The Making of Social Democracy

The Economic and Electoral Consequences of Norway's 1936 Folk School Reform*

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January 25, 2024

Abstract

Upon assuming power for the first time in 1935, the Norwegian Labour Party delivered on its promise for a major schooling reform. The reform raised minimum instruction time in less developed rural areas and boosted the resources available to rural schools, reducing class size and raising teacher salaries. We show that cohorts more intensively affected by the reform increased their education and experienced higher labor income. Our main result is that the schooling reform also boosted support for the Norwegian Labour Party in subsequent elections. This additional support persisted for several decades and was pivotal in maintaining support for the social democratic coalition in Norway. These results are not driven by the direct impact of education and are not explained by higher turnout, or greater attention or resources from the Labour Party targeted towards the municipalities most affected by the reform. Rather, our evidence suggests that cohorts that benefited from the schooling reform, and their parents, rewarded the party for delivering a major reform that was beneficial to them.

Keywords: education, human capital, labor, schooling reform, social democracy, voting.

JEL Classification: P16, I28, J24.

*This paper is dedicated to the memory of our dear friend and co-author, Tuomas Pekkarinen, who passed away while working on the revision. We are grateful to Ran Abramitzky, Eirik Berger, Jon Fiva, Matti Mitrunen, Karl Ove Moene, Bjarne Strøm, Janne Tukiainen, and numerous seminar participants for their comments and suggestions. We gratefully acknowledge financial support from the Academy of Finland and the Norwegian Research Council.

1 Introduction

Rising inequalities, economic and social insecurities, and a growing concentration of economic power have convinced many economists and commentators that major institutional reforms and new policies are necessary in Western nations and around the world (e.g. Esping-Andersen et al. 2002; Rajan 2019; Stiglitz 2019; Henderson 2021; Yang 2021; Bardhan 2022; Wolf 2023). There is little consensus, however, on what makes such reforms feasible. One of the most successful examples of ambitious institutional reforms comes from Workers' or Labour parties, later referred to as Social Democratic Parties, which rose to power in Scandinavia in the 1930s. These parties proceeded to build new institutions based on macroeconomic management, collective bargaining, fiscal redistribution, and social programs such as publicly-provided universal education, social security and national health care. Similar institutions were adopted in the UK starting in 1942 and in much of the rest of Western Europe after WWII.

These institutions were a sharp break with the past. Politics in Denmark, Norway and Sweden used to be dominated by parties representing large businesses or landowners, and their economies were rural and highly unequal (Atkinson and Søgaard 2016; Bengtsson 2019; Aaberge et al. 2020). For example, the pre-tax Gini coefficient in Norway was 0.57 in 1930, much higher than today's highly unequal Latin American societies. After the 1930s, Social Democratic Parties reshaped Scandinavian economics and politics. The political power of large businesses and landowners waned, and due to various redistributive policies and the active role of labor unions in wage setting, inequality began to diminish rapidly. The pre-tax Gini coefficient in Norway declined to 0.25 by 1970 (Aaberge et al. 2020). In addition, social mobility increased substantially for cohorts born after the 1930s in Sweden and Norway (Björklund et al. 2009; Pekkarinen et al. 2017).

In this paper, we investigate how the Norwegian Labour Party built long-term electoral support by focusing on their (successful) implementation of a major schooling reform. The Norwegian Labour Party came to power in 1935, under the leadership of Johan Nygaardsvold and with the support of the Agrarian Party.[1] It remained in government for most of the subsequent five decades. The formation of the first Labour government was preceded by the party's clear break from its earlier revolutionary Marxist ideology and a new strategy emphasizing a strong commitment to parliamentary democracy, a message of unity between rural and urban areas, and a willingness to seek compromises with other parties and with employer organizations. A central pillar of the Labour Party's economic agenda was a major school reform—listed in its program as the third priority, after democratic rights and equal justice. At the time, Norwegian education was highly decentralized and unequal, with rural areas having short school years and limited school resources. The Labour Party promised to

[1]Nygaardsvold's 1935 cabinet was Norway's second Labour government. The first in 1928 lasted only two weeks, without passing any legislation. During the German occupation in 1940–45, Nygaardsvold's cabinet acted as government-in-exile in London.

harmonize education and to raise school quality and instruction time in rural areas. As its first major reform, the new government launched an ambitious education reform, the Folk School Law of 1936, which increased funding and resources, reduced class size, expanded minimum instruction time, and raised teacher salaries for rural schools.

Although sometimes overlooked in the historical work on the Scandinavian labor movement, education was a key pillar of the social democratic agenda, and not just in Norway. The aim of the movement was to achieve greater social equality, and education was seen as an important tool for altering the distribution of opportunities (Rothstein 1998), using the "coercive power of the state" in the words of Lewin (1967). In Sweden, leading social democrats such as Tage Erlander, Olof Palme, and Alva Myrdal were deeply involved in the planning and implementation of education policy. Indeed, Myrdal saw education policy as "the primary strategic instrument for abolishing class barriers" (Rothstein 1998).

We find that the 1936 education reform in Norway had both economic and political effects. The reform did not alter the number of years of compulsory schooling, but it substantially increased instruction time and quality of schooling in the most disadvantaged parts of Norway. As a result, we estimate that birth cohorts of men most affected by the reform experienced significantly greater schooling, presumably because a higher quality of primary education improved their preparedness for further education. We also estimate a positive effect on earnings of these birth cohorts. Because these estimates are a little more sensitive to controlling for differential trends by region or 1930 municipality characteristics, we interpret them more cautiously than our education estimates. For women, we do not find a statistically significant effect on post-mandatory education but, consistent with improved quality of schooling, the affected female cohorts also enjoyed higher income. Our point estimates suggest that the reform had intergenerational effects as well, although many of these estimates are statistically insignificant due to our research design's relatively low statistical power for intergenerational analysis.

Our main focus is not on the effects of the reform on education or earnings, however, but on Norwegian institutions and politics. We show that the reform was critical for the support of the Labour Party in subsequent elections in rural and less developed parts of Norway. Before the reform, the Labour Party had less support in rural municipalities than in urban areas. After 1936, its vote share increased substantially in the more affected rural municipalities. These effects persisted for at least two decades and are robust to controlling for region and pre-reform industry structure and average income. A back-of-the-envelope calculation suggests that, without the schooling reform, the vote share of the Labour Party in rural areas, which still hosted more than half of the electorate, would have been 1.4–4.6 percentage points lower in 1945. The rise in rural support between 1933 and 1945—a total of 3.9 percentage points—was critical for the party since, concurrently, it lost 3.8 percentage points of its support in the cities. As a consequence, the traditionally higher support the Labour Party enjoyed in cities disappeared,

and the party has since been equally popular in rural and urban areas.

In the last part of the paper, we examine the mechanisms behind the impact of the reform on the Labour Party's electoral success. We establish that our electoral results are not driven by a "direct education effect", whereby the educated are more likely to be Labour Party supporters. In fact, during this period, highly educated Norwegians were more likely to vote for the more conservative parties. In addition, the electoral effect is largest in the first elections held after the reform, when most individuals directly impacted by the reform were not yet eligible to vote. Moreover, we show that the Labour Party's electoral success was not built on higher turnout which was already very high in Norway during this period nor on allocating greater resources or attention to the municipalities most affected by the reform. Rather, we argue our results are explained by the fact that the 1936 education reform was a major promise of the Labour Party and a central pillar of its program for helping the less advantaged parts of the country. The party delivered on its promise of major educational reform, and voters rewarded it with lasting electoral support—especially by broadening the social democratic coalition with the addition of previously more conservative rural voters.

We provide four pieces of evidence consistent with this interpretation. First, we find that the electoral effect is largely driven by municipalities that had not been previously exposed to Labour rule at the local level, indicating a switch from conservative to social democratic support in places that benefited from the education reform. Second, using individual-level survey data collected from the 1957 parliamentary elections, we find that voters who had personally experienced increased schooling and improved school quality were more likely to vote for the Labour Party. Third, rural Norwegians with children born into the cohorts who had benefited from the 1936 school reform were also much more likely to support the Labour Party than individuals with somewhat older children. This result implies that it was not just those receiving the education that became more pro-Labour, but also their family. Fourth, in the same survey more than 90% of the respondents agreed that the Norwegian Labour Party had been willing and able to implement its agenda, and those directly affected, and their parents, were particularly likely to hold this view.

Our paper is related to several literatures and its contribution to each one of these centers on the effects of the schooling reform on the broader Norwegian political equilibrium, as we discuss next.

First, one of the key questions in political economy is how voters reward parties and leaders that keep their promises and deliver public goods. Such support may result because of a change in beliefs about the "competence type" of the party, because of standard retrospective voting (e.g., Ferejohn 1986; Persson et al. 2000), or because voters feel reciprocal altruism towards the party. Each of these mechanisms can be seen in functioning democracies, but have also been at times associated with clientelistic or populist policies in nascent democracies (e.g., Acemoglu

et al. (2013) on the belief channel, Caprettini et al. (2021) on retrospective voting, and Finan and Schechter (2012) on reciprocal altruism). There are no clear results in this literature on what types of successful policies build long-term support for a party. We contribute to this literature by providing an instance of a major reform that built persistent and significant support for the governing party from both citizens who benefited from the school reform and their parents. Although our methodology does not distinguish whether voters updated their beliefs about the competency of the Labour Party or felt indebted to it, it is unique in showing how such support consolidated new Social Democratic institutions, rather than supporting clientelism. We argue that this was partly because of the broad-based nature and high visibility of the policy in question and partly owing to the efforts of the Labour Party to build a diverse coalition in support of its agenda.[2]

Second, there is by now a large number of papers in labor economics evaluating the effects of various schooling reforms, ranging from compulsory schooling and child labor laws to school building programs (e.g., Acemoglu and Angrist, 2000, Duflo, 2001, Black et al., 2005, Meghir and Palme, 2005, Oreopoulos, 2006, Pekkarinen et al., 2009, Fischer et al., 2020; see Oreopoulos and Salvanes, 2011, for a review). Studies such as Milligan et al. (2004) and Lindgren et al. (2019) have used education reforms to examine the effect of education on political participation and Marshall (2016) studies implications of the 1947 UK education reform, including its effects on the likelihood of voting for conservatives. We contribute to this literature by providing, to the best of our knowledge, the first investigation of how a major school reform affects the political equilibrium, both in the short and medium run.

Third, our paper relates to the literature on successful political reforms. In the context of democratic reforms, Acemoglu and Robinson (2006, 2012) study the role of collective action by politically excluded groups to force a transition away from non-democratic regimes, and emphasize the importance of fiscal redistribution, limited inequality and broad coalitions in order to ensure the consolidation of new democratic regimes. Fearon (2011) and Bidner and François (2013) explore the role of political accountability, bolstered by electoral institutions and collective action by citizens. Brender and Drazen (2007) stress the role of fiscal policies to reduce the fragility of new democracies. Giavazzi and Tabellini (2005) empirically investigate whether economic or political reforms come first in cross-country data. There is less systematic work on major institutional reforms within democratic political systems. Fernandez and Rodrik (1991) and Strulovici (2010) propose theoretical arguments for why economic reforms in democratic societies will be delayed or blocked, and the literature on special interest politics, e.g., Grossman and Helpman (2001), also offers various reasons for inefficient reforms. We contribute to this literature by studying how schooling reforms can form their own constituency by generating

[2]Conditional cash transfers (Manacorda et al. 2011; De La O 2013; Labonne 2013; Zucco Jr 2013) and even mere promises of transfers (Elinder et al. 2015) have also been found to have a positive effect on the support for the incumbents, but have not led to the consolidation of new institutions, as we observe in Norway.

persistent influences on subsequent political economy outcomes. Our results are also consistent with the recent findings in Acemoglu et al. (2023) showing that successful performance of democratic regimes increases support for democratic institutions.

Fourth, we contribute to the literature on the origins of social democracy in Scandinavia and Europe. Classic works in this area, such as Korpi (1983), Esping-Andersen (1990), Baldwin (1990) and Rothstein (1998), emphasize the role of labor unions and workers, though the central role of the coalition with agrarian interests has also received attention (e.g., Gourevitch 1986; Berman 2006). We contribute to this literature by providing empirical evidence on one relatively underemphasized mechanism leading to the persistent success of social democratic parties.

Fifth, many scholars have argued that education may increase support for democracy or certain types of institutions (e.g., Verba and Almond 1963; Lipset 1959). More recently, Glaeser et al. (2007) claim to find support for this hypothesis, though more systematic analysis in Acemoglu et al. (2005, 2008) show no impact of education or income on democracy once time trends are removed. Milligan et al. (2004) document that educated individuals are more likely to vote. However, Friedman et al. (2016) point out that greater turnout does not necessarily mean more pro-democracy behavior in general and show that disadvantaged Kenyans who received more education as a result of schooling reform increased their support for political violence. Relatedly, Bautista et al. (2020) show that Chilean cohorts that obtained less tertiary education because of General Pinochet's crackdown on universities and cuts to their budget after the 1973 coup were more likely to register to vote and support a return to democracy in the 1988 plebiscite. Our work contributes to this literature by proposing a new channel via which education affects political outcomes—support for parties that implemented major reforms.

The rest of the paper is organized as follows. In the next section, we provide the institutional background for Norway in the 1930s, outline the state of education and describe the Norwegian Labour Party's policy platform and the schooling reform it implemented upon assuming power in 1935. Section 3 describes our data sources, while Section 4 outlines our empirical approach. Our main results are presented in Section 5. Section 6 explores the mechanisms behind the growth in the support for Labour Party in areas and among cohorts benefiting from the schooling reform. Section 7 concludes, while the Online Appendix contains additional empirical results.

2 Norway's Labour Movement and Educational Policy

The roots of the Scandinavian welfare state models can be traced back to the policies of center-left governments that rose to power between the world wars.[3] From 1900 until the end of the First World War, several liberal governments introduced major labor laws that catered to workers in

[3]The Finnish welfare state, although it ultimately became similar to the Scandinavian model, followed a different path, partly due to the disruptive effects of the country's 1918 civil war; see Meriläinen et al. (2022).

the expanding manufacturing sector. However, these laws did not establish universal policies, which became a defining characteristic of the Nordic welfare states (Bull 1959; Bjørnson 2001).

During the 1930s, Swedish and Danish social democrats established lasting governments, typically in coalition with parties representing rural constituents. The policies introduced by these governments set the foundation for the welfare institutions that the same political forces continued to expand after WWII. These policies included the establishment of old age and disability pension, sickness leave, and unemployment insurance as well as large public investments in health and education. Norway followed this trend in 1935 when the Norwegian Labour Party formed a government with the support of the Agrarian Party.

2.1 Norway in the 1930s

Unlike what is sometimes claimed, the Nordic welfare states are not rooted in some underlying structural equality and consensus that predates the modern welfare state institutions. Quite the contrary, before the 1930s Norway, like other Nordic countries, was very unequal and experienced intense industrial conflict (Moene and Wallerstein 2006). In 1930, the Gini index in Norway, at 0.57, and the top 10% share of the national income, at 0.44, were similar to those in the United States around the same time and also today (Aaberge et al. 2020; WDI 2023). Regional inequalities were also striking. According to Falch and Tovmo (2003), the gap in income per capita between the poorest municipality and the richest city was 1 to 18 in 1930.

Although rapid structural change had already started by this point, almost 30% of the labor force still worked in agriculture, and more than half of the population lived in rural areas. The Norwegian economy had been severely impacted by the postwar recession in Europe in the early 1920s, and did not reach sustained recovery before it was hit again by the Great Depression in 1930. Due to the combination of deflationary policies and external shocks, GDP per capita grew only by 2.3% between 1919 and 1930 whereas the rest of the Scandinavian countries experienced solid growth (23.5% in Sweden, 28.3% in Denmark during the same period). The poor growth performance was reflected in a high unemployment rate which never dropped below 9–10% during the 1920s and reached 33% in 1933. Norwegian labor markets were also affected by high levels of industrial conflict. According to Moene and Wallerstein (2006), the number of working days lost due to strikes and lockouts in 1931 alone was three times larger than the total amount of days lost during the 25-year period between 1945 and 1970.

Norway's political landscape, mirroring other Scandinavian nations, was shaped by its proportional representation system from multi-seat constituencies. Beyond the Labour Party, which we discuss in the following subsection, three major parties stood out. The Conservative Party, rooted in urban and business interests, consistently prioritized business concerns, capturing vote shares between 20 and 27 percent during the three parliamentary elections held in the 1930s. The Liberal Party, which represented both the urban educated class and rural

landowners, had a platform emphasizing universal suffrage, labor reforms, and poverty relief. It secured vote shares of 16 to 20 percent during this period. Meanwhile, the Agrarian Party, representing large farmers, attracted 12 to 16 percent of the votes. In addition, there were several smaller parties, including the Communist Party and the fascist Nasjonal Samling, but they remained on the political margins with low single digit vote shares.

2.2 The Norwegian Labour Party

The development of the Norwegian labor movement followed the same broad pattern as similar parties in Northern Europe, particularly in other Scandinavian countries, although there are also some distinct characteristics (Bull 1959; Esping-Andersen 1985; Sejersted 2011). Founded in 1887, the Norwegian Labour Party entered parliament in 1904. Its early history was characterized by internal conflicts between the revolutionary and reformist factions. Until the 1930s, the party programs had a clear Marxist tone and an ambivalent attitude toward parliamentary democracy. Unlike the other Scandinavian Labour parties, the Norwegian Labour Party was also a member of the Soviet-led Comintern until the early 1920s.[4]

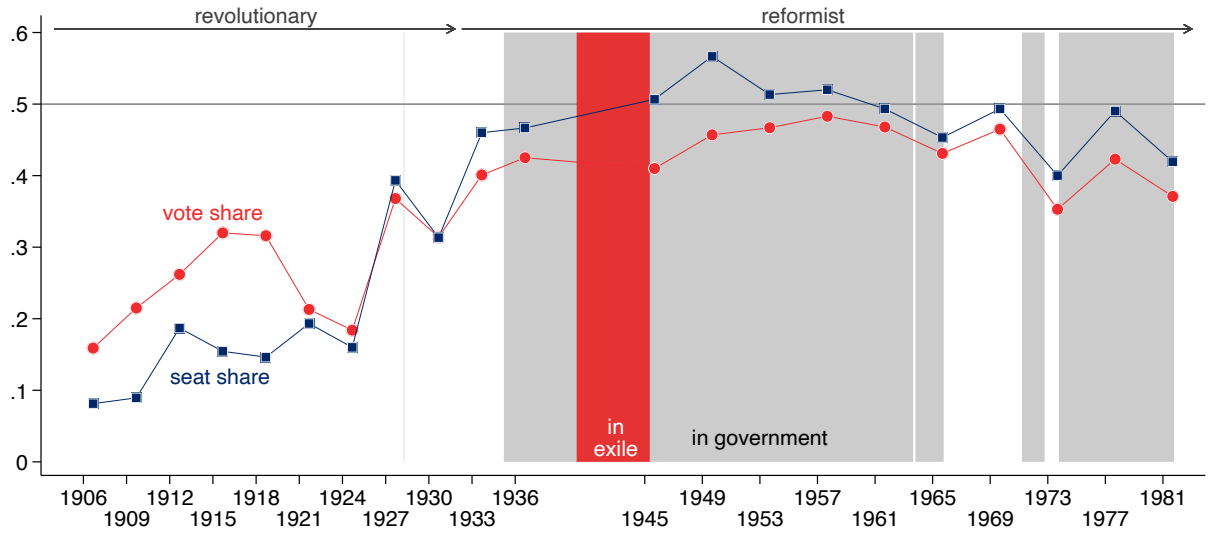
Following the poor performance in the 1930 parliamentary election, the Norwegian Labour Party altered its strategy and adopted a reformist agenda, following the earlier example of its sister parties in Denmark, Germany, and Sweden (Bull 1959; Esping-Andersen 1985). This shift was also motivated by the purges in the Soviet Union, the economic crisis which had severely affected the workers in the industrial, logging, and fishing industries, and the threat of fascism which was gaining support in Norway (led by the now infamous Vidkun Quisling).

The new strategy was built on three pillars. First, together with the main trade union, the Labour Party established a more cooperative approach towards the employer organizations and managed to convince them to recognize the National Confederation of Workers (LO) as a negotiating partner.[5] The party also shifted its economic policy by adopting a Keynesian program of stabilization policy following an influential pamphlet "a 3-year plan for Norway" (*En norsk 3-års plan*) by Ole Colbjørnsen and Axel Sømme. Second, the Labour Party moved from its earlier focus on industrial workers to a message of unity between rural and urban areas, as well as owners of small businesses and a part of the educated middle-class such as teachers and public sector workers (see Online Appendix Figure A1 for an illustration of this change between

[4]The reasons for the radicalization of the Social Democracy in Norway—contrasting with the experiences in Denmark and Sweden—are not well understood. One hypothesis is that the late industrialization in Norway, which began between 1905 and 1910, resulted in a younger workforce that was perhaps more inclined to support radical politics (Dahl 1971).

[5]This agreement was made just before the formation of the Labour government in 1935. It resembles the Saltsjøbad agreement of 1938 in Sweden and the agreements established already around the turn of the century in Denmark. The new national rules for wage negotiations were also signed by the National Confederation of Employers (NAF) and the National Confederation Workers. After WWII, the government also took an active part in the wage negotiations as a third party.

