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ABSTRACT

The Making of Social Democracy: The Economic and Electoral Consequences of Norway's 1936 Folk School Reform*

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 increasing teacher salaries. We document that cohorts more intensively affected by the reform significantly increased their education and experienced higher labor income. Our main result is that the schooling reform also substantially increased 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.

JEL Classification: P16, I28, J26

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

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1 Introduction

Workers or Labour parties, later referred to as Social Democratic Parties, rose to power in Scandinavia in the 1930s and 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 Europe after World War II.

These institutions were a sharp break from what prevailed before. In contrast to what is sometimes claimed, politics in Scandinavian societies were 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 very high unequal Latin American societies. After the 1930s, however, Social Democratic Parties dominated Scandinavian countries and reshaped their economies. The political power of large businesses and landowners waned, and as a result of various redistributive policies and the active role of labor unions in wage setting, inequality started diminishing rapidly. For example, the pre-tax Gini coefficient in Norway went from 0.57 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](#)).

From our 21st-century vantage point, the adoption and implementation of such radical reforms may appear surprising. How did these new parties, with fairly radical reform agendas, come to such prominence and received widespread electoral support? There is little research and certainly no consensus on this question, despite its importance, especially today when many commentators are calling for institutional reform throughout the Western and emerging world (e.g. [Esping-Andersen et al. 2002](#); [Rajan 2019](#); [Stiglitz 2019](#)).

In this paper, we investigate this question in the Norwegian context. The Norwegian Labour Party rose to power in 1935, under the leadership of Johan Nygaardsvold and with the support of the Agrarian Party. It remained in government for most of the subsequent five decades.¹ 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 building on 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—the reform of primary schools was 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

¹The Nygaardsvold’s cabinet of 1935 was in fact Norway’s second Labour government, with the first attempt—Hornsrud’s cabinet in 1928—lasting only two weeks, without passing any legislation. During the German occupation in 1940–45, Nygaardsvold’s cabinet acted as an exile government based in London.

and limited school resources. The Labour Party promised to harmonize education and to increase 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.²

Although sometimes overlooked in the historical work on the Scandinavian labor movement, education was a key pillar of social democratic agenda 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). It was envisaged that the initial conditions for individuals would be made more equal with the help of state intervention in education or, as formulated by Lewin (1967), "the coercive power of the state". In Sweden, for example, 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 the education policy as "the primary strategic instrument for abolishing class barriers" (Rothstein 1998).

We find that the 1936 education reform in Norway had distinct economic and political effects. We first show that years of education increased among the birth cohorts of boys living in areas most affected by the reform exactly at the time when the reform started to affect these cohorts. Importantly, the reform did not alter years of mandatory education nor the provision of non-mandatory education. Thus these results likely reflect better preparedness for further education. We also estimate a positive effect on earnings in later life. For women, we do not find a statistically significant effect on post-mandatory education but—consistent with improved quality of primary education having direct labor market returns—there is a strong impact on their later income. Furthermore, our findings suggest that the reform may have had major intergenerational effects, although many of these estimates are not statistically significant due to our research design's relatively low statistical power for intergenerational analysis.

Our main focus is not the effects of the reform on education or earnings, but on Norwegian institutions and politics. We show that the reform was critical for the support for 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 urban areas. After 1936, however, 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-an-envelope calculation suggests that the increase in the vote share of the Labour Party in rural areas would have been 1.4–4.6 percentage points lower in 1945 if the reform had not taken place. The increase in rural support—a total of 3.9 percentage points—was critical for the party, since in the meantime it lost 3.8 percentage points of its support in the cities.

²The other Nordic (Denmark, Sweden plus Finland) countries initiated and established similar school reforms for the rural and urban areas following the Norwegian reform (Mediås 2004).

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 this paper, we examine the mechanisms behind the impact of the reform on the Labour Party's electoral success. We show that it is not because of 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 of the individuals directly affected by the reform were not yet eligible to vote. We also show that the Labour Party did not increase its electoral success because of increased political participation in the form of higher turnout (which was already very high in Norway at this time) or because it devoted greater attention or resources 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 local level, indicating a switch from conservative to Labour politics in places that benefited from the education reform. Second, using individual-level survey data collected from 1957 parliamentary elections, we find that voters who themselves had experienced increased schooling and improved school quality were much more likely to support 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 (so that it was not just those receiving the education, but their entire family becoming more pro-Labour). 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 methodology does not distinguish whether voters updated their beliefs about the competency of the Labour Party or felt indebted to the Labour Party.³ Nevertheless, the evidence is fairly clear that both inhabitants who benefited from the school reform and their parents were

³Greater support for a party that has kept its promises and delivered public goods is consistent with several mechanisms. In addition to a change in beliefs about the "type" of the party, it could be 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 (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). We suspect that the reason why the school reform in Norway contributed to the formation of a social democratic coalition, rather than any type of clientelistic political dynamics, is both because of the broad-based nature of the policy in question and the efforts of the Labour Party to build a diverse coalition in support of its agenda.

much more likely to vote for the Norwegian Labour Party than other rural residents and thus provided the popular support for the Social Democratic institutions in Norway.

