SUPPORTING APPENDIX:

Enhancing Electoral Equality: Can Education Compensate for Family Background Differences in Voting Participation?

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A 1 A Theoretical Framework

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

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

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

As discussed at length in the literature on the paradox of voting, given the size of the

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

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

To analyze these issues more formally, let us proceed by approximating the net cost of voting by means of the following equation:

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

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

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

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

individual i in group g can thus be written:

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

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

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

Equation (4) can then be used to calculate the average turnout for each combination of socioeconomic status and school system, as is done in Table A1 (the bars denote mean values).

Table A1: Turnout by group and school system

School System

		Non-Reformed	*Reformed*
SES	*Low*	$\frac{c_0^l + \bar{a}_0^l + \delta^l \bar{e}_0^l + m}{2m}$	$\frac{c_1^l + \bar{a}_1^l + \delta^l \bar{e}_1^l + m}{2m}$
	High	$\frac{c_0^h + \bar{a}_0^h + \delta^h \bar{e}_0^h + m}{2m}$	$\frac{c_1^h + \bar{a}_1^h + \delta^h \bar{e}_1^h + m}{2m}$

The differences in turnout for each of the two SES groups before and after a school reform will thus be given by the column differences in Table A1. That is:

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

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

where Δ denotes the before and after difference in the variable of interest. A first thing to note is that the difference in turnout between the two points in time will not only

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

However, if we invoke the assumption that no other important changes occurred simultaneously with the school reform any differences in turnout across the columns of Table A1 will be due solely to the reform. In this case it is also straightforward to characterize the impact of the reform on voting inequality. For instance, if we use the difference in turnout shares between individuals from low and high SES homes to measure inequality the change in voting inequality induced by the reform is:

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

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

More importantly, however, this model highlights the fact that there are two different effects at work here. First there is what we will refer to as the *resource effect*, i.e., the reform may affect the allocation of education (the resource) between SES groups. Available empirical evidence suggest that both the sign and the magnitude of the resource effect can depend on the type of education reform being examined. Reforms that lengthens compulsory education, on the one hand, tend to have a larger effect on educational attainment of children from low SES homes ($\Delta \bar{e}^l > \Delta \bar{e}^h$) because they are less likely to pursue secondary education (Lindgren, Oskarsson, and Dawes, 2017). Blanden and Machin (2004), on the other hand, find that policies that expand higher education in the UK have served to widen the educational gap between children from rich and poor

backgrounds (i.e., $\Delta \bar{e}^l < \Delta \bar{e}^h$). Depending on the nature of the reform, the resource effect can therefore contribute to increasing or decreasing the voting gap.

However, even if both SES groups experience an equal increase in educational attainment as a result of the reform, so that the resource effect is zero, voting inequality could nevertheless change if the effect of education on voting differs across groups. We will refer to this as the *return effect*. If formal education and a stimulating socializing family environment are *substitutes* in the process of developing the type of skills, interests, and norms conducive to political participation a given increase in educational attainment should have a larger effect among individuals with low SES background (i.e., $\delta^l > \delta^h$). Or, conversely, if these two factors are *complements* in the production of political participation, increased schooling should have a more pronounced effect among individuals from high SES homes (i.e., $\delta^h > \delta^l$). In his study of the impact of civic education on political engagement, Campbell (2008) refers to these two possibilities as the *compensation* and *acceleration* hypothesis, respectively (cf., Langton and Jennings, 1968; Neundorf, Niemi, and Smets, 2016).

The extent to which education reforms can help mitigate political inequality will thus depend on both the resource and the return effects associated with particular reforms.

A 2   Details on Data and Measures

This section provides a description of the data availability, data sources and variables used for the paper "Can Increased Education Help Reduce the Political Opportunity Gap?"

A 2.1   Data Availability

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

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

A 2.2   Variables and Data Sources

Voter Turnout

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

In Sweden the electoral rolls are still maintained in paper form, and each roll lists all eligible voters living a particular voting district. The electoral rolls contain preprinted

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

After the elections the electoral rolls are archived at the municipality level. The first task in the data collection process was therefore to contact all 290 municipalities in Sweden and ask them if they could scan or copy the 2010 electoral rolls for us. In those cases where the municipalities were unable to do the work for us, our research assistants went to the municipalities to scan the material.[2] Using this strategy we were able to obtain digital copies of the electoral rolls for 282 out of the 290 municipalities. In 7 municipalities they were unable to locate the electoral rolls for the 2010 election, and in one municipality they would not let us scan or copy the electoral rolls. In addition, in a few cases the electoral rolls for specific electoral districts were missing, and, in a somewhat larger number of cases, individuals pages were accidentally neglected in the process of scanning.[3]

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

Digitizing the handwritten information on actual voting was somewhat more challenging. To do this, all the boxes in which the election officials keep their records were converted to binary images using a thresholding algorithm. That is, every pixel in these

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[3]We are currently in the process of trying to acquire as much as possible of the missing material.

boxes were assigned either a value of 0 or 1 depending on their relative darkness. In an ideal world empty boxes, indicating voting abstention, would then be represented by all zeros, whereas the images for voters would contain a larger number of ones, and the location of these ones should represent the pencil strokes associated with each of the three different symbols used for indicating voting (P, V, or $/$). In practice, however, there could be some black pixels (ones) also in empty boxes, due to stains on the paper or to imperfect scanning, and both the number and location of the black pixels associated with a particular symbol, such as P, will be highly dependent on the individual handwriting of the different election officials keeping the records. We therefore designed a procedure for making an initial classification of the content of all the individual boxes, by counting the number of black pixels in different directions of the individual images. In cases where the original image was of good quality this initial classification proved highly accurate. To improve the accuracy even further, we also developed a graphical user interface that was used to view and, when necessary, manually correct the automated classifications.[4] To save labor, we decided to focus on the classification of *voting* vs. *abstention*. Consequently, in the manual correction stage we did not attempt to correct misclassifications with respect to type of voting, e.g. voting in a polling place vs. postal voting.

Following this procedure we were able to retrieve information on voter turnout for 96.5 percent of those eligible to vote in the election to the national parliament in 2010 (6,873,661 out of 7,123,651 individuals). Figure A28 provides a first check of the quality of these data, by comparing aggregate voter turnout on the municipality level in our data with official election statistics.[5] As can be seen, aggregate turnout are virtually the same in the two data sources, and the only observation that is located visibly below the 45 degree line is one of the municipalities for which we, currently, lack information on a number of electoral districts.

Another way to check the quality of our data is to compare our indicator of voter

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

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

Figure A1: Voter Turnout at the Municipality Level
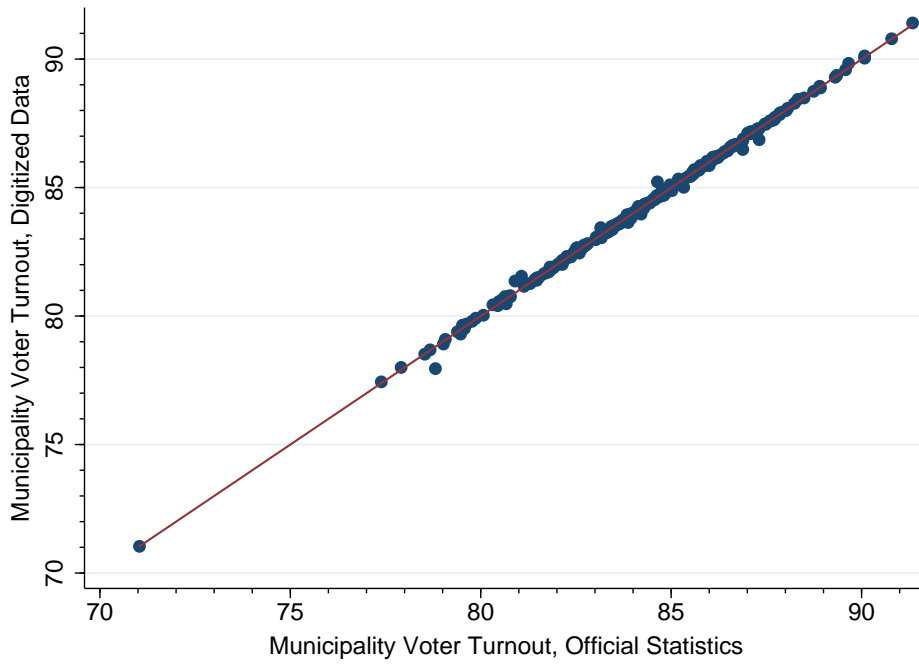
turnout with that of the random sample collected by Statistics Sweden (SCB) after the 2010 election. By using the unique social security number included in both data sets we were able to compare our classification with that of SCB for a total sample of 85,449 individuals. As can be seen from Figure A2, our digitized information on voter turnout conforms with SCB's manual codings in 99.7 percent of the cases (85,235/85,449). In practice, this means that very little, if anything, is lost by using our automated, and much less labor intensive, procedure for collecting data on voter turnout.

Figure A2: Comparing Classifications at the Individual Level

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

Data from Administrative Registers

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

`Reform intensity` — The share of available vocational programs in a municipality that were three years long. For municipalities not offering any vocational programs during the study period we use the reform intensity score for the municipality in which most students from the 1970 cohort (the cohort preceding the first reform cohort) attended a vocational program. For instance, if municipality A were not offering any vocational programs and most of the individuals born in 1970 from municipality A chose to attend their vocational studies at the upper secondary level in the nearby municipality B, the reform intensity score in municipality A will be the share of 3-year vocational programs in municipality B. The data necessary to construct the reform intensity measure were obtained from the Upper Secondary School Application Record (*Gymnasieskolans sökanderegister*). We are very grateful to Caroline Hall for sharing the stata code used to create this indicator.

`Home municipality` — Code for municipality of residence in December the year before the individual turn 16. Information is retrieved the Swedish Population Register.

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

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

`GPA` — Grade point average from compulsory school. The grades in each subjected is recorded on a scale from 1 to 5 with higher number indicated better grades. Information is retrieved from the Upper Secondary School Application Record.

`Vocational/Academic program` — Indicator identifying individuals enrolled in vocational and academic programs at age 16. Information is retrieved from information in the Upper Secondary School Application Record.

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

`Completed 3-year program` — Equal to 1 if the individual has completed at least three years of post-primary education at the age of 20.

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

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

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

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

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

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

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

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

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

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

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

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

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status have been set to its sample minimum value.

Family SES — In this study we use a measure of socioeconomic status that is closely related to the *PISA index of economic, social, and cultural status* (ESCS), developed by the OECD. This is a simple average of parental earnings, parental education, and parental occupational status. All items are assigned the same weight in calculating the SES index and is based on the two indicators for which data is available if information on one of the indicators in the index is missing. To adjust for differences in scales between the variables, all three sub-items were initially standardized to have a mean of 0 and a standard deviation of 1 in the sample under study. Parental earnings were top coded at the 99th percentile before being standardized. In case information on one of the indicators is missing the index is based on the two indicators for which data is available (complete data on all three sub-items are available for 94% of the observations). The scale reliability of the resulting index is .78.

Whereas information on parental education and labor income are gathered directly from the registers our measure of occupational status is based on census occupation codes. More precisely, we use the occupation codes to compute three well-known measures of occupational status: the *International Socio-Economic Index* (ISEI, Ganzeboom, Graaf, and Treiman, 1992)), the *Standard International Occupational Prestige Scale* (SIOPS, Treiman, 1977), and the *International Cambridge Scale* (ICAMS, Meraviglia, Ganzeboom, and Luca, 2016; Prandy and Jones, 2001).[6] As shown by Meraviglia, Ganzeboom, and Luca (2016), despite the differences in conceptual underpinnings these three measures are very highly correlated and appear to reflect a single underlying dimension. We therefore use the average of these three indicators to measure mothers' and fathers' occupational statuses. The scale reliability of this index is .96 for fathers and .93 for mothers. For a small number of individuals that have two non-employed parents, parental occupational status has been set at its sample

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[6]The code for translating census occupation codes into ICAMS, ISEI, and SIOPS was downloaded from `http://www.harryganzeboom.nl/isco88/index.htm`.

minimum value.

ESS Data

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

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

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

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

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

`Gender` — Equal to 1 for female respondents.

`Parental education` — Highest education, in years, of father and mother. In the original ESS file educational attainment of mothers and fathers are coded using the five-level

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ISCED-97 code, but we have translated these codes into years of education following the procedure devised by Okbay et al. (2016, Supplementary Table 1.2): Less than lower secondary education (7 years); Lower secondary education (10 years); Upper secondary education (13 years); Post-secondary non-tertiary education (15 years); Tertiary education (20 years).

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

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

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

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

Allow large immigration — Equal to 1 for respondents who answered either *Allow many to come and live here* or *Allow some* to the question "To what extent do

you think [country] should allow people of the same race or ethnic group as most [country] people to come and live here?". The indicator is based on the variable named IMDFETN in the original ESS data set.

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

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

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

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

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

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

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

original ESS data set.

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

Left party support — Equal to 1 for respondents who answered that they feel closer to one of the parties of the left bloc (i.e., the Social Democrats, the Left Party, and the Green Party). The indicator is based on the variable named PRTCLASE in the original ESS data set.

Left party support — Equal to 1 for respondents who answered that they feel closer to one of the parties of the right bloc (i.e., the Conservative Party, the Liberal Party, the Center Party, and the Christian Democratic Party). The indicator is based on the variable named PRTCLASE in the original ESS data set.

A 3 Additional Analyses

A 3.1 Descriptive Statistics

Table A2 presents summary statistics for the variables included in our analyses divided by educational enrollment and pilot intensity. We here distinguish between four different enrollment categories. The category *None* refers to those students who did not enroll in upper secondary school at age 16, Academic are those enrolling in academic upper secondary programs, and *2-year voc.* and *3-year voc.* are those enrolling in the two types of vocational programs under study.

Table A2: Descriptive Statistics by Reform Intensity and Educational Enrollment

Educ. enrollment	None		2-year voc.		3-year voc.		Academic	
Reform intensity	Low	High	Low	High	Low	High	Low	High
Gender	0.513 (0.500)	0.528 (0.499)	0.410 (0.492)	0.408 (0.491)	0.485 (0.500)	0.401 (0.490)	0.538 (0.499)	0.537 (0.499)
Immigrant background	0.202 (0.402)	0.200 (0.400)	0.151 (0.358)	0.138 (0.345)	0.146 (0.353)	0.118 (0.323)	0.149 (0.356)	0.143 (0.350)
Family SES	-0.285 (0.807)	-0.287 (0.834)	-0.294 (0.670)	-0.310 (0.663)	-0.170 (0.706)	-0.246 (0.663)	0.341 (0.847)	0.350 (0.867)
Student's year of birth	1972.0 (1.400)	1972.0 (1.411)	1971.9 (1.406)	1971.8 (1.391)	1973.0 (1.006)	1973.1 (0.889)	1972.0 (1.409)	1972.0 (1.412)
Father's year of birth	1943.0 (6.160)	1942.9 (6.290)	1943.0 (5.931)	1942.9 (6.040)	1944.1 (5.732)	1944.0 (5.872)	1942.5 (5.537)	1942.5 (5.660)
Mother's year of birth	1945.9 (5.355)	1945.9 (5.496)	1945.8 (5.194)	1945.8 (5.269)	1947.0 (4.910)	1946.9 (5.091)	1945.0 (4.790)	1945.0 (4.885)
Completing 3-year program	0.178 (0.382)	0.195 (0.397)	0.143 (0.350)	0.141 (0.348)	0.769 (0.422)	0.832 (0.374)	0.821 (0.383)	0.824 (0.381)
Turnout	0.801 (0.399)	0.807 (0.395)	0.867 (0.340)	0.865 (0.341)	0.882 (0.323)	0.872 (0.334)	0.928 (0.259)	0.925 (0.263)
Obs.	44861	52029	81741	84510	5593	14740	108616	121566
Compulsory school GPA	2.743 (0.686)	2.747 (0.681)	2.884 (0.528)	2.880 (0.527)	2.953 (0.545)	2.923 (0.532)	3.709 (0.494)	3.712 (0.493)
Obs.	29748	37334	70894	78265	4737	14060	89364	107891

As expected there are large differences in student characteristics when comparing across different educational groups. More importantly for our purpose, however, we can note that the differences within enrollment categories are generally very small. The only partial exception is the group of students attending 3-year vocational programs where we observe some slight differences between the groups, but this is to be expected since the students choosing to attend a 3-year program when most programs are of the 2-year type are likely to differ from those enrolling in 3-year programs when most vocational programs are of this type. The composition of vocational students as a whole is, however, very similar across both types of municipalities, and this is what matters for our identification strategy.