Figure 1: Labour party's election results and periods in government, 1905–1981



Note: This figure shows the vote share of the Norwegian Labour Party in parliamentary elections and the share of seats the party held in the parliament. The gray areas denote the periods when the Labour Party was in government, and the red area represents the period when the Labour government was in exile in London due to the Nazi occupation. The shift in the relationship between vote and seat shares in 1921 stems from a transition to proportional representation. The decline in the vote share from 1921 to 1924 is attributable to a temporary party split into the Social Democratic Party of Norway and the Norwegian Labour Party; see Cox et al. (2019) for details.

the 1930 and 1933 election campaigns). Third, the party made a clear break with revolutionary Marxist ideology and fully committed to advance its reformist agenda through parliamentary democracy and alliances with other parties.

These changes made the Norwegian Labour Party more appealing to moderate voters and more acceptable as a coalition partner for centrist parties. In the 1933 election, the party increased its vote share from 31% to 40%—the highest it had ever achieved. Although this electoral success did not immediately bring the party to power, the minority government led by the Liberal Party collapsed in 1935 when the Agrarian Party withdrew its support and agreed to support a Labour minority government with Johan Nygaarsdvold as its prime minister.

The 1935 government started a long period of social democratic rule. As shown in Figure 1, the Labour Party remained in power for most of the following 45 years. The exceptions were during the German occupation in 1940–1945 (when the Labour government was in exile in London) and short periods of center-right governments in the 1960s and 1970s. During this long period, the Labour Party laid the foundations of the modern Norwegian welfare state, introducing universal national social security, a comprehensive healthcare system, and, later, daycare and family leave policies.

2.3 Primary education in Norway before 1935

The Norwegian legislation on primary schools dates back to the 18th century. The first Law of Primary Education for the Kingdom of Denmark-Norway was introduced in 1739. Education was the purview of the church until the 1840s when regional federalism was introduced, and the responsibility of organizing primary education was delegated to municipalities. In 1861, the focus of primary schooling was shifted from preparing children for confirmation at the age of 15 to providing more general education, including subjects such as algebra, in addition to reading and writing.

Because the demand for primary education was understood to be lower in the rural areas and in fishing communities—sectors that were also exempt from child labor regulations—the law on primary schools stipulated shorter school years for rural areas than for urban areas, with a lower minimum and maximum number of school weeks in rural areas. Even though the school weeks included slightly more hours in rural areas, the restrictions on the number of weeks meant that rural primary school students received substantially less instruction time than their urban counterparts. As shown in Figure 2, in 1928 rural schools had to provide just 3,096 hours of education over seven years of mandatory education, whereas the corresponding figure was 4,912 hours in urban areas. In addition, the content of education varied widely across municipalities as the law on primary schools did not establish guidelines regarding the number of hours allocated to different subjects.

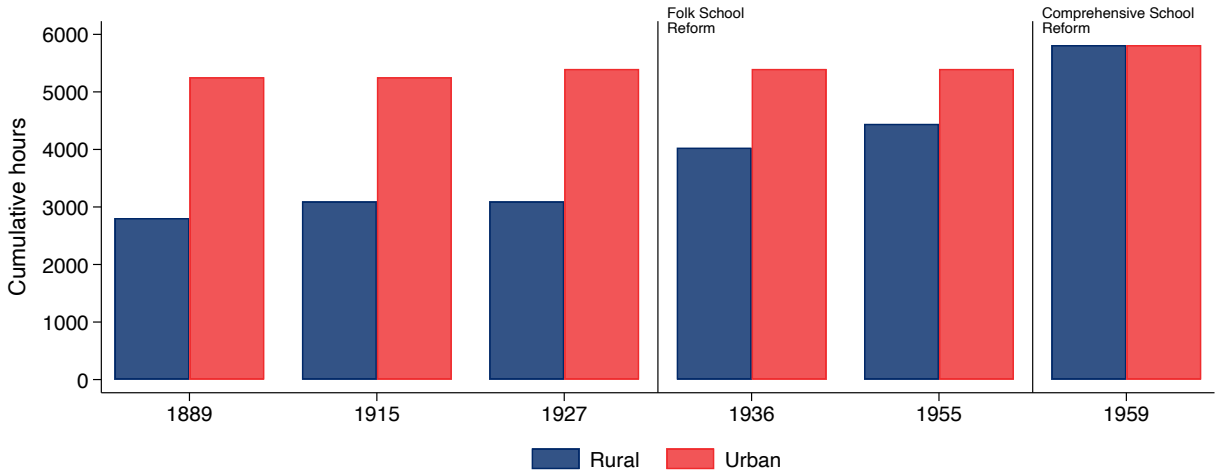
The differences in instruction time between rural and urban areas were considered a problem from early on. The law was revised in 1915, raising the minimum number of weeks of instruction time in the last four primary school grades to 14 weeks.[6] Nevertheless, the differences in the standards and the intensity of primary school education were exacerbated during the economic crises in the 1920s. As education was mostly locally funded, variation in the local economic conditions meant that the municipal authorities' ability to invest in primary education began to diverge. In 1935, urban areas provided 211 days of primary education, on average, while the average in rural areas was just 89.

2.4 Education policy in the Labour Party programs

Although often overlooked in the historical work on the Nordic labor movement, education policy was regarded as a key component in the political model for social change by the early social democrats. The movement's goal was to achieve greater social equality, for which education was seen as a critical tool, as it would alter the distribution of opportunities. The initial conditions for individuals would be made more equal with the help of state intervention in schooling. The

[6] The 1915 law increased the number of minimum and maximum hours of instruction time in rural schools by 11% and 40%, respectively.

Figure 2: Minimum cumulative hours in rural and urban primary schools, 1889–1959



Note: This figure shows the minimum cumulative hours of instruction over a span of seven years of mandatory education in rural and urban areas. Source: Norwegian Parliament Besl. O. No. 35, May 15th 1889; Besl. O. No. 36, May 28th 1889; Besl. O. No. 112, June 23rd 1915; Besl. O. No. 27, March 17th 1928; Besl. O. No. 114, June 7th 1936; Lov om folkeskulen pålandet Jun 16th, 1936.

primacy of educational policy was reflected in the fact that, in the Swedish case, many of the leading social democratic politicians and strategists were deeply involved in the planning and implementation of education policy from early on. (Rothstein 1998)

The importance of education reform is clear in the party programs of the Norwegian Labour Party. Already the very first program from 1885 called for "free and general education in state schools". Over time, these demands became more specific and clear emphasis was put on equal opportunities for children in different parts of Norway. In 1903, the party called for general primary school "for all children in the society", for "increase in the minimum hours of instruction", and that "the countryside primary schools should be brought to the same level as the town primary schools". By the 1930s, the urgency of education reform was so clear that the party program listed it as the third objective after democratic rights and equal justice. According to the program, "primary schools should be turned into a general comprehensive school that prepares children for further education". The party called for the central government to take over the financing of the schools and repeated the demand for uniform quality of primary schools and increased instruction time across the country.

2.5 The 1936 Folk School Law for rural areas

The Norwegian Labour Party's conviction that the country's education system was unequal and ill-suited for the demands of a rapidly changing economy shaped its reform priorities. To address the foundational problem of lack of equal access to high-quality primary education, the new

Labour government enacted the new law on primary schools in rural areas as one of its first major pieces of legislation in 1936. This reform marked the first step in a program that aimed at establishing a general comprehensive primary school which would prepare children for further education (Rust 1989).[7]

The new law on primary schools increased the minimum instruction time in rural areas to 16 weeks for the first three years of primary education and 18 weeks for the subsequent four years. In addition to these changes, the law decreased the maximum class size from 35 to 30. The funding from the central government was increased to cover a larger share of the base salaries of teachers and provided funding to pay teachers age and region related bonuses.[8] The state also took over other responsibilities that were previously carried out by the municipalities, such as school buildings, books, and inventories, as well as housing for the teachers. A new national curriculum was also introduced ("*Normalplanen for Folkeskolen*" from 1939) with a focus on skills rather than religious education and a ban on physical punishment. The new curriculum reduced the regional variation in the content of education (Rust 1989).[9]

The reform was a compromise, and the Labour Party decided not to advance some of its long-term goals like removing religious education from schools. Nevertheless, the 1936 reform was clearly ambitious, considering the state of primary schools in rural municipalities in the mid-1930s. Only 4% of the rural municipalities were providing more than 16 weeks of instruction in lower classes of primary schools in 1935. The percentage of municipalities fulfilling the criteria of the new law in higher grades was similarly low, just at 4%, while a mere 2% of the municipalities met the new requirements in all primary school grades. Thus, the new legislation forced a vast majority of rural municipalities to increase instruction time. The requirement on the maximum class size was also binding for most municipalities, with only 40% of municipalities meeting the provisions of the old law that there should be a maximum of 35 students per teacher. Just 22% had classes smaller than the new requirement of 30 students per teacher.

The law was passed swiftly, taking effect in July 1937. Municipalities were allowed to use five years to implement it fully. Hence, children starting school in August 1942 (born in 1935) were the first cohort for whom the new regime applied fully. The reform was, in fact, largely implemented in 1937–1938 and was greatly helped by an oversupply of teachers at the time. For example, the newspaper Arbeiderbladet (1936) reported that approximately 1,700 teachers were without work in fall 1935, while the expected number of new hires due to the reform was 700-800 teachers.

[7] Introducing a comprehensive school had been suggested by several "school commissions" from early-1900s onwards, but never gained enough support in the Parliament. Even as late as 1934, proposals to extend instructional hours in rural schools were voted down by conservative and agrarian parties.

[8] The law increased the share of central government funding from 45% of minimum teacher salary to 50%.

[9] See also Chapter 5 of "Lov om folkeskolen på landet", 1936.

3 Data

We built our main data set by linking together newly digitalized archival data on the rollout of the 1936 school reform, individual-level population-wide information on human capital and income, and municipality-level data on election results and pre-reform characteristics. To explore mechanisms, we also use survey data from 1957 on political preferences and data on candidate characteristics in national elections from Fiva and Smith (2017). We next describe each of these data sources in more detail.

3.1 Schools

We construct our treatment variable, discussed in detail in the next section, using municipality-level information on the provision of primary education, which we collected from Norwegian archives. These data originate from county-level primary school directors, who were tasked with sending a report every year to Statistics Norway. The information content of the data varies by year, but we can form a time-series for each municipality on the average weeks of school by grade from the 1920s onwards. For some years, we also observe the within-municipality distribution of children by weeks of education and student-teacher ratios. The data are reported separately for rural and urban municipalities, mirroring the separate legislation for rural and urban schools discussed in Section 2.3. We use this rural/urban definition throughout the paper.[10]

3.2 Human capital and income

We link the municipality-level measures of primary education to individual-level data using information on individual's municipality of birth. Our individual-level data contain population-wide information about educational attainment, earnings, demographics, and family links. In addition, for a subsample of men we have information from military records.

In our primary analysis, we focus on individuals born in rural municipalities. The "first generation" consists of individuals born between 1917–1940. We define the "second generation" as the children of the first generation (irrespective of their birthplace) and limit our analysis to those born between 1947–1976. We don't impose additional sample restrictions, but naturally exclude individuals with missing information.

We use completed years of education as our primary measure of human capital. This information is drawn from the 1960 and 1970 population censuses and Statistics Norway's educational database. These data are available for the full population. For men serving mandatory military service after 1969, we also observe IQ scores. Roughly 95% of Norwegian men from the relevant birth cohorts took arithmetics, vocabulary, and Raven Progressive Matrix

[10] Detailed description and aggregated data are available at https://www.ssb.no/a/histstat/publikasjoner/histemne-21.html.

tests between the ages of 18 and 20 during their draft board meetings for mandatory military service (see Sundet et al. 2004, for details). We use the composite score of these three tests as our second human capital measure. In addition, we observe annual income from 1969 onward as recorded in the pension register. This income measure includes labor earnings, taxable sick benefits, unemployment benefits, parental leave payments, and pensions. We construct proxies for lifetime income using average income over ages 50–64 for the first generation and average income over ages 30–34 for the second generation.

Table 1 presents a context and benchmark for our subsequent analysis by showing sample statistics for our main estimation sample. On average, first generation men have 9.0 years and women 8.4 years of education. Almost half of this cohort did not pursue education beyond primary school. The average annual income for men aged 50–64 is 180,000 Norwegian kroner (in 1998 prices), equivalent to roughly $21,000 (in 2020 prices). For women, the corresponding figures are 80,000 kroner or $9,000. There is a clear, albeit modest, gradient by treatment intensity, with those born in poorer (and thus more intensely treated, see Section 4.1) municipalities having somewhat lower education and income than those born in municipalities less affected by the reform. The lower panel of Table 1 confirms the second generation was better educated and had higher incomes than the first generation. Nevertheless, differences along the treatment intensity distribution remain evident.

3.3 Election results and municipality characteristics

Our primary election measures come from Municipality Database from the Norwegian Center for Research Data. These data provide municipality-level information on votes cast for the main political parties in national elections and on voter turnout. We complement these data with information on candidates in national elections, as reported in Fiva and Smith (2017), and follow them in dividing parties into six groups: (i) the Norwegian Labour Party, (ii) the Communist Party, (iii) the Agrarian Party, (iv) the Liberal Party, (v) the Conservative Party, and (vi) others.[11]

In some of our specifications, we control for per capita income in the municipality (collected from the tax records) and the share of the workforce in agriculture as recorded in the 1910 and 1930 Censuses. These data also come from the Municipality Database from the Norwegian Center for Research Data.

3.4 Survey data

We use individual-level data from a 1957 survey covering the Parliament election—the first election poll conducted in Norway (Rokkan et al. 1958). The survey collected information on

[11]The last group includes fringe parties, such as Christian Democrats, National Socialists, and from the 1970s onwards, a left-wing Maoist Party.

Table 1: Average Human Capital and Income

	Men				Women			
		Treatment Intensity				Treatment Intensity		
	All	Low	Medium	High	All	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: First-Generation								
Year of birth	1928.8	1928.5	1928.8	1929.3	1928.3	1928.0	1928.2	1928.6
Years of education	9.0	9.1	9.1	9.0	8.4	8.5	8.5	8.2
Post-mandatory education	0.56	0.58	0.57	0.54	0.50	0.54	0.53	0.45
Income at age 50–64	183,930	191,807	186,025	173,778	79,964	82,172	80,441	77,278
Observations	166,355	55,580	55,968	54,807	181,547	60,752	60,614	60,181
B: Second-Generation								
Year of birth	1961.0	1960.7	1960.9	1961.3	1961.2	1960.9	1961.2	1961.5
Years of education	12.2	12.2	12.2	12.2	12.5	12.4	12.5	12.5
Post-mandatory education	0.88	0.87	0.88	0.88	0.90	0.89	0.91	0.90
Income at age 30–34	241,514	245,205	244,855	234,434	138,570	138,738	138,283	138,680
Observations	181,061	61,484	59,372	60,205	169,088	57,527	55,269	56,292

Note: Sample averages for individuals born in 1917–1940 in rural municipalities (panel (a)) and their children born in 1947–1976 (panel (b)). Income is measured in Norwegian kroner (in 1998 prices) at ages 50–65 for the first generation and ages 30–34 for the second generation and includes labor earnings, taxable sick benefits, unemployment benefits, parental leave payments and pensions. Columns (2)–(4) and (6)–(8) report the averages separately by tertiles of the treatment intensity as defined in Section 4.1.

individual characteristics (age, gender, geographic location, family structure, education), voting, and several attitudinal questions. The survey also included questions on the respondents' children, and thus we can identify individuals whose children were affected by the 1936 Folk School reform.

4 Empirical approach

We follow an identification strategy similar to those used in Card (1992) and Acemoglu and Johnson (2007). This approach builds on the notion that the reform mattered more for rural municipalities that were further away from the new (national) standards and for the birth cohorts that underwent a larger share of their primary education under the new regime. We next discuss how we construct this treatment intensity measure and how we use it to estimate the impacts of the reform.

4.1 Treatment measures

Our identifying variation arises from two sources. First, the reform's impact varied across rural areas due to cross municipality differences in primary education provision before the reform. In particular, the reform had greater "bite" in municipalities that were far away from the post-reform requirements. In contrast, it had little impact on municipalities that already met or exceeded the new requirements. We measure this distance using information on instruction time shortly before the reform was passed. Specifically, we observe the share of children by instruction time brackets separately for grades 1–3 and 4–7 for each municipality in 1935 and summarize this information with the following municipality-level index

$$Z_j = \frac{3\sum_b s_{bj}\max(16-b,0) + 4\sum_b S_{bj}\max(18-b,0)}{28}, \tag{1}$$

where s_{bj} are the shares of children in grades 1–3 who received b weeks of education in municipality j in 1935, and S_{bj} are similar shares for grades 4–7. The numerator captures the average additional weeks of instruction a municipality would have to offer in order to meet the new requirements.[12] The denominator is a scaling factor corresponding to the cumulative change in minimum requirements induced by the reform (28 weeks over seven years of education). Thus, variable Z_j takes a value of one for a municipality at the pre-reform minimum in 1935 and zero for a municipality that already exceeded the new requirements before the reform.

The second source of identifying variation emerges between birth cohorts within a municipality. Those born before 1923 had completed primary education by the beginning of the implementation period in 1936 and thus were not exposed to the reform. Conversely, everyone born after 1935 started school after the implementation period and went through their entire primary education under the new requirements. Among 1923–1935 birth cohorts, the treatment intensity depends on the year of birth, the year the municipality implemented the reform, the "bite" the reform (Z_j), and the extent to which the municipality complied with the new requirements.[13]

We combine these sources of variation as a municipality–birth cohort level measure

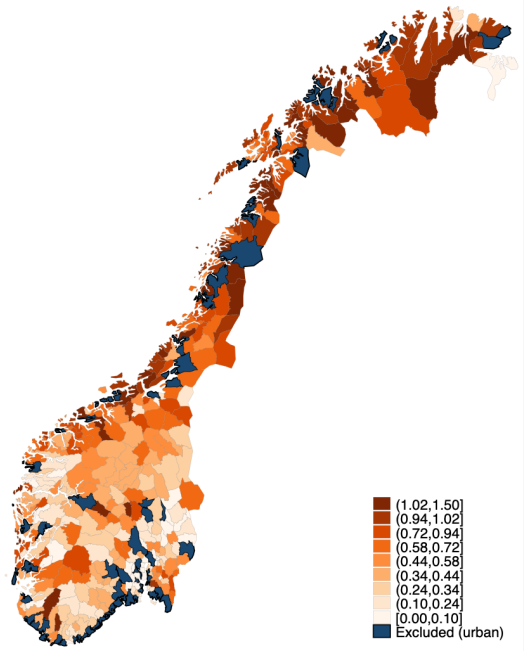
$$Z_{jc} = \pi_c Z_j \tag{2}$$

[12] Consider a municipality where half of the children in grades 1–3 received 12 weeks of education and the other half received 14 weeks of education in 1935. Assume also that all 4–7th graders received 18 weeks of education in 1935 in this municipality. Recall that the reform mandated that instruction time needed to be at least 16 weeks per year for grades 1–3 and to 18 weeks per year for grades 4–7. Thus the reform would have induced $3 \times [0.5 \times (16-12) + 0.5 \times (16-14)] = 9$ weeks of additional education for an average child living in this example municipality.

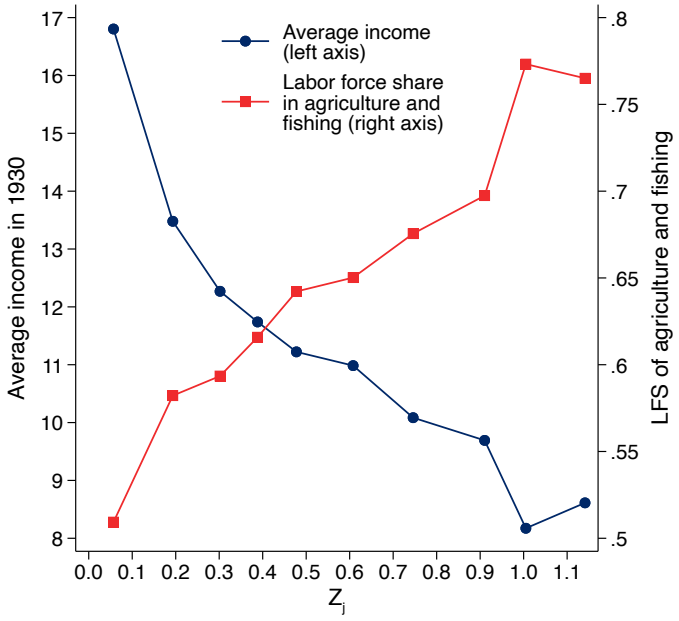
[13] For example, consider a municipality that followed the pre-reform minimum requirements before the reform and fully implemented the new requirements in 1938. In this case, children born in 1925 had started first grade at the age of 7 in 1932 and attended only their final (seventh) grade under the new requirements in 1938. Those born in 1926 attended school for two years after the reform, those born in 1927 for three years and so forth. Finally, everyone born in or after 1931 received the full treatment.

Figure 3: Treatment intensity

(a) Geographical distribution



(b) Pre-reform income and industrial structure



Note: Panel (a) presents the geographical distribution of the treatment intensity, Z_j, see equation (1). Panel (b) shows the average income and share of the labor force in agriculture and fishing in 1930 by deciles of the Z_j distribution; see Online Appendix Figure A2 for corresponding scatterplots.

where π_c is the share of years birth cohort c studied under the new requirements. As a baseline, we assume that all municipalities fully implemented the reform in 1938. That is, we set $\pi_c = 0$ for everyone born in or before 1924, $\pi_c = 1$ for everyone born in or after 1931, and $\pi_c = (c - 1924)/7$ for those born between 1925 and 1930. In the Online Appendix, we show that the results are robust to assuming slower implementation.

