulsory education was also to foster democratic and civic-minded citizens.

Despite the increase in years of schooling at the lower end of the education distribution, our results show that we are unable to reject a zero effect on voting, blood donations and charitable giving. In fact, the comparatively high precision of our estimates allows us to rule out even small positive effects for all outcomes. When we follow the approach of Cavaille and Marshall (2019) and analyze 14 compulsory schooling reforms in other European countries in the post-WWII period, we again find no evidence of increased civic-mindedness.

Both sets of results contradict a causal interpretation of the strong associations found between years of schooling and all measures of civic engagement used in the empirical analysis of this paper. In addition to the obvious explanation that measurement errors in the independent variables may lead to attenuation bias, there are at least two conceivable explanations to these seemingly contradictory results.

First, it may be that the observables that determine educational choice are also decisive for individual civic engagement and that additional education obtained by the reform leaves this engagement largely unchanged. A second possibility is that other elements of the comprehensive schooling reforms than the increase in years of schooling counteracted the effect on civic engagement, leaving the overall level in the population unchanged.

The policy debate preceding the Swedish reform stressed the importance of individual independence, democracy, and solidarity, as opposed to the pre-reform curriculum, which stressed duty, patriotism, and ethics based on Christianity. One possibility is that teaching based on the latter set of virtues promoted civic engagement more than those reflected in the new curriculum. If longer education increases civic engagement and the changes in the curriculum decrease these outcomes, it would rationalize the zero effects we estimate in this study.

An implication of the second possibility discussed above is that we would expect negative effects of the reform for individuals with a high SES background, since they only increased their years of schooling slightly from the reform, but were still exposed to the new curriculum (and classroom composition). This fits the pattern that we see for charitable giving (significant negative effect for individuals with high SES fathers). However, we find no negative reform effects for this group on the other two measures of civic engagement (voting and blood donations), which does not support the second interpretation of the zero results.

Margaryan et al. (2021), Siedler (2010) and Cantoni et al. (2017) show that education reforms may change self-reported attitudes and values. Our results confirm the general conclusion from these papers that such changes do not necessarily translate into changed behaviours. It is, therefore, important to continue to empirically assess the broader implications of how education reforms may foster attitudes and civic engagement of future citizens.

Acknowledgements. We thank Helena Holmlund, Esbjörn Larsson, Lisa Laun, Therese Nilsson, Martin Nybom, Lars Trägårdh, Björn Öckert, and participants at the 2022 IIPF conference, the 2024 Swedish Conference in Economics, the 2023 MPSA conference, and participants at various seminars at Uppsala University for valuable comments and suggestions.

Funding

Financial support from Jan Wallander och Tom Hedelius Stiftelse and Tore Browahlds stiftelse (P23-0225: Engström, P19-0138: Lundberg, W20-0029: Almén), and Stiftelsen Riksbankens Jubileumsfond (P19-0448: Elinder, Engström, Erixson) is gratefully acknowledged.

Conflict of interests

The authors have no competing interests to declare.

References

Abadie, A. 2020. Statistical nonsignificance in empirical economics. *American Economic Review: Insights 2*(2): 193–208. https://doi.org/10.1257/aeri.20190252 .

Acemoglu, D., S. Johnson, J.A. Robinson, and P. Yared. 2005. From education to democracy? *American Economic Review 95*(2): 44–49. https://doi.org/10.1257/000282805774669916 .

Algan, Y. and P. Cahuc. 2009. Civic virtue and labor market institutions. *American Economic Journal: Macroeconomics 1*(1): 111–45. https://doi.org/10.1257/mac.1.1.111 .

Algan, Y. and P. Cahuc. 2010. Inherited trust and growth. *American Economic Review 100*(5): 2060–92. https://doi.org/10.1257/aer.100.5.2060 .

Algan, Y. and P. Cahuc. 2014. Trust, growth and well-being: New evidence and policy implications, In *Handbook of Economic Growth*, eds. Aghion, P. and S. Durlauf, Chapter 2. Oxford: Elsevier.

Algan, Y., P. Cahuc, and A. Shleifer. 2013. Teaching practices and social capital. *American Economic Journal: Applied Economics* 5: 189–210 .

Allport, G.W. 1954. *The nature of prejudice*. Cambridge: Addison-Wesley Publishing Company.

Almén, D. 2020, 5. *Societal Impacts of Modern Conscription: Human Capital, Social Capital and Criminal Behaviour*. Ph. D. thesis, Stockholm University, Stockholm.

Althof, W. and M.W. Berkowitz. 2006. Moral education and character education: Their relationship and roles in citizenship education. *Journal of moral education 35*(4): 495–518 .

Angrist, N., K. Winseck, H.A. Patrinos, and J.G. Zivin. 2024. Human capital and climate change. *Review of Economics and Statistics*: 1–28 .

Barrios, J.M., E. Benmelech, Y.V. Hochberg, P. Sapienza, and L. Zingales. 2021. Civic capital and social distancing during the Covid-19 pandemic. *Journal of Public Economics* 193: 104310. https://doi.org/https://doi.org/10.1016/j.jpubeco.2020.104310 .

Berinsky, A.J. and G.S. Lenz. 2011. Education and political participation: Exploring the causal link. *Political Behavior* 33: 357–373. https://doi.org/10.1007/s11109-010-9134-9 .

Borusyak, K., X. Jaravel, and J. Spiess. 2021. Revisiting event study designs: Robust and efficient estimation. *arXiv preprint arXiv:2108.12419* .

Bratsberg, B., C.T. Dawes, A. Kotsadam, K.O. Lindgren, R. Öhrvall, S. Oskarsson, and O. Raaum. 2022. Birth order and voter turnout. *British Journal of Political Science 52*(1): 475–482. https://doi.org/10.1017/S0007123419000826 .

Brunello, G., M. Fort, and G. Weber. 2009. Changes in compulsory schooling, education and the distribution of wages in europe. *The Economic Journal 119*(536): 516–539 .

Callaway, B. and P.H. Sant'Anna. 2021. Difference-in-differences with multiple time periods. *Journal of Econometrics 225*(2): 200–230 .

Campbell, A., P.E. Converse, W.E. Miller, and D.E. Stokes. 1960. *The American Voter*. Wiley.

Campbell, D.E. 2006. What is education's impact on civic and social engagement. In *Measuring the effects of education on health and civic engagement: Proceedings of the Copenhagen symposium*, pp. 25–126.

Cantoni, D., Y. Chen, D.Y. Yang, N. Yuchtman, and Y.J. Zhang. 2017. Curriculum and ideology. *Journal of Political Economy 125*(2): 338–392 .