Overall, the student composition is thus rather similar in municipalities with high and low reform intensity. To further support this claim the last row of the table shows information on GPA from compulsory school, which is available for a subset of our sample (this variable can take on values between 1 and 5, higher values indicating better grades). As can be seen, the GPA within each student category is very similar regardless of reform intensity.

A 3.2   Reform Intensity and Educational Choices

As we discuss in the main text, an important question is how our measure of reform intensity relates to educational choices and to the selection of students into different types of upper secondary programs. Our identification strategy rests on the simple idea that students were more likely to enroll in 3-year programs in municipalities with a larger share of such programs. Figure A3 displays how the probability of different educational choices at age 16 varies as a function of reform intensity and indicates that this was indeed the case.

First, an important thing to note is that the share of individuals who did not enroll in upper secondary school at age 16 was the same for all levels of reform intensity (see the area labeled *None*). Our decision to exclude this group from the analysis should

Figure A3: Educational Choices by Reform Intensity
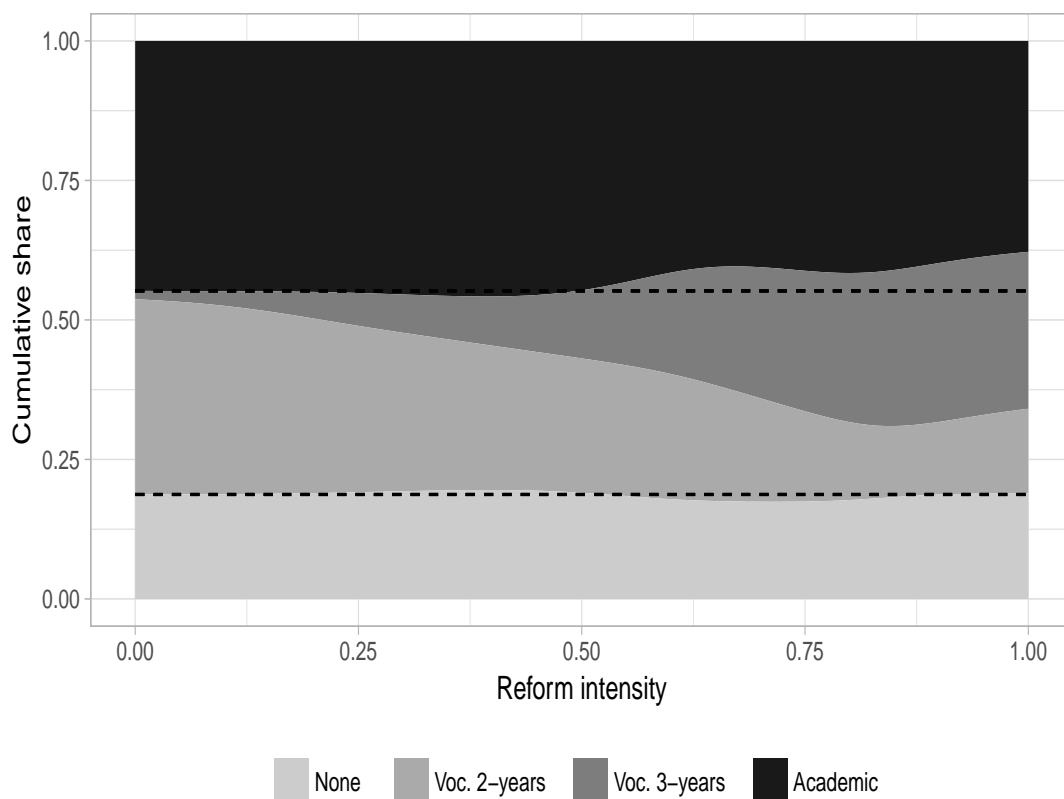
Note: The figure is a conditional density plot describing how the probability of different educational choices varies over reform intensity. The cdplot function in R (R Core Team, 2017), with a bandwidth of 0.1, has been used for the computations.

therefore not bias the estimates. Second, an as expected, the main effect of increasing reform intensity is to move students from 2-year to 3-year vocational programs (see the two areas in the middle of the graph). There is, however, also a slight tendency for students to shift from academic (the dark area) to 3-year vocational programs for high values of reform intensity. This is also the reason why we include students from both vocational and academic programs in the main analysis. Studying all upper secondary school students mitigates the risk that changes in the composition of vocational students affect our results (Åslund et al., 2017).

This being said, there are no obvious signs in the data that increased reform intensity actually altered the student composition of different programs. Figure A4, for instance, shows how the probability of enrolling in various types of programs varies over our measure

Figure A4: Educational Choices by Reform Intensity and SES

Low reform intensity

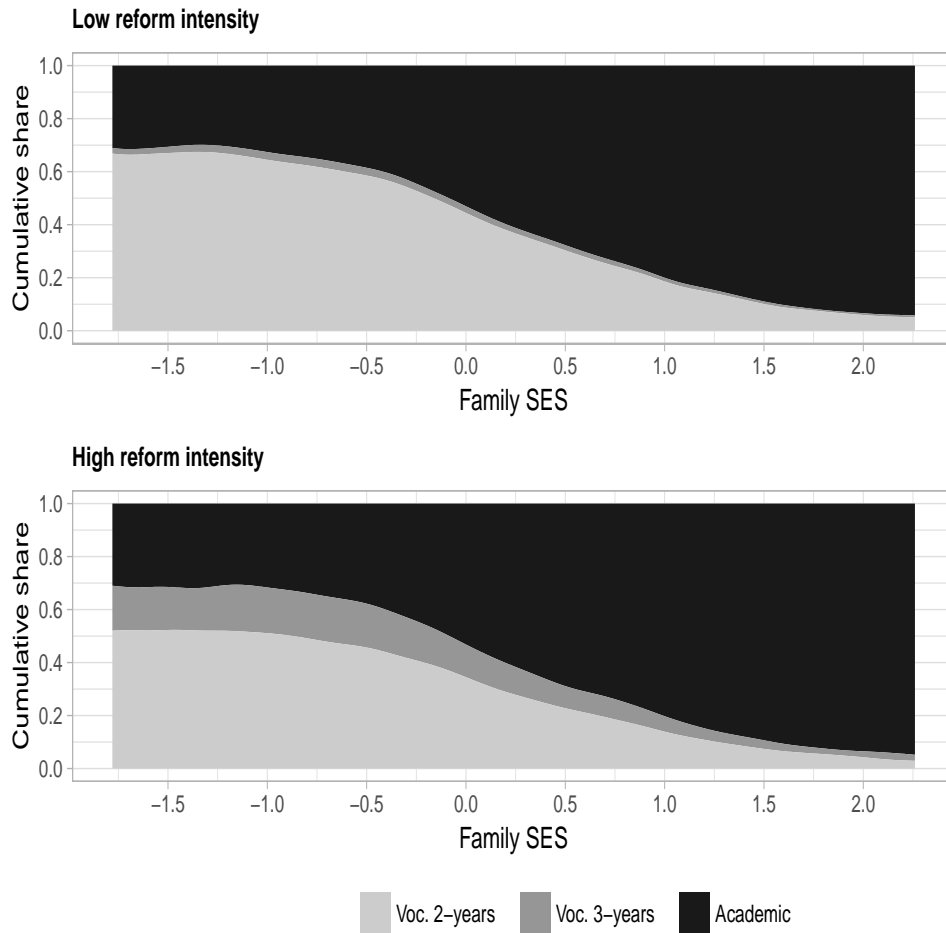

High reform intensity

Voc. 2–years   Voc. 3–years   Academic

Note: The figure is a conditional density plot describing how the probability of different educational choices varies over family SES and reform intensity. The cdplot function in R (R Core Team, 2017), with a bandwidth of 0.1, has been used for the computations.

of family SES when reform intensity is low (the upper graph) and when it is high (the lower graph).[7] The size and shape of the two dark areas in the figures are almost identical, which indicates that the socioeconomic composition of students in academic and vocational programs were the same regardless of reform intensity.

This conclusion is further corroborated by the regression results presented in Table A3. The table displays the effect of the reform on enrolling in upper secondary education (Panel A), enrolling in a vocational program (Panel B), enrolling in a 2-year vocational program (Panel C), enrolling in a 3-year vocational program (Panel D), completing a 3-

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[7]High reform intensity is here defined as values above 0.2, which is the median value of the reform indicator in the last year of the pilot.

year academic program (Panel E), or having completed at least one year of post-secondary education in 2010 (Panel F). In each case all remaining educational categories are lumped together; e.g., in Panel C we estimate the effect of reform intensity on enrolling in a 2-year vocational program rather than *not* enrolling in such a program.

As expected, the main effect of the reform was to decrease the share of individuals enrolling in 2-year vocational programs and increase those enrolling in 3-year vocational programs. We find no evidence that the reform affected the likelihood of enrolling in secondary education (Panel A). However, for some socioeconomic groups we find a small decrease in the probability of completing an academic program. Finally, in line with previous research we find that the reform did not alter the probability of pursuing post secondary education. It is only for students in the highest SES quartile that we find a small increase, but it is only statistically significant at the 0.1 level.

Table A3: The Reform and Educational Choices

	All	Q1	Q2	Q3	Q4
Panel A. Dependent Variable: Enroll in upper secondary program at age 16					
Reform intensity	−0.05	−0.52	−0.22	0.87	0.16
	(0.90)	(1.59)	(1.34)	(1.07)	(1.08)
Observations	513,656	126,404	128,440	129,434	129,378
Panel B. Dependent Variable: Enroll in vocational program					
Reform intensity	0.97	−0.00	0.87	1.16	1.59
	(1.09)	(1.92)	(1.67)	(1.83)	(1.39)
Observations	416,946	90,922	101,864	109,786	114,374
Panel C. Dependent Variable: Enroll in a 2-year vocational program					
Reform intensity	−28.80***	−39.91***	−35.57***	−26.83***	−10.64***
	(2.59)	(3.56)	(3.66)	(2.74)	(1.93)
Observations	416,946	90,922	101,864	109,786	114,374
Panel D. Dependent Variable: Enroll in a 3-year vocational program					
Reform intensity	31.59***	42.45***	39.38***	28.60***	12.84***
	(2.37)	(3.50)	(3.14)	(2.24)	(1.41)
Observations	416,946	90,922	101,864	109,786	114,374
Panel E. Dependent Variable: Completing 3-year academic track					
Reform intensity	−3.07***	−3.84**	−4.37***	−2.38	−1.84
	(1.13)	(1.87)	(1.60)	(1.72)	(2.07)
Observations	416,946	90,922	101,864	109,786	114,374
Panel F.Dependent Variable: Post-secondary education in 2010					
Reform intensity	−0.67	−0.94	−1.61	−1.88	2.72*
	(0.83)	(1.63)	(1.59)	(1.62)	(1.48)
Observations	416,425	92,994	102,440	108,964	112,027

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

A 3.3 Examining the Common Trend Assumption

In the main text we present two pieces of evidence in support for the common trend assumption. First of all, we show that whereas the school reform affected those enrolling in vocational programs it had no impact on those attending academic programs. Second, we show the results from a placebo regression in which we artificially pre- and post-date the reform by up to 15 years. For reasons of space we only discuss the placebo results for Quartile 1 in the main text. The placebo results for the remaining three quartile groups are therefore displayed here in figures A5, A6, and A7.

Figure A5: Placebo Graph, Q2
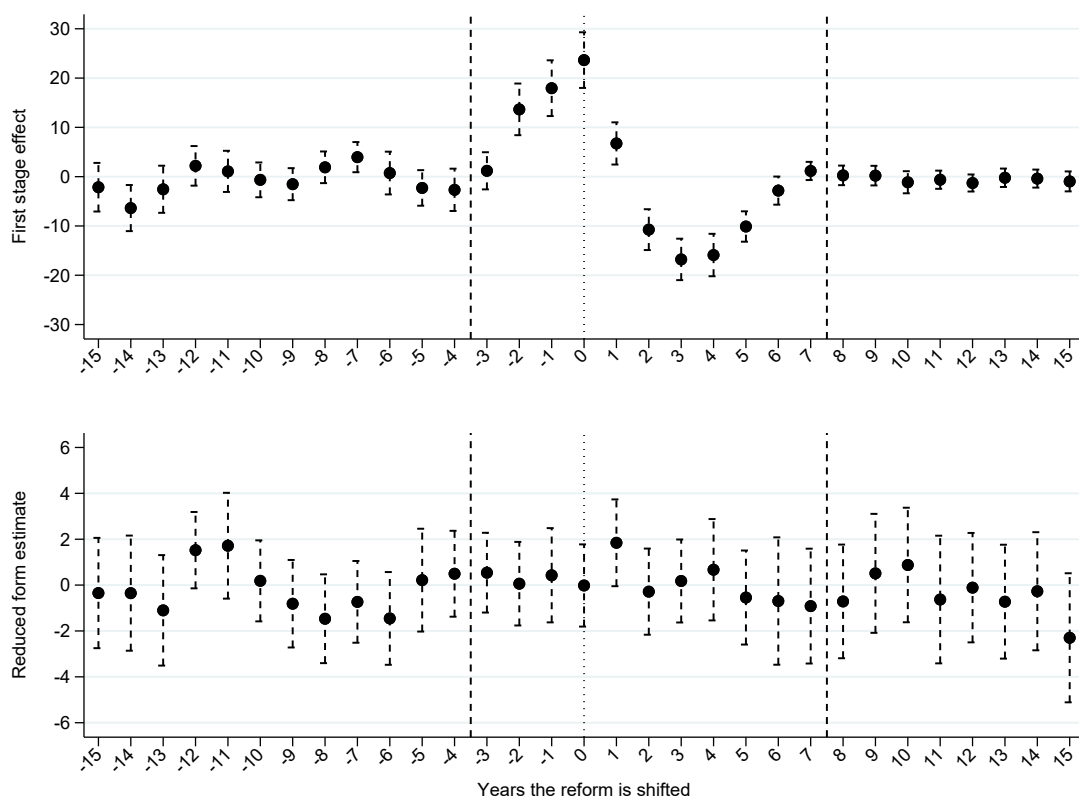
As discussed in the main text, all individuals who began upper secondary school before 1987 or after 1993 should be completely unaffected by the reform. Consequently, to obtain a pure "placebo sample" we need to pre-date the timing of the reform by at least 4 years or post-date it with at least 8 years. It is thus comforting to note that

Figure A6: Placebo Graph, Q3
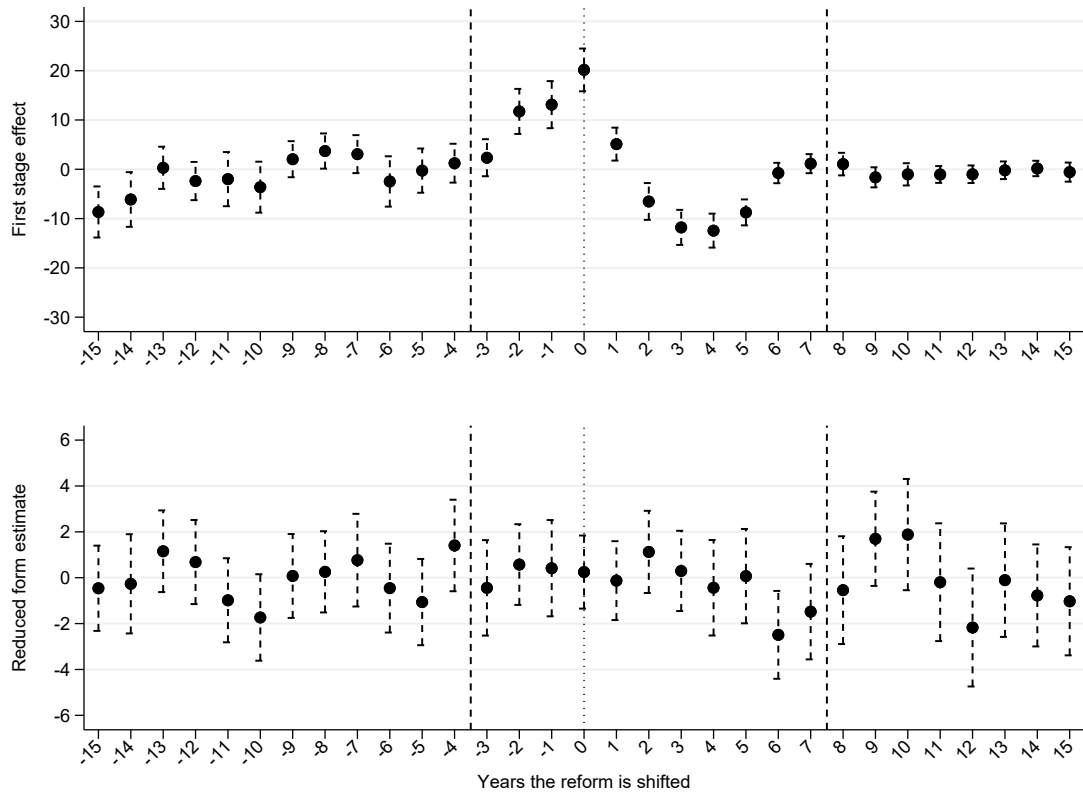
we only observe sporadic significant effects pointing in different directions for $t \leq -4$ or $t \geq 8$. Whereas the patterns of first stage estimates for Q2-Q4 are very similar to that reported for Q1 in the main text (although the magnitude of the estimates differ), we do not, as expected, find any statistically significant effect of reform intensity on voter turnout in the upper three quartiles.