Having defined the treatment intensity measures, we next examine their geographical distribution and association with the pre-reform municipality characteristics. Panel (a) of Figure 3 establishes that municipalities in the northern (less economically developed) parts of Norway tended to be further away from the post-reform minimums, and hence more affected by the reform, than those located farther to the south. However, there is also variation across the whole of Norway and sometimes large differences between neighboring municipalities. Nevertheless, as the reform was designed to improve education in more deprived areas, treatment intensity is naturally associated with pre-reform municipality characteristics. Panel (b) of Figure 3 illustrates these differences by plotting municipalities' average income and share of the labor force working in agriculture and fishing in 1930 by deciles of Z_j. It shows that municipalities that were providing the minimum (or less) pre-reform instruction time in 1935 ($Z_j \geq 1$) were substantially

poorer and had a much larger share of the labor force working in the primary sector. These differences motivate the differences-in-differences approach we discuss next.

4.2 Specifications

We start our analysis by asking how the reform impacted human capital and income of the directly affected individuals. Our first approach is to estimate event-study regressions of the form:

$$y_{icj} = \sum_{k \in K} \beta_k (Z_j \times \mathbb{1}[c = k]) + \sum_{k \in K} (X_{j0} \times \mathbb{1}[c = k])\theta_k + \mu_c + \mu_j + \epsilon_{icj} \tag{3}$$

where y_{icj} is the outcome of interest for individual i born in year c in municipality j. On the right-hand-side, K is a set of birth years ranging from 1917 to 1940 (apart from the omitted category), Z_j is the pre-reform distance from the new requirements as defined in equation (1), $\mathbb{1}[c = k]$ is an indicator function taking value one if the individual was born in year k, X_{j0} is a vector of municipality characteristics measured before the reform, μ_c is a vector of year of birth fixed effects, and μ_j is a vector of municipality of birth fixed effects. In order to increase precision, we aggregate birth years into two year bins, i.e., 1917–18, 1919–1920, and so forth and use birth cohorts 1923–24 as the omitted category.[14] The parameters of interest are β_k, which measure the extent to which the outcome grows differentially between birth cohort c and birth cohort 1923–24 across rural municipalities that were differentially affected by the reform. To control for differential trends across different types of municipalities, we add regional dummies and municipality-level measures of pre-reform average income and industry structure to vector X_{j0}.

The estimates for β_k measure the impact of the reform under the strong parallel trends assumption (Callaway et al. 2024). In our context, this means that changes in municipalities with a specific treatment intensity, Z_j, provide a good counterfactual for what average changes in all other municipalities would have been had they had the same treatment intensity. Importantly, we focus only on rural areas, and thus our estimates are not driven by the overall convergence between rural and urban areas. Nevertheless, we already saw a correlation between treatment intensity and pre-reform geographical location, income, and industrial structure within rural municipalities. Although municipality fixed effects capture time-invariant differences between municipalities, poorer or less industrialized municipalities could have evolved differently than the more prosperous ones even in the absence of the reform. Hence, we examine alternative specifications allowing for differential trends by geographical location, average income, and industrial structure. Allowing for these differential trends also mitigates the effects of other policies targeted at poorer municipalities[15], but simultaneously removes some of the useful

[14] Note that using municipality of birth and not municipality of residence helps reduce a potential bias due to selective mobility across municipalities. We do not include a subscript for calendar year here because our individual-level measures consist of education, income, and cognitive ability test scores, all recorded at a fixed age.

[15] Specifically, the Labour government also enacted the Tax Equalising Act (*Skatteutjevningsloven*) in 1936, introducing

cross-municipality variation in the impact of the reform.

An advantage of the event-study specification is that it does not impose any assumptions on the timing of the possible effects. Thus, it allows us to examine whether outcomes start to change differently in the affected municipalities at the time when the reform is implemented. It also provides a falsification exercise for the parallel pre-trends assumption required for a causal interpretation of β_c. In particular, we check whether outcomes behave differentially prior to the reform across municipalities that were later affected differentially by the reform. In order to efficiently summarize the results from the event-studies, we also look at a more parsimonious version where we estimate a single treatment effect, β, from the regression:

$$y_{icj} = \beta Z_{jc} + \sum_{k \in K} (X_{j0} \times \mathbb{1}[c = k])\theta_k + \mu_c + \mu_j + \epsilon_{icj}, \tag{4}$$

where Z_{jc} is the municipality-birth cohort level measure of treatment intensity, constructed under the assumption that the reform was fully implemented in 1938 (equation (2)). The other variables are defined as in the previous specification.

We analyze the impacts on electoral outcomes using similar approaches. However, here, we cannot utilize variation across birth cohorts, because electoral outcomes are available only at the municipality–year level. Thus, we start with event-study specifications of the form:

$$y_{ptj} = \sum_{h \in H} \beta_h (Z_j \times \mathbb{1}[t = h]) + \sum_{h \in H} \theta_h (X_{j0} \times \mathbb{1}[t = h]) + \mu_t + \mu_j + \epsilon_{ptj} \tag{5}$$

where y_{ptj} is the vote share of party p in year t at municipality j, H is a set of election years between years 1927 and 1965, and Z_j and X_{j0} are the same treatment intensity measure and pre-reform observable characteristics described above. The parameters of interest, β_h, now measure the extent to which the vote share of a party increased faster between the 1933 elections (the omitted category) and elections in year t in municipalities more affected by the reform. When conditioning on region and pre-reform municipality characteristics, we also add a full set of interactions with all election years to account for the long time period and any non-linearity in election results.

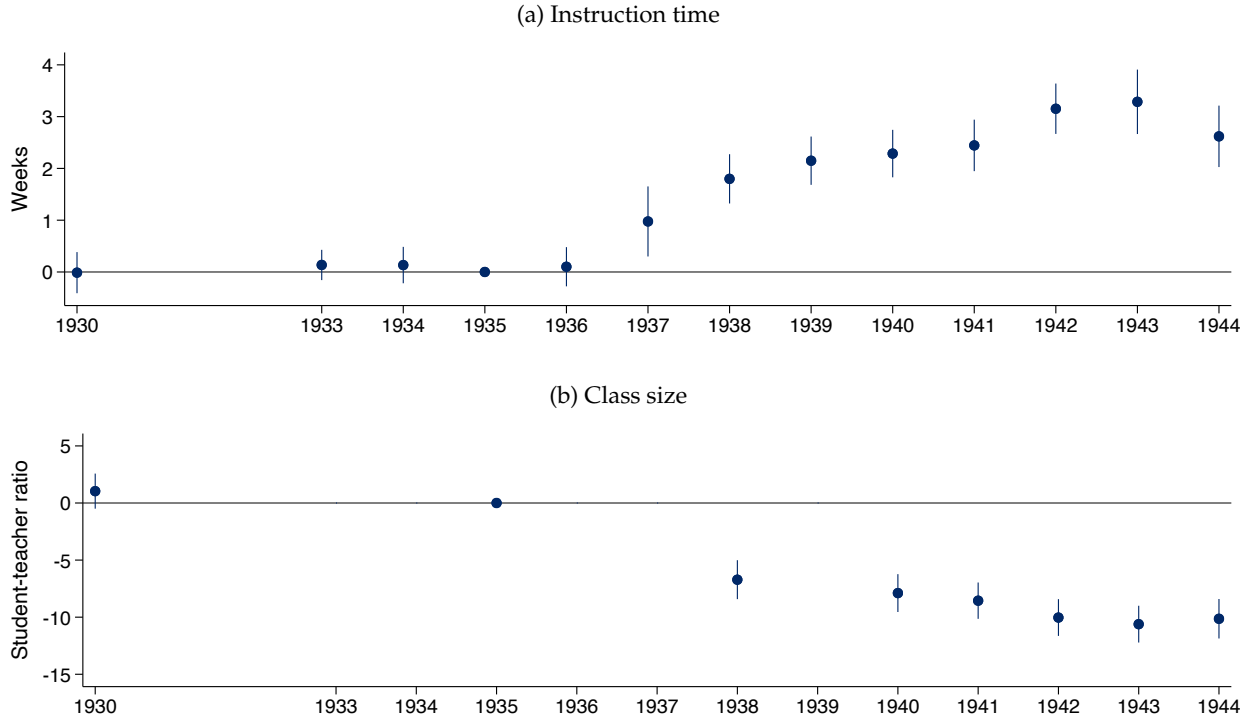
To summarize the estimates economically, we also report estimates from a differences-in-differences specification:

$$y_{ptj} = \beta (\mathbb{1}[t \geq 1945] \times Z_j) + \sum_{h \in H} \theta_h (X_{j0} \times \mathbb{1}[t = h]) + \mu_t + \mu_j + \epsilon_{ptj} \tag{6}$$

where $\mathbb{1}[t \geq 1945]$ is an indicator variable taking the value one for post-war years and zero for pre-war years, while other variables are as above.

new transfers to poor municipalities. However, this program was initially small, and Falch and Tovmo (2003) argue that it had a major impact only after the 1960s.

Figure 4: Event-Study Estimates for Instruction Time and Class Size



Note: This figure reports estimates for $y_{tj} = \sum_{h \in H} \beta_h (Z_j \times \mathbb{1}[t = h]) + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{tj} is educational input in year t at municipality j, H is a set of years for which data on the educational input is available, Z_j is our treatment intensity measure, and μ_t and μ_j are year and municipality fixed-effects, respectively. Panel (a) reports estimates when using weeks of education during an academic year as an outcome variable; panel (b) reports the estimates for average class size, approximated by dividing the number of students by the number of teachers at the municipality level. Standard errors are clustered at the municipality level. Information on class size was not collected in 1933, 1934, 1936, 1937, and 1939.

We interpret all our estimates as the intention-to-treat effect of the reform. We note that our measure of treatment intensity Z_j is constructed using data on instruction time. An alternative approach would be to define the treatment as instruction time and use Z_{cj} as an instrument. However, the exclusion restriction for this IV approach would require that the effects of the reform worked entirely via changes in instruction time, whereas, as is common also with other education reforms, the 1936 Folk school reform affected several dimensions of schooling simultaneously (see Section 2.5). Figure 4 illustrates this point by depicting event-study estimates for instruction time and class size. The top panel shows that instruction time evolved similarly in more and less exposed municipalities before the reform. As expected, more exposed municipalities expanded instruction time more from 1937 onwards than less exposed municipalities. In addition, they also significantly reduced class size, as shown in the bottom panel of Figure 4.[16]

[16]Online Appendix Figure A3 presents a complementary analysis by plotting the average instruction time and student-teacher ratio as functions of treatment intensity in the years 1930, 1935, 1938, and 1940-1944. It shows that

5 Results

This section presents our main results. We start by examining the impact of the Folk school reform on human capital and long-term income. This analysis is motivated by the reform's primary objective of harmonizing the standards of primary education across municipalities. Hence, if the reform was successful in increasing resources allocated to primary education in the municipalities most affected by it, we would expect an increase in years of education and earnings. Indeed, we find that this is the case. We then show that the reform may have had an intergenerational effect as well and increased the human capital and earnings of the children of those directly affected, although these estimates are less precise in some specifications. Finally, we present our core results showing that the reform increased the vote share of the Norwegian Labour Party in municipalities that were more affected by it. These effects are present both in the short and the long run and indicate that the reform played an important role in closing the rural-urban gap in the support for the Norwegian Labour Party. We return to the potential mechanisms behind these effects in the next section.

5.1 Direct impacts on education and income

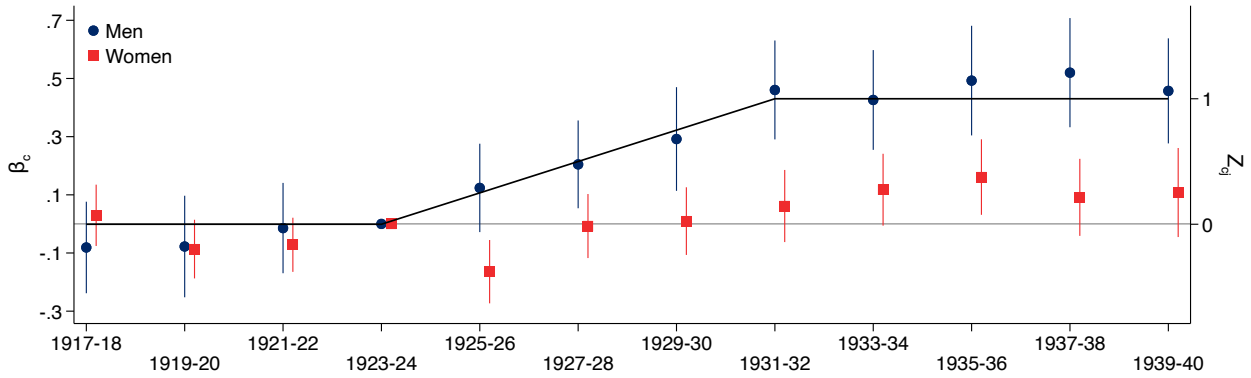
Figure 5 reports the baseline event-study estimates for the effect of the reform on years of education. Importantly, the reform did not change mandatory years of education, and hence it affected years of education only through the likelihood of continuing in post-primary education. We do not find pre-trends: there is no association between changes in years of education and treatment intensity among the birth cohorts that left primary education before the reform was implemented—those born 1923-24 and before (see also Online Appendix Figure A4). In line with the aims of the reform, men's years of education begin to increase faster in municipalities that were more affected precisely at the time the reform was launched. Reassuringly, the impacts are smallest for the cohorts that already had completed most of their primary education and largest for the birth cohorts whose entire primary education took place after the reform—those born in 1931-32 and subsequently. We also see that after these cohorts, the estimates level off, consistent with our interpretation that what we are observing are the effects of the reform, and not another process leading to educational convergence across these municipalities.

In short, the pattern presented in Figure 5 suggests that increasing the length of the school year and allocating more resources to primary education had a positive effect on post-mandatory education among men. The estimates for women, while qualitatively similar, are smaller and less consistently statistically significant at conventional levels.

The first row of Table 2 summarizes the effects on education using our second specification,

the pre-reform values of all inputs were highly correlated with our treatment intensity variable, but this correlation clearly declined after the reform was implemented in 1938.

Figure 5: Event-Study Estimates for First Generation's Years of Education



Note: Estimates for β_k from regression $y_{icj} = \sum_{k \in K} \beta_k (Z_j \times \mathbb{1}[c = k]) + \mu_c + \mu_j + \epsilon_{icj}$, where y_{ijc} is years of post-mandatory education of individual i born in municipality j in year c, Z_j is treatment intensity for municipality j, μ_c is a vector of year of birth fixed-effects, and μ_j is a vector of municipality of birth fixed-effects. Standard errors are clustered at the municipality of birth level. The solid black line shows treatment intensity for each birth cohort. The first cohorts impacted by the reform are those born in 1925-26, and cohorts born in 1931-32 and after are fully affected by the reform.

equation (4). The baseline estimates suggest that full exposure to the reform increased post-mandatory education of men by 0.47 years, corresponding to a 5% increase from the baseline of 9.0 years.[17] For women, the point estimate indicates an increase of 0.16 years, or a 2% increase from a baseline of 8.2 years. Columns (2) and (6) present results from specifications that allow differential trends for each of Norway's 20 regions and hence control for overall regional convergence that may have been correlated with the reform. Consistent with this correlation, the estimates are now smaller, but remain statistically significant for men. In the rest of the table, we allow for differential trends by 1930 average taxable income, changes in average taxable income between 1915 and 1930, and the industrial structure of the municipality in 1930 (see also the table notes). Online Appendix Figure A5 presents the corresponding event-study estimates using the same control variables. The most demanding specification incorporating differential trends by region, income, and industry suggests that full exposure to the reform had a statistically and economically significant (and precisely estimated) impact on men's education—an increase of 0.29 years (p-value $<$0.001). The impact on women is insignificant in the more demanding specifications.

We present further robustness checks in the Online Appendix. First, we show that the estimates are very similar when we use alternative approaches for estimating standard errors (Online Appendix Table A1) and assumptions for when the reform was implemented (Online Appendix Table A2). Second, we contrast our quasi-experimental results based on variation created by the reform to those using the observed weeks of education as the treatment variable.

[17] Full exposure refers to $Z_{jc} = 1$—that is, being born after 1931 in a municipality that provided only the pre-reform minimum weeks of education in 1935 (see equation (4)).

Table 2: Differences-in-Differences Estimates for the First Generation

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years of education	0.473	0.231	0.220	0.302	0.291	0.163	0.052	-0.015	0.004	-0.016
	(0.051)	(0.073)	(0.078)	(0.088)	(0.086)	(0.036)	(0.048)	(0.057)	(0.057)	(0.057)
Log income (age 50–64)	0.143	0.088	0.051	0.048	0.043	0.156	0.102	0.086	0.055	0.065
	(0.015)	(0.020)	(0.022)	(0.021)	(0.022)	(0.022)	(0.026)	(0.029)	(0.031)	(0.031)
Controlling for:										
Region	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Income	no	no	yes	no	yes	no	no	yes	no	yes
Industry	no	no	no	yes	yes	no	no	no	yes	yes

Note: Estimates for β from regression $y_{icj} = \beta Z_{jc} + \sum_{k \in K}(X_{j0} \times \mathbb{1}[c = k])\theta_k + \mu_c + \mu_j + \epsilon_{icj}$, where Z_{jc} is treatment intensity in municipality j for birth cohort c, X_{j0} is a vector of pre-reform covariates, μ_c is a vector of cohort fixed-effects, and μ_j is a vector of municipality of birth fixed-effects. Each regression stems from a separate regression, which differ in the dependent variable (rows) and specification (columns). Columns (2) to (5) and (7) to (10) condition on trends by 20 regions; columns (3) and (8) add controls for trends by quintiles of municipality's 1930 average taxable income and income growth between 1915 and 1930; columns (4) and (9) for quintile dummies of municipality's labor force shares in agriculture, fishing, manufacturing, and services in 1930; and columns (5) and (10) for income and industry structure. Each entry is from a separate regression. Number of observations: 164,286 (men) and 179,685 (women) for years of education; 161,924 (men) and 156,092 (women) for log income.

These estimates would be biased if, for example, the local provision of primary education increased with the local demand for post-mandatory education. Furthermore, the quality of the archival data on weeks of education varies across years and was not collected prior to 1930 or in 1931–32. Thus, measures for the earlier birth cohorts are largely based on extrapolations, and the estimates are likely to suffer from a substantially larger attenuation bias than our main estimates. Nevertheless, we find a similar pattern as in our quasi-experimental results: the estimates for men are positive and statistically significant, while all estimates for women are small, insignificant, and centered around zero (Online Appendix Table A3).

The remaining rows of Table 2 turn to average log income at ages 50–64 as the outcome variable.[18] The estimates are statistically and economically significant but sensitive to controlling for differential trends by region or 1930 municipality characteristics. This sensitivity suggests that average incomes in areas more affected by the reform converged towards incomes of other regions, and that this was most likely not just due to the causal effects of the reform. Consistent with this interpretation, we also see a small pre-trend among men in the baseline specification (Online Appendix Figure A6). Although this pattern suggests that baseline specifications that do not control for such convergence will lead to biased results, the estimates that control for differential trends by region, income, and/or industrial structure are quite stable. For example, the most demanding specification that controls for all of these differential trends implies that full

[18] To improve precision, we Winsorize log income at the 1st and the 99th percentile.

exposure to the reform raised the long-term income of men by 4.3 log points (p-value 0.053). Interestingly, in contrast to education, the estimates indicate that the reform also increased women's income. In the most demanding specification, the point estimate is 6.5 log points (p-value 0.035). This effect on women's income is consistent with the hypothesis that women benefited from the higher quality of schooling but, for a variety of reasons, did not pursue further schooling in these least developed rural parts of Norway.[19] For example, Online Appendix Table A4 shows that Norway's Folk school reform increased the likelihood of women to live in urban areas significantly, which could be interpreted as higher quality schooling enabling them to migrate to higher wage labor markets. Overall, we conclude that the Folk school reform likely increased long-term income, both for women and men, but this evidence has to be interpreted with greater caution than the education results, given the convergence trends in Norway at the time.

5.2 Intergenerational effects

The Labour Party's aim was to improve overall social mobility, by providing education opportunities for families from less advantaged areas. We now turn to examine whether the schooling reform also increased the human capital and income of the children of cohorts impacted by the reform.