This paper is related to a number of literatures. First, 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](#); see [Oreopoulos and Salvanes, 2011](#), for a review). To the best of our knowledge, none of these works investigate the political implications of these reforms.

Second and more directly, 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 contribution of the coalition with agrarian interests has also received attention (e.g., [Gourevitch 1986](#); [Berman 2006](#)). These emphases are different from but complementary to ours, since many of these authors also recognize the importance of the public services provided by the Social Democratic parties.

Third, our paper relates to the literature on successful political reforms. In the context of democratic reforms, [Acemoglu and Robinson \(2006, 2012\)](#) emphasize the role of collective action by politically excluded groups to force a transition away from non-democratic regimes, but also 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 \(2007\)](#) explore 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 are not aware of theoretical or empirical work in economics or political science that investigates the impact of major school reforms on the political equilibrium. Consistent with this result, recent work by [Acemoglu et al. \(2021\)](#) finds that cohorts that have lived longer under democracy, especially when a democracy is economically successful and delivers public goods, tend to support democratic institutions and oppose non-democratic rule.

Fourth, 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, and points out

the problems in their empirical study. [Milligan et al. \(2004\)](#) show that educated individuals are more likely to vote, but as pointed out in [Friedman et al. \(2016\)](#), this does not necessarily mean more pro-democracy behavior in general. These authors show that disadvantaged Kenyans who received more education because of schooling reform may have actually increased their support for political violence. Our work is very different from this literature, however, since we are not claiming that education effects support for democracy or social democracy, but that education reforms, which were the main electoral promise of the Labour Party before their 1936 victory, made Norwegians more trusting and supportive of this party and their social democratic agenda.

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 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 centre-left governments that rose to power in between the world wars.⁴ Several liberal governments introduced major labor laws, covering workers in the developing manufacturing sector from 1900 until the end of the First World War. However, these laws did not enact universal policies, which became to be a defining characteristic of the Nordic welfare states ([Bull 1959](#); [Bjørnson 2001](#)).

Social democrats formed durable governments during the 1930s, typically in coalition with parties representing rural voters. The policies of these governments laid the foundations of the welfare institutions that the same political forces continued to build after the WWII. These policies included 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 all the Nordic countries, was a highly unequal country with

⁴The Finnish welfare state, though ultimately ending up similar to the Scandinavian model, followed a different path owing to the disruptive effects of the 1918 civil war; see [Meriläinen et al. \(2020\)](#).

high levels of industrial conflict (Moene and Wallerstein 2006). In 1930, the Gini index in Norway was higher than the current Anglo-Saxon levels at 0.57 and the top 10 % share of the national income was similar to the contemporaneous level in the United States at 0.44 (Aaberge et al. 2020). The regional inequalities were also striking, especially along rural-urban axis. According to Falk and Tovmo (2000), 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 45% of the population lived in urban 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, the GDP per capita grew only by 2.3% between 1919 and 1930 whereas the rest of the Scandinavian countries experienced solid growth, ranging from 23.5% in Sweden to 28.3% in Denmark during the same period. The poor growth performance was reflected in 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 than the total amount of days lost during the 25-year period between 1945 and 1970.

2.2 The Norwegian Labour Party

The development of the Norwegian labor movement followed the same broad pattern as similar parties in Northern Europe, and in particular in other Scandinavian countries, although there are also some distinct characteristics (Bull 1959; Esping-Andersen 1985; Sejersted 2011). The Norwegian Labour Party was founded in 1887 and entered the 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 towards parliamentary democracy. Unlike the other Scandinavian Labour parties, Norwegian Labour Party was also a member of the Soviet led Comintern until the early 1920s.⁵

Following the poor performance in the 1930 election, the Norwegian Labour Party changed its strategy and adopted a reformist agenda following the 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 industrial, logging and fishing industries, and the threat of fascism which was gaining support in

⁵The 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 it is rooted in the age composition of the industrial workforce in Norway, where the relatively late industrialization, taking off only between 1905 and 1910, meant that workers were much younger and perhaps more willing to support radical politics (Dahl 1971).

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 compel the employer organizations to recognize the National Confederation of Workers (LO) as a negotiating partner.⁶ 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 business and part of the educated middle-class such as teachers and public sector workers (see Appendix Figure A1 for an illustration of this change between 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 centerist parties. In the 1933 election, the party increased its vote share from 31% to 40%, the highest share it had ever gained. Although the electoral success did not immediately lead to the formation of a Labour government, the minority government led by the Liberal Party collapsed in 1935, when the Agrarian Party withdrew its support and agreed to support the Labour minority government with Johan Nygaardsvold as its prime minister.