Figure A7: Placebo Graph, Q4
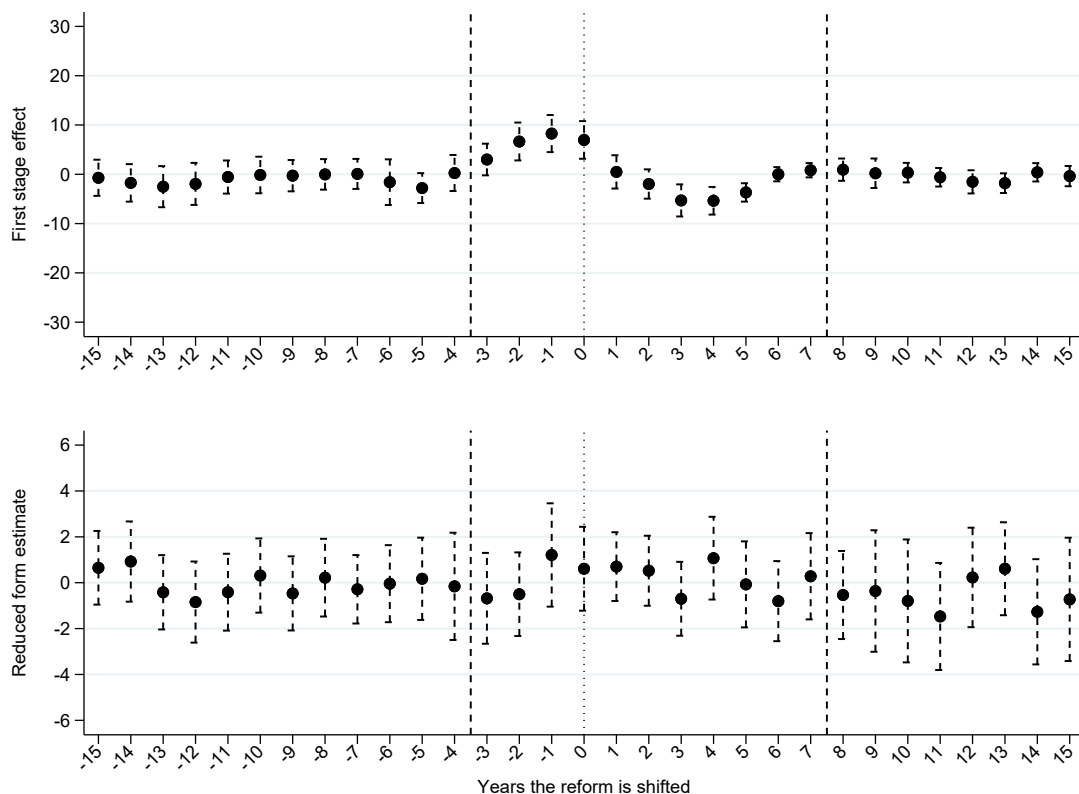
An alternative approach to evaluate the plausibility of the common trend assumption is to examine whether changes in reform intensity are related to changes in important pre-determined student characteristics. In Table A4, we therefore use our preferred model specification to regress compulsory school GPA (which is available for a subset of our sample), parental earnings, parental occupational status, parental education, and family SES on reform intensity. To ease interpretation, the first four of these variables have been standardized to have mean 0 and standard deviation 1.[8]

To judge from these results, reform intensity is not related to any important changes in pre-determined student characteristics. We find no relationship at all between reform intensity and GPA from compulsory school. And whereas we find some statistically significant, although very weak, correlations between reform intensity and the parental SES indicators for the sample as a whole, these correlations generally lose their statistical

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[8]To mitigate the risk associated with grade inflation over time, GPA was standardized by cohort.

significance once we study the different quartiles separately (which we do in the main analysis).

Table A4: Reform intensity and individual characteristics

	All	Q1	Q2	Q3	Q4
Compulsory school GPA	0.02	0.01	-0.01	0.00	0.04
	(0.02)	(0.04)	(0.03)	(0.03)	(0.03)
Parental earnings	0.03*	0.01	-0.02	0.02	-0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.03)
Parental occ. status	0.04***	-0.00	0.01	0.02	0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Parental education	0.04**	0.04	0.01	-0.01	-0.00
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Family SES	0.05***	0.02	-0.00	0.01**	-0.01
	(0.02)	(0.01)	(0.00)	(0.01)	(0.02)

Notes: All models include controls for gender and immigrant background, as well as a full set of fixed effects for birth year, home municipality, and father's and mother's birth years. Standard errors, shown in parentheses, allow for clustering at the municipality level. ***/**/*, indicates significance at the 1/5/10% level. Results are presented as percentage points.

So far we have used individual level data to assess the plausibility of the common trend assumption. But given that the reform was implemented at the municipality level we may, however, also be interested in whether there were any important signs of non-random selection into the pilot at the municipality level. Most importantly, since the implementation of the reform included elements of voluntarism, we could fear that the reform intensity could be correlated with municipality specific factors that also affected political activity. More precisely, our identification strategy hinges on the assumption that the changes in reform intensity over time is uncorrelated with *changes* in important municipality characteristics. Time-invariant differences between municipalities with high and low reform intensity will, however, not jeopardize identification since these differences will be accounted for by the municipality fixed effects included in our analyses.

What is important is thus that reform intensity is not related to *trends* in the development of municipality characteristics. In order to further evaluate this assumption

figures A8 to A13 present a series of graphs in which we have plotted trends in high and low reform intensity municipalities over time in the upper panels. We have used the median reform intensity in 1990 (0.2) as our cut off to distinguish high reform intensity municipalities from low reform intensity municipalities. In the lower panel in the graphs we present the difference between the high and low reform intensity municipalities for the respective factors, as well as the 95 percent confidence bounds. The crucial thing here is that any differences between the high and low intensity municipalities stay similar over time since this would indicate that the trends in high reform intensity municipalities are not significantly different from the trends in low intensity municipalities.

Looking first at the amount of support for labor parties in Figure A8 it is evident that the support for Labor Parties (the Left Party and the Social Democrats) was somewhat higher in high intensity municipalities. However, this difference is stable over the entire time span for which we have data (1976-2010) and we find no trend differences between the two types of municipalities.

Next, looking at voter turnout in Figure A9 the trends are very similar and the differences are indistinguishable. This is even more evident in the lower bar of the graph indicating that the trends are parallel over time.

Figures A10 to Figure A13 show the trend for four socioeconomic indicators from 1970 to 2010 (measured every fifth year). The graphs display trends in educational attainment (the mean years of education) in Figure A10, the employment share in Figure A11, the share of citizens with immigrant background in Figure A12 and occupational status (the mean occupational status among individuals in the same category) in Figure A13. All of these measures are calculated for the adult population in a municipality (i.e., residents who are 18 years or older).

For all four socioeconomic indicators we find strikingly small differences between low and high reform intensity municipalities, and the differences that do exist are very stable over time. The only partial exception is the share with immigrant background for which the difference tend to become slightly larger over time. The absolute magnitude of this

change is, however, small, in particular in relation to the size of the confidence intervals. Moreover, we control for immigrant background in our analyses, so any trend shift in this variable that coincides with changes in reform intensity should be accounted for.

Figure A8: Reform Intensity and Left Party Support, Municipality Level

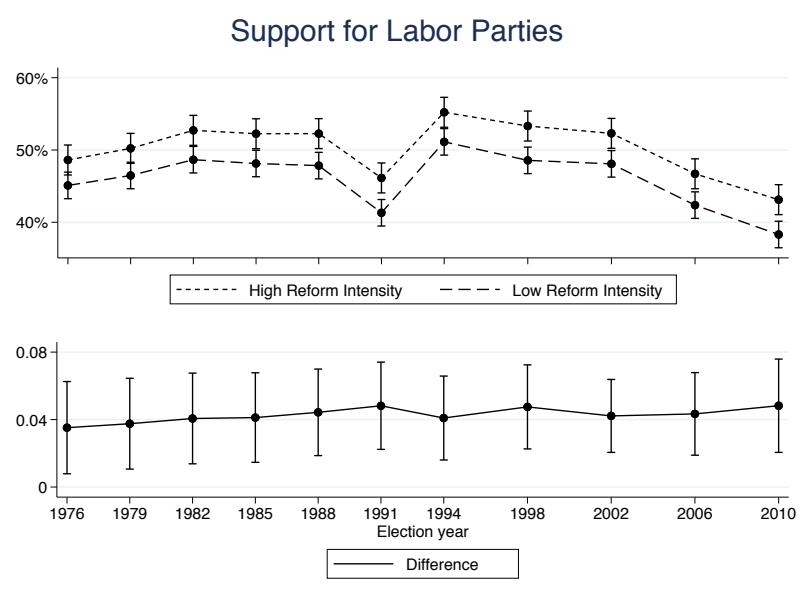Figure A9: Reform Intensity and Voter Turnout, Municipality Level
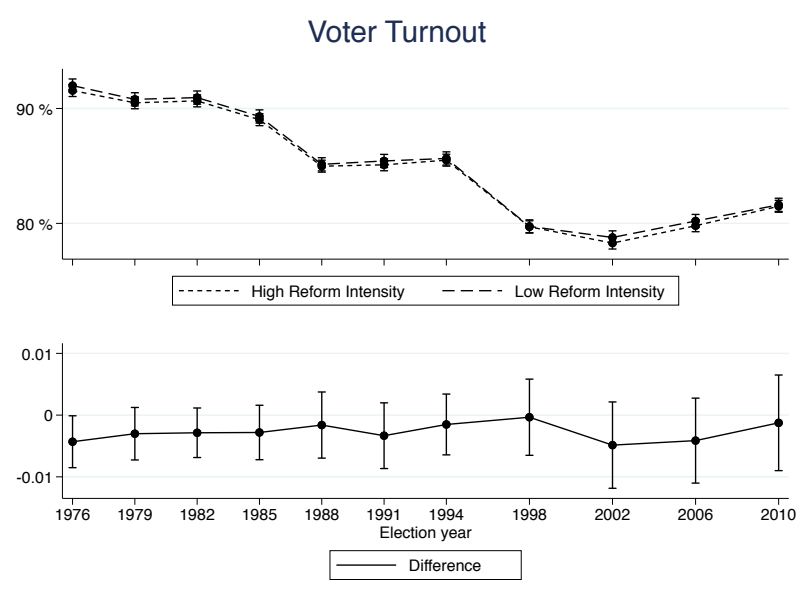
Figure A10: Reform Intensity and Educational Attainment, Municipality Level

Educational attainment
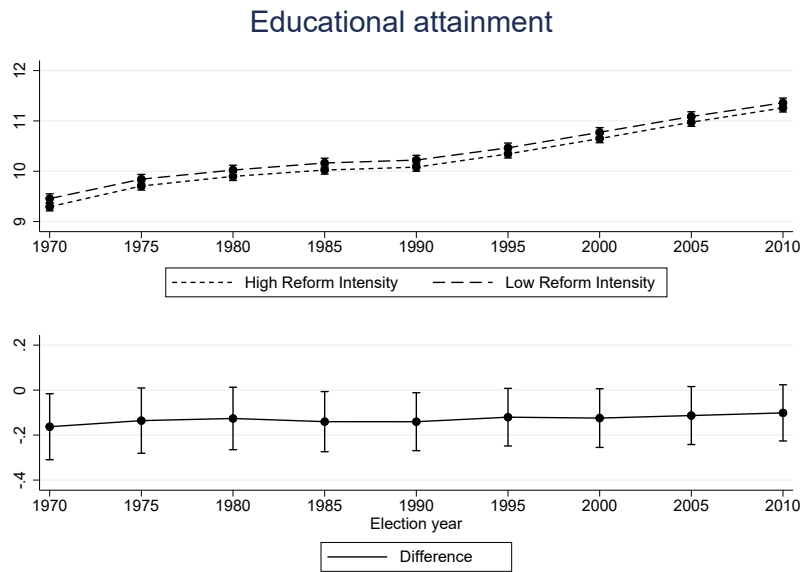
Figure A11: Reform Intensity and Employment Share, Municipality Level