Caplan, B. 2018. *The case against education: Why the education system is a waste of time and money.* Princeton University Press.

Cappelen, A., J. List, A. Samek, and B. Tungodden. 2020. The effect of early-childhood education on social preferences. *Journal of Political Economy 128*(7): 2739–2758 .

Cavaille, C. and J. Marshall. 2019. Education and anti-immigration attitudes: Evidence from compulsory schooling reforms across Western Europe. *American Political Science Review 113*(1): 254–263 .

Cesarini, D., M. Johannesson, and S. Oskarsson. 2014. Pre-birth factors, post-birth factors, and voting: Evidence from Swedish adoption data. *American Political Science Review 108*(1): 71–87. https://doi.org/10.1017/S0003055413000592 .

Dawes, C.T., A. Okbay, S. Oskarsson, and A. Rustichini. 2021. A polygenic score for educational attainment partially predicts voter turnout. *Proceedings of the National Academy of Sciences 118*(50): e2022715118. https://doi.org/10.1073/pnas.2022715118. https://www.pnas.org/doi/pdf/10.1073/pnas.2022715118 .

DeBell, M., J. Krosnick, K. Gera, D. Yeager, and M.P. McDonald. 2018. The turnout gap in surveys: Explanations and solutions. *Sociological Methods Research* 20: 1–30 .

Dee, T.S. 2004. Are there civic returns to education? *Journal of Public Economics 88*(9): 1697 – 1720. https://doi.org/https://doi.org/10.1016/j.jpubeco.2003.11.002 .

Dee, T.S. 2020. Education and civic engagement, In *The Economics of Education* (Second Edition ed.)., eds. Bradley, S. and C. Green, Chapter 8, 103–108. Academic Press. https://doi.org/https://doi.org/10.1016/B978-0-12-815391-8.00008-2.

Dewey, J. 1916. *Democracy and Education: An Introduction to the Philosophy of Education.* New York: Macmillan.

Dill, J.S. 2007. Durkheim and Dewey and the challenge of contemporary moral education. *Journal of moral education 36*(2): 221–237 .

Dinesen, P.T., C.T. Dawes, M. Johannesson, R. Klemmensen, P. Magnusson, Asbjørn, S. Nørgaard, I. Petersen, and S. Oskarsson. 2016. Estimating the impact of education on political participation: Evidence from monozygotic twins in the United States, Denmark and Sweden. *Political Behavior* 38: 579–601. https://doi.org/10.1007/s11109-015-9328 .

Doyle, W.R. and B.T. Skinner. 2017. Does postsecondary education result in civic benefits? *The Journal of Higher Education 88*(6): 863–893 .

Egmond, M.v., W.v.d. Brug, S. Hobolt, M. Franklin, and E.V. Sapir 2017. European parliament election study 2009, voter study. Technical report, GESIS Data Archive, Cologne.

Elinder, M. and O. Erixson. 2024. An inquiry into the relationship between intelligence and prosocial behavior: Evidence from Swedish population registers. *The Economic Journal*: ueae105. https://doi.org/https://doi.org/10.1093/ej/ueae105 .

ESS ERIC 2002. Ess1 data documentation. Technical report, European Social Survey European Research Infrastructure (ESS ERIC), Sikt - Norwegian Agency for Shared Services in Education and Research.

ESS ERIC 2010. Ess5 data documentation. Technical report, European Social Survey European Research Infrastructure (ESS ERIC), Sikt - Norwegian Agency for Shared Services in Education and Research.

ESS ERIC 2023. Ess1 - integrated file, edition 6.7 [data set]. Technical report, European Social Survey European Research Infrastructure (ESS ERIC), Sikt - Norwegian Agency for Shared Services in Education and Research.

Eurobarometer 2015. Special eurobarometer 426 "blood and cell and tissue donation". Technical report, European Comission.

Fischer, M., U.G. Gerdtham, G. Heckley, M. Karlsson, G. Kjellsson, and T. Nilsson. 2021. Education and health: Long-run effects of peers, tracking and years. *Economic Policy* 36: 3–49 .

Fischer, M., G. Heckley, M. Karlsson, and T. Nilsson. 2022. Revisiting Sweden's comprehensive school reform: Effects on education and earnings. *Journal of Applied Econometrics 37*(4): 811–819. https://doi.org/https://doi.org/10.1002/jae.2881 .

Gaertner, S.L. and J.F. Dovidio. 2000. *Reducing Intergroup Bias: The Common Ingroup Identity Model*. Philadelphia: Psychology Press.

Gill, B., E.R. Whitesell, S.P. Corcoran, C. Tilley, M. Finucane, and L.I. Potamites. 2020. Can charter schools boost civic participation? The impact of democracy prep public schools on voting behavior. *American Political Science Review* 114: 1386–1392. https://doi.org/10.1017/S000305542000057X .

Guiso, L., P. Sapienza, and L. Zingales. 2004. The role of social capital in financial development. *American Economic Review 94*(3): 526 – 556 .

Guiso, L., P. Sapienza, and L. Zingales. 2011. Civic capital as the missing link, In *Handbook of Social Economics*, ed. Jess Benhabib, Alberto Bisin, M.J., Chapter 10. Amsterdam: North Holland.

Helliwell, J.F. and R.D. Putnam. 2007. Education and social capital. *Eastern Economic Journal 33*(1): 1–19 .

Hillygus, D.S. and N. Nie. 2001. Education and democratic citizenship. *Making good citizens: Education and civil society*: 30–57 .

Hjalmarsson, R., H. Holmlund, and M.J. Lindquist. 2015. The effect of education on criminal convictions and incarceration: Causal evidence from micro-data. *The Economic Journal 125*(587): 1290–1326 .

Holmlund, H. 2020. A researcher's guide to the Swedish compulsory school reform. *Journal of the Finnish Economic Association* 1/2020: 25–50 .

Holmlund, H., M. Lindahl, and E. Plug. 2011. The causal effect of parents' schooling on children's schooling: A comparison of estimation methods. *Journal of Economic Literature 49*(3): 615–651 .

Kam, C.D. and C.L. Palmer. 2008. Reconsidering the effects of education on political participation. *Journal of Politics* 70: 612–631. https://doi.org/10.1017/S0022381608080651 .

Knack, S. and P. Keefer. 1997. Does social capital have an economic payoff? A cross-country investigation. *The Quarterly Journal of Economics 112*(4): 1251–1288. https://doi.org/10.1162/003355300555475. http://oup.prod.sis.lan/qje/article-pdf/112/4/1251/5393975/112-4-1251.pdf .