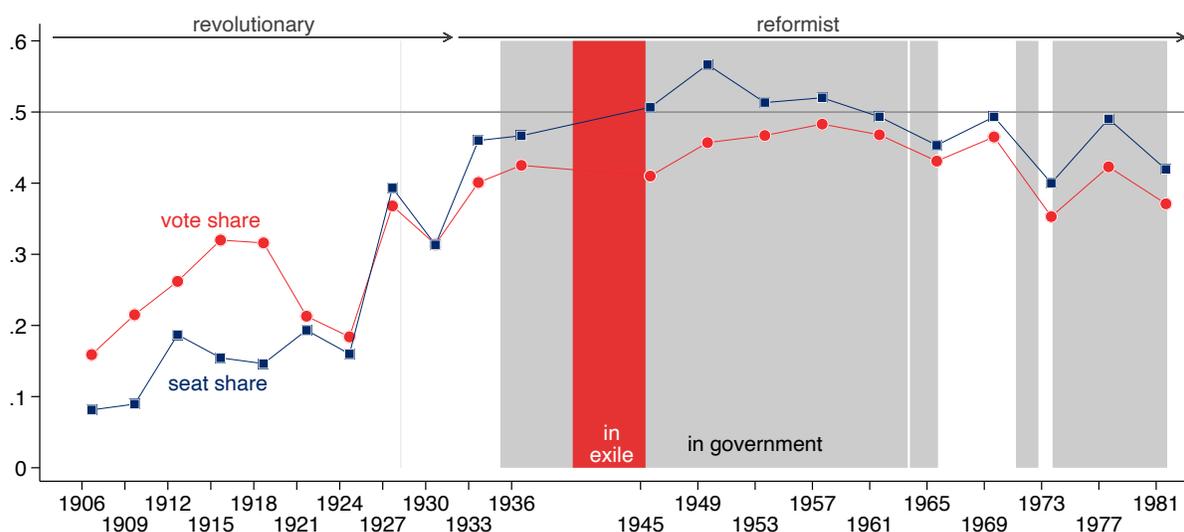
The 1935 Labour government started a long period of social democratic rule. As shown in Figure 1, the Labour Party held power for most of the following 45 years apart from the German occupation in 1940–1945 (when the Labour government was in exile in London) and short periods of center-right governments in the 1960s and 1970s. During this long period, the Labour Party implemented an ambitious program for developing the welfare state that included the introduction of universal national social security, health care system, and, later, day care 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 responsibility 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 changed from preparing children for confirmation at the age of 15 to

⁶This agreement was made just before the formation of the Labour government in 1935. It resembled the Saltsjöbad 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 started to take 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 reports the vote shares of the Norwegian Labour Party in parliamentary elections and the share of seats the party held in the parliament. The gray areas present the periods, when the Labour party was in government, and the red area the period that the Labour government spent in exile in London following the Nazi occupation. The change in the link between vote and seat shares in 1921 is due to a move to a proportional representation. The drop in the vote share in 1921–1924 is due to the temporary split of the party into the Social Democratic Party of Norway and Norwegian Labour Party, see [Cox et al. \(2019\)](#) for details.

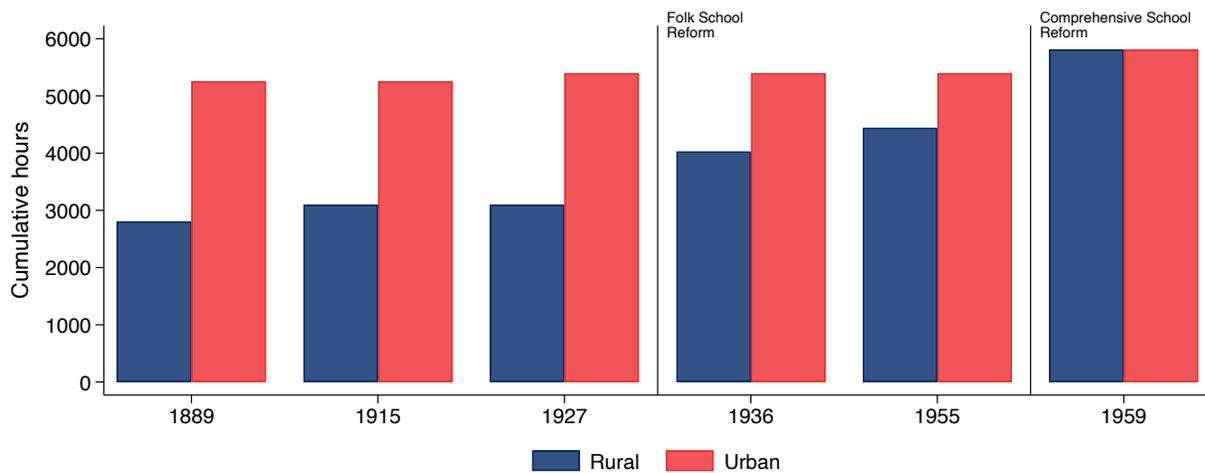
preparing children in general including algebra in addition to reading and writing.

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 the child labor laws—the law on primary schools stipulated shorter school years for rural than for urban areas, in particular, with lower minimum and maximum number of school weeks in rural parts. Even though the school weeks included slightly more hours in rural areas, the restrictions on the number of weeks implied that rural primary school students received substantially less instruction time than their urban counterparts. As shown in Figure 2, the requirement in 1928 was that 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 about 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, when the minimum number of weeks of instruction time in the last four primary school grades was increased to 14 weeks.⁷ However, the differences

⁷The 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 reports minimum cumulative hours over 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 folkeskolen på landet Jun 16th, 1936.

in the standards and the quantity 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 only 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 aim of the movement was to achieve greater social equality, for which education was going to be a critical tool as it would alter the distribution of opportunities (Rothstein 1998). The initial conditions for individuals would be made more equal with the help of state intervention in education or, as formulated by Lewin (1967): "the coercive power of the state". The primacy of educational policy was reflected in the fact that, in the Swedish case, many of the leading social democratic politicians and strategists, such as Tage Erlander, Olof Palme, and Alva Myrdal, were deeply involved in the planning and implementation of education policy from early on. Indeed, Myrdal saw the education policy as "the primary strategic instrument for abolishing class barriers" (Rothstein 1998).