Employment share
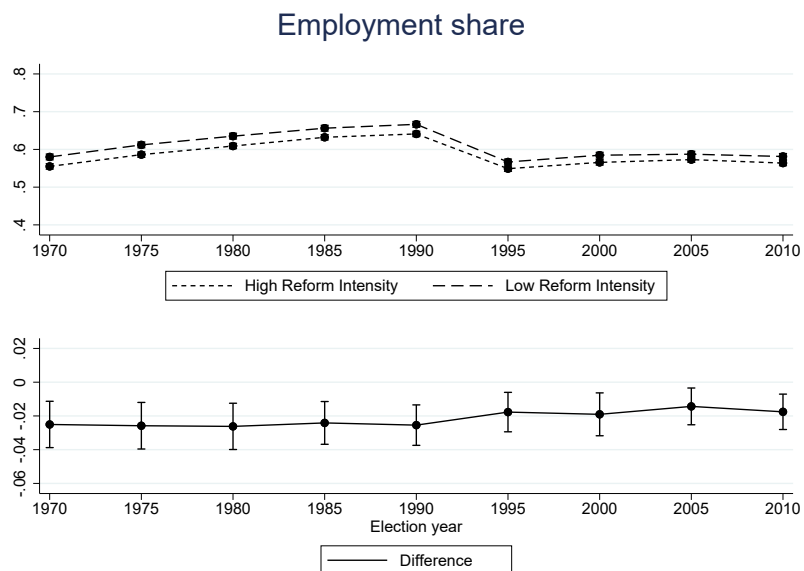
Figure A12: Reform Intensity and Immigrant Background Share, Municipality Level
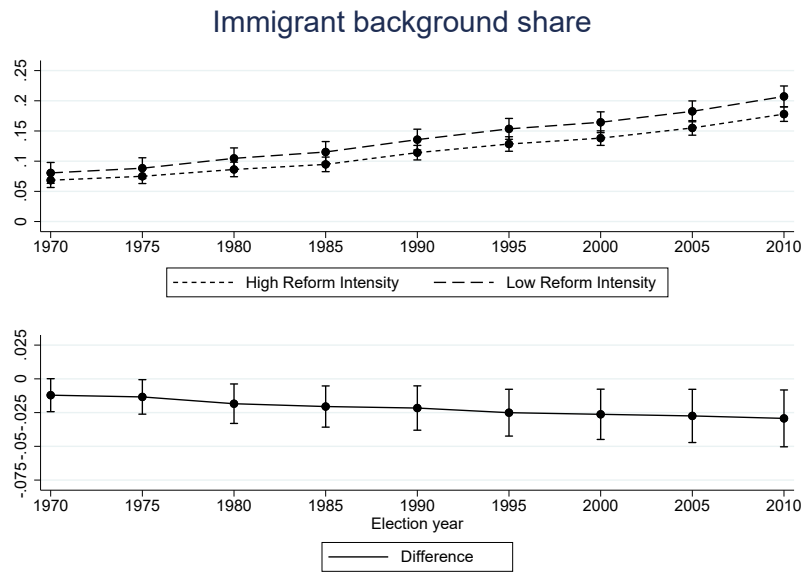
Figure A13: Reform Intensity and Occupational Status, Municipality Level
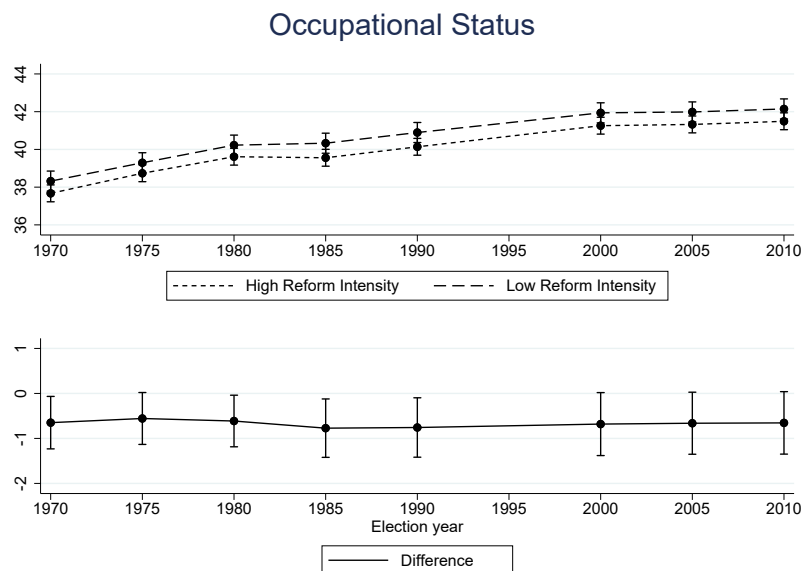
A 3.4 Results: Vocational Students

In the main analysis we study all individuals born between 1970 and 1974 enrolling in upper secondary school between 1986 and 1990, despite the fact that the pilot did not affect the length and content of the academic training programs. The main advantage with this approach is that we need to be less worried that the results are driven by compositional changes between different types of programs. Yet, given that students in vocational training programs were those primarily affected by the reform we should expect the effect on voting to be more pronounced in this group. In Table A5 we therefore present separate results for individuals who enrolled in a vocational upper secondary program at age 16.

Overall, the results for the sample focusing on individuals attending vocational programs are very similar to those obtained for the full sample. As was the case in the main analysis the pattern of results in Table A5 indicates that the positive reform effect on turnout is restricted to the lower end of the SES distribution. The main difference is that the coefficient of the reform intensity variable for the lowest quartile group (Q1) increases from 3.1 to 4.5, which is to be expected since it was the vocational programs that were affected by the reform. It is also noteworthy that the IV estimate of the effect of completing a 3-year program (11.8) is very similar to the one obtained in the full sample (11.6).

Table A5: Reform Effect on Voter Turnout, Vocational Programs

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
(Reduced form)					
Reform intensity	0.61	4.48***	−0.43	−2.34	−2.84
	(0.84)	(1.42)	(1.30)	(1.59)	(2.41)
Gender	2.84***	4.01***	2.89***	1.93***	1.11***
	(0.18)	(0.33)	(0.29)	(0.31)	(0.43)
Immigrant background	−6.65***	−7.73***	−6.40***	−5.53***	−4.95***
	(0.32)	(0.46)	(0.52)	(0.57)	(0.77)
Family SES	3.25***				
	(0.14)				
Panel B. Dependent variable: $\geq$ 3 years of post-primary educ. at age 20					
(First-stage 2SLS)					
Reform intensity	36.55***	38.13***	36.40***	37.96***	30.19***
	(3.45)	(3.75)	(3.87)	(4.13)	(4.86)
Gender	−2.69***	−2.46***	−3.21***	−2.72***	−2.01**
	(0.57)	(0.55)	(0.60)	(0.78)	(0.90)
Immigrant background	−1.12***	−0.98**	−0.94*	−1.32**	−2.70***
	(0.31)	(0.46)	(0.49)	(0.53)	(1.03)
Family SES	4.58***				
	(0.17)				
Panel C. Dependent variable: Voting.					
(Second-stage 2SLS)					
Completed 3-year program	1.66	11.75***	−1.18	−6.18	−9.41
	(2.32)	(3.86)	(3.55)	(4.30)	(7.87)
Gender	2.88***	4.30***	2.85***	1.76***	0.92**
	(0.20)	(0.35)	(0.30)	(0.34)	(0.45)
Immigrant background	−6.63***	−7.61***	−6.41***	−5.61***	−5.20***
	(0.32)	(0.47)	(0.52)	(0.57)	(0.82)
Family SES	3.17***				
	(0.17)				
Observations	186,764	60,900	59,516	44,851	21,497

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

A 3.5 Results: Entire Cohorts

In our main empirical analysis we excluded all individuals not enrolling in upper secondary education at age 16. In Table A3 above we presented results indicating that the reform did not affect the probability of enrolling in upper secondary school. However, in order to check whether our results are driven by any lingering compositional effects of the reform Table A6 presents estimates based on all students completing compulsory schooling between 1986 and 1990, including those that did not enroll in upper secondary school at age 16. The pattern of estimates in Table A6 is very similar to the one presented in the main analysis. Thus, these results do not provide any cause for concern.

Table A6: Reform Effect on Voter Turnout, All Individuals

|	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
(*Reduced form*)					
Reform intensity	0.71	3.45***	−0.76	−0.20	0.09
	(0.50)	(1.11)	(0.90)	(0.84)	(0.98)
Gender	2.39***	4.01***	3.21***	1.75***	0.59***
	(0.11)	(0.21)	(0.20)	(0.17)	(0.12)
Immigrant background	−6.44***	−8.26***	−6.27***	−5.01***	−4.93***
	(0.26)	(0.42)	(0.33)	(0.37)	(0.28)
Family SES	3.84***				
	(0.10)				
Panel B. Dependent variable: $\geq$ 3 years of post-primary educ. at age 20					
(*First-stage 2SLS*)					
Reform intensity	17.51***	21.88***	19.63***	17.80***	6.67***
	(1.97)	(2.75)	(2.59)	(2.05)	(2.03)
Gender	2.98***	3.78***	3.77***	3.55***	0.90***
	(0.31)	(0.41)	(0.46)	(0.44)	(0.33)
Immigrant background	0.57	2.28***	1.95***	−0.76	−4.53***
	(0.41)	(0.57)	(0.61)	(0.65)	(0.39)
Family SES	19.31***				
	(0.14)				
N	513656	126404	128440	129434	129378
Panel C. Dependent variable: Voting.					
(*Second-stage 2SLS*)					
Completed 3-year program	4.05	15.76***	−3.88	−1.14	1.42
	(2.94)	(5.30)	(4.51)	(4.75)	(14.70)
Gender	2.27***	3.42***	3.35***	1.79***	0.57***
	(0.14)	(0.30)	(0.26)	(0.25)	(0.18)
Immigrant background	−6.46***	−8.62***	−6.19***	−5.02***	−4.86***
	(0.27)	(0.47)	(0.33)	(0.37)	(0.73)
Family SES	3.06***				
	(0.57)				
Observations	513,656	126,404	128,440	129,434	129,378

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

A 3.6 Results: Municipalities With Vocational Schools

As mentioned in the main text, it is not obvious how to define reform intensity in municipalities where no vocational programs were offered. However, municipalities without own upper secondary schools usually had an agreement with a neighboring municipality that their students could attend upper secondary school there. In the main analysis we therefore assigned the students in municipalities without vocational programs a "school municipality" based on information on where the majority of the vocational students in the 1970 cohort went to school (more formally we use the modal value to identify school municipality).

In Figure A14 we show how educational choices vary with reform intensity in municipalities with and without vocational schools. As can be seen the overall pattern looks very similar in these two types of municipalities. Moreover, the share of individuals attending 3-year tracks is closely related to reform intensity also in municipalities that lack vocational programs, which indicates that the procedure that we use to identify "school municipalities" works very well.

Figure A14: Upper secondary program by reform intensity and municipality type
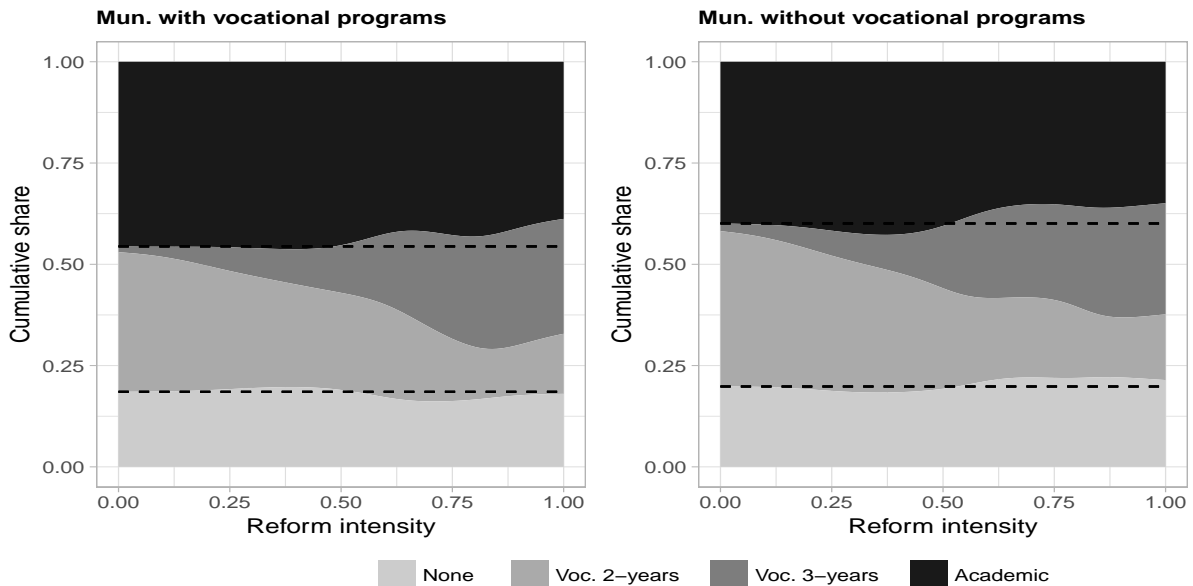
Note: The figure is a conditional density plot describing how the probability of different educational choices varies over reform intensity. The cdplot function in R (R Core Team, 2017), with a bandwidth of 0.1, has been used for the computations.

Nonetheless, to make sure that our results are not unduly driven by our procedure to handle municipalities without vocational schools we have re-estimated our main results including only the 185 municipalities that offered vocational programs in all years between 1986 and 1990. The results are reported in Table A7. The results for this restricted sample are very similar to those obtained for the larger sample in the main analysis.

Table A7: Results, Municipalities With Vocational Schools

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
Reform intensity	0.65	3.55***	−0.63	0.54	−0.71
	(0.57)	(1.17)	(1.06)	(0.98)	(1.05)
Gender	2.03***	3.88***	2.91***	1.43***	0.45***
	(0.12)	(0.26)	(0.22)	(0.20)	(0.14)
Immigrant background	−5.71***	−7.69***	−5.73***	−4.31***	−4.41***
	(0.28)	(0.42)	(0.42)	(0.42)	(0.32)
Family SES	2.76***				
	(0.09)				
Panel B. Dependent variable: At least 3 years of post-primary educ. at age 20.					
Reform intensity	19.91***	26.50***	22.72***	20.64***	6.37***
	(2.29)	(3.75)	(3.30)	(2.20)	(2.11)
Gender	3.74***	5.33***	4.93***	4.35***	0.87**
	(0.36)	(0.59)	(0.52)	(0.48)	(0.38)
Immigrant background	1.21**	3.65***	2.69***	−0.08	−2.98***
	(0.47)	(0.69)	(0.71)	(0.77)	(0.38)
Family SES	18.03***				
	(0.17)				
Panel C. Dependent variable: Voting.					
Completed 3-year program	3.25	13.39***	−2.77	2.59	−11.08
	(2.87)	(4.52)	(4.65)	(4.74)	(17.42)
Gender	1.91***	3.17***	3.05***	1.32***	0.54**
	(0.16)	(0.38)	(0.33)	(0.28)	(0.22)
Immigrant background	−5.75***	−8.18***	−5.66***	−4.31***	−4.74***
	(0.28)	(0.48)	(0.43)	(0.42)	(0.62)
Family SES	2.18***				
	(0.52)				
Observations	356,595	74,752	85,571	94,814	101,278

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

A 3.7 Results: Using Logit Regression

For reasons discussed in the main text, we decided to use a linear probability model in our main empirical analysis. However, in Table A8 we estimate both the reduced form and the first-stage equations by means of a logit regression. The coefficients reported in the

table are odds-ratios. Moreover average marginal effects – which are directly comparable to the coefficient estimates from the linear probability model estimated in the main text – are presented within brackets.

As is evident from comparing these results with those presented in the main text, we obtain very similar results when using a logit model instead of a linear probability model. Unfortunately, there is no easy analog to the 2SLS model in the case when both the outcome of interest and the endogenous variable are binary. Available options, such as the bivariate probit, rest on very stringent identification assumptions and have proved to be difficult to estimate due to their numerical instability (Freedman and Sekhon, 2010). In line with these theoretical results, when attempting to estimate a bivariate probit model we had problems to get the models to converge for some groups and the results that we did obtain tended to be highly sensitive to different model specifications and sample restrictions (unlike the first-stage and reduced form results presented in Table A8). We therefore decided not to report these results here.

On a more substantive note, the finding that the positive effect of the reform is restricted to individuals from low SES-homes holds true also when interpreting the logit coefficients in terms of odds-ratios. This suggests that the lower return to education in higher SES-groups is not primarily due to a ceiling effect since odds-ratios, unlike probabilities, are not affected by the mean of the dependent variable (Mare, 1980).

Table A8: Logit Results, Odds ratios and Marginal Effects

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
Reform intensity	1.08	1.30***	0.99	0.98	0.98
	[0.64]	[3.09]	[−0.13]	[−0.16]	[−0.10]
Gender	1.26***	1.38***	1.34***	1.18***	1.09***
	[2.04]	[3.82]	[2.88]	[1.34]	[0.57]
Immigrant background	0.59***	0.57***	0.60***	0.62***	0.56***
	[−4.74]	[−6.61]	[−5.03]	[−3.83]	[−3.73]
Family SES	1.39***				
	[2.91]				
Observations	416,928	90,902	101,835	109,739	114,278
Panel B. Dependent variable: ≥ 3 years of post-primary educ. at age 20.					
Reform intensity	2.64***	3.19***	2.68***	2.57***	1.64***
	[20.43]	[24.83]	[22.68]	[21.75]	[8.52]
Gender	1.21***	1.30***	1.25***	1.22***	1.06***
	[3.97]	[5.56]	[5.10]	[4.68]	[1.04]
Immigrant background	1.05**	1.16***	1.10***	0.99	0.84***
	[1.09]	[3.16]	[2.27]	[−0.34]	[−2.98]
Family SES	2.40***				
	[18.39]				
Observations	416,940	90,918	101,857	109,771	114,358

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

A 3.8 Results: Disaggregating the SES Components

We have also examined the sensitivity of our results with respect to our measure of family background. In line with much previous research we have used a composite measure of SES in our main analysis. Although we believe that there are good theoretical reasons for doing so, it can nevertheless be interesting to disaggregate the effect of family SES into its component parts. Towards this end, we have estimated separate models for each of the three sub-items making up our family SES measure. The results are reported in Tables A9, A10 and A11. As can be seen the reform effect in the first family

background quartile shrinks somewhat in magnitude (the coefficients range from 2.1 to 2.8 percentage points) when considering the different indicators in isolation as compared to the corresponding effect when using the composite SES indicators (3.1). This could be taken to indicate that the individuals that are situated in the bottom quartile on one of these variables, e.g., parental earnings, is on average less disadvantaged than individuals that are situated in the bottom quartile of the composite measure (since the negative effect of low parental earnings may be offset by somewhat higher parental education or occupational status). This being said, the results of this disaggregated analysis closely mimics the results we obtain when using the composite index.