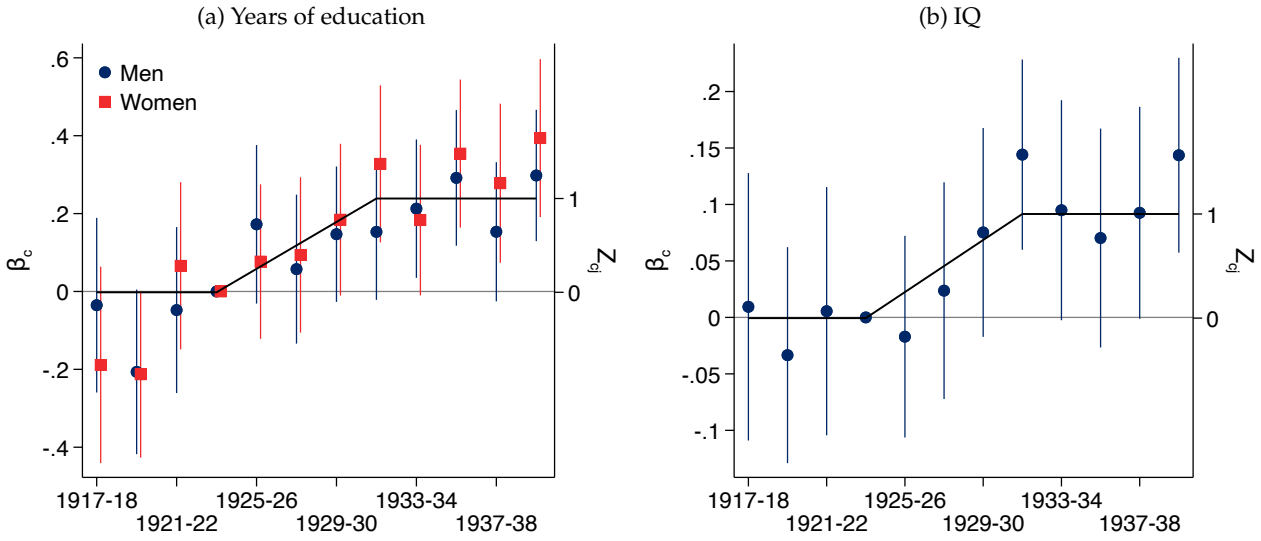
Figure 6 presents event-study estimates examining the effects for the second generation. We estimate equation (3) using data on the outcomes of children whose fathers were directly affected by the reform. The right-hand-side variables refer to the municipality and birth cohort of the person's father. As with our first generation results, we use years of education as the primary measure of human capital. For second generation men, we additionally have cognitive test (IQ) scores taken as part of the mandatory military service, which can be impacted by the quality of education.

We find that human capital of the children whose fathers were too old to have been directly affected by the reform develops similarly across municipalities. In contrast, for birth cohorts whose fathers were affected by the reform, we see a gradual increase in years of education. This effect levels off around the first birth cohorts fully exposed to the reform. The results for men and women are generally very similar for the second generation. The results for IQ present a similar, albeit noisier, pattern, indicating a positive effect on the second generation.

Table 3 presents the differences-in-differences estimates. The baseline estimates for men (column (1)) indicate that moving a father's municipality of birth from the pre-reform to the

[19] These results align with an earlier finding by Fischer et al. (2020), who examine two Swedish reforms: one in 1936 that increased the mandatory education duration from six to seven years, and another in 1937 that raised instruction time. Their results suggest that a 39 week cumulative extension in instruction time over mandatory education increased long-term income by 2.4 log points for men and 9.5 log points for women. Our estimates correspond to a 28 weeks cumulative extension of instruction time. When transformed to equivalent units, the estimates from Fischer et al. (2020) are quite similar: $28/39 * 2.4 = 1.7$ log points for men, and likewise, 6.8 log points for women.

Figure 6: Event-Study Estimates for the Second Generation



Note: Estimates for β from regression $y_{icj} = \sum_{k \in K} \beta_k (Z_j^P \times \mathbb{1}[c = k]) + \mu_c^P + \mu_j^P + \epsilon_{icj}$, where y_{ijc} is child's years of post-mandatory education (panel (a)) or IQ score in SDs (panel (b)), Z_j^P is treatment intensity in the father's municipality of birth j, μ_c^P is a vector of fixed-effects for the father's year of birth, and μ_j^P is a vector of fixed-effects for the father's municipality of birth. Standard errors are clustered at the municipality of birth level. The solid black line shows treatment intensity.

post-reform minimum requirements raises his sons' years of education by roughly 0.21 years, a 2% increase from a baseline of 12.3. The estimate using mother's exposure is almost identical. Similar to the first generation results, conditioning for trends by the father's birth municipality's region, pre-reform income, or pre-reform industrial structure reduces the point estimates (columns (2) to (5)). However, unlike in the case of the first generation, the estimates from more demanding specifications lose statistical significance. The pattern of estimates for IQ test scores and log income at ages 30 to 34 are similar, with point estimates varying between 0.03–0.11 standard deviations for IQ and between -1.4 and 3.4 log points for income.

Notably, the effects are stronger for women than for men. While the estimates for the years of education of daughters are similar to those for sons, the estimates for their log income are large and statistically significant in most specifications.

Overall, these results suggest that the 1936 Folk school reform impacted social mobility and educational opportunities more broadly within Norwegian society. These effects most likely reflect the within-transmission of human capital from parents in exposed municipalities to their children. Nevertheless, our findings for the second generation should be interpreted with caution, because many of the estimates from the more demanding specifications are not statistically significant.

Table 3: Differences-in-Differences Estimates for the Second Generation by Parent's Exposure

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: By father's exposure										
Years of education	0.208	0.185	0.129	0.070	0.049	0.306	0.146	0.118	0.069	0.074
	(0.048)	(0.071)	(0.077)	(0.076)	(0.078)	(0.053)	(0.077)	(0.085)	(0.083)	(0.086)
IQ	0.109	0.071	0.079	0.029	0.035	.	.	.	.	.
	(0.023)	(0.035)	(0.036)	(0.034)	(0.035)					
Log income	0.034	0.015	0.009	-0.014	-0.011	0.041	0.064	0.071	0.045	0.052
	(0.013)	(0.016)	(0.019)	(0.020)	(0.020)	(0.019)	(0.024)	(0.028)	(0.029)	(0.030)
B: By mother's exposure										
Years of education	0.207	0.117	0.117	0.074	0.110	0.278	0.079	0.038	0.042	0.024
	(0.048)	(0.069)	(0.073)	(0.081)	(0.082)	(0.052)	(0.067)	(0.078)	(0.078)	(0.081)
IQ	0.087	0.009	0.020	-0.013	-0.006	.	.	.	.	.
	(0.023)	(0.032)	(0.034)	(0.040)	(0.040)					
Log income	0.028	0.009	-0.009	-0.004	-0.012	0.024	0.058	0.062	0.060	0.061
	(0.011)	(0.016)	(0.018)	(0.018)	(0.018)	(0.017)	(0.024)	(0.028)	(0.033)	(0.033)
Controlling for:										
Region	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Income	no	no	yes	no	yes	no	no	yes	no	yes
Industry	no	no	no	yes	yes	no	no	no	yes	yes

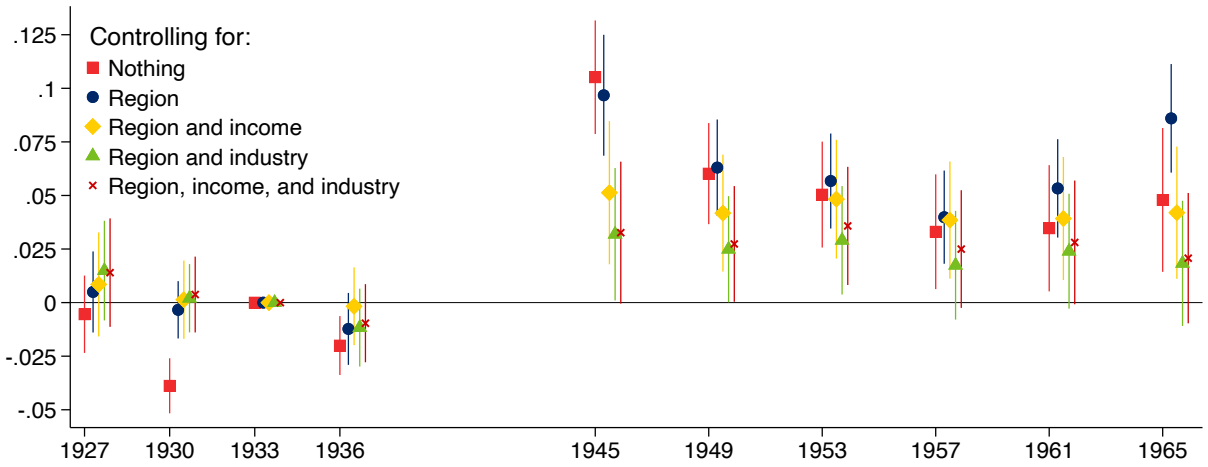
Note: Estimates for β from regression $y_{ijc} = \beta Z_{jc}^{P} + \mu_{c}^{P} + \mu_{j}^{P} + \epsilon_{ijc}$, where Z_{j}^{P} is the treatment intensity of the reform in the father's (panel (a)) or mother's (panel (b)) municipality of birth j, μ_{c}^{P} is a vector of fixed-effects for parent's year of birth, and μ_{j}^{P} is a vector of fixed-effects for parent's municipality of birth. Standard errors are clustered at the parent's municipality of birth level. Each regression stems from a separate regression, which differ in the dependent variable (rows) and specification (columns), see note for Table 2 for details of the specifications. Number of observations, panel (a): 175,971 (men) and 165,647 (women) for years of education; 17,3160 (men) and 157,316 (women) for log income; 147,180 for IQ. Number of observations, panel (b): 196,579 (men) and 181,648 (women) for years of education; 194,062 (men) and 172,022 (women) for log income; 154,415 for IQ.

5.3 Elections

Our results so far suggest that the Folk school reform generated important and long-term economic benefits. Since this reform was implemented during a critical juncture in Norwegian history and was the bedrock of the policies the Labour Party promised, could it also have changed Norwegian politics in a fundamental way? We now provide evidence that the answer to this question is yes. To do so, we focus on the Labour Party's vote share in national (parliamentary) elections during the period when the Norwegian welfare state was built and consolidated. We also discuss how the reform affected the electoral success of other political parties.

Figure 7 presents our event-study estimates for the Labour Party's vote share in the national

Figure 7: Event-Study Estimates for the Vote Shares of the Labour Party



Note: This figure presents estimates for $y_{ptj} = \sum_{h \in H} \beta_h (Z_j \times \mathbb{1}[t = h]) + \sum_{h \in H} \theta_h (X_{j0} \times \mathbb{1}[t = h]) + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{ptj} is the vote share of the Norwegian Labour Party in year t at municipality j, Z_j is our treatment intensity measure and X_{j0} is a vector of pre-reform observable characteristics that vary across specifications. All background characteristics are entered in the form of quintile dummies and are interacted with year fixed effects. The blue dots show results when controlling for 20 regions; the yellow diamonds when Municipality's industry structure is measured by the labor force shares in agriculture and fishing, manufacturing, and services. The estimates measure the extent to which the vote share of a party increased faster between the 1933 elections and elections in year t in municipalities more affected by the reform. Standard errors are clustered at the municipality level. Online Appendix Figure A7 reports similar estimates for the other major parties.

elections. We use 1933 as the reference category and report estimates without additional controls, and while controlling for differential trends for larger regions, pre-reform income quintiles, and pre-reform industrial structure of the municipality. We do not find any pre-reform trends after controls are included (and only some weak evidence for pre-trends in the specification without controls).

Most importantly, we find that the vote share of the Norwegian Labour Party increased substantially faster between 1933 and 1945 in municipalities that were more affected by the reform. Depending on the specification, the point estimates suggest that the Labour Party's vote share rose between 3.2 and 10.5 percentage points more in municipalities that were at the pre-reform minimum in comparison to municipalities that met the post-reform requirements already in 1935. These estimates imply a large relative effect given that the Labour Party's baseline vote share in municipalities most affected by the reform (defined as $Z_j \geq 1$) was 32 percent in 1933. While the estimates are smaller for later elections, all specifications yield large, positive, and statistically significant estimates for every election until 1965.

The first row of Table 4 summarizes the effects on the Labour Party's vote share using standard differences-in-differences regressions (equation (6)). The point estimates for the Labour Party vary between 2.3 and 7.0 percentage points in municipalities that were fully exposed to the reform.

Table 4: Differences-in-Differences Estimates for the Vote Shares

	Vote share				
	(1)	(2)	(3)	(4)	(5)
Labour	0.070	0.068	0.042	0.023	0.027
	(0.013)	(0.010)	(0.013)	(0.012)	(0.013)
Communists	-0.012	-0.013	-0.008	-0.003	-0.005
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)
Agrarian	-0.005	-0.041	-0.016	0.005	0.000
	(0.010)	(0.012)	(0.014)	(0.012)	(0.012)
Liberal	-0.089	-0.053	-0.022	-0.018	-0.011
	(0.013)	(0.013)	(0.014)	(0.014)	(0.015)
Conservatives	-0.005	-0.027	-0.026	-0.028	-0.026
	(0.012)	(0.012)	(0.014)	(0.012)	(0.012)
Time trends by:					
Region	no	yes	yes	yes	yes
Income	no	no	yes	no	yes
Industry	no	no	no	yes	yes

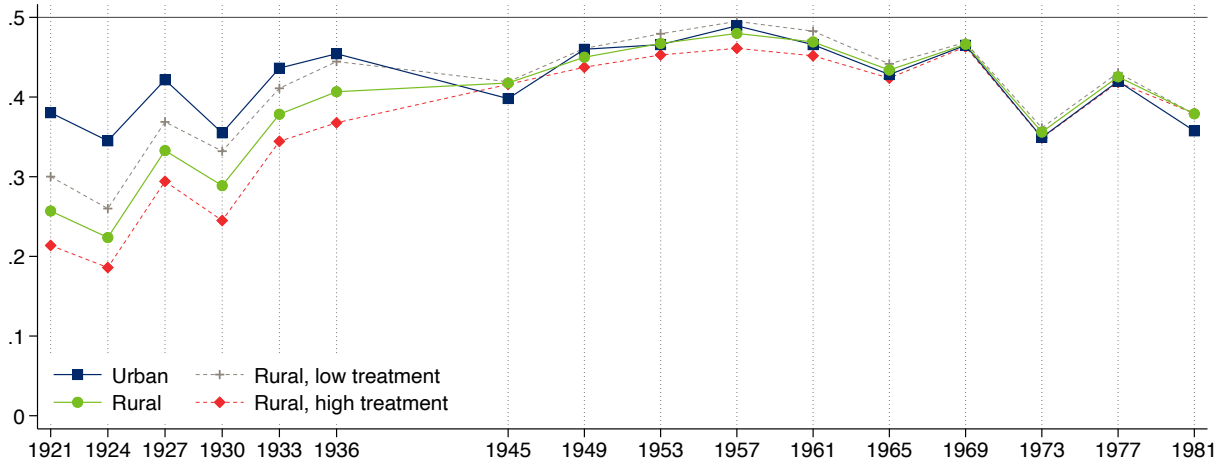
Note: Point estimates and standard errors (in parentheses) for β from regression $y_{ptj} = \beta(\mathbb{1}[t \geq 1945] \times Z_j) + \sum_{h \in H} \theta_h(X_{j0} \times \mathbb{1}[t = h]) + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{ptj} is the vote share for party p in municipality j in year t, Z_j measures treatment intensity (see equation (6)), $\mathbb{1}[t \geq 1945]$ is an indicator variable taking the value one for post-war and zero for pre-war years, X_{j0} is a vector of pre-reform characteristics, and μ_t and μ_j are year and municipality fixed-effects. Each regression stems from a separate regression, which differ in the dependent variable (rows) and specification (columns). Standard errors are clustered at the municipality level. Number of observations: 6,590.

These gains appear to be largely driven by losses of the Liberal Party, the Conservatives and the Communists, even if these effects are not as robustly significant as those for the Labour Party. We do not find any consistent pattern for the Agrarian Party.

To put these results into context, we estimate their implications for the overall vote share of the Labour Party in rural areas. A simple back-of-an-envelope calculation suggests that the Labour Party's rural vote share grew by 1.4–4.6 percentage points between 1933 and 1945 due to the reform.[20] For comparison, Figure 8 shows that the support for the Labour Party in rural Norway increased and caught up with its vote share in urban areas precisely after the school reform was enacted. For example, between 1933 and 1945, the party gained 3.9 percentage points in rural areas, while it lost 3.8 percentage points of its support in the cities. As a consequence, the traditionally higher support the Labour Party enjoyed in cities disappeared and the party has hence been equally popular in rural and urban areas. Within rural areas, this increase in support for the Labour Party entirely came from municipalities more affected by the reform.

[20] We conduct this calculation by multiplying the event-study estimates for 1945 with the "bite" of the reform for municipalities, Z_j, and then calculate population weighted averages of the implied effect over all rural municipalities.

Figure 8: Labour Party's vote shares in rural and urban areas



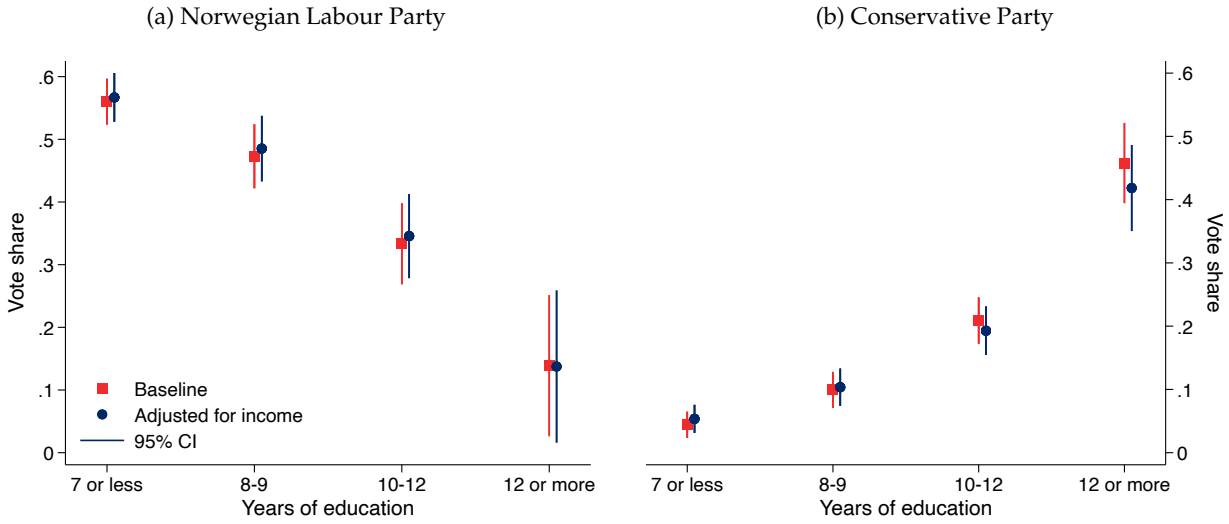
Note: This figure reports the vote shares of the Norwegian Labour Party in parliamentary elections separately for rural and urban areas. For rural areas, we also report vote shares separately for municipalities that had below and above median ($Z_j = 0.38$) treatment intensities.

Overall, our results indicate that the major educational reform, promised and swiftly implemented in 1936 by the Norwegian Labour Party, profoundly influenced the political landscape of the country, boosting the support for the party in the rural municipalities where it had a greater impact. In the next section, we will provide further evidence suggesting that this impact was a response to the party's successful implementation of a major reform that it had promised and that altered social mobility in the country in the coming decades. This successful reform implementation shifted the allegiance of parents and their children towards the Labour Party.

6 Mechanisms

In this section, we discuss potential mechanisms for the political effects of the 1936 education reform. We first reject two possible channels: (i) education directly increasing support for the Norwegian Labour Party, and (ii) the reform affecting voter turnout or the supply of local candidates. We then present two pieces of evidence supporting the interpretation that the reform led voters to adjust their views about and allegiance to the Labour Party. Specifically, we show that the impact on Labour Party vote share is substantially larger in municipalities that had no previous experience of Labour rule at a local level. Furthermore, we find that rural residents who attended primary schools after the reform harmonized resources and the length of the school year in rural areas—as well as their parents—were more likely to report voting for Labour and to agree that the party had effectively implemented its program in the 1957 electoral survey.

Figure 9: Labour Party and Conservative Party Support by Educational Attainment in 1957



Note: Estimates for μ_e from regression $y_i = \mu_e + \gamma X_i + \epsilon_i$, where μ_e is a vector of years of education (four categories, see x-axis labels in the figure). In panel (a), y_i is an indicator for voting for the Labour Party in the 1957 elections. In panel (b), y_i is an indicator for voting for the Conservative Party in 1957. The baseline specification does not include any control variables. In the second specification, we condition for the respondent's self-reported income.

6.1 Education and political preferences

We start with the competing hypothesis that the growth in the support for the Norwegian Labour Party is a direct effect of education (for example, because the more educated are more likely to support social democratic parties). Our results do not indicate any support for this hypothesis. First, it is inconsistent with the fact that the largest effects on the Labour Party vote took place in 1945 when the oldest cohort impacted by the reform was only 22 years old, meaning that the majority of the electorate had not been directly affected by the reform.

Second, and more directly, educational attainment and support for the Norwegian Labour Party was, in fact, negatively correlated during this period. Panel (a) of Figure 9 shows this association in 1957 when Norway's first post-electoral surveys was conducted. Among voters who had only primary education, 56% responded that they had voted for Labour. This share decreases monotonically with years of education to only 14% among those with 12 or more years of education. Conditioning on self-reported income yields almost identical results. Indeed, the more educated were substantially more likely to vote for the Conservative Party (panel (b)).[21] Given this negative correlation, and the timing of the effects, the electoral results we estimate are very unlikely to be driven by the direct effects of the reform on educational attainment.

[21] Online Appendix Figure A8 shows that the association between education and support for the Labour Party also holds when we restrict the sample only to rural areas.

6.2 Political participation

We next ask whether the reform's impact might be working through the increased political participation in affected municipalities. There are three channels of political participation that might be at work. The first is a turnout effect among the population, while the other two would be via the election strategies of the Labour Party, which could have decided to field more candidates in these municipalities or allocate more of its resources to such municipalities. We do not find support for any of these channels.