The importance of the education reform is very 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". These demands became more specific over time and clear emphasis was put on equal opportunities for children in different parts of Norway. Already 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 country side primary schools should be brought to the same level as the town primary schools". By 1930s, the urgency of the education reform was so clear that the 1936 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 the equal 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 primary education of sufficiently high quality, the new Labour government passed the new law on primary schools in rural areas as one of its first major pieces of legislation in 1936. This reform was the first step in a program that aimed at establishing a general comprehensive primary school which would prepare children for further education (Rust 1989).⁸

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 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.⁹ The state also took over other responsibilities that were previously carried out by the municipalities, such a school buildings, books and inventories, as well as housing for the teachers. Furthermore, a new national curriculum was 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).¹⁰

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 goals of the 1936 reform were ambitious, considering the state of primary schools in rural municipalities in the

⁸Establishing 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 in 1934 extensions of increased hours in rural schools was voted down with support from conservatives as well as the agrarian party.

⁹The law increased the share of central government funding from 45% of minimum teacher salary to 50%.

¹⁰See also Chapter 5 of "Lov om folkeskolen på landet", 1936.

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 small at 4%, and 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 requirements 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 and came into force from the school year starting in August 1936. Municipalities were allowed to use five years to implement it fully. Hence, children born in 1935, and consequently starting school in August 1942, are the first cohort for whom the new regime was fully implemented.

3 Data

We created our main data set by linking together newly digitalized archival data on the roll out of the 1936 primary school reform, individual-level population-wide information on human capital and income, and municipality-level data on election results and pre-reform characteristics. In order 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 create 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 and digitized. These data originate from county-level primary school directors, who were obliged to send 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, the extent to which several grades were taught in the same class, the gender composition, education and compensation of the teachers, and the type of school buildings available.¹¹

¹¹Detailed description and aggregated data are available at <https://www.ssb.no/a/histstat/publikasjoner/histemne-21.html>.

3.2 Human capital and income

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

We conduct separate analyses for two groups. The “first-generation” consists of individuals born in rural Norwegian municipalities in 1917–1940. Second, we define the “second-generation” as the children of the first-generation (regardless of their own place of birth), and restrict the analysis to those born in 1947–1976. We do not impose any further sample restrictions, although some individuals with missing information naturally drop out of our sample.

We use completed years of education as our primary measure for human capital. This information is drawn from the 1960 and 1970 population censuses and Statistics Norway's educational database and is thus available for the full population. For men serving the mandatory military service after 1969, we also observe IQ scores. Roughly 95% of Norwegian men in the relevant birth cohorts took arithmetics, vocabulary, and Raven Progressive Matrix tests at the age of 18–20 at the draft board meeting 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 onwards 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 sample averages by treatment intensity (as defined in the next section) for our main estimation sample. On average, the first generation men have 9.0 and women 8.4 years of education and almost half of them did not continue their education after leaving primary school. Men's average annual income at ages 50–64 is 180,000 Norwegian kronas (in 1998 prices) corresponding to about \$21,000 (in 2020 prices). For women, the corresponding figures are 80,000 kronas or \$9,000. There is a clear, although relatively mild, gradient by treatment intensity with those born in poorer (and thus more intensely treated) municipalities having somewhat less education and lower income than those born in municipalities less affected by the reform. As shown in the lower panel, the second-generation has more education and higher incomes than the first-generation, and part of the differences along the treatment intensity distribution are still present.

3.3 Election results and municipality characteristics

Our primary election measures are drawn from Municipality Data Base of 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 in-

Table 1: Average Human Capital and Income

	Men				Women			
	Treatment Intensity				Treatment Intensity			
	All (1)	Low (2)	Medium (3)	High (4)	All (5)	Low (6)	Medium (7)	High (8)
<i>A: First-Generation</i>								
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
<i>B: Second-Generation</i>								
Year of birth	1961.2	1962.4	1962.0	1962.3	1961.6	1962.7	1962.4	1962.6
Years of education	12.3	12.6	12.3	12.3	12.5	12.8	12.6	12.7
Post-mandatory education	0.87	0.89	0.89	0.89	0.89	0.90	0.91	0.91
Income at age 30–34	246,986	262,439	254,144	245,380	141,229	153,407	146,819	146,417
Observations	402,557	143,946	124,387	134,224	374,745	134,009	116,139	124,597

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 year 1998 kronas and includes labor earnings, taxable sick benefits, unemployment benefits, parental leave payments and pensions. Columns (2)–(4) and (6)–(8) report

formation on candidates in national elections as reported in [Fiva and Smith \(2017\)](#) and use their categorization to divide parties into six categories: (i) the Norwegian Labour Party, (ii) the Communist Party, (iii) the Agrarian party, (iv) the Liberal Party, (v) the Conservative Party, and (vi) others.¹²

In some of our specifications, we control for income per capita 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 are drawn from the Municipality Data Base of the Norwegian Center for Research Data as well.