Table A9: Results Measuring Family SES by Parental Earnings

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
(*Reduced form*)					
Reform intensity	0.74	2.14**	0.65	0.04	0.08
	(0.52)	(1.02)	(0.93)	(0.83)	(1.25)
Gender	2.04***	3.13***	2.57***	1.91***	0.71***
	(0.11)	(0.24)	(0.20)	(0.19)	(0.16)
Immigrant background	−5.88***	−7.35***	−5.94***	−5.36***	−4.67***
	(0.26)	(0.39)	(0.41)	(0.36)	(0.30)
Parental earnings	0.00***				
	(0.00)				
Panel B. Dependent variable: ≥ 3 years of post-primary educ. at age 20					
(*First-stage 2SLS*)					
Reform intensity	19.96***	22.72***	23.82***	19.44***	8.29***
	(2.03)	(2.91)	(2.78)	(2.16)	(1.85)
Gender	4.03***	4.77***	4.71***	4.65***	2.11***
	(0.32)	(0.52)	(0.44)	(0.42)	(0.35)
Immigrant background	−0.43	1.61***	0.14	−1.91***	−4.23***
	(0.42)	(0.58)	(0.63)	(0.58)	(0.49)
Parental earnings	0.03***				
	(0.00)				
Panel C. Dependent variable: Voting.					
(*Second-stage 2SLS*)					
Completed 3-year program	3.70	9.43**	2.71	0.22	0.93
	(2.59)	(4.37)	(4.01)	(4.28)	(15.02)
Gender	1.89***	2.68***	2.44***	1.89***	0.69*
	(0.15)	(0.32)	(0.28)	(0.27)	(0.36)
Immigrant background	−5.86***	−7.50***	−5.95***	−5.36***	−4.63***
	(0.26)	(0.41)	(0.41)	(0.38)	(0.71)
Parental earnings	0.00***				
	(0.00)				
Observations	416,946	93,044	104,640	108,779	110,483

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

Table A10: Results Measuring Family SES by Parental Occupational Status

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
(Reduced form)					
Reform intensity	0.91*	2.83**	0.26	0.25	0.48
	(0.50)	(1.18)	(1.30)	(0.93)	(1.01)
Gender	2.03***	3.45***	3.14***	1.56***	0.62***
	(0.11)	(0.24)	(0.24)	(0.17)	(0.16)
Immigrant background	−5.68***	−7.39***	−5.88***	−3.98***	−4.93***
	(0.26)	(0.44)	(0.39)	(0.36)	(0.33)
Parental occ. status	0.13***				
	(0.00)				
Panel B. Dependent variable: ≥ 3 years of post-primary educ. at age 20					
(First-stage 2SLS)					
Reform intensity	20.10***	24.91***	23.10***	20.35***	9.06***
	(1.97)	(2.99)	(2.92)	(2.08)	(1.91)
Gender	4.16***	4.88***	5.98***	4.42***	1.73***
	(0.33)	(0.53)	(0.50)	(0.37)	(0.41)
Immigrant background	0.38	2.54***	2.28***	−0.32	−2.96***
	(0.37)	(0.63)	(0.66)	(0.57)	(0.43)
Parental occ. status	0.87***				
	(0.01)				
Panel C. Dependent variable: Voting.					
(Second-stage 2SLS)					
Completed 3-year program	4.55*	11.34**	1.13	1.24	5.28
	(2.52)	(5.05)	(5.63)	(4.58)	(10.86)
Gender	1.84***	2.89***	3.08***	1.51***	0.52**
	(0.15)	(0.35)	(0.41)	(0.27)	(0.25)
Immigrant background	−5.70***	−7.68***	−5.90***	−3.98***	−4.78***
	(0.26)	(0.47)	(0.40)	(0.37)	(0.45)
Parental occ. status	0.09***				
	(0.02)				
Observations	387,204	79,308	81,796	119,751	106,349

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

Table A11: Results Measuring Family SES by Parental Education

	All	Q1	Q2	Q3	Q4
Panel A. Dependent variable: Voting.					
(*Reduced form*)					
Reform intensity	0.72	2.19**	−0.17	0.89	−0.08
	(0.52)	(1.07)	(1.03)	(0.99)	(0.98)
Gender	2.05***	3.51***	2.58***	1.46***	0.65***
	(0.11)	(0.22)	(0.21)	(0.16)	(0.16)
Immigrant background	−6.05***	−7.44***	−5.96***	−5.41***	−5.27***
	(0.27)	(0.46)	(0.34)	(0.36)	(0.42)
Parental educ. status	0.68***				
	(0.02)				
Panel B. Dependent variable: ≥ 3 years of post-primary educ. at age 20					
(*First-stage 2SLS*)					
Reform intensity	19.68***	26.42***	21.44***	18.69***	8.81***
	(2.01)	(3.30)	(2.72)	(1.95)	(2.12)
Gender	4.10***	6.20***	5.38***	3.88***	0.57
	(0.33)	(0.50)	(0.48)	(0.37)	(0.40)
Immigrant background	−1.28***	1.44***	−0.78	−2.76***	−4.25***
	(0.28)	(0.48)	(0.59)	(0.49)	(0.44)
Parental educ. status	5.05***				
	(0.04)				
Panel C. Dependent variable: Voting.					
(*Second-stage 2SLS*)					
Completed 3-year program	3.67	8.29**	−0.79	4.77	−0.88
	(2.66)	(4.08)	(4.81)	(5.21)	(11.17)
Gender	1.90***	2.99***	2.62***	1.27***	0.66***
	(0.15)	(0.33)	(0.33)	(0.26)	(0.17)
Immigrant background	−6.00***	−7.56***	−5.97***	−5.28***	−5.31***
	(0.27)	(0.46)	(0.34)	(0.42)	(0.61)
Parental educ. status	0.49***				
	(0.13)				
Observations	416,413	92,137	114,018	124,702	85,556

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

A 3.9 Treatment Heterogeneity Across Family SES

In the main text we present graphs displaying results from a flexible regression model in which a cubic spline function of family SES is interacted with reform intensity. In these models we use a spline regression with 5 knots. Below we present a set of graphs (Figures A15 through A20) showing results from a number of spline regressions corresponding to the ones presented in the main text in order to check the degree to which our results are dependent on the number of knots used. Figures A15, A17 and A19 are based on all students enrolled in upper secondary school whereas Figures A16, A18 and A20 only include students on vocational programs. Each panel displays results from four different interaction specifications: the reform indicator is i) linearly interacted with family SES; ii) interacted with a cubic spline function with 3 knots; iii) interacted with a cubic spline function with 5 knots; and iv) interacted with a cubic spline function with 7 knots. The vertical dotted lines correspond to the first, second and third quartiles of the family SES variable.

Two things should be noted in these graphs. First of all, irrespective of specification and number of knots in the spline regressions the reform effect is stronger in the lower end of the family SES distribution. Second, the linear and 3-knot spline specifications cannot fully capture the evident nonlinear treatment effects and IV estimates. The choice between using five or more knots in the spline regression is, admittedly, somewhat arbitrary. However, in order to reduce the risk for overfitting we settled for presenting the results using 5 knots in the main text.

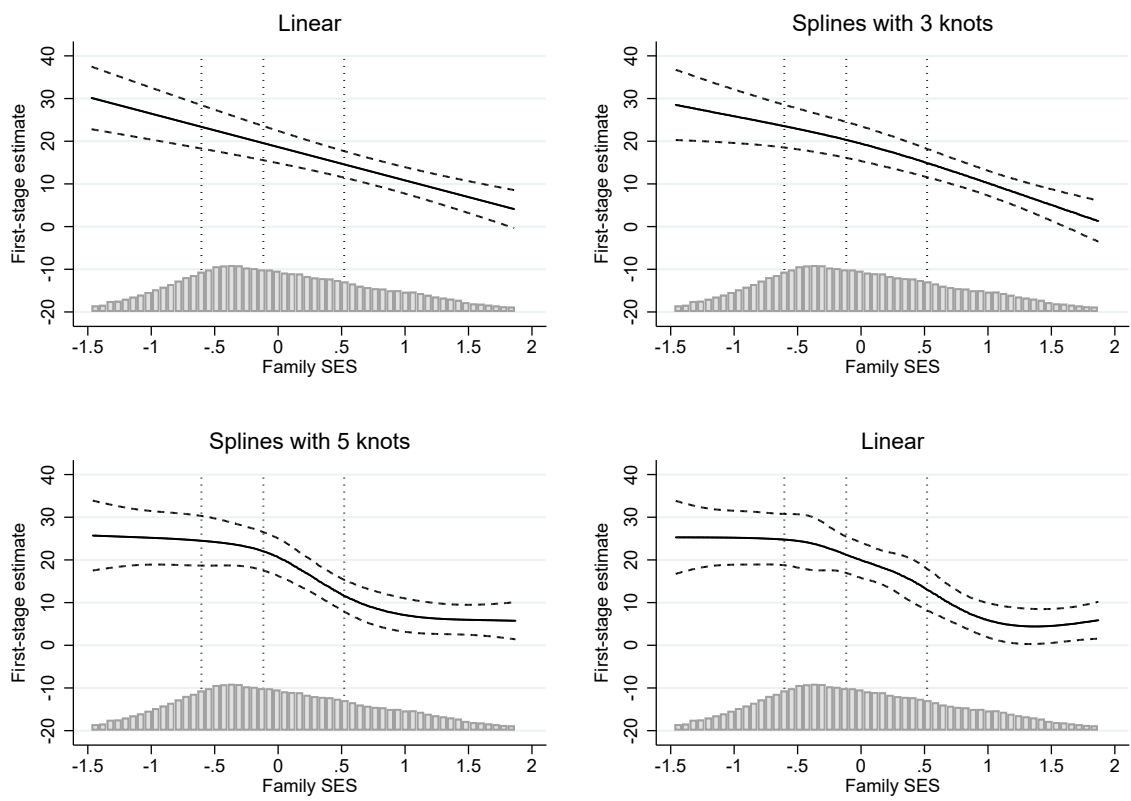Figure A15: First Stage Estimates, Vocational and Academic Programs

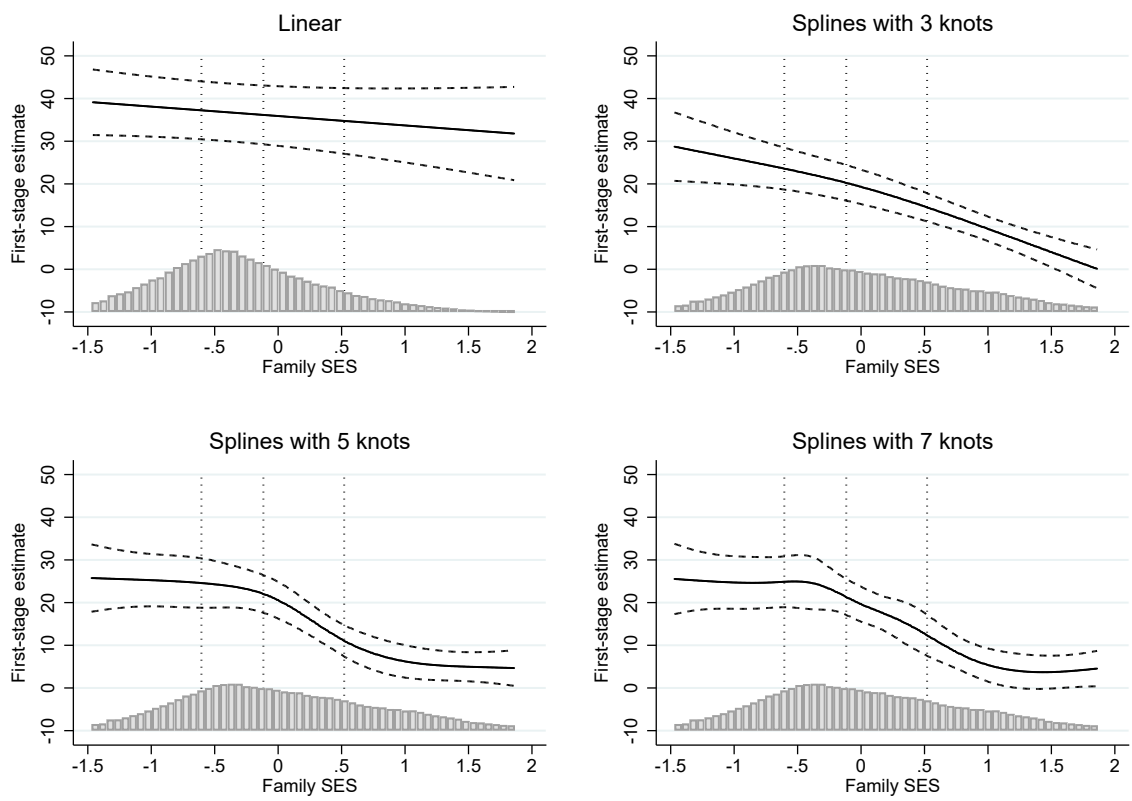Figure A16: First Stage Estimates, Vocational Programs

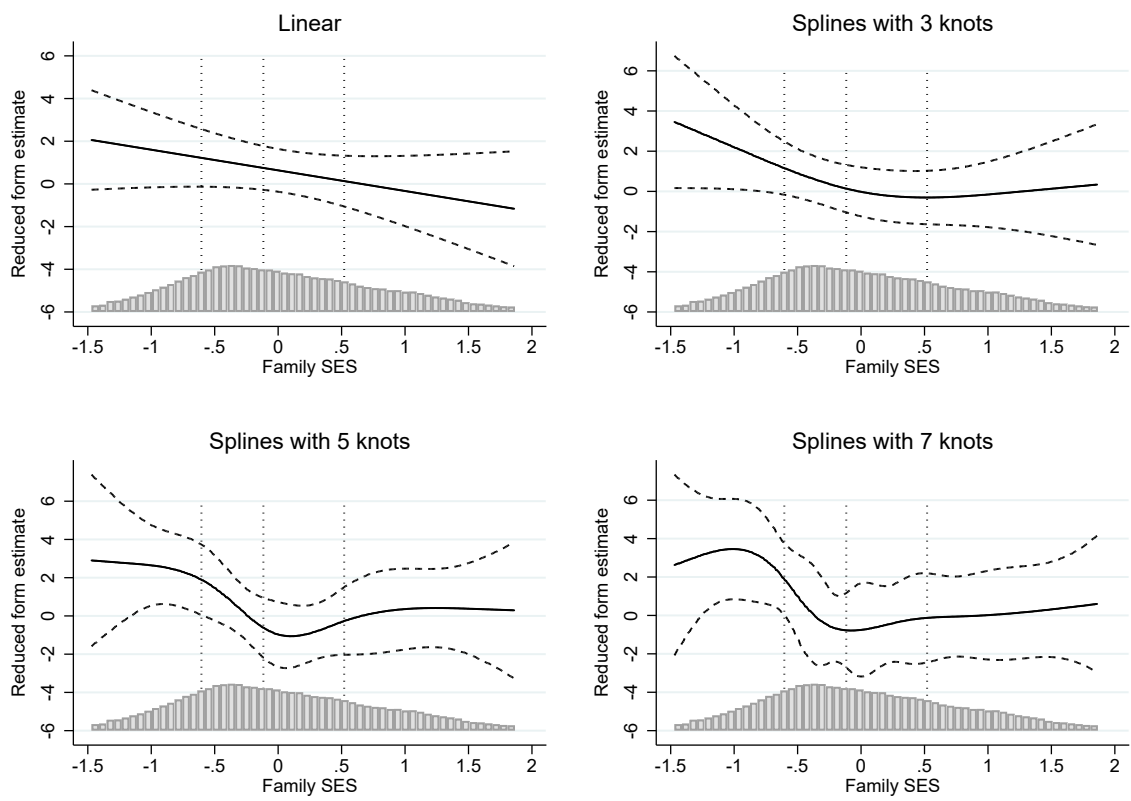Figure A17: Reduced Form Estimates, Vocational and Academic Programs