Figure 10 panel (a) presents event-study estimates for municipality-level turnout in national elections. The baseline estimates are large and significant for both the pre- and the post-period, reflecting other pre-existing differences across municipalities. Once we condition on the same pre-reform characteristics we used in our analysis in the previous section (any combinations of region, industry composition and municipality income), we do not find any indication on the reform affecting turnout.

The remaining panels of Figure 10 examine whether the reform affected the composition of candidates. Panel (b) explores the possibility that the share of Labour candidates in the exposed municipalities increased (this might result because of the party's strategy, the willingness of individuals in these areas to run for office as Labour candidates, or discouragement of potential candidates from other parties). However, our event-study estimates, using data from Fiva and Smith (2017) on candidates running in parliamentary elections between 1927 and 1965, provide no support for this hypothesis. All of our estimates are indistinguishable from zero in these regressions.

Finally, panel (c) of Figure 10 investigates the possibility that the Labour Party might have strategically allocated more candidates to heavily-affected municipalities at the expense of other areas, leading to a greater share of Labour candidates in the exposed municipalities. Once again, we do not find any evidence for such an impact.

In summary, we conclude that our estimated political effects do not arise from participation channels.

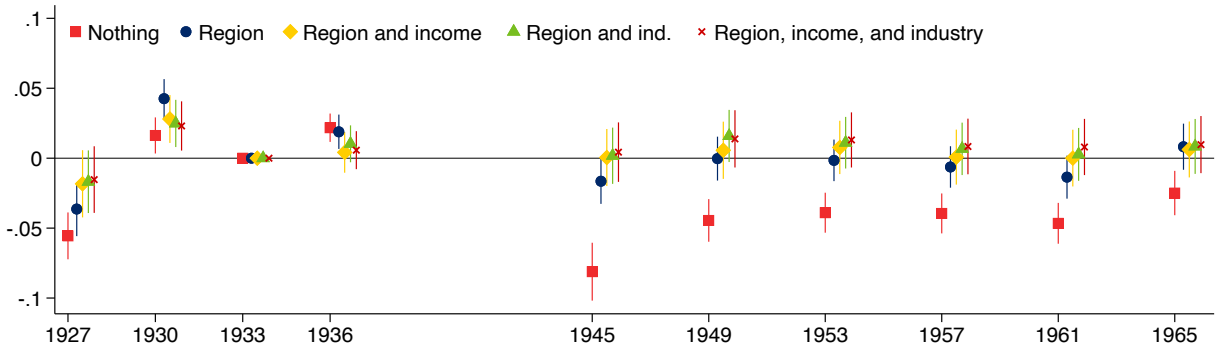
6.3 Local Labour rule

We now explore another potential mechanism behind our political results, by investigating whether the effects of the schooling reform were differential depending on recent experience of (local) Labour rule in the municipality. Specifically, we exploit the fact that the Labour Party governed more than a quarter of rural municipalities at the time of the reform.[22]

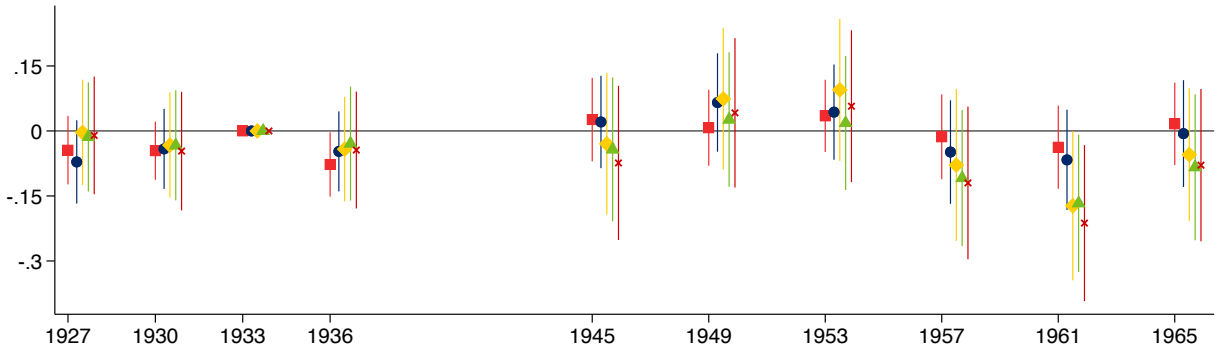
[22]In our main analysis, we define local Labour rule using data on the political affiliation of the mayor after the 1934 local elections. While some geographical clustering is evident (Online Appendix Figure A9, panel (a)), there are municipalities under Labour control in all parts of the country. Municipalities in both groups are also present over the entire support of the treatment intensity distribution (Online Appendix Figure A10).

Figure 10: Event-Study Estimates for Political Participation

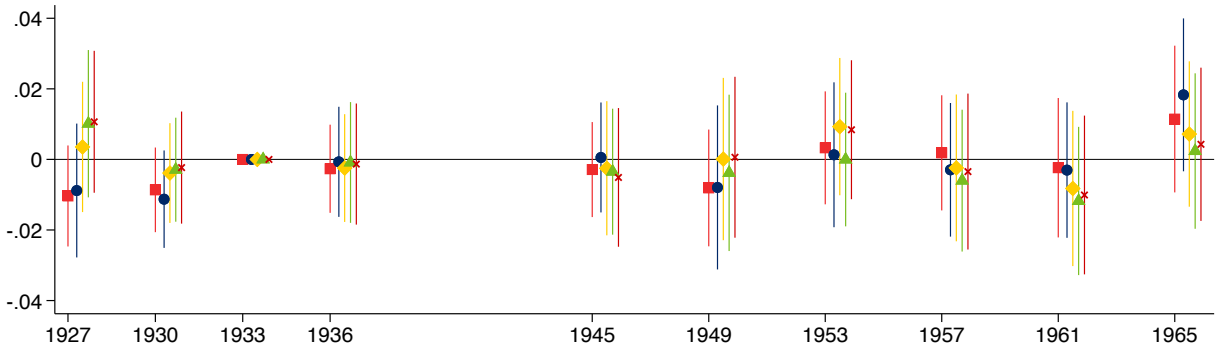
(a) Turnout



(b) Labour share of municipality's candidates



(c) Municipality's share of Labour candidates

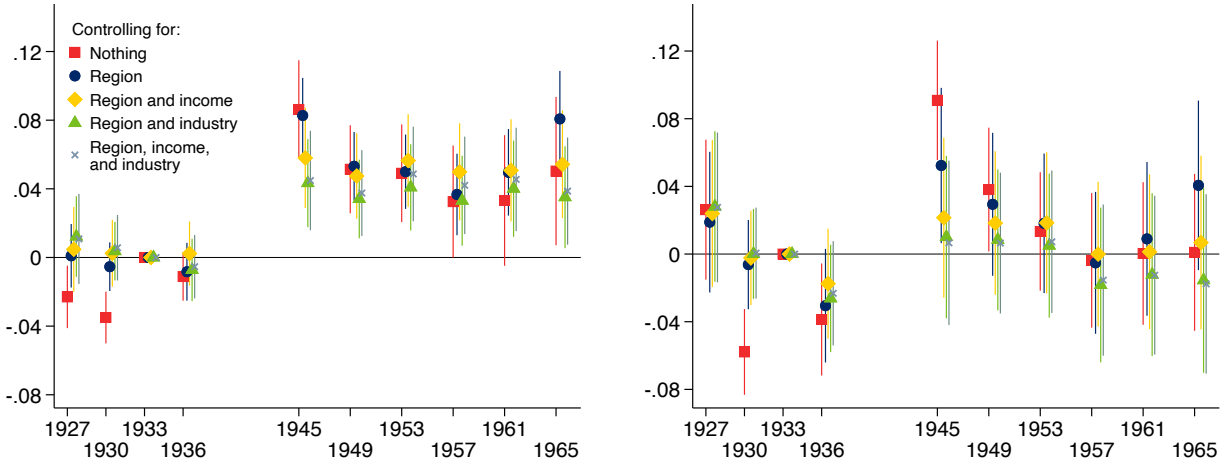


Note: This figure reports estimates for $y_{tj} = Z_j\beta_t + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{tj} is the outcome of interest in municipality j at year t, Z_j is our treatment intensity measure and X_{j0} is a vector of pre-reform observable characteristics that vary between specifications (see figure legend). The outcomes are turnout in national elections (panel (a)), share of Labour candidates out of all candidates from municipality j (panel (b)), and the share of Labour candidates coming from municipality j out of all Labour candidates in the election district (panel (c)). See Online Appendix Table A7 for a differences-in-differences version of these results.

Figure 11: Labor Vote Share Estimates by Earlier Exposure to Local Labour Rule

(a) Non-Labour Mayor in 1934

(b) Labour Mayor in 1934



Note: This figure reports results from regression $y_{ptj} = Z_j\beta_t + W_j\gamma_t + (Z_j \times W_j)\delta_t + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{ptj} is the vote share of the Norwegian Labour Party in year t at municipality j, Z_j treatment intensity, W_j is an indicator for the municipality having a Labour mayor in 1934, X_{j0} is a vector of pre-reform observable characteristics that vary between specifications (see figure legend), and μ_t and μ_j are year and municipality fixed-effects, respectively. Panel (a) reports estimates for β_t, i.e., the impact of the reform on municipalities with no prior exposure to Labour rule. Panel (b) reports estimates for $\beta_t + \delta_t$, i.e., effects for other municipalities that had a Labour mayor in 1934. See Online Appendix Table A8 for a differences-in-differences version of these results.

The event-study estimates reported in Figure 11 show that the effects of the 1936 school reform are significantly larger in non-Labour municipalities. We construct the figure by first estimating

$$y_{ptj} = Z_j\beta_t + W_j\gamma_t + (Z_j \times W_j)\delta_t + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj} \tag{7}$$

where y_{ptj} is the vote share of the Labour party in the national elections in year t at municipality j, W_j is an indicator variable taking value one if the local mayor in 1934 was from the Labour Party and zero otherwise, and Z_j and X_{j0} are the same pre-reform distance and pre-reform observable characteristics as in our baseline analysis. Panel (a) of Figure 11 reports estimates for $\hat{\beta}$, which captures the effect of the schooling reform on municipalities that did not have Labour local government. Similarly, panel (b) of Figure 11 reports estimates for $\hat{\beta} + \hat{\delta}$, corresponding to the effect of schooling reform on municipalities that had a Labour mayor in 1934.

The most plausible interpretation for the relative increase in Labour vote share in areas that did not have a mayor from the party relates to a "learning channel": rural municipalities that previously did not vote for a Labour mayor appear to have updated more positively from the party's successful implementation of its national reform agenda.

We employ three strategies to validate the robustness of our findings and the credibility of

our interpretation. First, we demonstrate that the results are robust for utilizing data on the affiliation of municipalities' mayors from 1928, either in conjunction with or as an alternative to 1934, and when applying a lagged dependent variable specification (Appendix Figures A11 and A12; Appendix Table A8). Second, we assess the possibility that the differential effects of the reform in municipalities with and without a Labour mayor might arise from the Labour Party typically losing votes in areas where it held local authority. We find no signs that local Labour governance led to a decline in the party's vote share prior to the reform, but, in line with our proposed mechanism, local Labour rule predicts a subdued rise in the party's vote share between 1936 and 1945 (Online Appendix Table A9 and Online Appendix Figure A9, panel (b)).

6.4 Support for the Labour Party among the directly affected and their parents

Finally, we use individual-level data from the 1957 election survey, the first of its kind conducted in Norway, to investigate whether those who directly benefited from the schooling reform, or their parents, were more likely to support the Labour Party. This survey does not include data on the respondents' municipality of residence, preventing us from calculating treatment intensity at the individual level in this dataset. However, we leverage information regarding whether a municipality had a low or high population density in 1957 to approximate the likelihood of respondents residing in affected areas. This categorization effectively segregates the data into urban and rural areas, with the latter, as we have observed so far, generally experiencing a higher intensity of treatment. In addition, we observe the respondents' and their youngest children's ages.

The first column of Table 5 examines the likelihood of voting for the Labor Party in the 1957 elections. In the top panel, we divide the respondents into four groups defined by their age and type of residential municipality. Specifically, we set the age threshold to 33 years because these younger birth cohorts attended primary school during and after the implementation of the reform. The estimates presented in the first column of Table 5 show that among older cohorts, those living in rural areas were 13 percentage points less likely to support the Labour Party than those living in urban areas. Remarkably, the gap is reversed for younger cohorts, among whom the Labour Party has a (-.133+.186=) 5.3 percentage point higher support in rural than in urban areas.[23] This comparison can be interpreted as a differences-in-differences estimate, where (part of) the younger rural birth cohorts were directly affected by the reform, while other groups were not. Naturally, this is a less fine-grained comparison than the ones we presented in Section 5 and should be interpreted with greater caution. Two potential concerns are worth considering: first, there might be greater attenuation, and second, other differential trends between rural and urban areas might confound this strategy. Nevertheless, as Table 5 shows, estimates using this strategy

[23]Online Appendix Figure A13 reports the corresponding vote shares also for the other main parties.

Table 5: Support for the Labour Party in the 1957 Election Survey Data

	Voted the Labour Party in 1957		Voted the Labour Party in first elections		Labour has implemented its agenda	
	(1)	(2)	(3)	(4)	(5)	(6)
A: Children						
Constant	0.614	0.624	0.600	0.607	0.521	0.527
	(0.023)	(0.023)	(0.022)	(0.022)	(0.023)	(0.023)
Low density	-0.133	-0.160	-0.122	-0.141	0.001	-0.012
	(0.034)	(0.035)	(0.032)	(0.033)	(0.034)	(0.036)
Young	-0.036	-0.033	-0.008	-0.002	-0.085	-0.078
	(0.045)	(0.045)	(0.047)	(0.046)	(0.044)	(0.045)
Low density ×	0.186	0.192	0.153	0.156	0.071	0.059
Young	(0.069)	(0.068)	(0.073)	(0.072)	(0.069)	(0.069)
B: Parents						
Constant	0.613	0.634	0.576	0.592	0.529	0.548
	(0.032)	(0.032)	(0.030)	(0.031)	(0.031)	(0.032)
Low density	-0.187	-0.225	-0.139	-0.163	-0.009	-0.046
	(0.048)	(0.049)	(0.044)	(0.046)	(0.048)	(0.050)
Young child	-0.014	-0.027	0.051	0.036	-0.029	-0.038
	(0.046)	(0.046)	(0.043)	(0.043)	(0.045)	(0.045)
Low density ×	0.128	0.136	0.034	0.048	0.041	0.052
Young child	(0.068)	(0.066)	(0.062)	(0.061)	(0.067)	(0.067)
Observations: children	1,105	1,103	1,218	1,214	1,166	1,162
Observatons: parents	852	851	1,011	1,008	899	897
Region FEs	no	yes	no	yes	no	yes

Note: This table reports results from differences-in-differences estimates using the 1957 election survey from specification $y_i = \alpha + \beta A_i + \gamma R_i + \delta(A_i \times R_i) + \epsilon_{ptj}$, where y_i is an outcome variable, A_i is an indicator for being potentially affected by the reform, R_i is an indicator for living in a low density (rural) area. In columns (1) and (2), the outcome is an indicator variable for person i voting the Norwegian Labour Party in 1957 and in columns (3) and (4) voting the Labour Party in the first elections where the person cast a vote. In columns (5) and (6), the outcome is an indicator for replying "Yes, absolutely" to the question: "Would you say that the Labor Party has shown the will and ability to implement this program in the years it has had government power?" In panel (a), A_i is one for individuals who were 33 years old or younger in 1957 and zero for other respondents. In panel (b), A_i takes value one if the respondent's youngest child is 25 or younger and zero otherwise. We exclude individuals born after 1925 from the parent sample. In columns (2), (4) and (6), we control for the respondent's region of residence.

are statistically significant (except for the reduction of Labour support in urban areas across birth cohorts). Furthermore, column (2) shows that the results are robust to controlling for region-fixed effects.

The estimates in panel (b) of Table 5 present a parallel analysis for parents whose children

were influenced by the reform. Specifically, we divide the sample into two groups: those who had children younger than 25 years old in 1957 and the others.[24] The estimates uncover a 19 percentage point gap in Labour Party support between rural and urban voters who either had no children or had children too old to be directly affected by the reform. In contrast, the discrepancy between rural and urban areas is 13 percentage points smaller among voters with children young enough to be impacted by the reform. Again, these differences-in-differences estimates remain statistically significant and robust, even when accounting for region fixed-effects.

These results suggest that this less fine-grained differences-in-differences strategy still captures the relevant source of variation and motivates us to look at other variables that are available in the 1957 survey using the same strategy.

Most notably, the 1957 election survey collected information about which party the respondents supported when they first voted. The third and fourth columns of Table 5 repeat the analysis for this outcome. The parents of affected children do not show an increased likelihood of voting for the Labour Party in their initial elections. But, their children—all of whom attained voting eligibility post-reform—are much more likely to vote for Labour in their first election, with magnitude of estimates very similar to those for their 1957 elections votes. These results reinforce our interpretation that the reform altered the voting behavior of those directly influenced by it.

The survey also contained questions concerning the reasons behind respondents' voting decisions. The final columns of Table 5 reports results based on the question: "*Would you say that the Labour Party has shown the will and ability to implement this program in the years it has had government power?*". Once again, we split the sample by respondent's (or her children's) age and rural-urban status.

Table 5 shows that respondents who were not affected by the reform had very similar opinions on the implementation of the party program both in rural and urban areas. However, rural respondents who were either young enough to have been directly affected or who had children young enough to have been impacted are more likely to strongly agree with this statement than respondents in the same categories living in urban areas. This difference seems to reflect movement from the category "Yes, with reservations" to "Yes, absolutely" (see Online Appendix Figure A14). However, these results should be interpreted with greater caution, since the differences-in-differences estimates reported in columns (5)-(6) of Table 5 are not statistically significant.

Overall, we interpret these results as shedding considerable light on the mechanisms via which the Norwegian Labour Party reaped the benefits of its successful implementation of the 1936 Folk schooling reform. The reform was implemented during a critical period of Norway's

[24]The age categories in the survey do not align perfectly with the age ranges of the affected respondents and their children. The category "Younger than 25 years old" is the closest we can get to the affected children in this survey.

history, as the Labour Party formed its first effective government. Against the background of an economy with widespread regional and class inequalities, the party promised a radical agenda, with a highly publicized schooling reform at its center. The fact that it implemented this reform swiftly and successfully convinced (rural) voters with no previous experience of direct Labor rule that it was competent and devoted to improving their schooling and economic conditions. More generally, the reform built allegiance from families it directly benefited. These voters, and their children, then supported the party during this formative phase of the making of Norwegian social democracy. Naturally, this is just an interpretation, and some parts of it cannot be tested directly, but the evidence provided in this section supports its broad outlines.

7 Conclusions

The 19th and early 20th centuries witnessed major challenges to the then-prevailing "capitalist systems". While some of these challenges were strongly influenced by Marxist/socialist ideas and attempted to overthrow the existing system, others took a decidedly reformist approach, working through democratic process and seeking power in order to implement fundamental reforms within the broader institutional structure of their countries. This reformist approach is most closely identified with the social democratic parties in Scandinavia, which were initially influenced by the same socialist ideas but then broke away from the Marxist tradition. Starting with Sweden in 1932, social democratic (labor or worker) parties in these countries came to power and implemented sweeping reforms. The key social democratic policy and institutional reforms included the strengthening of trade unions; the development of a corporatist model of wage setting with negotiations between trade unions, employers, and the government; active macroeconomic management; progressive taxation; national health care systems; social security; various other social programs; and also centrally, greater investment in publicly-provided education.

The Norwegian Labour Party, which assumed power in 1935, provides an ideal case study for understanding how the social democratic system came to be formed, in part because it campaigned with the promise to undertake a major schooling reform. As soon as it was in power, it implemented its promised reform, which harmonized the school year, raising minimum instruction time in economically less developed rural areas and boosting the resources available to rural schools.

We document that cohorts that were more intensively treated by this school reform—because of their location and age at the time of reform—achieved significantly more education. Our results suggest that also their subsequent labor income rose, though these estimates are sometimes less precise because of other confounding trends favoring rural areas.

More importantly, we find that residents of municipalities that benefited from the 1936

schooling reform became much more likely to vote for the Labour Party. Perhaps even more strikingly, the additional support for this party persisted for several decades, with both individuals directly impacted by the reform and their children becoming more likely to vote for the party.

Our evidence suggests that these results are not mediated by the direct effects of education (in fact, more educated individuals were less likely to support the Labour Party at the time). Nor are they explained by greater participation or greater attention from the Labour Party towards these municipalities. Rather, our results suggests that cohorts that benefited from the schooling reform, as well as their parents, rewarded the party for having kept its promise and for delivering a major reform that altered the nature of social mobility Norway.

Several economists and commentators are presently calling for major institutional reforms in Western nations and beyond (e.g. Esping-Andersen et al. 2002; Rajan 2019; Stiglitz 2019; Henderson 2021; Yang 2021; Bardhan 2022; Wolf 2023). A key question is whether the Norwegian experience in the 1930s is relevant for the current context and whether a credible promise and later delivery of major reforms could be the basis of a broad coalition that supports these reforms and the parties that implement them. While we have no direct answer to this question, we believe that a systematic analysis of the interplay between economic and social reforms and the formation of different political coalitions is an important area for future research.