3.4 Survey data

We use individual-level data from a 1957 covering the Parliament election—the first election poll conducted in Norway ([Rokkan et al. 1958](#)). These surveys collect information on individual characteristics (age, gender, geographic location, family structure, educational), voting and a number of attitudinal questions. The survey also included questions on the respondents' children and thus we can identify individuals whose children were likely affected by the 1936 Folk School reform.

¹²The "others" category includes fringe parties in the Norwegian context, such as Christian Democrats, National Socialists, and from the 1970s onwards a left-wing Maoist Party.

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 municipalities that were further away from the new (national) standards and for the birth cohorts that spent a larger share of their primary education under the new regime. We next discuss how we measure this treatment intensity 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 municipalities because of 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 to the post-reform minimum requirements using information on instruction time just 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 a municipality-level index:

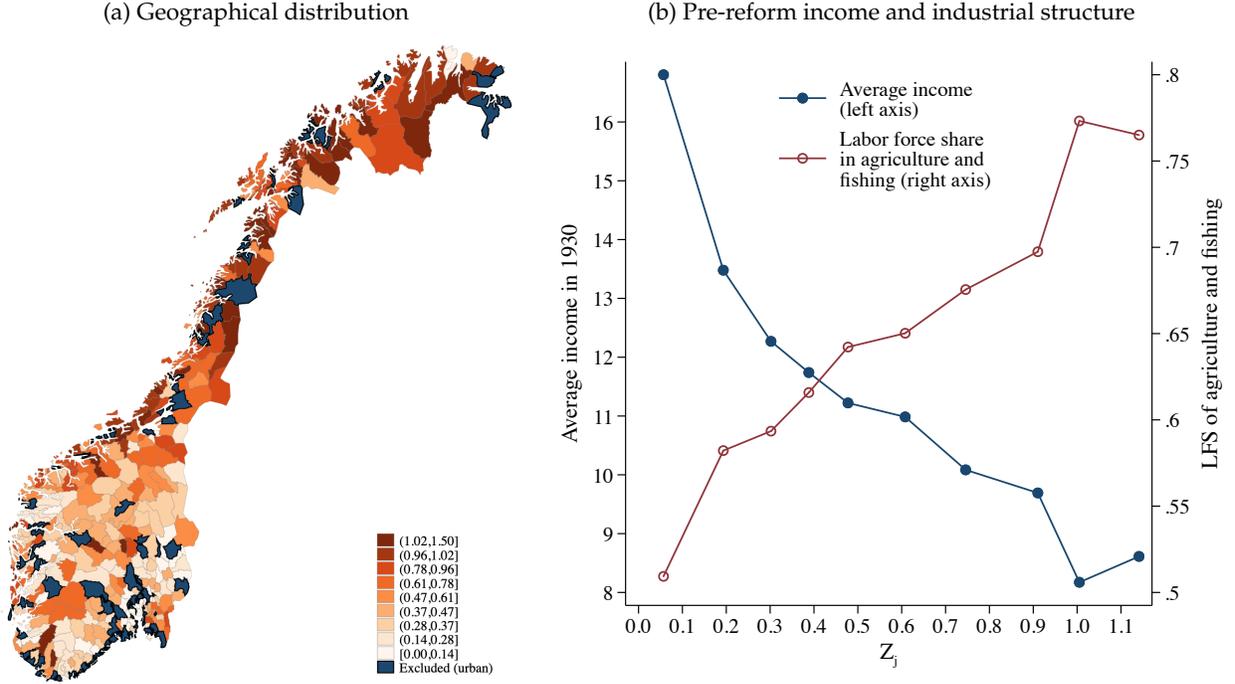
$$Z_j = \frac{3 \sum_b s_{bj} \max(16 - b, 0) + 4 \sum_b S_{bj} \max(18 - b, 0)}{28}, \quad (1)$$

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 nominator captures the average additional weeks of instruction a municipality would have to offer in order to meet the new requirements.¹³ 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, Z_j takes the 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 occurs between birth cohorts within a municipality. Those born before 1923 had left primary education by the beginning of the implementation period in 1936 and thus were not exposed to the reform. On the other hand, everyone born after 1935 started school after the implementation period and went through their entire primary education under the new requirements. Among the 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),

¹³For example, think of a municipality, where half of the children in grades 1–3 got 12 weeks of education and the other half got 14 weeks of education in 1935. Let's further assume 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 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 municipality.

Figure 3: Treatment intensity



Note: Panel (a) presents the geographical distribution of the treatment intensity, Z_j , see equation 1. Panel (b) shows the association between Z_j and average income of municipalities.

and the extent to which the municipality complied with the new requirements.¹⁴

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

$$Z_{jc} = \sum_c \pi_c Z_j \quad (2)$$

where π_c is the share of years birth cohort c studied under the new requirements. We do not know when each municipality implemented the reform. Thus, as a baseline, we assume that all municipalities fully implemented the reform in 1938, and 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. We show that the results are robust to assuming other implementation years.