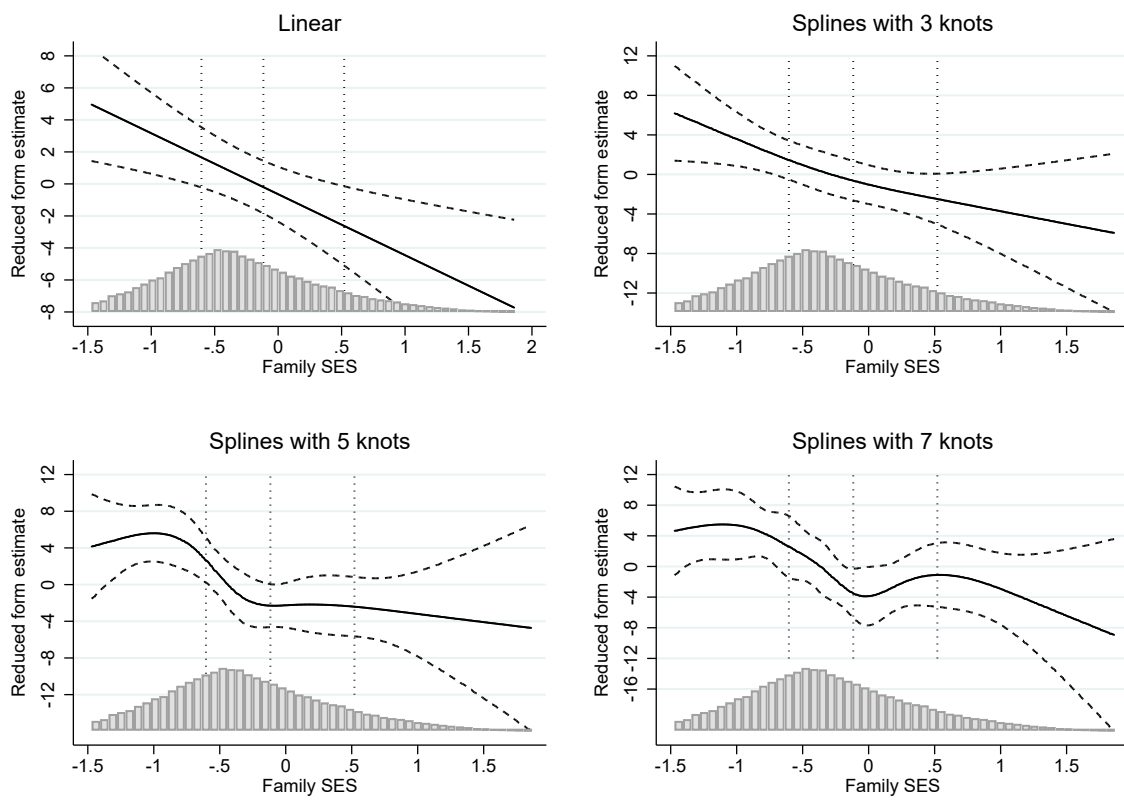Figure A18: Reduced Form Estimates, Vocational Programs

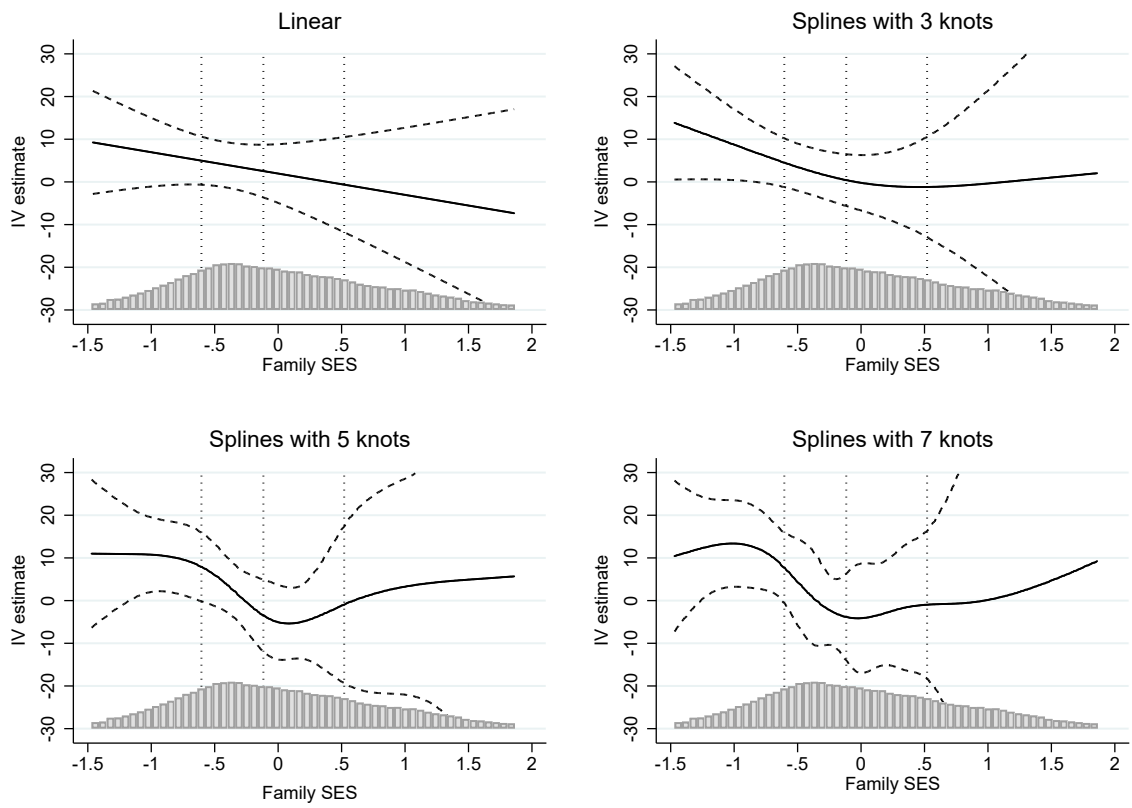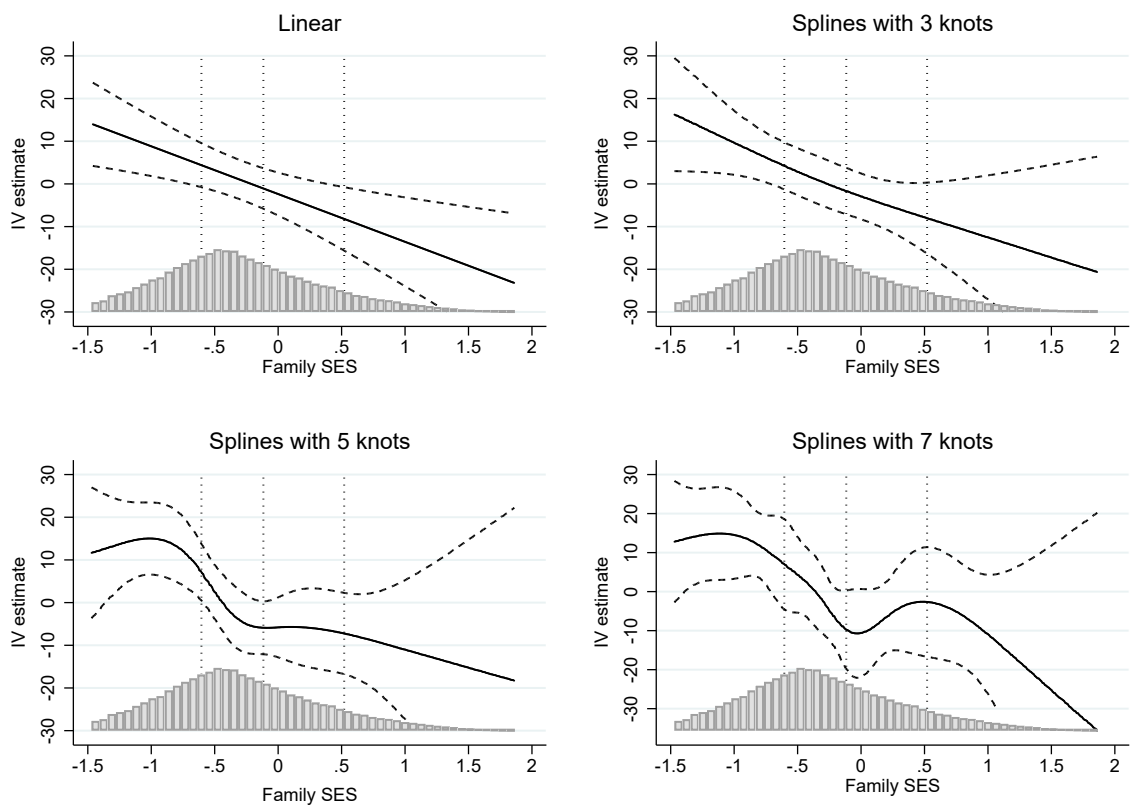Figure A19: IV Estimates, Vocational and Academic Programs

Figure A20: IV Estimates, Vocational Programs

A 3.10 Examining the Linearity Assumption

As discussed in the main text, by using 2SLS with a continuous instrument we have implicitly invoked the assumptions that the treatment effect of interest is (conditionally) linear and homogeneous. We analyze the tenability of the latter assumption in the main text. In this section we will focus on the assumption of linearity.

The best way to assess the assumption of linearity is often by means of graphical inspection. We will therefore provide simple partial regression plots that can be used to determine whether the first stage and the reduced form appear approximately linear within each SES quartile.

The graphs display the relationships between, on the one hand, reform intensity and, on the other, having completed a 3-year upper secondary program at age 20 (Figure A21) and voter turnout (Figure A22). In both panels we show separate graphs for each family SES quartile. The graphs are based on a first degree local polynomial regressions of residuals from regressions of reform intensity (the x-axes), completing a 3-year program (y-axes, Figure A21) and voter turnout (y-axes, Figure A22) on a set of municipal and cohort fixed effects. We also include scatter plots displaying the relationship between the reform indicator and the outcomes (jittered dummy indicators for completing a 3-year program (Figure A21) and voter turnout (Figure A22)). These graphs thus show the relationship between the outcome of interest and reform intensity once municipality and cohort fixed effects have been adjusted for.

In line with the results presented in the main text the graphs indicate that reform intensity is more strongly associated with both completing a 3-year upper secondary program (Figure A21) and, in particular, voter turnout (Figure A22) among individuals in the lowest SES quartile. More important for our purposes, though, there are no strong signs of non-linearities in these graphs (the only signs of non-linearities are in the extreme tails of the reform intensity variable). We thus find that the relationships between reform intensity and both outcomes are approximately linear within each SES quartile group.

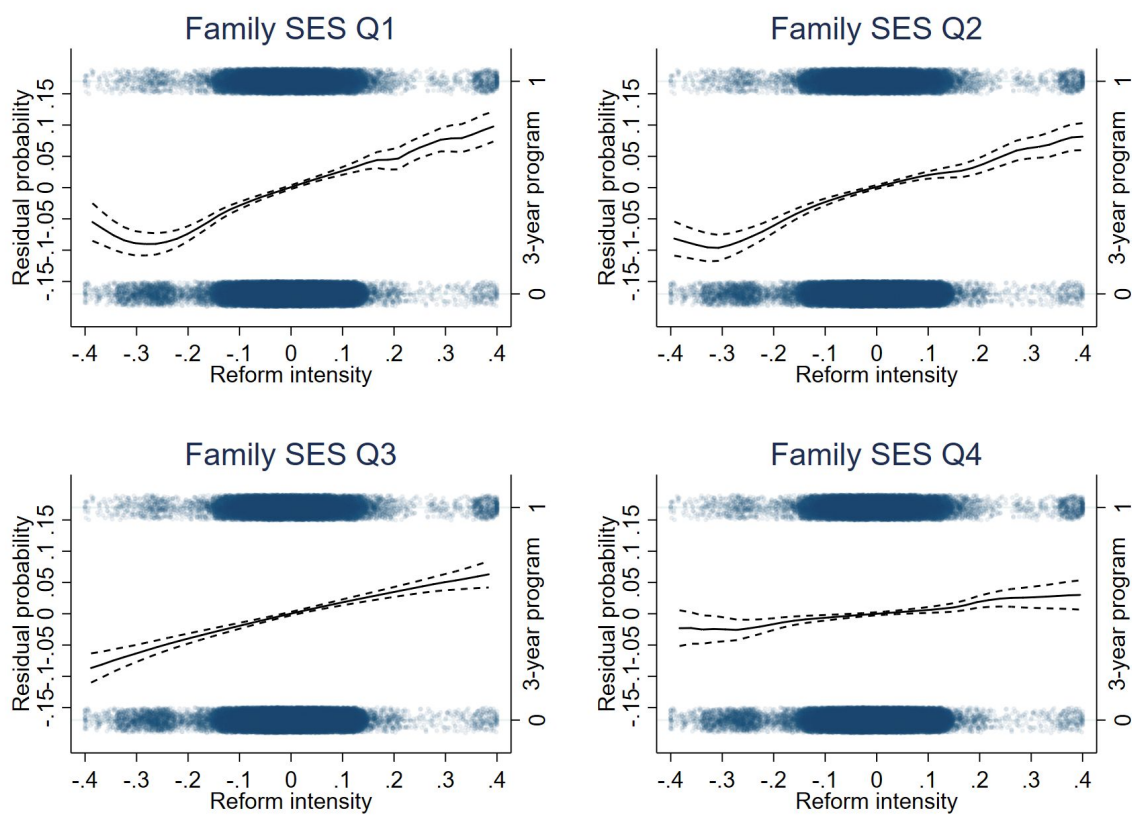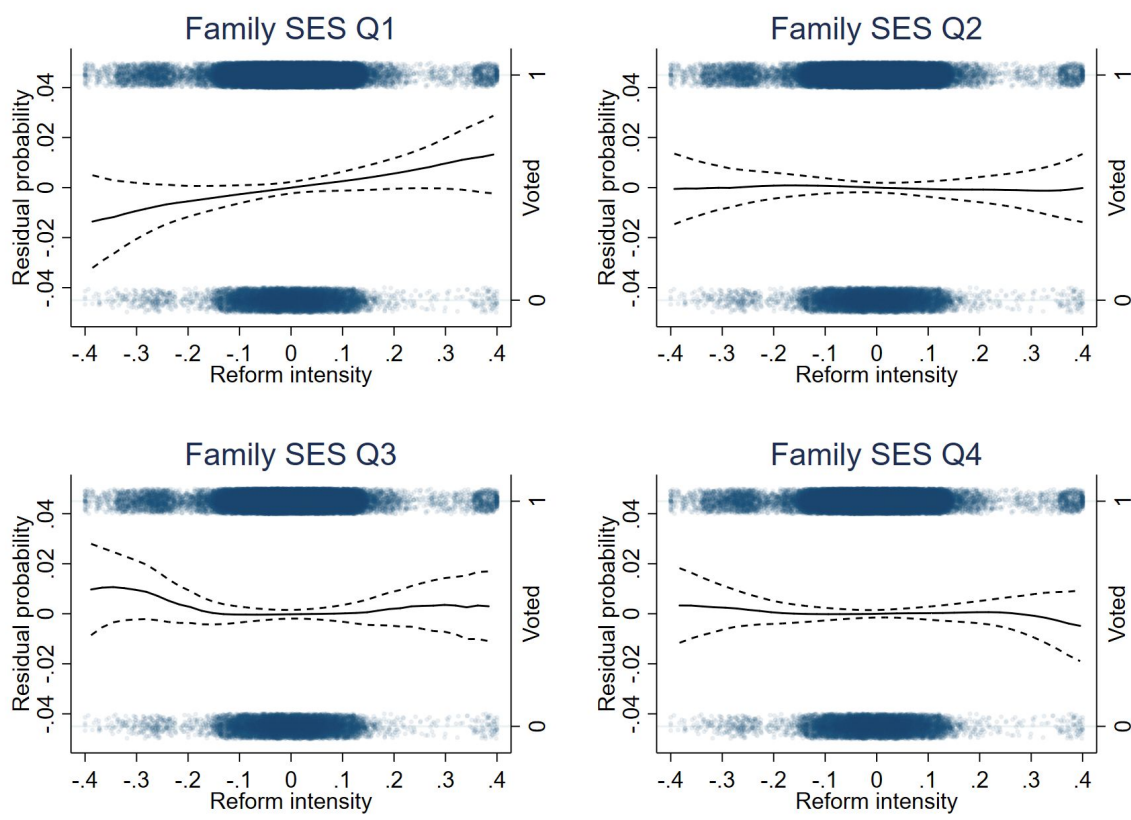Figure A21: Partial Regression Plot, First Stage Estimate

Figure A22: Partial Regression Plot, Reduced Form Estimate

A 3.11   The Reduction of the Voting Gap

To further clarify the meaning of these results for the socioeconomic voting gap, the leftmost graph in Figure A23 displays how the expected turnout rates in the four groups vary with reform intensity.[9] As can be seen, there are remaining substantial inequalities in voting as the share of three-year programs starts to increase. In particular, the differences between the three highest socioeconomic groups hardly change at all as a result of the reform (these lines are more or less parallel). The relative turnout of those from the most disadvantaged homes, however, clearly improved as a result of the reform. According to these estimates, in the absence of any three-year vocational programs the expected voting gaps between individuals in the lowest quartile and those in the other three quartiles would be 3.2 (Q2 vs Q1), 5.4 (Q3 vs Q1), and 7.4 (Q4 vs Q1) percentage points, whereas the corresponding figures would be 0, 2.2, and 4.2 percentage points if all vocational training programs were three years in length.