Kosse, F., T. Deckers, P. Pinger, H. Schildberg-Hö, A. Falk, S. Alan, Z. Bašić, M. Bauer, A. Becker, T. Boneva, and B. Born. 2020. The formation of prosociality: Causal evidence on the role of social environment. *Journal of Political Economy* 128 .

La Porta, R., F. Lopez-de Silanes, A. Shleifer, and R.W. Vishny. 1997. Trust in large organizations. *The American Economic Review 87*(2): 333–338 .

Lager, A., D. Seblova, D. Falkstedt, and M. Lövdén. 2017. Cognitive and emotional outcomes after prolonged education: A quasi-experiment on 320 182 Swedish boys. *International Journal of Epidemiology 46*(1): 303–311 .

Lager, A.C.J. and J. Torssander. 2012. Causal effect of education on mortality in a quasi-experiment on 1.2 million Swedes. *Proceedings of the National Academy of Sciences 109*(22): 8461–8466 .

Larsson, U. 2001. Bilden av sverige: Värner rydéns bok om medborgarkunskap, In *Vad är Sverige? Röster om svensk nationell identitet*, ed. Johansson, A.W., 136–156. Prisma.

Leschinsky, A. and K.U. Mayer. 1999. *The comprehensive school experiment revisited: Evidence from Western Europe*. Lang.

Lindgren, K.O., S. Oskarsson, and C.T. Dawes. 2017. Can political inequalities be educated away? Evidence from a large-scale reform. *American Journal of Political Science 61*(1): 222–236 .

Lindgren, K.O., S. Oskarsson, and M. Persson. 2019. Enhancing electoral equality: Can education compensate for family background differences in voting participation? *American Political Science Review 113*(1): 108–122 .

Lipset, S.M. 1959. Some social requisites of democracy: Economic development and political legitimacy. *American Political Science Review 53*(1): 69–105 .

Lundborg, P., A. Nilsson, and D.O. Rooth. 2014. Parental education and offspring outcomes: Evidence from the Swedish compulsory school reform. *American Economic Journal: Applied Economics 6*(1): 253–278 .

Margaryan, S., A. Paul, and T. Siedler. 2021. Does education affect attitudes towards immigration? Evidence from Germany. *Journal of Human Resources 56*(2): 446–479 .

Marshall, J. 2019. The anti-democrat diploma: How high school education decreases support for the democratic party. *American Journal of Political Science 63*(1): 67–83 .

Meghir, C. and M. Palme. 2005. Educational reform, ability, and family background. *American Economic Review 95*(1): 414–424. https://doi.org/10.1257/0002828053828671 .

Meghir, C., M. Palme, and M. Schnabel. 2012. The effect of education policy on crime: An intergenerational perspective. *National Bureau of Economic Research*. https://doi.org/https://EconPapers.repec.org/RePEc:nbr:nberwo:18145 .

Meghir, C., M. Palme, and E. Simeonova. 2018. Education and mortality: Evidence from a social experiment. *American Economic Journal: Applied Economics 10*(2): 234–56. https://doi.org/10.1257/app.20150365 .

Milligan, K., E. Moretti, and P. Oreopoulos. 2004. Does education improve citizenship? Evidence from the United States and the United Kingdom. *Journal of Public Economics 88*(9): 1667 – 1695. https://doi.org/https://doi.org/10.1016/j.jpubeco.2003.10.005 .

Nannicini, T., A. Stella, G. Tabellini, and U. Troiano. 2013. Social capital and political accountability. *American Economic Journal: Economic Policy 5*(2): 222–50. https://doi.org/10.1257/pol.5.2.222 .

Oskarsson, S., C.T. Dawes, and K.O. Lindgren. 2018. It runs in the family. *Political Behavior 40*(4): 883–908. https://doi.org/https://doi.org/10.1007/s11109-017-9429-1 .

Palme, M. and E. Simeonova. 2015. Does women's education affect breast cancer risk and survival? Evidence from a population based social experiment in education. *Journal of health economics* 42: 115–124 .

Paluck, E.L., S.A. Green, and D.P. Green. 2019. The contact hypothesis re-evaluated. *Behavioural Public Policy 3*(2): 129–158 .

Pasek, J., L. Feldman, D. Romer, and K.H. Jamieson. 2008. Schools as incubators of democratic participation: Building long-term political efficacy with civic education. *Applied Developmental Science* 12: 26–37. https://doi.org/10.1080/10888690801910526 .

Paxton, P. and R.W. Ressler. 2018. Trust and participation in associations, In *The Oxford Handbook of Social and Political Trust*, ed. Uslaner, E.M., 149–172. Oxford University Press. https://doi.org/10.1093/oxfordhb/9780190274801.013.6.

Pelkonen, P. 2012. Length of compulsory education and voter turnout-evidence from a staged reform. *Public Choice* 150: 51–75. https://doi.org/10.1007/s11127-010-9689-3 .

Persson, M. 2014. Testing the relationship between education and political participation using the 1970 British Cohort Study. *Political Behavior* 36: 877–897. https://doi.org/10.1007/s11109-013-9254-0 .

Persson, M. 2015. Education and political participation. *British Journal of Political Science 45*(3): 689–703 .

Persson, M. and H. Oscarsson. 2010. Did the egalitarian reforms of the Swedish educational system equalise levels of democratic citizenship? *Scandinavian Political Studies* 33: 135–163. https://doi.

org/10.1111/j.1467-9477.2009.00244.x .

Pettigrew, T.F., L.R. Tropp, U. Wagner, and O. Christ. 2011. Recent advances in intergroup contact theory. *International Journal of Intercultural Relations 35*(3): 271–280. https://doi.org/https://doi.org/10.1016/j.ijintrel.2011.03.001 .

Putnam, R.D. 1993. *Making Democracy Work: Civic Traditions in Modern Italy.* Princeton: Princeton University Press.

Putnam, R.D. 2000. *Bowling Alone: The Collapse and Revival of American Community.* New York: Simon Schuster Paperbacks.

Rao, G. 2019. Familiarity does not breed contempt: Generosity, discrimination, and diversity in Delhi schools. *American Economic Review* 109: 774–809. https://doi.org/10.1257/aer.20180044 .

Schmitt, H., S. Bartolini, W.v.d.E. Brug, F. Cees van der, F. Mark, Dieter, G. Toka, M. Marsh, and J. Thomassen 2009. European election study 2004 (2nd edition). Technical report, GESIS Data Archive, Cologne.

Schroeder, D.A. and W.G. Graziano. 2014. The field of prosocial behavior. Oxford University Press. https://doi.org/10.1093/oxfordhb/9780195399813.013.32.