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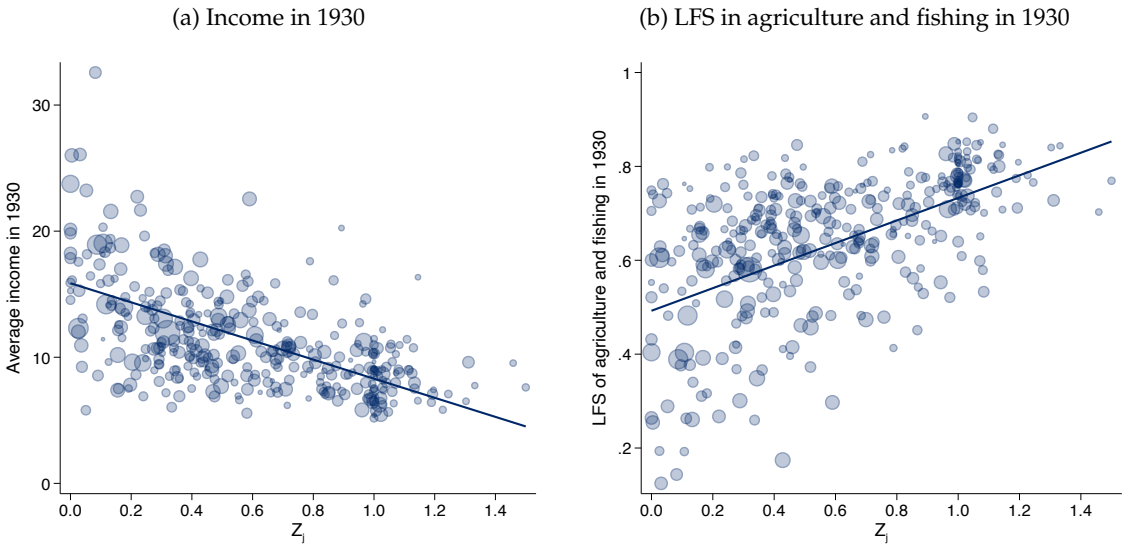
Figure A1: Norwegian Labour Party's Election Posters

(a) 1930

(b) 1933

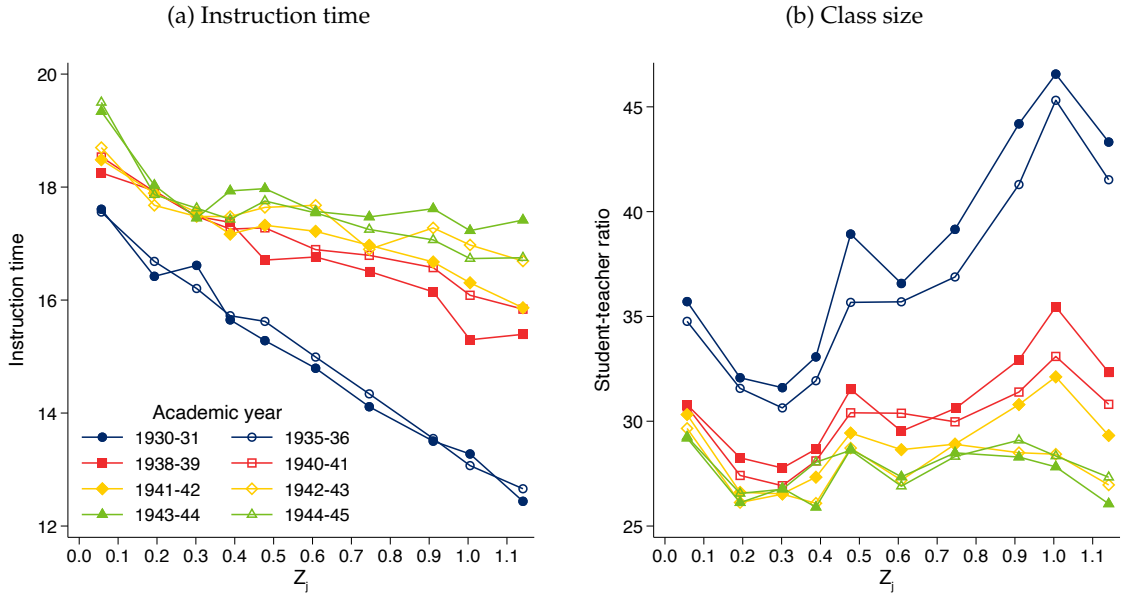
Note: The change in the rhetoric of the Norwegian Labour Party as illustrated by election posters. Both are designed by the same artist, Erling Nielsen. The 1930 campaign focused on urban industrial workers and the poster design is heavily influenced by Soviet art. The 1933 campaign led with a message of unity and the poster includes references also to fishing and agriculture with slogans "Cities and the countryside, hand in hand" and "The whole people in work". Source: Nasjonalbiblioteket (GOFP00005767 and P8), © Arbeiderpartiet/Arbark Arbeiderbevegelsens.

Figure A2: Treatment intensity and pre-war characteristics



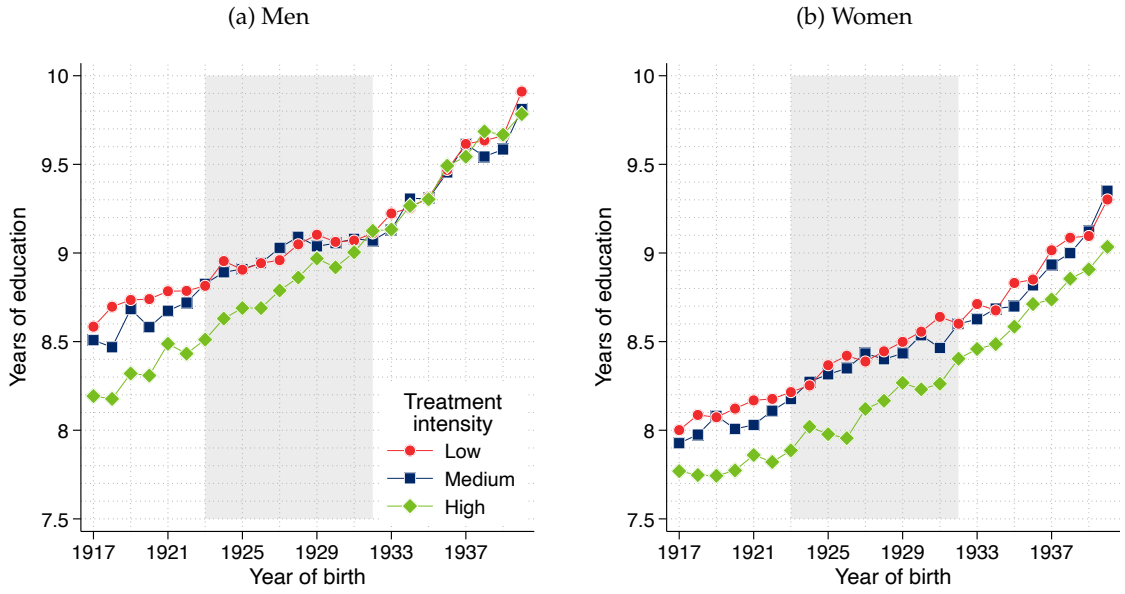
Note: This figure plots municipalities' average income (panel (a)) and labor force working in agriculture and fishing (panel (b)) in 1930 on Z_j.

Figure A3: Changes in instruction time and student-teacher ratio by treatment intensity



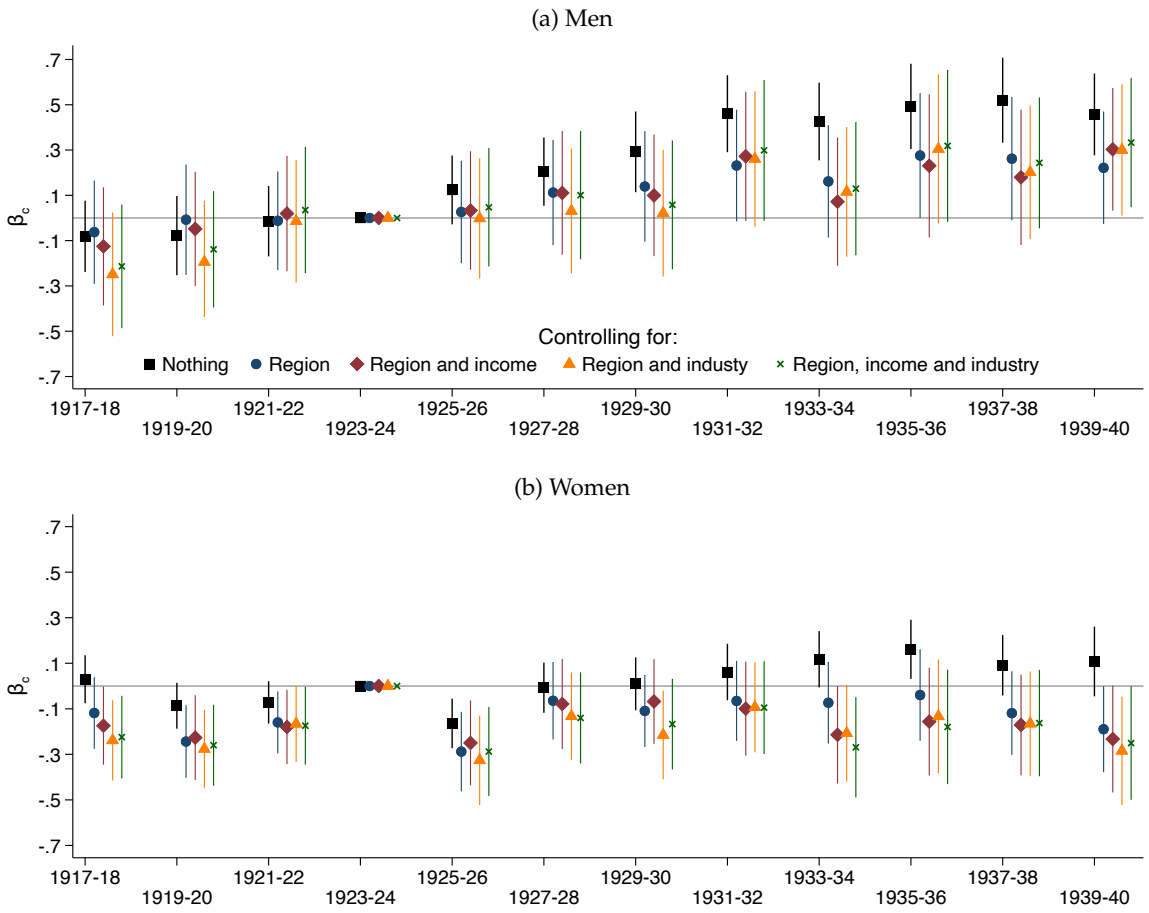
Note: Panel (a) shows average instruction time (vertical axis) by deciles of our treatment intensity measure (horizontal axis) in years 1930, 1935, 1938, and 1940-1944. Average instruction time in 1935–36 and our treatment intensity measure are closely connected by construction (see equation (1)). Panel (b) presents similar illustration of the association between our treatment intensity measure and average class size.

Figure A4: Average years of education



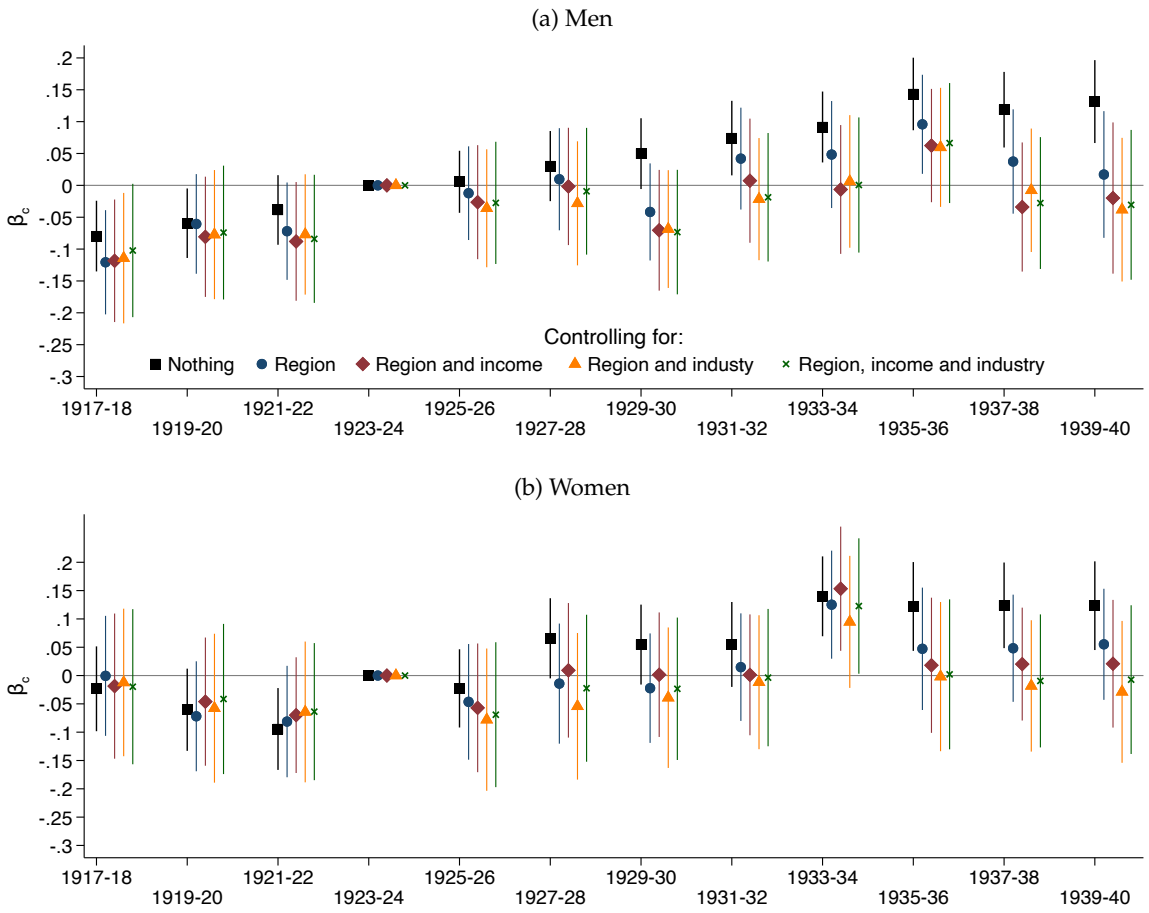
Note: Average years of education by year of birth and by tertiles of the treatment intensity as defined in Section 4.1

Figure A5: Event-study estimates for years of education, alternative specifications



Note: Estimates for β_k from regression $y_{icj} = \sum_{k \in K} \beta_k (Z_j \times \mathbb{1}[c = k]) + \sum_{k \in K} (X_{j0} \times \mathbb{1}[c = k])\theta_k + \mu_c + \mu_j + \epsilon_{icj}$, where y_{ijc} is years of post-mandatory education of individual i born in municipality j in year c, Z_j is treatment intensity for municipality j, X_{j0} is a vector of pre-reform observable characteristics (see figure legend), μ_c is a vector of year of birth fixed-effects, and μ_j is a vector of municipality of birth fixed-effects. Standard errors are clustered at the municipality of birth level.

Figure A6: Event-study estimates for log income, alternative specifications



Note: Estimates for β_k from regression $y_{icj} = \sum_{k \in K} \beta_k (Z_j \times \mathbb{1}[c = k]) + \sum_{k \in K} (X_{j0} \times \mathbb{1}[c = k]) \theta_k + \mu_c + \mu_j + \epsilon_{icj}$, where y_{ijc} is log income at ages 50–64 of individual i born in municipality j in year c, Z_j is treatment intensity for municipality j, X_{j0} is a vector of pre-reform observable characteristics (see figure legend), μ_c is a vector of year of birth fixed-effects, and μ_j is a vector of municipality of birth fixed-effects. Standard errors are clustered at the municipality of birth level.

Table A1: Diff-in-Diff Estimates for the First Generation: Alternative Standard Errors

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Years of education										
Point estimate	0.473	0.231	0.220	0.302	0.291	0.163	0.052	-0.015	0.004	-0.016
Baseline SE	(0.051)	(0.073)	(0.078)	(0.088)	(0.086)	(0.036)	(0.048)	(0.057)	(0.057)	(0.057)
Conley SE (50km)	(0.051)	(0.072)	(0.078)	(0.089)	(0.086)	(0.036)	(0.048)	(0.057)	(0.059)	(0.059)
Conley SE (100km)	(0.051)	(0.070)	(0.074)	(0.085)	(0.083)	(0.036)	(0.051)	(0.060)	(0.063)	(0.062)
Conley SE (150km)	(0.050)	(0.067)	(0.071)	(0.085)	(0.082)	(0.037)	(0.051)	(0.061)	(0.063)	(0.063)
B: Log income (age 50–64)										
Point estimate	0.143	0.088	0.051	0.048	0.043	0.156	0.102	0.086	0.055	0.065
Baseline SE	(0.015)	(0.020)	(0.022)	(0.021)	(0.022)	(0.022)	(0.026)	(0.029)	(0.031)	(0.031)
Conley SE (50km)	(0.016)	(0.020)	(0.022)	(0.022)	(0.022)	(0.023)	(0.027)	(0.030)	(0.033)	(0.032)
Conley SE (100km)	(0.016)	(0.020)	(0.022)	(0.021)	(0.022)	(0.025)	(0.028)	(0.032)	(0.034)	(0.034)
Conley SE (150km)	(0.017)	(0.021)	(0.023)	(0.022)	(0.022)	(0.024)	(0.029)	(0.032)	(0.035)	(0.035)
Controlling for:										
Region	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Income	no	no	yes	no	yes	no	no	yes	no	yes
Industry	no	no	no	yes	yes	no	no	no	yes	yes

Note: Estimates for β from regression $y_{icj} = \beta Z_{cj} + X_{j0}\theta_c + \mu_c + \mu_j + \epsilon_{icj}$, where Z_{jc} is treatment intensity in municipality j for birth cohort c, μ_c is a vector of cohort fixed-effects, and μ_j is a vector of municipality of birth fixed-effects. Columns (2) and (6) condition on trends by 20 regions; columns (3) and (7) for trends by quintiles of municipality's 1930 average taxable income and income growth between 1915 and 1930; columns (4) and (6) for trends by quintiles of municipality's labor force share in agriculture, fishing, manufacturing, and services in 1930. Each entry stems from a separate regression. Baseline standard errors are clustered at the municipality level. In addition, we report Conley (1999) standard errors that adjust for spatial correlation using 50, 100, and 150 kilometers distance cutoffs.

Table A2: Diff-in-Diff Estimates: Alternative Assumed Implementation Years

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Years of education										
1938	0.473	0.231	0.220	0.302	0.291	0.163	0.052	-0.015	0.004	-0.016
(baseline)	(0.051)	(0.073)	(0.078)	(0.088)	(0.086)	(0.036)	(0.048)	(0.057)	(0.057)	(0.057)
1936	0.483	0.231	0.222	0.291	0.287	0.137	0.046	-0.003	0.001	-0.013
	(0.051)	(0.071)	(0.077)	(0.087)	(0.085)	(0.035)	(0.048)	(0.055)	(0.056)	(0.056)
1940	0.457	0.230	0.219	0.310	0.299	0.176	0.053	-0.023	0.011	-0.012
	(0.052)	(0.077)	(0.081)	(0.093)	(0.089)	(0.038)	(0.050)	(0.059)	(0.061)	(0.061)
1942	0.430	0.207	0.199	0.289	0.279	0.179	0.048	-0.026	0.019	-0.002
	(0.055)	(0.084)	(0.088)	(0.098)	(0.095)	(0.041)	(0.054)	(0.063)	(0.066)	(0.066)
Most likely	0.447	0.242	0.254	0.312	0.315	0.172	0.063	0.018	0.054	0.039
	(0.051)	(0.069)	(0.068)	(0.076)	(0.073)	(0.035)	(0.043)	(0.048)	(0.047)	(0.047)
B: Log income (age 50–64)										
1938	0.143	0.088	0.051	0.048	0.043	0.156	0.102	0.086	0.055	0.065
(baseline)	(0.015)	(0.020)	(0.022)	(0.021)	(0.022)	(0.022)	(0.026)	(0.029)	(0.031)	(0.031)
1936	0.144	0.088	0.054	0.045	0.043	0.156	0.094	0.081	0.046	0.058
	(0.016)	(0.021)	(0.023)	(0.022)	(0.023)	(0.023)	(0.028)	(0.031)	(0.032)	(0.032)
1940	0.145	0.092	0.055	0.057	0.051	0.156	0.109	0.089	0.063	0.072
	(0.015)	(0.021)	(0.023)	(0.022)	(0.022)	(0.022)	(0.026)	(0.030)	(0.032)	(0.033)
1942	0.147	0.092	0.056	0.062	0.055	0.151	0.106	0.077	0.056	0.061
	(0.016)	(0.023)	(0.026)	(0.025)	(0.025)	(0.022)	(0.028)	(0.032)	(0.034)	(0.035)
Most likely	0.142	0.086	0.060	0.066	0.060	0.142	0.079	0.054	0.041	0.046
	(0.015)	(0.019)	(0.021)	(0.021)	(0.021)	(0.020)	(0.023)	(0.025)	(0.027)	(0.028)
Controlling for:										
Region	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Income	no	no	yes	no	yes	no	no	yes	no	yes
Industry	no	no	no	yes	yes	no	no	no	yes	yes

Note: This table reports the results corresponding to Table 4 using alternative definitions of Z_{cj} (equation (2)), where we alter the assumption of when municipalities implemented the reform. The top row of each panel is the baseline specification, where we assume that all municipalities implemented the reform in 1938. The next rows report the results based on assumptions that all municipalities implemented the reform in 1936, 1940 and 1942, respectively. The bottom row is based on the most likely implementation year as inferred by sudden changes in average weeks of schooling the municipalities reported to Statistics Norway.