Figure A23: Decomposing the Voting Gap
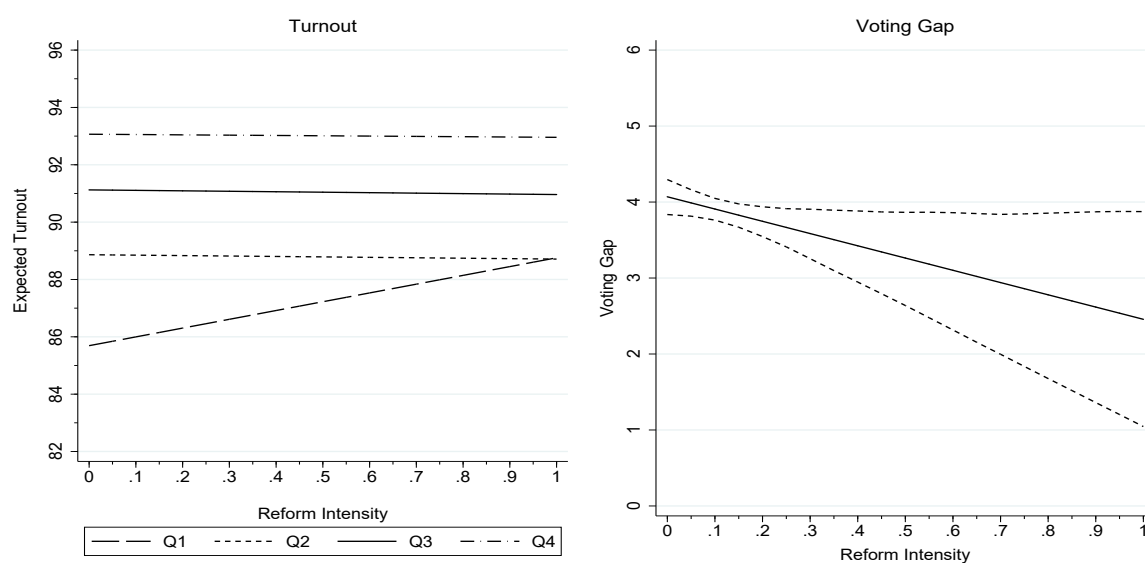
Note: The dotted lines in the graph to the right represent 95% confidence intervals obtained through bootstrapping.

In the rightmost graph of Figure A23 we use the same predictions to show how the

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[9]Expected turnout is calculated on the basis of the results presented in Table 1 of the main text averaging over the sample values of all other variables in the model.

aggregate socioeconomic voting gap varies with reform intensity. That is, the graph shows the average absolute difference in voting probability across the six possible quartile comparisons for different values of reform intensity.[10] More substantively, we can think of these differences as the expected (absolute) difference in turnout between two randomly selected individuals representing two different quartile groups. To judge from our results the average voting gap decreases from 4.1 to 2.5 percentage points as reform intensity increases from 0 to 1 (although the calculations become rather uncertain for high levels of reform intensity). However, and as the leftmost graph makes clear, this overall reduction is mainly driven by the fact that the relative position of the lowest quartile group improved as a result of the reform.

The results presented in the main text imply that the return effect is more important than the resource effect in explaining the reduction in the socioeconomic voting gap. To characterize the relative importance of these two factors in accounting for the reduction in the overall voting gap, Figure A24 presents the results from two empirically informed thought experiments.

We first imagine a situation in which the return to education is set to the sample average for all socioeconomic groups, but where the effects of the reform on educational attainment (the resource effects) are the ones previously estimated from the data. The development of the voting gap in this scenario is shown in the left graph. The right graph instead shows how the voting gap would vary with reform intensity in a situation where we leave the return effects of the different groups intact, but assign the average resource effect to all four groups.

In line with what is expected based on the results in the main text, the differences in return effects across groups are more important than differences in resource effects in explaining the reduction in the voting gap (although the calculations become rather uncertain for high levels of reform intensity). Under the assumption of equal return

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

Figure A24: Decomposing the Voting Gap
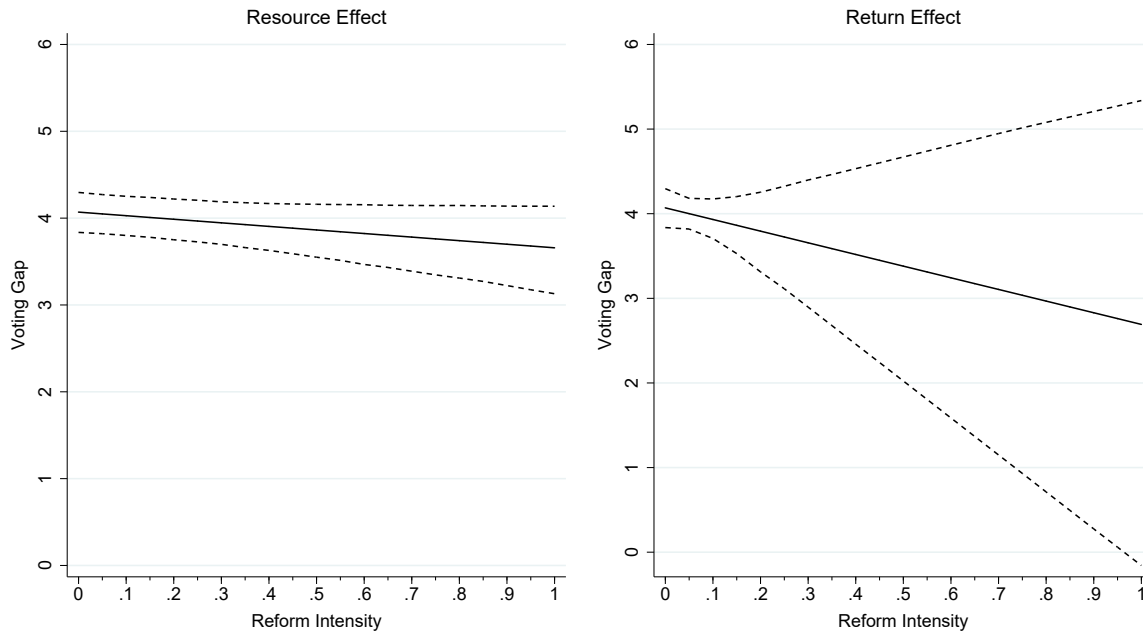
Note: The dotted lines represent 95% confidence intervals obtained through bootstrapping.

effects in all groups an increase in reform intensity from 0 to 1 would only have reduced the average voting gap by 0.4 percentage points, whereas the corresponding decrease under the assumption of equal resource effects is 1.3 percentage points.

A 3.12   Examining Potential Mechanisms

A number of mechanisms have been suggested as explanations to why education may influence political participation. First, education may have a direct effect on individuals' propensity to engage in the political sphere. According to this hypothesis the lengthening of vocational training programs from two to three years—and the increased focus on civic education—strengthened attitudinal factors shown to predict voter turnout in earlier studies such as political knowledge (Galston, 2001), interest in politics (Verba, Schlozman, and Brady, 1995), internal as well as external political efficacy (Finkel, 1985), support for the norm of voting (Blais and Young, 1999), and individuals' expectations about cooperative behavior and trust (Huang, Maassen van den Brink, and Groot, 2009). Second,

education may have a more indirect effect on political participation by influencing individuals' social and economic status. These intervening factors will in turn determine social and political network centrality. Thus, individuals with higher education will be more closely connected and exposed to networks that boost participation (Verba, Schlozman, and Brady, 1995; Nie, Junn, and Stehlik-Barry, 1996).

Due to data constraints, a more in-depth analysis of the causal mechanisms underlying the observed relationship between reform status and turnout is unfortunately outside the scope of the current study, but we will use the available data to try to shed at least some light on this important issue. Above all, our data permits a simple test of the second and more indirect link between education and political participation through possible mechanisms such as income, occupation, family status, and political activity in surrounding social networks. In Table A12 we therefore present results from a mediation analysis in which we sequentially control for a number factors. This analysis is based on a somewhat smaller sample than before because we have invoked the requirement to have complete data on all potential mediators. In the first column we therefore re-estimate the reduced form model for individuals of low social background (Q1) with complete data records. In this restricted sample the reform is estimated to increase the probability of voting by about 2.9 percentage points, which is very close to the effect previously found for the unrestricted sample (3.1).

In Column 2 we add controls for marital status (1=married), the number of children below 19 years of age, monthly labor income (in 1,000 SEK), and occupational dummies (based on four-digit occupational codes). The results suggest that all these variables are significantly related to voting. Moreover, the effect of increasing the share of 3-year training programs decreases by about 24 percent when including them among the regressors. In the remaining columns we also add controls for the political activity of surrounding networks. More precisely, in columns 3–5 we, in turn, control for the average turnout level among other individuals living in the same voting district (neighbors), the average turnout level among other individuals working at the same establishment (colleagues),

Table A12: Mediation Analysis: Social Position

	(1)	(2)	(3)	(4)	(5)	(6)
Reform intensity	2.86**	2.18*	2.19*	2.13*	2.10*	2.06*
	(1.12)	(1.11)	(1.11)	(1.11)	(1.10)	(1.10)
Gender	4.41***	2.90***	2.73***	2.63***	3.31***	3.04***
	(0.23)	(0.30)	(0.30)	(0.29)	(0.29)	(0.28)
Immigrant background	−5.97***	−5.43***	−5.06***	−5.32***	−3.99***	−3.79***
	(0.34)	(0.33)	(0.32)	(0.33)	(0.30)	(0.29)
Earnings		0.19***	0.18***	0.14***	0.17***	0.13***
		(0.02)	(0.02)	(0.01)	(0.02)	(0.01)
Martial status		4.27***	4.01***	4.25***	−2.69***	−2.73***
		(0.26)	(0.26)	(0.26)	(0.27)	(0.27)
Number of children		2.34***	2.17***	2.29***	−2.01***	−2.06***
		(0.12)	(0.12)	(0.12)	(0.14)	(0.13)
Turnout neighbors			37.88***			14.04***
			(2.36)			(2.49)
Turnout colleagues				5.28***		3.91***
				(0.46)		(0.45)
Turnout family					25.17***	24.87***
					(0.38)	(0.39)
Occupational dummies	No	Yes	Yes	Yes	Yes	Yes
Family SES	Q1	Q1	Q1	Q1	Q1	Q1
Observations	89,148	89,148	89,148	89,148	89,148	89,148

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

and the average turnout among other eligible voters belonging to the same household (family members). In the last column we control for all these factors simultaneously. As is to be expected there is a positive association between the probability of voting and the political activity of one's social networks. To judge from these results, much discussed factors such as income, occupation, and social networks can account for around 28 percent ($(2.86 - 2.06)/2.86$) of the overall educational effect.

Thus, the lion's share of the reform effect seems to be mediated via other pathways. One possibility is that the reform impact on voting behavior is not driven by schooling. Instead the positive effect of the reform on turnout propensities may reflect the reform-induced increase in average age at which the individuals typically moved out of their parents' home. A closer look at if and how the reform effect on turnout varies by birth-cohorts can provide some insights into this alternative pathway.

Previous studies have shown that young adults living with their parents vote more often than those who have left the nest (Bhatti and Hansen, 2012). Furthermore, research on the persistence in turnout suggests that voting is habitual (Plutzer, 2002; Denny and Doyle, 2009; Coppock and Green, 2016; Fujiwara, Meng, and Vogl, 2016).[11] Voting in one election increases the probability of voting in subsequent elections. In line with these findings it could be expected that the reform led to a boost in turnout among individuals that, as a consequence of the reform, were still living with their parents at the time of their first election in the beginning of the 1990's and that this resulted in an initial increase in voting probability that persisted into the 2010 election.

Above all, the 1973 cohort is set apart from the other birth cohorts by the fact that the extra year in upper secondary school among the treated individuals (autumn 1991 to spring 1992) coincided with the general election in September 1991 whereas the untreated students in this birth-cohort completed their two years of schooling (autumn 1989 to spring 1991) in between the two elections in 1988 and 1991. The treated individuals in the other three treated cohorts—born in 1971, 1972 and 1974—completed their third

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[11]However, recent work by Bechtel, Hangartner, and Schmid (2018) find little evidence for voting as a habit-forming activity.

year in upper secondary school in off-election academic years (1989/1990, 1990/1991 and 1992/1993).

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

To test this hypothesis Figure A25 plots point estimates and 95% confidence intervals from a model in which the reform effect is allowed to vary by birth-cohort and, for the 1973 cohort, by voting eligibility in the 1991 election.[12] As a benchmark the leftmost point displays the average reform effect in the full sample.

The pattern of cohort-conditional effects does not support the pathway assuming long-term effects of living with one's parents at the time of the first election. Under such a scenario we would have found significantly stronger reform effects among those born between January and August 1973. Instead we find positive and overlapping effects across the different cohorts and we cannot reject the null hypothesis that all four cohort-specific effects are of the same magnitude ($p = 0.368$).

Thus, we need to look elsewhere for factors mediating the reform effect. The most

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

Figure A25: Reduced Form Estimate by Cohort
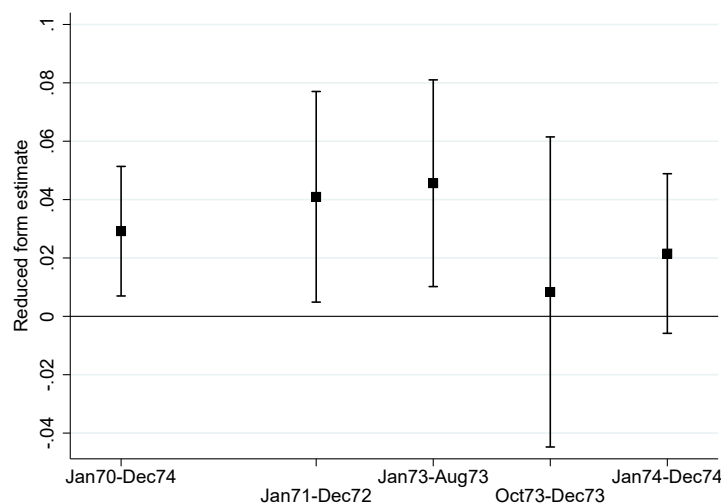
likely possibility is that the effect is driven by various factors more directly related to the nature and content of education, such as the skills and norms that the individuals learn in school. Previous studies on the effects of civic education and educational attainment on precursors to turnout behavior such as political efficacy, political interest and political knowledge lend support to this putative causal mechanism (Jackson, 1995; Verba, Schlozman, and Brady, 1995; Galston, 2001; Green et al., 2011). Relatedly, schooling may improve individuals' expectations about cooperative behavior and trust which makes them more likely to contribute to public goods of which turnout is one example (Huang, Maassen van den Brink, and Groot, 2009). Unfortunately our data does not permit a direct test of the degree to which the reform effect is mediated by these factors.