Siedler, T. 2010. Schooling and citizenship in a young democracy: Evidence from postwar Germany. *The Scandinavian Journal of Economics* 112: 315–338. https://doi.org/10.1111/j.1467-9442.2010.01604.x .

Sondheimer, R.M. and D.P. Green. 2010. Using experiments to estimate the effects of education on voter turnout. *American Journal of Political Science* 54: 174–189. https://doi.org/10.1111/j.1540-5907.2009.00425.x .

Tenn, S. 2007. The effect of education on voter turnout. *Political Analysis* 15: 446–464. https://doi.org/10.1093/pan/mpm012 .

Van Dijk, E. 2014. The Economics of Prosocial Behavior, In *The Oxford Handbook of Prosocial Behavior*, eds. Schroeder, D.A. and W.G. Graziano. Oxford University Press. https://doi.org/10.1093/oxfordhb/9780195399813.013.015.

Wagner, P. and T. Kössler. 2022. Moulding democratic citizens: Democracy and education in modern European history – an introduction. *European Review of History: Revue européenne d'histoire 29*(6): 859–883. https://doi.org/10.1080/13507486.2022.2133683 .

Weinschenk, A.C., C.T. Dawes, and S. Oskarsson. 2021. Does education instill civic duty? Evidence from monozygotic twins in the United States and Sweden. *International Journal of Public Opinion Research* 33: 183–195. https://doi.org/10.1093/ijpor/edaa006 .

Willeck, C. and T. Mendelberg. 2022. Education and political participation. *Annual Review of Political Science* 25: 89–110 .

Wolfinger, R.E. and S.J. Rosenstone. 1980. *Who Votes?* Yale University Press.

Yang, S. 2019. Does education foster trust? Evidence from compulsory schooling reform in the UK. *Economics of Education Review* 70: 48–60. https://doi.org/10.1016/j.econedurev.2019.03.007 .

Zak, P.J. and S. Knack. 2001. Trust and growth. *The Economic Journal 111*(470): 295–321. https://doi.org/10.1111/1468-0297.00609. https://onlinelibrary.wiley.com/doi/pdf/10.1111/1468-0297.00609 .

Österman, M. 2021. Can we trust education for fostering trust? Quasi-experimental evidence on the effect of education and tracking on social trust. *Social Indicators Research* 154: 211–233. https://doi.org/10.1007/s11205-020-02529-y .

Online Supplementary Material

Appendix A Details on method

A.1 Testing the exogeneity assumption

In this section, we investigate whether the (conditional) exogeneity assumption seems to hold. In other words, conditional on municipality fixed effects and cohort fixed effects, is the assignment to the compulsory schooling reform as good as random among the individuals in our sample? To test this we regress our reform variable using different background characteristics as outcomes in separate regressions using Equation (1).

The characteristics that we investigate are parental education, parental income, which sector the head of the household (usually the father) is working in, car ownership in the family and the quality of housing.

Parental education is gathered from the 1970 census and is a dummy taking value one if the parent has more than primary schooling. The reason that we are taking this outcome from the 1970 Census (post-reform implementation) is due to data limitation. There is no information on education on micro-level prior to this Census. Parental income is earned income gathered from the 1968 income and taxation register (*Inkomst- och taxeringsregistret*) and is transformed using a hyperbolic sin transformation to account for zero earnings and the log-normal distribution of this variable. As with parental education, this is the earliest data on income accessible in Swedish registers. The parental sector is obtained from the 1960 Census and is a dummy taking value one if the head of household (usually the father) is working in the blue-collar sector.[32] Also car ownership and housing quality are gathered from the 1960 Census and measured at the same level. Car is dummy taking value one if the head of household owns at least one car and the quality of housing is a dummy taking value one if the house the family is living in fulfils all the criteria.[33]

The results from this exercise are presented in Table A.1. We see that, conditional on municipality and cohort fixed effects, students who are assigned to the reform do not significantly differ from those that are in not assigned, implying that the reform is conditionally exogenous to student background.

[32] The following working groups are defined in the 1960 census: 1. Business owner in agriculture and forestry 2. Worker in agriculture and forestry 3. Business owner in industry, trade etc. 4. Business owner in academic jobs 5. Business manager 6. Clerk 7. Worker (other than in group 2.) 8. An employee in service 9. Military 10. Non-identified 11. Student 12. Unemployed. Blue collar worker is a dummy taking value one if the head of the household has a job categorized as 2., 7. or 12. (unemployed), and takes value zero for the rest of the categories except 10. (non-identified) and 11. (student).

[33] The criteria is that the apartment or house has a water supply, drain, own water closet, central heating, own bath or shower room, electric or gas stove with oven, and a refrigerator.

Table A.1: Effects of the compulsory school reform on pre-determined characteristics

	Father Edu (1)	Mother Edu (2)	F log inc (3)	M log inc (4)	Blue collar (HH) (5)	Car (HH) (6)	Quality of home (HH) (7)
Reform	0.001	0.001	0.017	-0.011	-0.002	0.002	0.002
	(0.002)	(0.002)	(0.013)	(0.013)	(0.002)	(0.002)	(0.002)
Observations	806,537	1,006,253	1,032,310	743,228	1,250,028	1,254,604	1,231,801
Mean	0.34	0.26	8.98	8.43	0.49	0.49	0.56
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses. Clustered standard errors at the municipality level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. The coefficients are obtained from separate regression using Equation (1) and Data set 1, where each characteristic is used as an outcome variable. Parental education is from the 1970 Census and is a dummy taking value 1 if a parent has more than primary education. Parental log income is from the Income and Taxation Register 1968 and is transformed using hyperbolic sin transformation. Work type (Blue collar), car ownership (Car) and home quality (QoH) are all gathered at the household level from the 1960 Census. Blue collar is a dummy taking value 1 if the head of household (the father) has a blue-collar job. A car is a dummy taking the value of one if the head of household has at least one car. QoH is a dummy taking value one if the house fulfils all the criteria of quality.

A.2 Educational outcomes

In this section, we assess how the compulsory schooling reform affected years of schooling and educational attainment. The Swedish Educational register has information on the individuals' highest attained degrees, traditionally serving as a basis for imputing schooling years by aligning them with the typical duration required for those degrees. However, this method, as highlighted by Fischer et al. (2022), tends to underestimate the reform's impact on years of schooling. It fails to consider the broader effect of the compulsory schooling shift, not only on those who would have quit education after seven or eight years in the absence of the nine-year mandate but also on some students pursuing secondary vocational training, which mandated only seven years of primary schooling.