Having defined the treatment intensity measures, we next examine their geographical distribution and association with pre-reform municipality characteristics. Panel (a) of Figure 3 shows that municipalities in the northern (less economically developed) parts of Norway tended to be

¹⁴For 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.

further away from the post-reform minimums, and hence more affected by the reform, than those located further 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 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 approaches we next discuss.

4.2 Specifications

We start our analysis by asking how the reform affected human capital and income of the directly affected individuals and estimate event-study regressions of the form:

$$y_{icj} = Z_j\beta_c + X_{j0}\theta_c + \mu_c + \mu_j + \epsilon_{icj} \quad (3)$$

where y_{icj} is the outcome of interest for individual i born in year c in municipality j .¹⁵ On the right-hand-side, Z_j is the pre-reform distance from the new requirements (see equation (1)), 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.¹⁶ The parameters of interest are β_c , which measure the extent to which the outcome grows differentially between birth cohort c and birth cohort 1923–24 (the omitted category) across municipalities that were differentially affected by the reform.

We report estimates from several specifications that differ in terms of what is included in the vector X_{j0} . The specifications are motivated by the correlation between treatment intensity and pre-reform geographical location, income and industrial structure discussed above. While the municipality fixed effects capture all time-invariant differences between municipalities, it is conceivable that poorer or less industrialized municipalities would 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.

The advantage of the event-study specification is that it allows us to examine whether the timing of the possible changes is consistent with the timing of the reform. It also provides a

¹⁵We 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.

¹⁶Note that using municipality of birth and not municipality of residence helps reducing a potential bias due to mobility across municipalities.

falsification exercise for the parallel pre-trends assumption required for a causal interpretation of β_c . However, this flexibility comes with the cost of a large number of parameters. In order to efficiently summarize the results from the event-studies, we also estimate a more parsimonious version where we estimate a single parameter specifying the effect of all post-reform years:

$$y_{icj} = \beta Z_{jc} + X_{j0}\theta_c + \mu_c + \mu_j + \epsilon_{icj}, \quad (4)$$

where Z_{jc} is the municipality–birth cohort level measure of treatment intensity (see equation (2)) and other variables are as in the previous specification.

We analyze the impacts on electoral outcomes using a similar approach. 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:

$$y_{ptj} = Z_j\beta_t + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj} \quad (5)$$

where y_{ptj} is the vote share of party p in year t at municipality j , and Z_j and X_{j0} are the same treatment intensity measure and pre-reform observable characteristics as above. The parameters of interest, β_t , now 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.

In order to increase statistical power and to summarize the estimates into a single number, we also report estimates from a standard differences-in-differences specification:

$$y_{ptj} = \beta(\mathbb{1}[t \geq 1945] \times Z_j) + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj} \quad (6)$$

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.

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 instrumental variable for it. We do not use this strategy because we believe the exclusion restriction necessary for such an IV approach is not valid. This exclusion restriction would require that the effects of the reform worked entirely via changes in instruction time, whereas, as is common with other education reforms, the Norwegian reform affected several dimensions of educational inputs at the same time (see Section 2.5). Appendix Figure A2 illustrates this point by plotting average instruction time and student-teacher ratio as functions of treatment intensity in years 1930, 1935, 1938 and 1940-1944. It shows that the pre-reform values of all inputs were highly correlated with our treatment intensity variable, but this correlation clearly declines after the reform was implemented in 1938, implying that the main dimensions of the reform were correlated with each other.

5 Results

This section presents our main results. We start by examining the impact of the reform on human capital and long-term income of the individuals who were directly affected by it. This analysis is motivated by the 1936 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 in earnings. We find that this is, indeed, the case. We then show that the reform may have had an intergenerational effect also on human capital and earnings on the children of those directly affected, although these estimates are much 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 the reform. These effects are present both in the short and in 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 impact on human capital and income

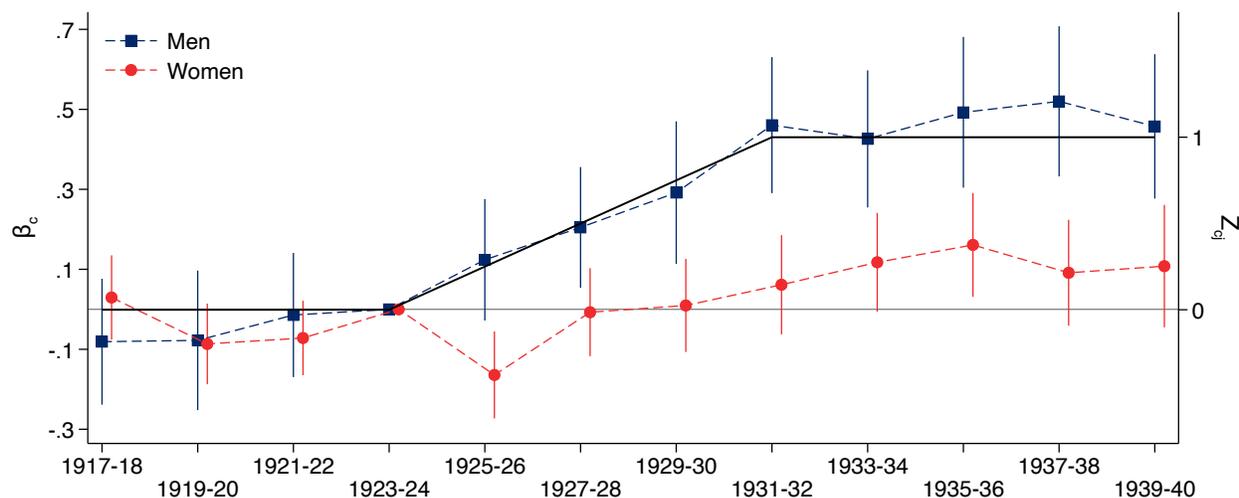
Figure 4 reports the baseline event-study estimates for the effect of the reform on years of education. Importantly, the reform did not change mandatory years of education and hence it affected years of education only through the likelihood of continuing in post-primary education. We find no indication of pre-trends. Specifically, 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. In line with the aims of the reform, men’s years of education start to increase faster in municipalities that were more affected precisely at the time the reform was launched. Reassuringly, the impact is 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. Furthermore, the estimates level off at around the birth cohorts who were the first to be fully exposed to the reform and thus went through similar primary education as the later birth cohorts.