Table A3: Differences-in-Differences Estimates for the First Generation using Observed/Extrapolated Weeks of Primary Education

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years of education	0.238 (0.037)	0.080 (0.038)	0.072 (0.036)	0.091 (0.037)	0.097 (0.036)	0.025 (0.028)	-0.035 (0.029)	-0.058 (0.029)	-0.039 (0.029)	-0.053 (0.029)
Log income (age 50–64)	0.065 (0.012)	0.026 (0.011)	0.016 (0.012)	0.018 (0.011)	0.016 (0.012)	0.060 (0.016)	0.012 (0.015)	0.009 (0.014)	-0.003 (0.015)	0.001 (0.014)
Controlling for:										
Region	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Income	no	no	yes	no	yes	no	no	yes	no	yes
Industry	no	no	no	yes	yes	no	no	no	yes	yes

Note: Estimates for β from regression $y_{icj} = \beta W_{jc} + \sum_{k \in K}(X_{j0} \times \mathbb{1}[c = k])\theta_k + \mu_c + \mu_j + \epsilon_{icj}$, where W_{jc} is the observed (or extrapolated) cumulative weeks of primary education in municipality j for birth cohort c, X_{j0} is a vector of pre-reform covariates, μ_c is a vector of cohort fixed-effects, and μ_j is a vector of municipality of birth fixed-effects. Each entry is from a separate regression. See Table 2 for details of the pre-reform covariates. Municipalities reported weeks of education in the academic year 1930/31 and annually from the academic year 1933/34 onwards. Values for the missing years, as well as for missing values for some municipalities in other years, are approximated using linear interpolation/extrapolation. In order to reduce outliers, most likely due to particularly severe cases of measurement error, we Winsorize W_{jc} at the 1st and 99th percentile. Furthermore, to ease comparisons to our main estimates, we have rescaled W_{jc} so that a one unit change in W_{jc} corresponds to a 28 weeks cumulative increase over the seven years of primary education.

Table A4: Diff-in-Diff Estimates for the First Generation: Additional outcomes

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Employed										
1980	0.035	0.007	0.004	0.004	0.002	0.032	-0.010	-0.003	0.000	0.003
	(0.007)	(0.009)	(0.010)	(0.010)	(0.010)	(0.008)	(0.010)	(0.011)	(0.012)	(0.011)
1970	-0.005	-0.013	-0.018	-0.012	-0.015	0.029	0.009	0.006	-0.008	-0.005
	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.008)	(0.011)	(0.013)	(0.013)	(0.013)
B: Works in agriculture										
1980	-0.007	-0.013	-0.006	-0.002	0.000	0.001	0.005	0.012	0.018	0.017
	(0.005)	(0.008)	(0.009)	(0.009)	(0.009)	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)
1970	-0.014	-0.035	-0.017	-0.002	-0.001	-0.029	-0.029	-0.008	0.001	0.004
	(0.007)	(0.010)	(0.011)	(0.010)	(0.010)	(0.007)	(0.008)	(0.009)	(0.009)	(0.009)
C: Works in manufacturing										
1980	-0.002	-0.003	-0.004	-0.002	-0.003	-0.001	0.001	0.002	0.002	0.003
	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
1970	0.027	0.040	0.031	0.015	0.017	-0.020	-0.002	-0.007	0.001	-0.004
	(0.009)	(0.013)	(0.014)	(0.015)	(0.014)	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)
D: Lives in urban area										
1980	0.057	0.033	0.026	0.012	0.014	0.067	0.056	0.044	0.015	0.020
	(0.010)	(0.015)	(0.018)	(0.018)	(0.018)	(0.010)	(0.011)	(0.013)	(0.014)	(0.013)
1970	0.055	0.036	0.014	0.009	0.004	0.073	0.065	0.048	0.028	0.030
	(0.011)	(0.014)	(0.016)	(0.017)	(0.017)	(0.010)	(0.012)	(0.014)	(0.016)	(0.016)
Controlling for:										
Region	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Income	no	no	yes	no	yes	no	no	yes	no	yes
Industry	no	no	no	yes	yes	no	no	no	yes	yes

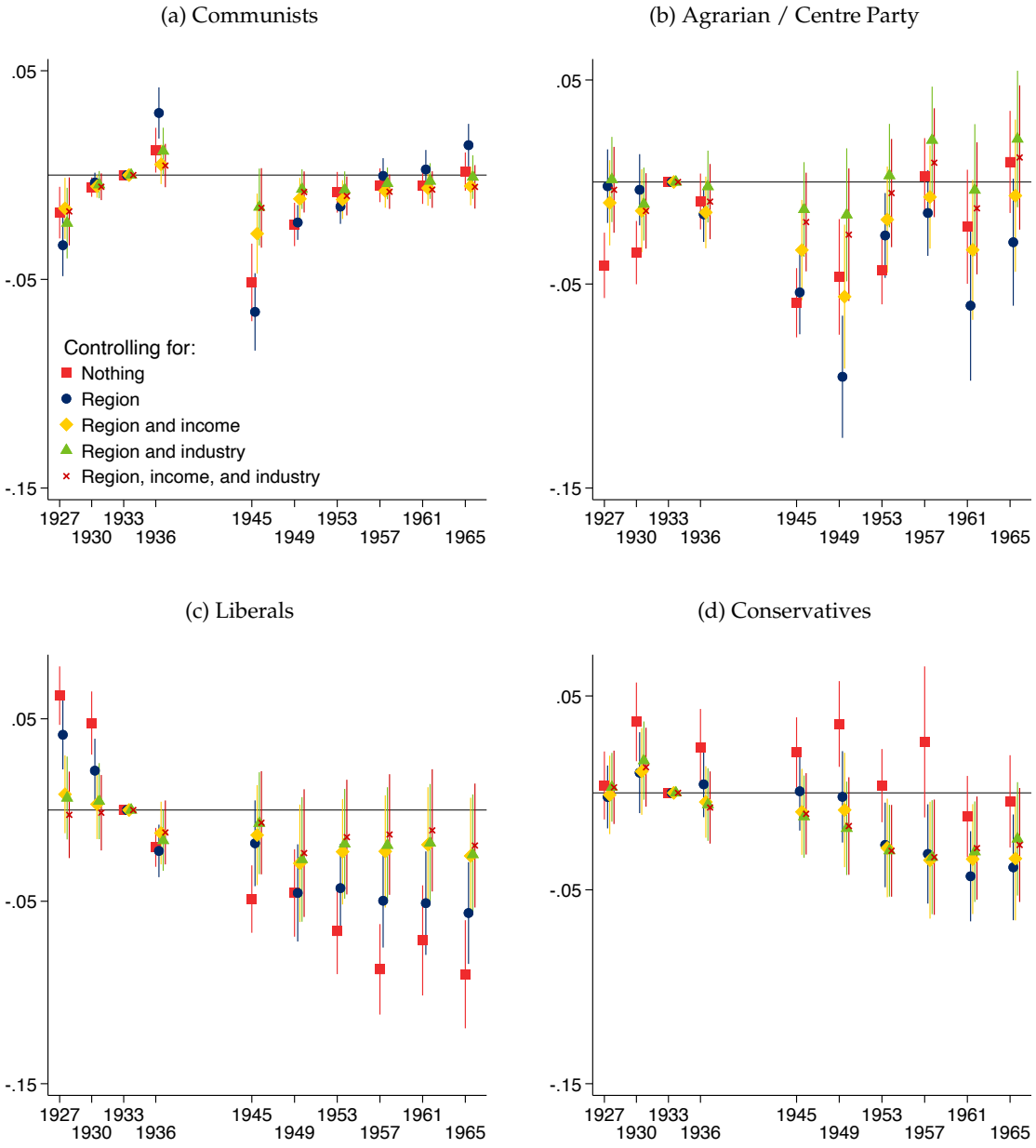
Note: Estimates for β from regression $y_{icj} = \beta Z_{cj} + X_{j0}\theta_c + \mu_c + \mu_j + \epsilon_{icj}$, where Z_{jc} is treatment intensity in municipality j for birth cohort c, μ_c is a vector of cohort fixed-effects, and μ_j is a vector of municipality of birth fixed-effects. Columns (2) and (6) condition on trends by 20 regions; columns (3) and (7) for trends by quintiles of municipality's 1930 average taxable income and income growth between 1915 and 1930; columns (4) and (6) for trends by quintiles of municipality's labor force share in agriculture, fishing, manufacturing, and services in 1930. Each entry stems from a separate regression.

Table A5: Diff-in-Diff Estimates for the Second Generation: Alternative Standard Errors

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A: Years of education: father's exposure										
Point estimate	0.208	0.185	0.129	0.070	0.049	0.306	0.146	0.118	0.069	0.074
Baseline SE	(0.048)	(0.071)	(0.077)	(0.076)	(0.078)	(0.053)	(0.077)	(0.085)	(0.083)	(0.086)
Conley SE (50km)	(0.047)	(0.065)	(0.069)	(0.069)	(0.070)	(0.053)	(0.070)	(0.077)	(0.075)	(0.077)
Conley SE (100km)	(0.047)	(0.063)	(0.066)	(0.065)	(0.065)	(0.053)	(0.069)	(0.077)	(0.072)	(0.076)
Conley SE (150km)	(0.045)	(0.062)	(0.065)	(0.065)	(0.065)	(0.054)	(0.072)	(0.081)	(0.076)	(0.079)
B: IQ: father's exposure										
Point estimate	0.109	0.071	0.079	0.029	0.035					
Baseline SE	(0.023)	(0.035)	(0.036)	(0.034)	(0.035)					
Conley SE (50km)	(0.022)	(0.032)	(0.033)	(0.031)	(0.032)					
Conley SE (100km)	(0.022)	(0.031)	(0.031)	(0.030)	(0.030)					
Conley SE (150km)	(0.021)	(0.031)	(0.032)	(0.030)	(0.031)					
C: Log income: father's exposure										
Point estimate	0.034	0.015	0.009	-0.014	-0.011	0.041	0.064	0.071	0.045	0.052
Baseline SE	(0.013)	(0.016)	(0.019)	(0.020)	(0.020)	(0.019)	(0.024)	(0.028)	(0.029)	(0.030)
Conley SE (50km)	(0.013)	(0.015)	(0.017)	(0.017)	(0.017)	(0.018)	(0.023)	(0.025)	(0.026)	(0.027)
Conley SE (100km)	(0.013)	(0.015)	(0.016)	(0.017)	(0.017)	(0.018)	(0.022)	(0.025)	(0.025)	(0.026)
Conley SE (150km)	(0.012)	(0.015)	(0.016)	(0.017)	(0.017)	(0.019)	(0.022)	(0.026)	(0.026)	(0.027)
D: Years of education: mother's exposure										
Point estimate	0.207	0.117	0.117	0.074	0.110	0.278	0.079	0.038	0.042	0.024
Baseline SE	(0.048)	(0.069)	(0.073)	(0.081)	(0.082)	(0.052)	(0.067)	(0.078)	(0.078)	(0.081)
Conley SE (50km)	(0.053)	(0.070)	(0.077)	(0.075)	(0.077)	(0.207)	(0.124)	(0.112)	(0.072)	(0.095)
Conley SE (100km)	(0.053)	(0.069)	(0.077)	(0.072)	(0.076)	(0.207)	(0.124)	(0.112)	(0.072)	(0.095)
Conley SE (150km)	(0.054)	(0.072)	(0.081)	(0.076)	(0.079)	(0.207)	(0.124)	(0.112)	(0.072)	(0.095)
E: IQ: mother's exposure										
Point estimate	0.087	0.009	0.020	-0.013	-0.006					
Baseline SE	(0.023)	(0.032)	(0.034)	(0.040)	(0.040)					
Conley SE (50km)	(0.023)	(0.030)	(0.031)	(0.036)	(0.036)					
Conley SE (100km)	(0.024)	(0.031)	(0.033)	(0.038)	(0.038)					
Conley SE (150km)	(0.023)	(0.031)	(0.033)	(0.038)	(0.038)					
F: Log income: mother's exposure										
Point estimate	0.028	-0.001	-0.015	-0.014	-0.018	0.024	0.071	0.076	0.069	0.070
Baseline SE	(0.011)	(0.015)	(0.017)	(0.016)	(0.017)	(0.017)	(0.022)	(0.024)	(0.027)	(0.026)
Conley SE (50km)	(0.011)	(0.016)	(0.017)	(0.017)	(0.017)	(0.015)	(0.021)	(0.022)	(0.025)	(0.024)
Conley SE (100km)	(0.011)	(0.016)	(0.017)	(0.017)	(0.017)	(0.015)	(0.020)	(0.022)	(0.024)	(0.024)
Conley SE (150km)	(0.011)	(0.015)	(0.016)	(0.016)	(0.016)	(0.014)	(0.020)	(0.021)	(0.025)	(0.024)
Controlling for:										
Region	no	yes	yes	yes	yes	no	yes	yes	yes	yes
Income	no	no	yes	no	yes	no	no	yes	no	yes
Industry	no	no	no	yes	yes	no	no	no	yes	yes

Note: Estimates for β from regression $y_{ijc} = \beta Z_{jc}^{P} + \mu_{c}^{P} + \mu_{j}^{P} + \epsilon_{ijc}$, where Z_{j}^{P} is the treatment intensity of the reform for father's (panel (a)) or mother's (panel (b)) municipality of birth j, μ_{c}^{P} is a vector of fixed-effects for parent's year of birth, and μ_{j}^{P} is a vector of fixed-effects for parent's municipality of birth. Standard errors are clustered at the parent's municipality of birth level. In addition, we report Conley (1999) standard errors that adjust for spatial correlation using 50, 100, and 150 kilometers distance cutoffs.

Figure A7: Vote shares of other parties



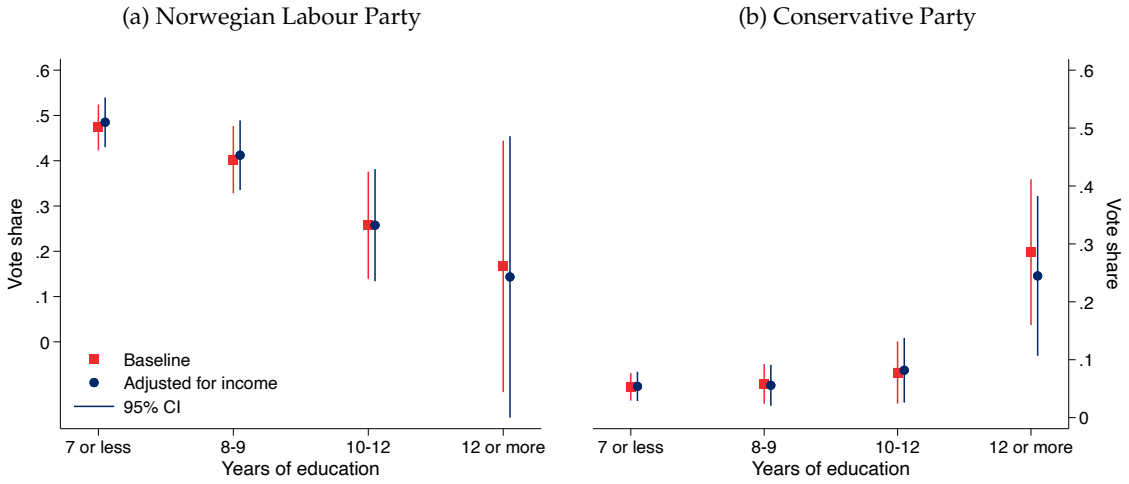
Note: This figure reports estimates for $y_{ptj} = \sum_{h \in H} \beta_h (Z_j \times \mathbb{1}[t = h]) + \sum_{h \in H} \theta_h (X_{j0} \times \mathbb{1}[t = h]) + \mu_t + \mu_j + \epsilon_{ptj}$ where y_{ptj} is the vote share of part p in year t at municipality j, Z_j is our treatment intensity measure and X_{j0} is a vector of pre-reform observable characteristics that vary between specifications (see figure legend). The estimates measure the extent to which the vote share of a party increased faster between the 1933 elections and elections in year t in municipalities more affected by the reform.

Table A6: Differences-in-Differences Estimates for the Vote Shares: Alternative Standard Errors

	Vote share				
	(1)	(2)	(3)	(4)	(5)
A: Labour					
Point estimate	0.0703	0.0685	0.0415	0.0232	0.0266
Baseline SE	(0.0133)	(0.0105)	(0.0125)	(0.0116)	(0.0126)
Conley SE (50km)	(0.0166)	(0.0115)	(0.0127)	(0.0115)	(0.0123)
Conley SE (100km)	(0.0174)	(0.0128)	(0.0127)	(0.0114)	(0.0120)
Conley SE (150km)	(0.0178)	(0.0136)	(0.0130)	(0.0117)	(0.0122)
B: Communists					
Point estimate	-0.0121	-0.0126	-0.0078	-0.0025	-0.0047
Baseline SE	(0.0046)	(0.0040)	(0.0047)	(0.0048)	(0.0049)
Conley SE (50km)	(0.0066)	(0.0056)	(0.0049)	(0.0050)	(0.0049)
Conley SE (100km)	(0.0086)	(0.0079)	(0.0050)	(0.0055)	(0.0050)
Conley SE (150km)	(0.0096)	(0.0090)	(0.0054)	(0.0058)	(0.0053)
C: Agrarian					
Point estimate	-0.0049	-0.0407	-0.0158	0.0052	0.0001
Baseline SE	(0.0100)	(0.0123)	(0.0135)	(0.0119)	(0.0123)
Conley SE (50km)	(0.0169)	(0.0136)	(0.0140)	(0.0146)	(0.0149)
Conley SE (100km)	(0.0187)	(0.0137)	(0.0142)	(0.0144)	(0.0149)
Conley SE (150km)	(0.0190)	(0.0142)	(0.0144)	(0.0144)	(0.0148)
D: Liberal					
Point estimate	-0.0895	-0.0532	-0.0216	-0.0175	-0.0107
Baseline SE	(0.0127)	(0.0126)	(0.0135)	(0.0144)	(0.0147)
Conley SE (50km)	(0.0169)	(0.0136)	(0.0140)	(0.0146)	(0.0149)
Conley SE (100km)	(0.0187)	(0.0137)	(0.0142)	(0.0144)	(0.0149)
Conley SE (150km)	(0.0190)	(0.0142)	(0.0144)	(0.0144)	(0.0148)
E: Conservatives					
Point estimate	-0.0049	-0.0274	-0.0264	-0.0279	-0.0265
Baseline SE	(0.0117)	(0.0118)	(0.0143)	(0.0120)	(0.0125)
Conley SE (50km)	(0.0158)	(0.0127)	(0.0145)	(0.0117)	(0.0119)
Conley SE (100km)	(0.0171)	(0.0143)	(0.0149)	(0.0118)	(0.0120)
Conley SE (150km)	(0.0178)	(0.0153)	(0.0151)	(0.0118)	(0.0120)
Time trends by:					
Region	no	yes	yes	yes	yes
Income	no	no	yes	no	yes
Industry	no	no	no	yes	yes

Note: Point estimates and standard errors for β from regression $y_{ptj} = \beta(Z_j \times \mathbb{1}[t \geq 1945]) + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{ptj} is the vote share for party p in municipality j in year t, Z_j measures treatment intensity (see equation (6)), $\mathbb{1}[t \geq 1945]$ is an indicator variable taking the value one for post-war and zero for pre-war years, X_j0 is a vector of pre-reform characteristics, and μ_t and μ_j are year and municipality fixed-effects. Baseline standard errors are clustered at the municipality level. In addition, we report Conley (1999) standard errors that adjust for spatial correlation using 50, 100, and 150 kilometers distance cutoffs.

Figure A8: Labour Party and Conservative Party Support by Educational Attainment in 1957: Rural sample



Note: Estimates for μ_e from regression $y_i = \mu_e + \gamma X_i + \epsilon_i$, where μ_e is a vector of years of education (four categories, see x-axis labels in the figure). In panel (a), y_i is an indicator for voting for the Labour Party in the 1957 elections. In panel (b), y_i is an indicator for voting for the Conservative Party in 1957. The baseline specification does not include any control variables. In the second specification, we condition for the respondent's self-reported income. The sample includes only respondents living in rural areas.