To mitigate this potential measurement error in schooling years, Fischer et al. (2022) propose an alternative approach. They advocate for constructing an educational trajectory by using additional data from the 1970 Census, specifically incorporating details about primary or secondary education. This supplementary information is then combined with the educational register's records encompassing the highest degree attained.[34] We adopt this idea when assessing the impact of the reform on years of schooling.

More specifically, the 1970 Census includes information on the type of secondary education (gymnasium) or primary education for individuals who hadn't acquired secondary education by 1970. Every individual in our sample should have completed primary education but not necessarily secondary education by 1970, as the individuals are between 16 and 27 years old that year. For those with secondary education in this data, we assume nine years of primary education. We then impute two or three years of secondary education, depending on the duration associated with the specific type of secondary education completed.

For instances where primary education data (spanning seven, eight, or nine years) is visible in the 1970 Census (i.e. those who either have not completed their secondary degree yet or those who have not started one), we use this information with the Educational register for individuals with no higher degree than secondary education. This enables us to impute years of secondary education depending on the type of secondary education (ranging from one to three years of vocational or theoretical education). In cases where individuals have completed theoretical secondary education, our approach imputes the necessary additional years of primary education (as nine years are a requisite for this type of study).[35] Last, for individuals with completed tertiary education, our imputation process involves attributing nine years to primary schooling, three years to secondary schooling, and then assigning the specific number of tertiary education years as per their degree outlined in the educational register.

To assess the impact of the compulsory schooling reform on years of schooling, we regress the reform dummy on years of schooling and educational attainment using Equation (1). The effect of years of schooling is presented in Panel A in Table A.2. In column 1, we see that reform increased the average schooling by 0.28 years. When we separate the effect between individuals depending on their socio-economic status (SES), we see that the effect is almost four times larger for individuals from low SES households. Their average increase in years of schooling is 0.38 years while the average treatment effect for individuals from high SES households is 0.1 years. The effects for all groups are significant at the 1% level.

Was there any spillover to higher education? As the reform made all individuals eligible for theoretical secondary education, we look at whether the reform made individuals more likely to obtain a higher degree. We create a dummy taking value one if the individual has obtained at least a secondary degree and regress the reform dummy on this outcome using Equation (1). The results are

[34] As we are measuring many long-term outcomes, we update the information of highest completed degree up to year 2013.

[35] We also update the number of primary education if they have obtained a longer primary education according to the educational register compared to the census

shown in Panel C in Table A.2. In column 1, we see that the reform increased the average probability of obtaining at least a secondary degree by 0.77 percentage points (or 1.04%), and the effect is significant at the 5% level. Again, we see that the average treatment effect is roughly four times larger for individuals from low SES households compared to individuals from high SES households. The effect is significant for the low SES group at the 1% level, while it is not statistically different from zero for the high SES group. We do not find any effect of the reform on the probability of obtaining a tertiary/University degree (not shown here), and hence, we can conclude that the reform only affected the probability of obtaining a secondary degree.

Using the conventional method of assigning years of schooling (based on the years of highest obtained education level), we obtain effects that are substantially smaller. The effect of the reform on this measure of years of schooling is seen in Panel B in Table A.2. Due to data limitation in Data Set 2 [36], we cannot use the new way of assigning years of schooling when assessing the association between years of schooling and voting and blood donation. Hence, we will use the conventional way of assigning years of schooling when investigating the association between years of schooling and our outcomes. For those outcomes, we have both measures of years of schooling; there are small differences in the estimates when using different measures of years of schooling.[37] The new way of assigning years of schooling tends to yield slightly larger effects compared to the conventional one when looking at the full sample and for the LSES group, while for the HSES group, the effects are generally smaller. However, even though the estimates are very precise, these differences tend not to be statistically different from each other.

[36] We lack the variable from the 1970 Census, which includes information on the type of secondary education (gymnasium) or primary education for individuals who hadn't acquired secondary education by 1970.

[37] Results are available upon request.

Table A.2: Effects of the compulsory school reform on years of schooling and educational attainment

	All (1)	LSES (2)	HSES (3)
A. New YoS			
School Reform	0.28***	0.38***	0.10***
	(0.0446)	(0.0307)	(0.0321)
Obs.	1,151,211	506,766	254,561
Mean	11.25	10.75	12.64
F-statistics	41.43	153.017	9.63
B. Old YoS			
School Reform	0.18***	0.26***	0.045*
	(0.0306)	(0.0217)	(0.0239)
Obs.	1,196,981	522,131	265,076
Mean	11.39	10.90	12.69
F-statistics	35.62	147.87	3.53
C. Educational Attainment (at least upper secondary)			
School Reform	0.74**	1.31***	0.39
	(0.34)	(0.31)	(0.25)
Obs.	1,196,981	522,131	265,076
Mean (%)	74.2	69.7	87.5
Municipality FE	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes

Notes: Standard errors in parentheses. Clustered standard errors at the municipality level. * p<0.10, ** p<0.05, *** p<0.01. The outcome of interest is years of schooling and the coefficients are obtained using Equation (1). Panel A presents the estimate when using the new way of assigning years of schooling while Panel B presents the estimate when using the conventional way of assigning years of schooling. Panel C presents the effect of on upper secondary degree (*gymnasie*). The outcome of interest is a dummy taking value one if an individual has a secondary degree or higher where the coefficients and standard errors are scaled to represent percentage points. The columns show the estimates for the full sample, individuals from low and high socio-economic background, respectively.

A.3 Alternative estimators

In this section we re-estimate our main results using the newly developed estimators for settings with staggered treatment as a Two-Way Fixed Effects (TWFE) estimator may produce misleading estimates if the reforms's effect is heterogeneous between municipalities or over cohort (see De Chaisemartin and d'Haultfoeuille (2023) for a survey). We present the results for all main outcomes in a table format. In these tables, the first column shows the estimate from our preferred specification (TWFE). Column 2 shows the estimate using the estimator by Borusyak et al. (2021) (BJS (2021)). This estimator uses only never treated units as controls and drop always treated municipalities, i.e. those municipalities where the first treated cohort is born in 1943 or earlier. The reform's effect is the average weighted difference between (all) pre-periods and post-periods. Column 3 and 4 show the estimator developed in Callaway and Sant'Anna (2021) using only never treated (C & S (Never)), and never treated and not yet treated (C & S (Not yet)) as controls, respectively. As with BJS (2021), always-treated municipalities are dropped. The treatment effects using these two estimators are the average weighted difference between (only) the last non-treated period and all post-periods, and hence relies on the less strict assumption of parallel trends for all post-treatment periods rather than parallel trends in all pre and post-treatment periods.