A 3.13   Examining External Validity

To situate our study and improve the understanding of the generalizability of the results, this section presents a brief descriptive analysis of the inequality of political voice in 25 modern democracies using data from the European Social Survey (ESS).[13] A first important question concerns the representativeness of the Swedish case with regard to the relationship between family background and voter turnout. Towards this end, the

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[13]See the table notes for details on data and methods

Figure A26: Family Background and Political Participation, ESS data
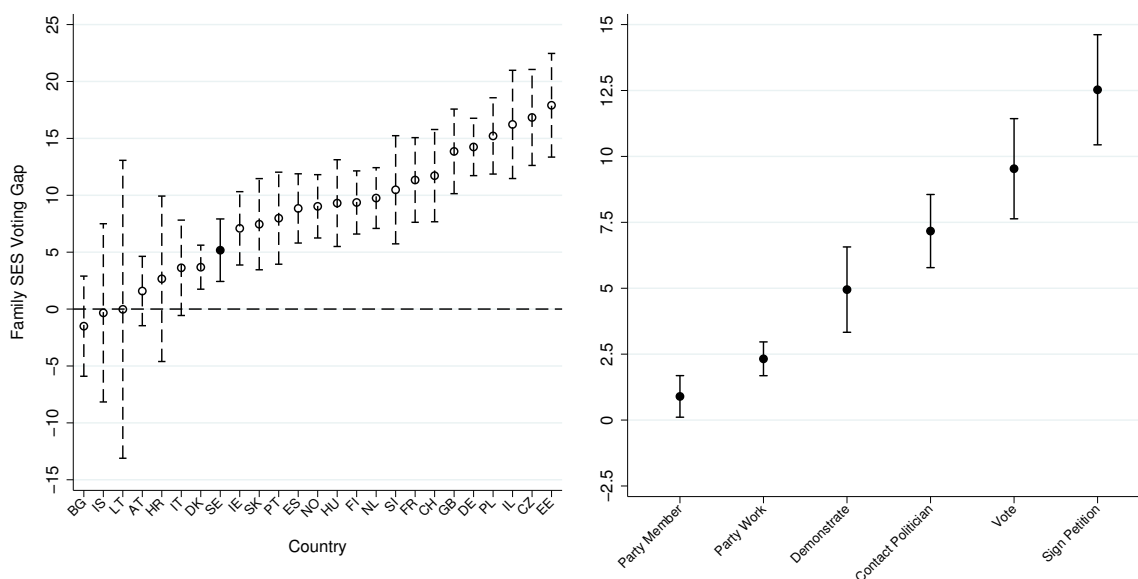
Note: The graphs are based on the results from various regression analyses in which the dependent variable of interest, e.g., voting, is regressed on dummies for family SES quartiles together with a set of controls including gender, survey year, year of birth fixed effects (left graph), and country by year of birth fixed effects (right graph). We then plot the expected differences in outcomes between the highest and lowest quartile of the family SES distribution. All individuals included in the analyses are aged 25–65. Standard errors are clustered at the country level and post-stratification weights are used make the samples representative. The confidence intervals are calculated at the 95% level.

leftmost graph in Figure A26 displays the *interquartile difference*, i.e., the difference in expected turnout between an individual coming from a home in the highest quartile (Q4) of the family SES distribution and one coming from a home located in the lowest quartile (Q1), for each of the 25 countries.[14] In most countries individuals from more advantaged social backgrounds vote, or at least claim to vote, to a much higher extent than individuals from less well off backgrounds. The interquartile difference in the sample as a whole is about 9.5 percentage points whereas the corresponding figure for Sweden (labeled *SE*) is 5.2 percentage points. Although slightly more politically equal than the average European country, Sweden is not exceptional as can be seen from the fact that the confidence interval for the Swedish point estimate overlaps with those of most other

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

countries.

Another issue concerns the representativeness of the participatory act of voting. The rightmost graph of Figure A26 therefore displays how family background relates to other forms of political participation, such as working for a political party, contacting elected politicians, or signing petitions. As can be seen, individuals from privileged social backgrounds are more likely to undertake all these political acts compared with those from less privileged backgrounds. The large variation in the baseline probabilities of performing these different acts makes it difficult to compare the relative importance of family background across different types of political participation. The important point, however, is that individuals of low social origin are less likely to exercise political voice, *regardless of participatory channel*. The strong relationship between family background and voting thus seems to hold true for different acts of political participation more generally. This simple analysis indicates that the lessons drawn in this study can be expected to travel beyond the particular case of voting in Sweden.

A 3.14   Policy Implications

The focus on initial social circumstances could be questioned on the ground that the principle of political equality does not require "that all individuals be equally active, only that participant publics be representative in their politically relevant characteristics" (Schlozman et al. 2012:178). So the question then becomes whether family background is such a politically relevant characteristic. One way to attempt to answer this question is by examining whether individuals of different social origin hold conflicting political attitudes. In Figure A27, we therefore report the interquartile gap in family background for five attitudinal questions included in the ESS.

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

Figure A27: Gaps in Political Attitudes
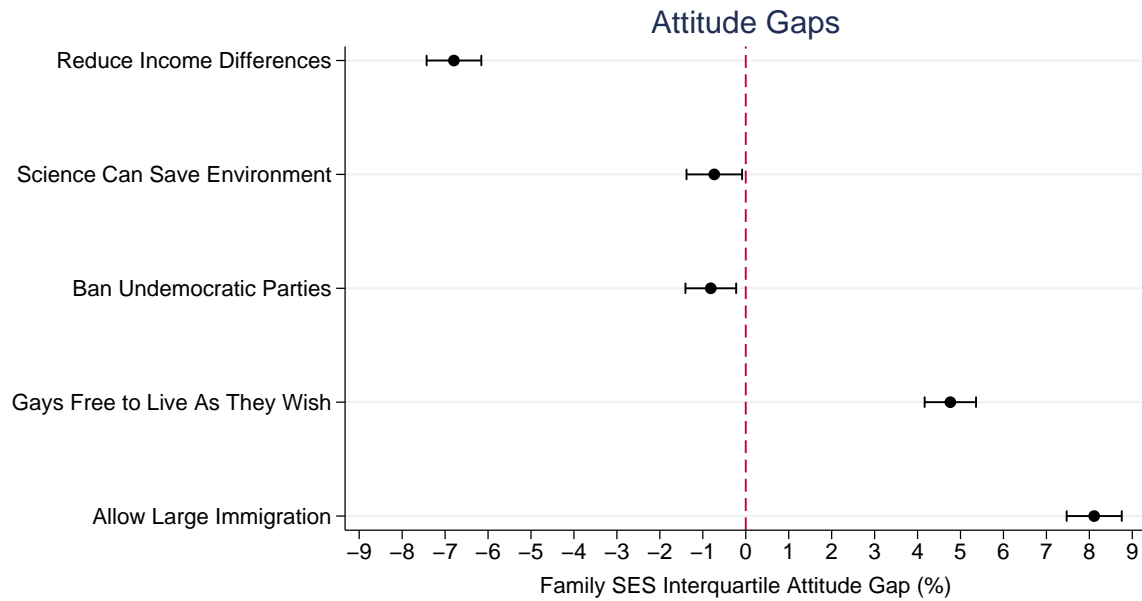
Note: See the note to Figure A26 for a description of the method used for creating this graph.

the right for individuals of different ethnic belonging to settle in their country. We also see that individuals from more advantaged backgrounds are considerably more likely to support the statement that gays and lesbians should be free to live their lives as they wish. Although smaller in magnitude, there are also discernible differences with respect to the probability of agreeing with the statements that science can be trusted to solve environmental problems and that undemocratic political parties should be banned.

This analysis thus indicates that there are important differences in political attitudes between individuals of different social origin. We may, however, also be interested in the extent to which these differences are reflected in the party choices of the different groups. This relates to the important question whether the reform under study affected representational inequality.

There is a widespread scholarly belief, dating back at least to the writings of Tingsten (1937), that variation in voter turnout will have electoral consequences. The underlying argument for this goes as follows. Because individuals at lower levels of the social ladder are overrepresented among non-voters, increased turnout will mainly benefit parties

representing more disadvantaged socioeconomic groups. A recent study from Sweden confirms the relationship between the level of voter turnout and the socioeconomic composition of the voters; when voter turnout is low it is less likely that low SES citizens vote (Persson, Solevid, and Öhrvall, 2013). As for the effects on party support, findings are somewhat mixed (Hansford and Gomez, 2010), but recent studies employing different types of natural experiments tend to support the view that higher turnout benefits the left (Hansford and Gomez, 2010; Bechtel, Hangartner, and Schmid, 2016; Finseraas and Vernby, 2014).

Since we do not have access to individual level data on party choice we cannot study the electoral consequences of the reform directly. The share of students affected by the pilot scheme was also too small for us to be able to study the effects based on aggregate election results. However, we present a set of analyses that provide indirect evidence supporting the idea that the reform may have increased the vote share for left parties, leading to a decrease in representational inequality. However, this evidence is purely correlational and as such it should be interpreted with some caution.

The analyses aim to establish two relationships; *a)* that voting for the left parties was more likely among citizens from a low SES family background, and *b)* that there is a relationship between aggregate level voter turnout among the lowest SES quartile and support for left parties. Figure A28 establishes the first point using data from the European Social Survey from 2010. It shows the difference in left-right party support by family background quartile. The results provide evidence that it was only among the citizens in the lowest quartile of SES family background that the left parties gained more support than the right parties.

In Table A13 we provide evidence in support of the second relationship using electoral district data from the 2010 election. The table presents analyses of the relationship between the vote share difference between the left and right bloc and voter turnout.[15]

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[15]The left bloc consists of the Social Democrats, the Left Party and the Green Party, whereas the right bloc is comprised of the Conservatives, the Liberal Party, the Center Party, and the Christian Democratic Party.

Figure A28: Difference in Left-Right Support (%) by Family SES, ESS 2010
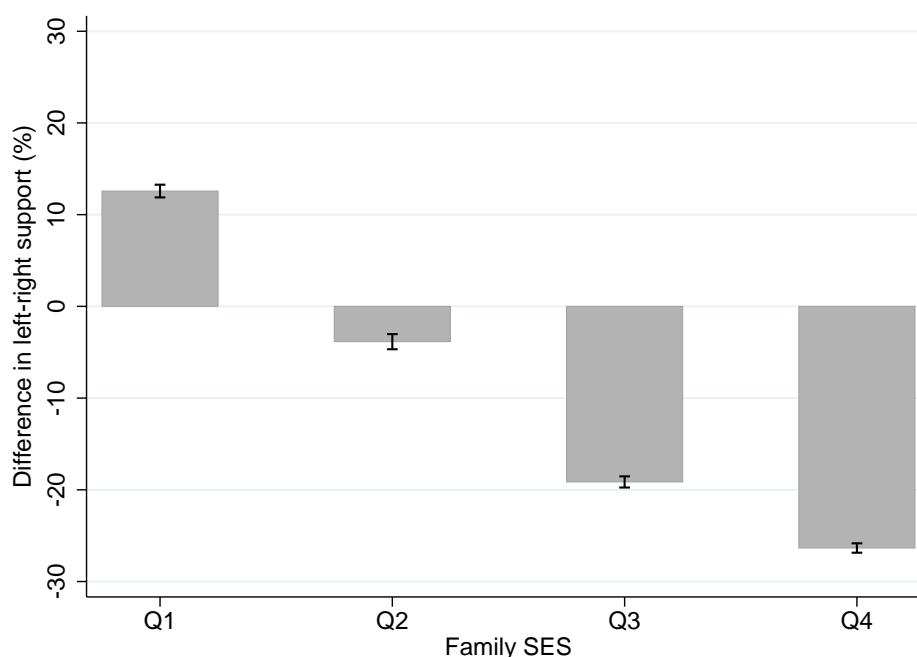
Note: The difference in the share of respondents that say that they feel closer to a party in left bloc and those that say that they feel closer to a party in the right bloc.

The first two columns of the table show the correlation between overall turnout in a district and the difference in left-right support. As can be seen, it is only when controlling for municipality fixed effects that we observe a positive correlation between party support and overall turnout. According to the estimates in model 2 a one percentage point higher overall turnout is associated with 0.2 percentage points higher vote share difference between the left and right bloc.

In column 3 we instead examine how left-right support relates to turnout in different quartiles of the family SES variable[16] The results are rather striking. If we hold turnout in the other quartiles constant, a one percentage point higher turnout among individuals of the lowest family background is associated with a 0.3 percentage points higher difference in the left-right support. For all other quartile groups the corresponding difference is negative. In column 4 we add municipality fixed effects to the model, so now all identifi-

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[16]For reasons of data availability the turnout in each quartile group is based on information on individuals born between 1955 and 1990. To make the results less sensitive to outliers we only include electoral districts where there are at least 50 individuals in each quartile group.

Table A13: Voter turnout and party choice at the electoral district level

	(1)	(2)	(3)	(4)	(5)	(6)
Overall turnout	−0.02	0.20***				
	(0.08)	(0.07)				
Turnout Q1			0.31***	0.20***	0.32***	0.21***
			(0.07)	(0.05)	(0.00)	(0.00)
Turnout Q2			−0.11	−0.08	−0.13***	−0.08***
			(0.08)	(0.06)	(0.00)	(0.00)
Turnout Q3			−0.21**	0.09	−0.16***	0.09***
			(0.10)	(0.07)	(0.00)	(0.00)
Turnout Q4			−0.25***	−0.02	−0.25***	−0.01***
			(0.09)	(0.06)	(0.00)	(0.00)
Observations	4546	4546	4546	4546	6,053,233	6,053,233
Municipality FE	No	Yes	No	Yes	No	Yes
Population weights	No	No	No	No	Yes	Yes

cation comes from differences between electoral districts within municipalities. Although this analysis may push the data to its limit we see that there is still a rather strong correlation between turnout in Q1 and left-right support.[17] In the last two columns we weight the electoral district results by the number of eligible voters within each district, but this does not affect the results. If we take these results at face value, a quick back of the envelope calculation thus suggests that a 3 percentage point increase in turnout in Q1 (our estimated reform effect) can be expected to increase the total vote share difference between the left and right bloc by between 0.5 and 1 percentage points.

To summarize, these results show that it is more likely that citizens with a disadvantaged SES background support left parties and that higher voter turnout among citizens from low SES background is associated with increased support for left parties. Moreover, in the analyses in the main paper we show that the education reform increased turnout among those from low SES backgrounds. Hence, while we have no data on how the specific students who participated in the reform voted in the 2010 election, the set of analyses presented here is indicating that the reform is likely to have benefited left parties. Ad-

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[17]The positive coefficient for Q3 is mainly driven by support for the Green Party, and not by support for the traditional leftist parties.

mittedly, these results are purely correlational, but they square well with the findings of some other recent studies using more credible identification strategies (Finseraas and Vernby, 2014; Bechtel, Hangartner, and Schmid, 2016). We thus find at least tentative support for the view that reforms, such as the one under study, that contribute to the reduction of the socioeconomic voting gap can help foster representational equality by increasing the vote share of leftist parties. However, since the individuals affected by this particular reform constituted a fairly small part of the total electorate the overall effect of the reform on election results was likely fairly small. Yet, in very close elections it may be a factor that can help tipping the majority in favor of the left parties.

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