In short, the pattern presented in Figure 4 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 are qualitatively similar, but smaller and less consistently significant at conventional levels.

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

¹⁷By full exposure we mean $Z_{jc} = 1$, i.e., being born after 1931 in a municipality that provided only the pre-reform

Figure 4: Event-Study Estimates for First-Generation's Years of Education



Note: Estimates for β_c from regression $y_{ijc} = Z_j\beta_c + \mu_c + \mu_j + \epsilon_{ijc}$, where y_{ijc} is years of post-mandatory education, 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 municipality of birth level. The solid black line shows treatment intensity for each birth cohort when $Z_j = 1$ and the reform was implemented in 1938.

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 (0.051)	0.262 (0.067)	0.267 (0.070)	0.336 (0.076)	0.325 (0.075)	0.163 (0.036)	0.092 (0.045)	0.068 (0.050)	0.088 (0.048)	0.084 (0.048)
Log income (age 50–64)	0.143 (0.015)	0.080 (0.019)	0.048 (0.020)	0.044 (0.020)	0.039 (0.020)	0.156 (0.022)	0.099 (0.025)	0.083 (0.026)	0.059 (0.027)	0.067 (0.027)
Linear time trends by:										
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 linear trends by 20 regions; columns (3) and (7) for linear trends by quintiles of municipality's 1930 average taxable income and income growth between 1915 and 1930; columns (4) and (6) for linear trends by quintiles of municipality's labor force share in agriculture, fishing, manufacturing and services in 1930. Each entry stems from a separate regression.

For women, the point estimate suggest a 0.16 years or a 2% increase from the baseline of 8.2 years. Columns (2) and (6) report results from specifications that allow differential linear trends for each of Norway’s 20 regions and hence control for overall regional convergence that may have been correlated with the reform. The estimates are now 0.26 years for men and 0.09 for women and remain statistically significant. In the rest of the table, we allow for differential linear trends by 1930 average taxable income, changes in average taxable income between 1915 and 1930, and the industrial structure of the municipality 1930 (see the table note for details). Appendix Figure A3 presents the corresponding event-study estimates using the same control variables. The most demanding specification allowing for differential trends by region, income and industry suggest that full exposure to the reform increased the post-mandatory education of men by 0.33 years (p-value <0.001) and that of women by 0.084 years (p-value 0.084).

The remaining of Table 2 repeats the analysis using average log income at ages 50–64 as an outcome variable. 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 not just due to the effects of the reform on education. Most likely, there would have been some amount of convergence even without the reform, and as a result, we also see a small pre-trend among men in the baseline specification (Appendix Figure A4). Thus, the baseline estimates for men’s income are likely to be biased upwards. However, the estimates are relatively stable in specifications allowing for differential trends by region and either income or industrial structure (or both). These most demanding specification suggests that a full exposure to the reform increased long-term income of men by 3.9 log points (p-value 0.053) and that of women by 6.7 log points (p-value 0.013). Thus, we conclude that the reform’s impact was likely to increase long-term income, although this evidence is somewhat less conclusive than in the case of years of education. The results for women also suggest that improved primary education was valuable in the labor market even when it did not increase the likelihood of further education.