In Table A.3, the different estimates for our main outcomes[38] using the different estimators are shown. By comparing our TWFE estimate with the estimates from the alternative estimators we see small differences for all outcomes. The only notable systematic difference is that both C & S estimators (columns 3 and 4) tend to yield less precise estimates compared to the TWFE and BJS (2021) estimators.

We conclude that our TWFE estimates are not sensitive for heterogeneous treatment effects or negative weights.

Table A.3: Effects of the compulsory school reform on civic engagement: alternative estimators

	TWFE (1)	BJS(2021) (2)	C & S (Never) (3)	C & S (Not yet) (4)
A. Share Elect. voted				
School Reform	0.070	0.073	-0.068	-0.007
	(0.083)	(0.093)	(0.125)	(0.123)
Obs.	1,221,523	1,165,231	1,165,231	1,165,231
B. Blood donor, 57-62				
School Reform	-0.120*	-0.222**	-0.168	-0.130
	(0.073)	(0.087)	(0.113)	(0.115)
Obs.	1,179,492	1,125,149	1,125,149	1,125,149
C. CG: Any				
School Reform	-0.535**	-0.492**	-0.462	-0.305
	(0.228)	(0.208)	(0.300)	(0.280)
Obs.	1,101,599	1,051,020	1,051,020	1,051,020

Notes: Standard errors in parentheses. Clustered standard errors at the municipality level. * p<0.10, ** p<0.05, *** p<0.01. Coefficients and standard errors are scaled to represent percentage points. Column 1 shows the coefficient obtained using Equation (1), which is the conventional TWFE-estimator. Column 2 shows the coefficient obtained using the estimator developed by Borusyak et al. (2021). Column 3 and 4 show the coefficients obtained using the estimator developed by Callaway and Sant'Anna (2021) when using never treated, and never treated and not yet treated control units, respectively.

[38] This test on all outcomes is available upon request.

A.4 Comparison with previous studies on voting

In this section, we present the data gathered from the comparisons with previous studies in Figure 2. The inclusion criteria are: i) the outcome should be voting and ii) the necessary information, in order to translate the estimated effect into the Cohen's D of 0.3 more years of schooling (YoS), should be possible to extract from the paper and iii) the studies should have a clear identification strategy, i.e. have a causal approach.

Milligan et al. (2004) (US – NES): the effect estimate is taken from Table 3, model 3. The estimate refers to the effect of a high-school diploma, which the authors interpret as 4 more YoS (page 1686).

Milligan et al. (2004) (UK): the effect estimate is taken from Table 6, second column. The estimate refers to the effect of one more YoS.

Dee (2004) (GSS): the effect estimate is taken from Table 4, model 4. The estimate refers to the effect of one more YoS.

Siedler (2010) (ALLBUS and ForsaBus): the effect estimates are taken from Table 3, model 2. The estimates refers to the effect of one more YoS.

Berinsky and Lenz (2011): the effect estimate is taken from Table 4, model 4. The estimate refers to the effect of "some college" vs no college which we assume implies 4 YoS.

Pelkonen (2012): the effect estimates are taken from Table 4, model 3. The estimates refers to the effect of one more YoS.

Persson (2014): the effect estimate is taken from Table 2. The standard error of the estimate is inferred by the p-value combined with the size of the estimated effect. The estimate refers to the effect of college degree vs no college degree. Since it is not reported how many YoS this implies we use the 4 years assumed in Milligan et al. (2004).

Doyle and Skinner (2017) (2010 estimate): the effect estimate is taken from Table 2, for year 2010 (instrument: 2 year public college). The estimate refers to the effect of one more YoS.

Appendix B Details on data

B.1 Sample restrictions

Our target population is the full population of Swedes born between 1943 and 1954. However, some key information is needed to run our analyses which we do not have for every individual. As we are assigning treatment based on the individuals' place of residence in the 1960 Census, we exclude individuals which we do not observe in that Census. We also exclude municipalities (and some parishes in Stockholm, Gothenburg and Malmö) where the implementation year is unclear, according to Holmlund (2020). The former restriction reduces the sample size from 1 830 697 individuals to 1 405 954, and the latter reduces the sample to 1 254 615 million individuals (in Data Set 1). This sample will serve as our main sample.

As we have also missing information for the outcome variable, the sample sizes differ depending on which outcome is used. See Table C.1 for how many observations we have for each outcome.

B.2 Voting

Uppsala researchers at the Department of Government, Uppsala University, have digitized election rolls for the general elections of 1970, 1994 and 2010, as well as the EU elections of 1994, 2009 and 2019. The data on the 2010 general election was first validated, used and published in the paper Lindgren et al. (2019). They were able to obtain digital copies of the electoral rolls for 282 out of the 290 municipalities. Their procedure resulted in information on voter turnout for 96.5% of those eligible to vote in the election to the national parliament in 2010. For a more detailed description of their data collection procedure and assessment of the quality of the data, we refer to the supplementary materials of Lindgren et al. (2019). The data collection from the other elections followed the same procedure as the 2010 election.

For the 2018 general election, Statistics Sweden collected information on individual voter turnout on the entire electorate. To ensure high quality of the data, Statistics Sweden participated in the design of the electoral rolls in order to facilitate the scanning, and the measurement errors are negligible (Statistics Sweden, 2019).

B.3 Blood donation

The blood donation data was first collected and used in the study by Almén (2020). The data was stored on servers at 30 regional organizational bodies, which belong to the 21 regional councils responsible for public health care in Sweden. To retrieve the donor data, the author first contacted each body and submitted a formal application. Each head of these bodies had to approve the request, which they all did. Once permission was granted, he contracted the two companies procured by the regional councils to manage their data. These companies extracted the personal identity numbers of all individuals who donated blood from 1990 to 2016. Unfortunately, the region of Skåne lacked data before 2000. Finally, each body sent the data to Statistics Sweden, which then replaced the personal identification number with another number and delivered the data together with other register data needed for the study.

B.4 Charitable giving

The data on recipient organizations were obtained through a special request from Statistics Sweden. This was necessary because the information is not included in the Tax Agency's Income and Tax Register but is available in tax statements from the Swedish Tax Agency. Access to the data was granted following a thorough extradition assessment, with the condition that we do not disclose the identities of specific organizations. As a result, we have categorized the organizations into broader groups.