Table A7: Differences-in-Differences Estimates on Political Participation

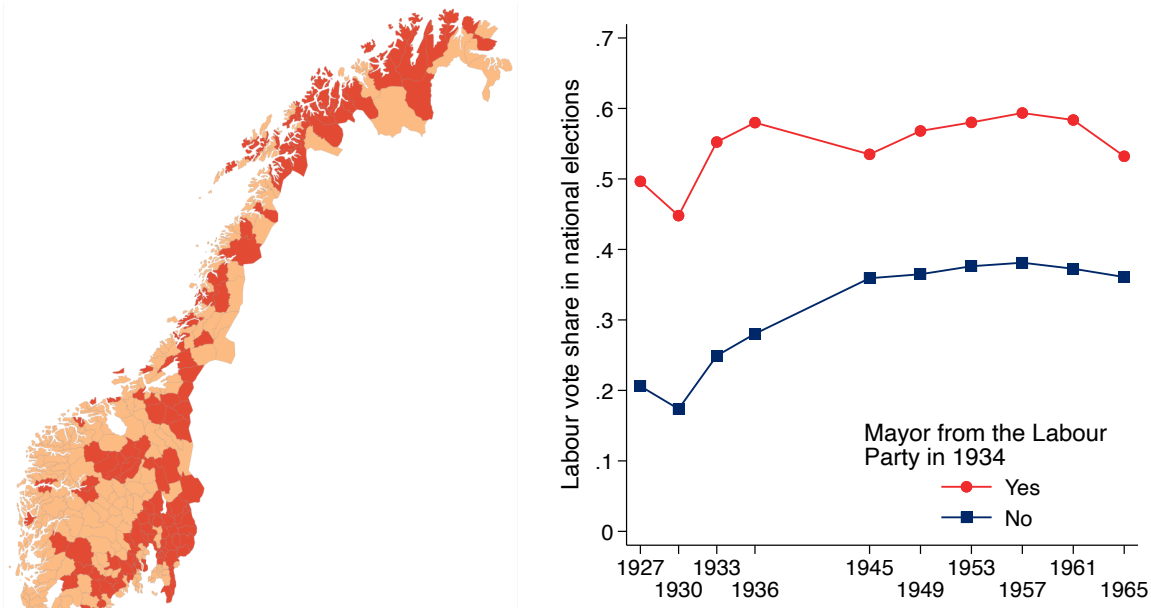
	(1)	(2)	(3)	(4)	(5)
Turnout	-0.042	-0.012	0.000	0.003	0.006
	(0.006)	(0.006)	(0.008)	(0.008)	(0.008)
Labour candidate share (out of all municipality's candidates)	0.048	0.040	-0.007	-0.038	-0.037
	(0.028)	(0.036)	(0.052)	(0.049)	(0.054)
Candidate share (out of all districts Labour candidates)	0.006	0.006	0.001	-0.005	-0.002
	(0.005)	(0.005)	(0.006)	(0.006)	(0.006)
Time trends by:					
Region	no	yes	yes	yes	yes
Income	no	no	yes	no	yes
Industry	no	no	no	yes	yes

Note: Point estimates and standard errors (in parentheses) for β from regression $y_{tj} = \beta(Z_j \times \mathbb{1}[t \geq 1945]) + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{tj} is either the share of Labour candidates out of all candidates from municipality j (panel (a)) or the share of Labour candidates coming from municipality j out of all Labour candidates in the election district (panel (b)), Z_j measures treatment intensity (see equation (6)), $\mathbb{1}[t \geq 1945]$ is an indicator variable taking the value one for post-war and zero for pre-war years, X_{j0} is a vector of pre-reform characteristics, and μ_t and μ_j are year and municipality fixed-effects.

Figure A9: Local Politics

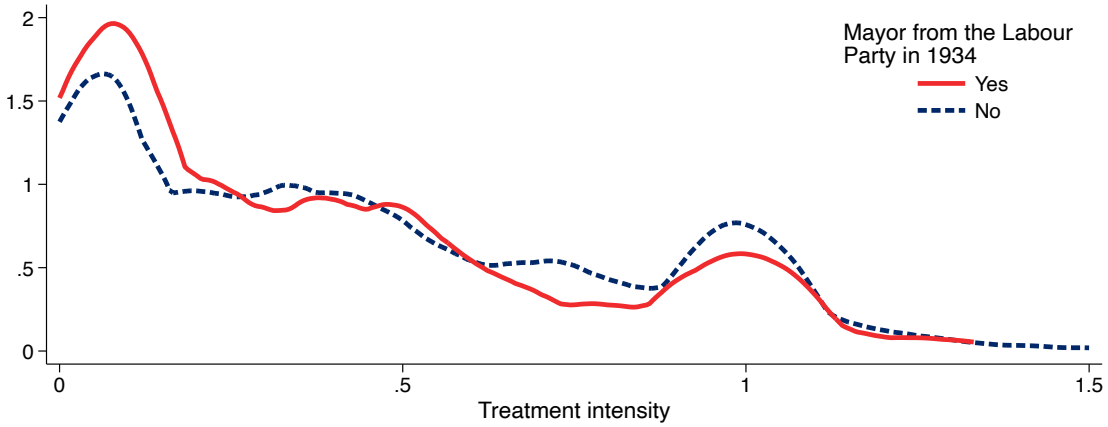
(a) Earlier Exposure to Local Labour Rule

(b) Labour vote share in national elections



Note: In panel (a), municipalities that had experienced local labor in 1934 are marked with the darker color. Panel (b) shows the support for the Norwegian Labour Party in national elections over time for these two types of municipalities.

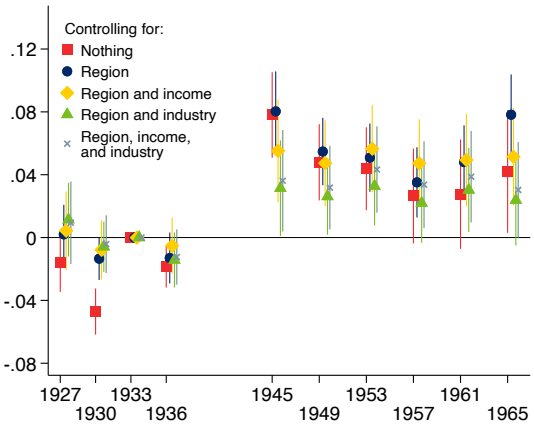
Figure A10: Overlap in Treatment Intensity by Earlier Exposure to Local Labour Rule



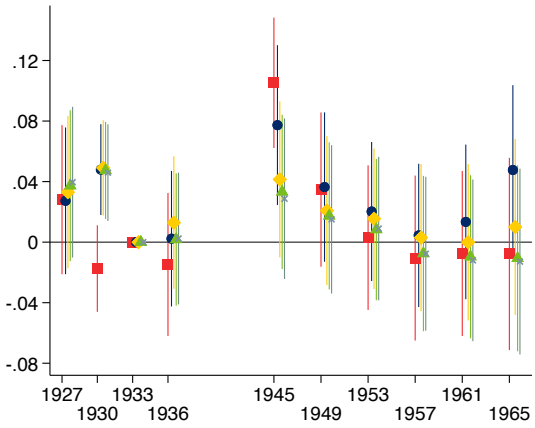
Note: The red solid line shows the distribution of our treatment intensity measure, Z_j, for municipalities that had experienced local Labour rule in 1934, and the blue dashed line for other municipalities.

Figure A11: Alternative definitions for local Labour rule

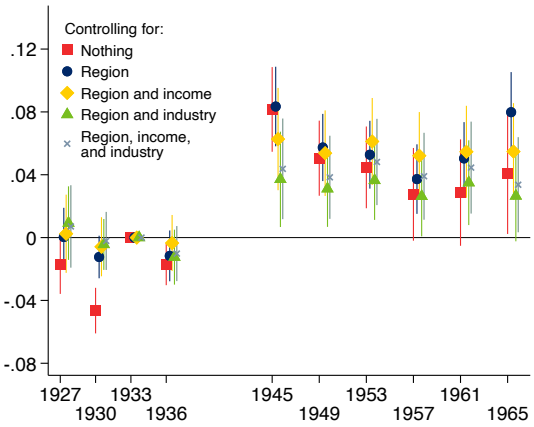
(a) Non-Labour Mayor in 1928



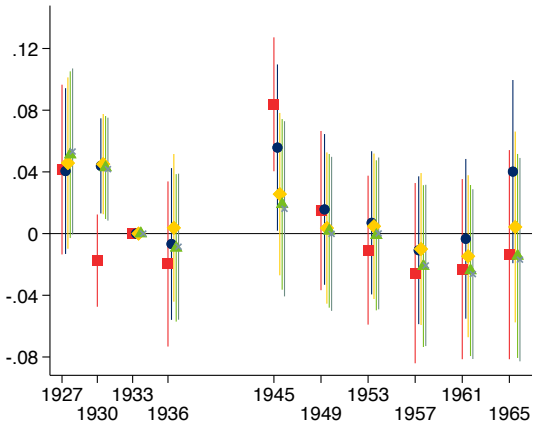
(b) Labour Mayor in 1928



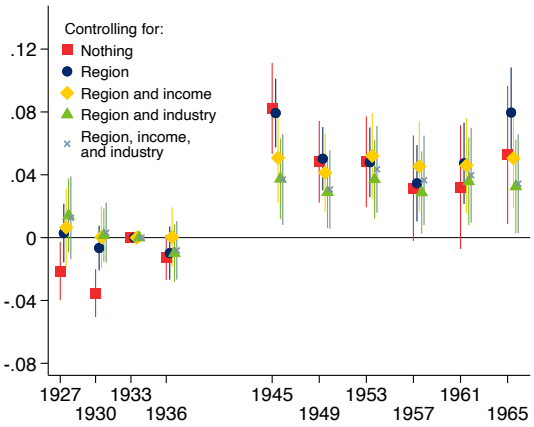
(c) Non-Labour Mayor in 1928 or 1934



(d) Labour Mayor in 1928 and 1934



(e) Non-Labour Mayor in 1928 and 1934



(f) Labour Mayor in 1928 or 1934

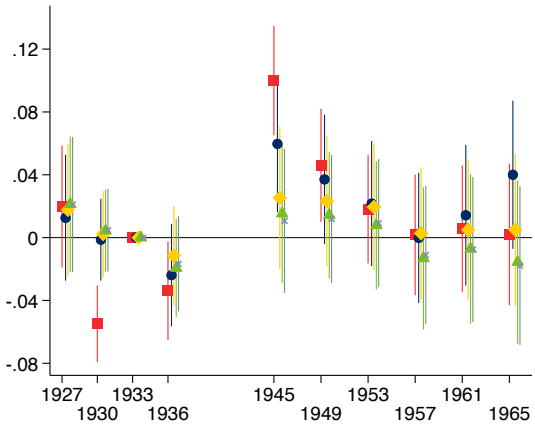


Table A8: Impact on Vote Shares by Local Politics

	Labour Party Vote Share				
	(1)	(2)	(3)	(4)	(5)
A: Labour Mayor in 1934					
Main effect (β)	0.065	0.060	0.046	0.033	0.036
	(0.017)	(0.011)	(0.012)	(0.011)	(0.012)
Interaction (γ)	-0.021	-0.028	-0.035	-0.034	-0.038
	(0.024)	(0.021)	(0.020)	(0.020)	(0.020)
B: Labour Mayor in 1928					
Main effect (β)	0.067	0.062	0.051	0.036	0.041
	(0.017)	(0.010)	(0.012)	(0.011)	(0.012)
Interaction (γ)	-0.027	-0.033	-0.040	-0.040	-0.046
	(0.024)	(0.021)	(0.020)	(0.020)	(0.020)
C: Labour Mayor in 1928 or 1934 (or both)					
Main effect (β)	0.064	0.064	0.053	0.030	0.038
	(0.015)	(0.010)	(0.012)	(0.011)	(0.012)
Interaction (γ)	-0.045	-0.050	-0.062	-0.047	-0.056
	(0.027)	(0.022)	(0.022)	(0.022)	(0.022)
D: Labour Mayor in 1928 and 1934					
Main effect (β)	0.065	0.066	0.058	0.034	0.043
	(0.015)	(0.010)	(0.012)	(0.011)	(0.012)
Interaction (γ)	-0.063	-0.068	-0.079	-0.062	-0.072
	(0.026)	(0.021)	(0.021)	(0.022)	(0.022)
Time trends by:					
Region	no	yes	yes	yes	yes
Income	no	no	yes	no	yes
Industry	no	no	no	yes	yes

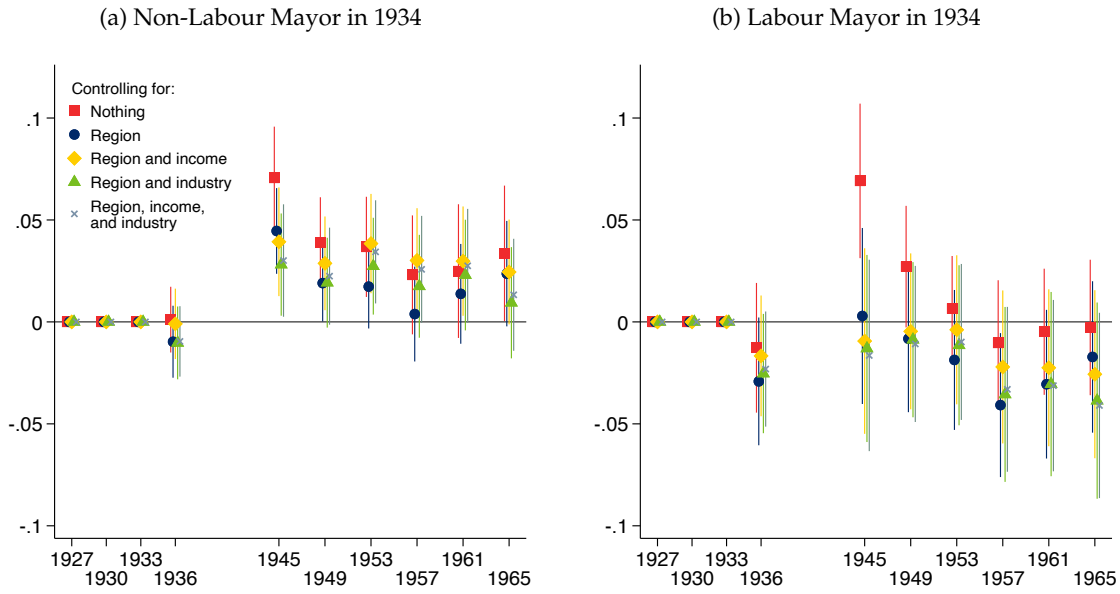
Note: This table reports estimates from differences-in-differences specifications $y_{ptj} = \beta(post \times Z_j) + \gamma(post \times W_j) + \delta(post \times Z_j \times W_j) + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{jt} is the Labour Party's vote share in municipality j in year t, Z_{jc} is treatment intensity, W_j is an indicator for the municipality having experienced Labour rule prior to the reform (see panel labels for definitions), X_{j0} is a vector of municipality's pre-war characteristics, and μ_t and μ_j are year and municipality fixed-effects, respectively. For each regression, we report estimates for $\hat{\beta}$ and $\hat{\delta}$.

Table A9: Local Labour rule and change in Labour vote share

	Change in Labour vote share in					
	1927–1930		1933-1936		1935–1945	
	(1)	(2)	(3)	(4)	(5)	(6)
Local Labour rule at $t-1$	0.002	0.060	0.006	0.015	-0.139	-0.034
	(0.009)	(0.010)	(0.006)	(0.011)	(0.011)	(0.016)
Labour vote share at $t-1$	.	-0.234	.	-0.029	.	-0.356
		(0.020)		(0.025)		(0.040)
Constant	-0.036	0.025	0.030	0.038	0.073	0.171
	(0.003)	(0.005)	(0.003)	(0.006)	(0.007)	(0.010)

Note: This table reports estimates from regression $\Delta y_j t = \alpha + \beta W_{j,t-1} + \gamma y_{j,t-1} + \epsilon_{jt}$, where Δy_{jt} is the change in Labour Party's vote share in the national elections between years t and $t-1$, W_j is an indicator for the municipality having local Labour rule between $t-1$ and t, and $y_{j,t-1}$ is the vote share of the Labour part at $t-1$ (used in specifications reported in columns (2), (4) and (6)). The records for the 1931 local elections are not available.

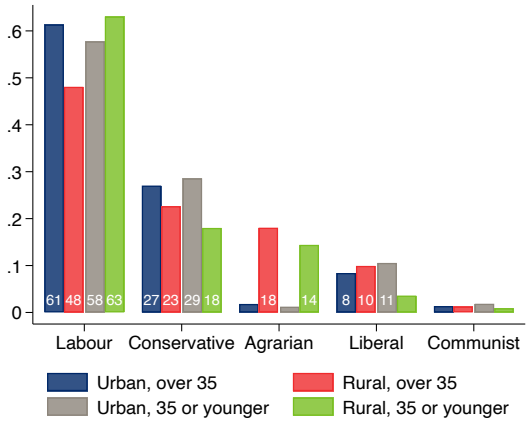
Figure A12: Labor Vote Share Estimates by Earlier Exposure to Local Labour Rule, Lagged Dependent Variable Specification



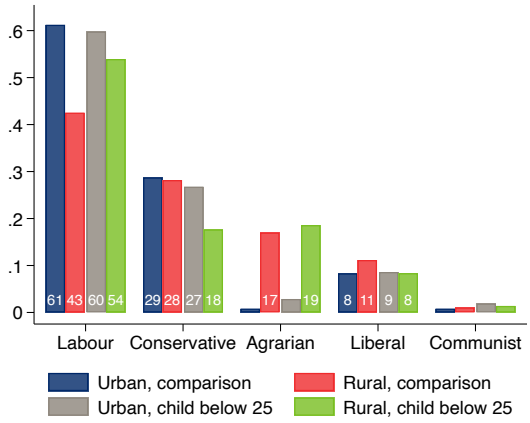
Note: This figure corresponds to Figure 11 but using lagged dependent variable specification, i.e., a series of cross-sectional regressions of the form $y_{jt} = \alpha + \beta Z_j + X_{j0}\theta + \sum_{l=27,30,33} [\gamma_l y_{jl} + \theta_l y_{jl}^2] + \epsilon_{jt}$ where we control for for Labour vote share (and its square) in 1928, 1930, and 1933.

Figure A13: Vote Shares by Age and Residential Location

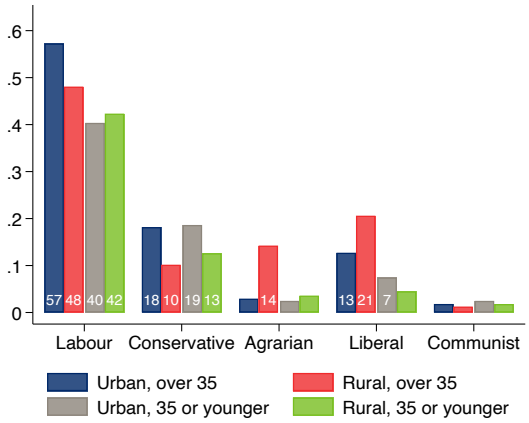
(a) By Age, the 1957 election



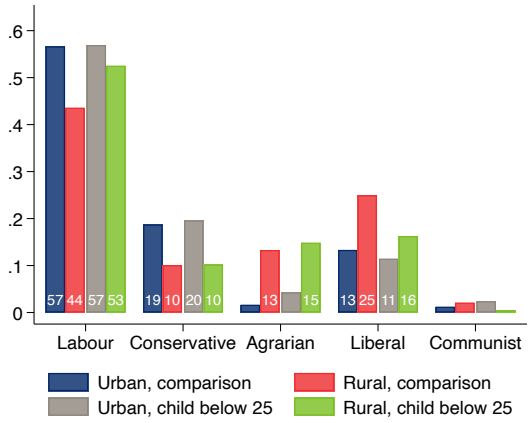
(b) By Children's Age, the 1957 election



(c) By Age, first elections

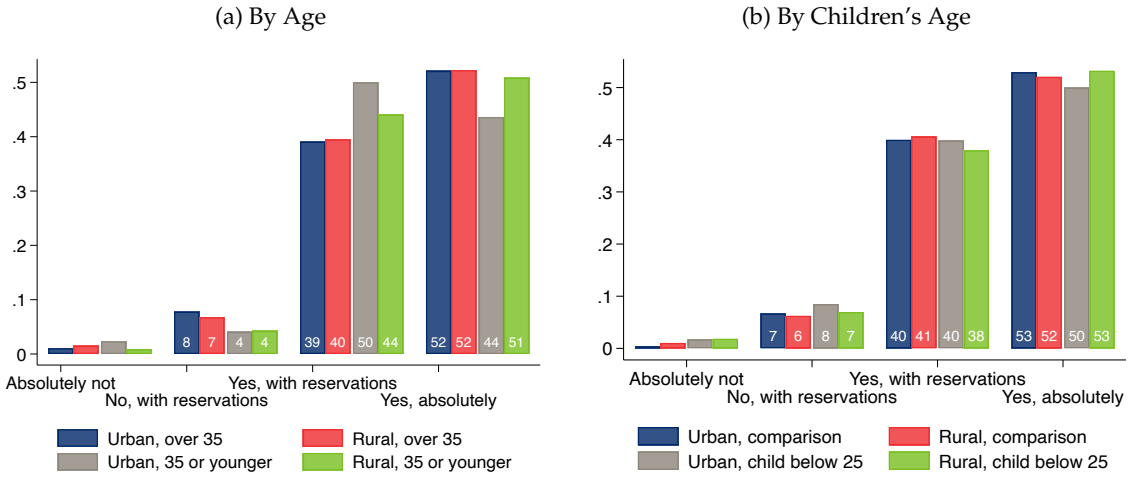


(d) By Children's Age, first elections



Note: This figure shows the distribution of answers to the questions "Which party will you vote for?" (panels (a) and (b)) and "Which party did you vote for the first time you participated in a parliamentary election?" (panels (c) and (d)) in the 1957 election survey. The category "Conservative" includes both the Conservative Party and the Christian Democratic Party. Categories "Unanswered" and "Did not vote" are omitted. The numbers inside the bars are percentage shares within each age–residence municipality.

Figure A14: Has the Labour Party Implemented It's Agenda?



Note: This figure shows the distribution of answers to the question "Would you say that the Labor Party has shown the will and ability to implement this program in the years it has had government power?" in the 1957 election survey.