B.5 Data availability

The analyses are based on data from administrative registers held and maintained by Statistics Sweden (SCB) and the regional bodies of the public health care that manages blood donations in Sweden. According to our agreements with these institutions, we are not permitted to share the data directly. However, a description, in Swedish, of how to order data from SCB is available at: $http://www.scb.se/sv_/Vara-tjanster/Bestalla-mikrodata$). Contact information for the regional bodies that manage blood donation in Sweden can be obtained by contacting the Swedish Blood Alliance (SweBA) ($https://www.sweba.se/$), which is a non-profit association that works for cooperation between the blood centres in Sweden. All data sources require permission from a Swedish Ethical Review Board in order to hand out the data. The research is approved by the Swedish Ethical Review Board.

Appendix C Additional results

C.1 Descriptive statistics by SES

Table C.1: Descriptive statistics by SES

	All			Low SES			High SES		
	Obs.	Mean	St. dev	Obs.	Mean	St. dev	Obs.	Mean	St. dev
Panel A: Data set 1									
Reform variable:									
Reform Status	1,254,615	0.353	0.478	532,435	0.383	0.486	274,102	0.470	0.499
Individual Characteristics:									
High SES	806,537	0.340	0.474	532,435	0.000	0.000	274,102	1.000	0.000
Female	1,254,615	0.487	0.500	532,435	0.488	0.500	274,102	0.490	0.500
Birth Year	1,254,615	1948.251	3.423	532,435	1948.880	3.339	274,102	1949.206	3.309
Outcomes:									
Years of Schooling	1,196,981	11.391	2.789	522,131	10.897	2.556	265,076	12.693	2.696
YoS (Fischer et al. 2022)	1,151,211	11.249	2.820	506,766	10.749	2.530	254,561	12.641	2.792
Charitable Giving	1,101,599	0.327	0.469	483,255	0.301	0.459	247,379	0.375	0.484
Ch. Giving: Foreign Aid	1,101,599	0.165	0.371	483,255	0.134	0.341	247,379	0.227	0.419
Ch. Giving: Religion	1,101,599	0.020	0.139	483,255	0.015	0.123	247,379	0.025	0.155
Ch. Giving: Social Work	1,101,599	0.032	0.176	483,255	0.024	0.153	247,379	0.042	0.200
Ch. Giving: Research	1,101,599	0.216	0.412	483,255	0.212	0.408	247,379	0.219	0.413
Panel B: Data set 2									
Reform variable:									
Reform Status[a]	1,256,848	0.353	0.478	537,465	0.383	0.486	276,724	0.469	0.499
Individual Characteristics:									
High SES[a]	814,189	0.340	0.474	537,465	0.000	0.000	276,724	1.000	0.000
Female[a]	1,256,848	0.487	0.500	537,465	0.487	0.500	276,724	0.490	0.500
Birth Year[a]	1,256,848	1948.251	3.423	537,465	1948.878	3.342	276,724	1949.199	3.313
Outcomes:									
Years of Schooling[a]	1,198,792	11.390	2.786	524,629	10.897	2.554	266,170	12.691	2.691
Share Voted Elections	1,221,523	0.803	0.252	527,484	0.789	0.250	271,246	0.840	0.233
Voted, 1970	639,339	0.839	0.368	243,572	0.833	0.373	128,373	0.866	0.341
Voted, 1994	1,068,045	0.909	0.287	465,506	0.905	0.293	239,792	0.925	0.264
Voted, 2010	1,083,142	0.914	0.281	475,660	0.908	0.289	244,193	0.930	0.255
Voted, 2018	1,038,373	0.924	0.266	456,617	0.921	0.270	238,544	0.938	0.241
Voted EU, 2009	1,085,927	0.533	0.499	476,036	0.485	0.500	245,147	0.618	0.486
Voted EU, 2019	1,020,281	0.689	0.463	449,218	0.656	0.475	234,613	0.761	0.426
Voted EU Ref., 1994	1,065,400	0.899	0.302	464,623	0.893	0.309	239,079	0.920	0.272
Blood Donor, age 47-62	1,215,745	0.084	0.277	525,933	0.087	0.282	271,265	0.088	0.283
Blood Donor, age 57-62	1,179,492	0.047	0.212	510,363	0.049	0.216	264,330	0.049	0.217

Notes: [a]Variable is available in both data sets

C.2 Association between years of schooling and our remaining outcome variables

Table C.2: Association between years of schooling and our remaining outcome variables

	(1) Voted 1970	(2) Voted 1994	(3) Voted 2010	(4) Voted 2018	(5) EU Ref. 1994	(6) EU 2009
Panel A: Full Sample						
Years of Schooling	1.64***	1.40***	1.40***	1.23***	1.56***	4.98***
	(0.048)	(0.021)	(0.014)	(0.014)	(0.018)	(0.025)
Observations	614,018	1,064,497	1,076,040	1,030,586	1,061,873	1,079,319
Mean (%)	84.7	91.1	91.7	92.6	90.0	53.5
Panel B: Low SES						
Years of Schooling	1.51***	1.40***	1.44***	1.26***	1.56***	4.78***
	(0.046)	(0.026)	(0.020)	(0.021)	(0.025)	(0.032)
Observations	238,710	464,526	474,018	454,708	463,644	474,492
Mean (%)	83.7	90.7	91.0	92.3	89.4	48.6
Panel C: High SES						
Years of Schooling	1.64***	1.35***	1.28***	1.12***	1.37***	4.83***
	(0.059)	(0.028)	(0.022)	(0.020)	(0.024)	(0.053)
Observations	123,843	238,820	241,720	235,656	238,110	242,892
Mean (%)	87.3	92.6	93.4	94.3	92.1	62.2
	(7) EU 2019	(8) Blood Donor, 47-62	(9) CG: Foreign	(10) CG: Religion	(11) CG: Social Work	(12) CG: Research
Panel A: Full Sample						
Years of Schooling	4.31***	0.31***	3.36***	0.53***	0.64***	1.25***
	(0.040)	(0.016)	(0.036)	(0.011)	(0.034)	(0.032)
Observations	1,013,083	1,190,168	1,098,809	1,098,809	1,098,809	1,098,809
Mean (%)	69.1	8.6	16.5	2.0	3.2	21.7
Panel B: Low SES						
Years of Schooling	4.35***	0.44***	2.87***	0.42***	0.46***	1.41***
	(0.038)	(0.017)	(0.027)	(0.014)	(0.015)	(0.027)
Observations	447,462	520,624	482,462	482,462	482,462	482,462
Mean (%)	65.7	8.8	13.4	1.5	2.4	21.2
Panel C: High SES						
Years of Schooling	3.96***	0.09***	3.85***	0.59***	0.78***	0.93***
	(0.059)	(0.022)	(0.045)	(0.017)	(0.050)	(0.041)
Observations	231,961	264,231	246,624	246,624	246,624	246,624
Mean (%)	76.6	9.0	22.7	2.5	4.2	21.9
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses. Clustered standard errors at the municipality level. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Coefficients and standard errors are scaled to represent percentage points. Each column in each separate panel presents separate regressions using Equation (1), except that we replace the reform dummy with years of schooling (using the conventional way of assigning years of schooling, see A.2 for a discussion of this measure). In Panel A, the estimates are obtained using the full sample while Panel B and C show the estimates for individuals from low and high socio-economic backgrounds, respectively.

C.3 Effects of the compulsory school reform on our remaining outcome variables

Table C.3: Effects of the compulsory school reform on our remaining outcome variables

	(1) Voted 1970	(2) Voted 1994	(3) Voted 2010	(4) Voted 2018	(5) EU Ref. 1994	(6) EU 2009
School Reform	0.357	0.068	0.002	-0.076	-0.047	-0.144
	(0.341)	(0.104)	(0.099)	(0.101)	(0.110)	(0.200)
Obs.	639,339	1,068,045	1,083,142	1,038,373	1,065,400	1,085,927
Mean (%)	83.9	90.9	91.4	92.4	89.9	53.3
Panel B: Low SES						
School Reform	-0.142	0.009	-0.017	-0.104	-0.083	-0.268
	(0.396)	(0.145)	(0.137)	(0.142)	(0.152)	(0.277)
Obs.	243,572	465,506	475,660	456,617	464,623	476,036
Mean (%)	83.3	90.5	90.8	92.1	89.3	48.5
Panel C: High SES						
School Reform	0.489	0.161	0.096	0.205	0.075	0.035
	(0.440)	(0.201)	(0.204)	(0.168)	(0.195)	(0.334)
Obs.	128,372	239,792	244,193	238,544	239,079	245,147
Mean (%)	86.6	92.5	93.0	93.8	92.0	61.8
	(7) EU 2019	(8) Blood Donor, 47-62	(9) CG: Foreign	(10) CG: Religion	(11) CG: Social Work	(12) CG: Research
Panel A: All						
School Reform	0.102	-0.386***	-0.397**	-0.039	-0.193**	-0.309
	(0.179)	(0.141)	(0.165)	(0.048)	(0.084)	(0.189)
Obs.	1,020,281	1,215,745	1,101,599	1,101,599	1,101,599	1,101,599
Mean (%)	68.9	8.4	16.5	2.0	3.2	21.6
Panel B: Low SES						
School Reform	0.063	-0.525***	-0.070	-0.064	-0.07	-0.104
	(0.273)	(0.164)	(0.165)	(0.063)	(0.075)	(0.226)
Obs.	449,268	525,933	483,255	483,255	483,255	483,255
Mean (%)	65.6	8.7	13.4	1.5	2.4	21.2
Panel C: High SES						
School Reform	0.044	0.131	-0.821***	0.146	-0.291**	-0.587**
	(0.281)	(0.210)	(0.314)	(0.107)	(0.139)	(0.272)
Obs.	234,613	271,265	247,379	247,379	247,379	247,379
Mean (%)	76.1	8.8	22.7	2.5	4.2	21.9
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Birth Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Standard errors in parentheses. Clustered standard errors at the municipality level. * p<0.10, ** p<0.05, *** p<0.01. Coefficients and standard errors are scaled to represent percentage points. Each column in each separate panel presents separate regressions using Equation (1). In Panel A, the estimates are obtained using the full sample while Panel B and C show the estimates for individuals from low and high socio-economic backgrounds, respectively.

References

Almén, D. 2020, 5. *Societal Impacts of Modern Conscription: Human Capital, Social Capital and Criminal Behaviour.* Ph. D. thesis, Stockholm University, Stockholm.

Berinsky, A.J. and G.S. Lenz. 2011. Education and political participation: Exploring the causal link. *Political Behavior* 33: 357–373. https://doi.org/10.1007/s11109-010-9134-9 .

Borusyak, K., X. Jaravel, and J. Spiess. 2021. Revisiting event study designs: Robust and efficient estimation. *arXiv preprint arXiv:2108.12419* .

Callaway, B. and P.H. Sant'Anna. 2021. Difference-in-differences with multiple time periods. *Journal of Econometrics 225*(2): 200–230 .

De Chaisemartin, C. and X. d'Haultfoeuille. 2023. Two-way fixed effects and differences-in-differences with heterogeneous treatment effects: A survey. *The Econometrics Journal 26*(3): C1–C30 .

Dee, T.S. 2004. Are there civic returns to education? *Journal of Public Economics 88*(9): 1697 – 1720. https://doi.org/https://doi.org/10.1016/j.jpubeco.2003.11.002 .

Doyle, W.R. and B.T. Skinner. 2017. Does postsecondary education result in civic benefits? *The Journal of Higher Education 88*(6): 863–893 .

Fischer, M., G. Heckley, M. Karlsson, and T. Nilsson. 2022. Revisiting Sweden's comprehensive school reform: Effects on education and earnings. *Journal of Applied Econometrics 37*(4): 811–819. https://doi.org/https://doi.org/10.1002/jae.2881 .

Holmlund, H. 2020. A researcher's guide to the Swedish compulsory school reform. *Journal of the Finnish Economic Association* 1/2020: 25–50 .

Lindgren, K.O., S. Oskarsson, and M. Persson. 2019. Enhancing electoral equality: Can education compensate for family background differences in voting participation? *American Political Science Review 113*(1): 108–122 .

Milligan, K., E. Moretti, and P. Oreopoulos. 2004. Does education improve citizenship? Evidence from the United States and the United Kingdom. *Journal of Public Economics 88*(9): 1667 – 1695. https://doi.org/https://doi.org/10.1016/j.jpubeco.2003.10.005 .

Pelkonen, P. 2012. Length of compulsory education and voter turnout-evidence from a staged reform. *Public Choice* 150: 51–75. https://doi.org/10.1007/s11127-010-9689-3 .

Persson, M. 2014. Testing the relationship between education and political participation using the 1970 British Cohort Study. *Political Behavior* 36: 877–897. https://doi.org/10.1007/s11109-013-9254-0 .

Siedler, T. 2010. Schooling and citizenship in a young democracy: Evidence from postwar Germany. *The Scandinavian Journal of Economics* 112: 315–338. https://doi.org/10.1111/j.1467-9442.2010.01604.x .

Statistics Sweden 2019. *Voter turnout in the General Elections 2018.* Statistics Sweden.