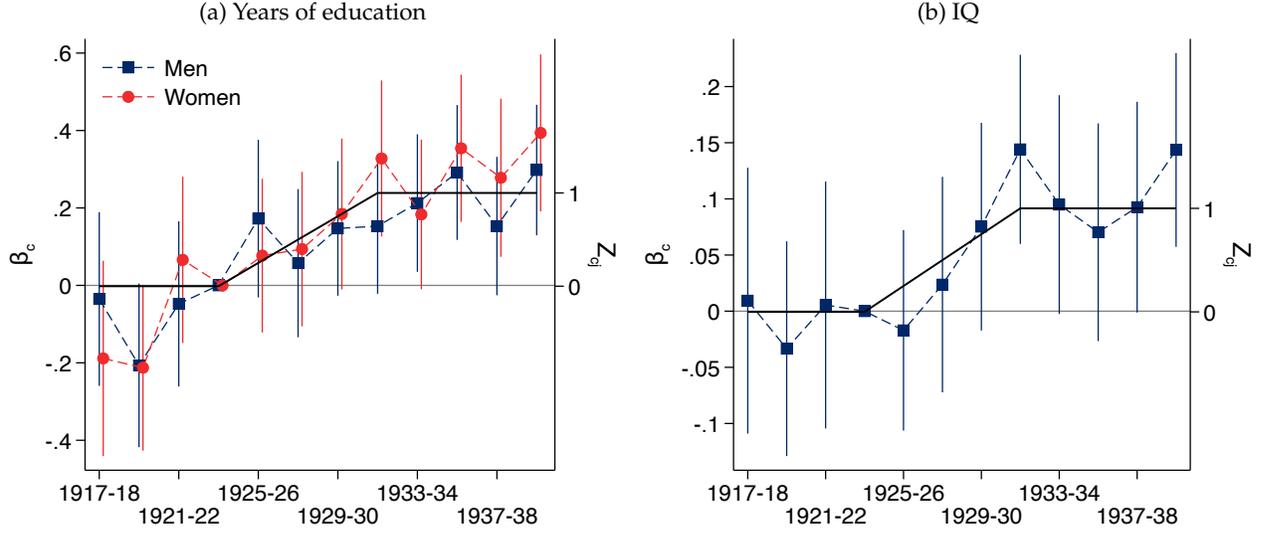
5.2 Intergenerational effects

In line with the Norwegian Labour Party’s objective of improving overall social mobility, we find evidence that the schooling reform also increased the educational attainment of the children of cohorts directly impacted by the reform.

Figure 5 presents event-study estimates for the second-generation. Specifically, we estimate equation (3) using data on the outcomes of children whose fathers were directly affected by the reform. The right-hand-side variables still refer to the municipality and birth cohort of person’s father. As with our first-generation results, we use years of education as the primary measure

minimum weeks of education in 1935 (see equation 4).

Figure 5: Event-Study Estimates for the Second-Generation



Note: Estimates for β from regression $y_{ijc} = Z_j^F \beta_c + \mu_c^F + \mu_j^F + \epsilon_{ijc}$, where y_{ijc} is years of post-mandatory education (panel A) or IQ score in SDs (panel B), Z_j^F is treatment intensity in father's municipality of birth j , μ_c is a vector of fixed-effects for father's year of birth, and μ_j is a vector of fixed-effects for father's municipality of birth. Standard errors are clustered at municipality of birth level. The solid black line shows treatment intensity for each father birth cohort when $Z_j^F = 1$ and the reform was implemented in 1938.

of human capital. For second-generation men, we additionally have cognitive test (IQ) scores taken as part of the mandatory military service. The results using this IQ index present a similar, although noisier, pattern, indicating a positive effect on the second-generation.

The results show that, reassuringly, human capital of the children whose fathers are too old to have been directly affected by the reform evolve similarly across municipalities. In contrast, for birth cohorts whose fathers were affected by the reform, we see a gradual increase in human capital. This effect levels off around the first birth cohorts fully exposed to the reform, which is the pattern we should expect given that the impact of the reform is uniform from this cohort onwards. The results for men and women are very similar for the second-generation, and if anything, a bit stronger for women. The results on IQ for the second-generation men also corroborate this picture.

Table 3 reports the corresponding differences-in-differences estimates. The baseline estimates suggest that pushing father's municipality of birth from the pre- to the post-reform minimum requirements increased their sons' years of education by about 0.21 years corresponding to a 2% increase from the baseline of 12.3. Similar to the first-generation results, conditioning for linear trends by father's birth municipality's region, pre-reform income or pre-reform industrial structure somewhat reduces point estimates, which now vary between 0.07–0.15 years and mostly lose statistical significance. Interestingly, the results for IQ test scores are less sensitive to specification.

Table 3: Differences-in-Differences Estimates for the Second Generation by Father’s Exposure

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years of education	0.208 (0.048)	0.150 (0.066)	0.095 (0.069)	0.109 (0.065)	0.071 (0.068)	0.306 (0.053)	0.127 (0.071)	0.082 (0.077)	0.131 (0.072)	0.091 (0.078)
IQ	0.109 (0.023)	0.065 (0.032)	0.068 (0.033)	0.059 (0.031)	0.058 (0.032)
Log income	0.034 (0.013)	0.012 (0.016)	0.004 (0.017)	-0.003 (0.016)	-0.004 (0.017)	0.041 (0.019)	0.067 (0.023)	0.064 (0.025)	0.072 (0.024)	0.072 (0.026)
Linear time trends by:										
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 $y_{ijc} = \beta Z_{jc}^F + \mu_c^F + \mu_j^F + \epsilon_{ijc}$, where Z_j^F is the treatment intensity of the reform for father’s municipality of birth j , μ_c^F is a vector of fixed-effects for father’s year of birth, and μ_j^F is a vector of fixed-effects for father’s municipality of birth. Standard errors are clustered at municipality of birth level. See note for Table 2 for details of the specifications and Appendix Table A1 for similar analysis using mother’s treatment intensity.

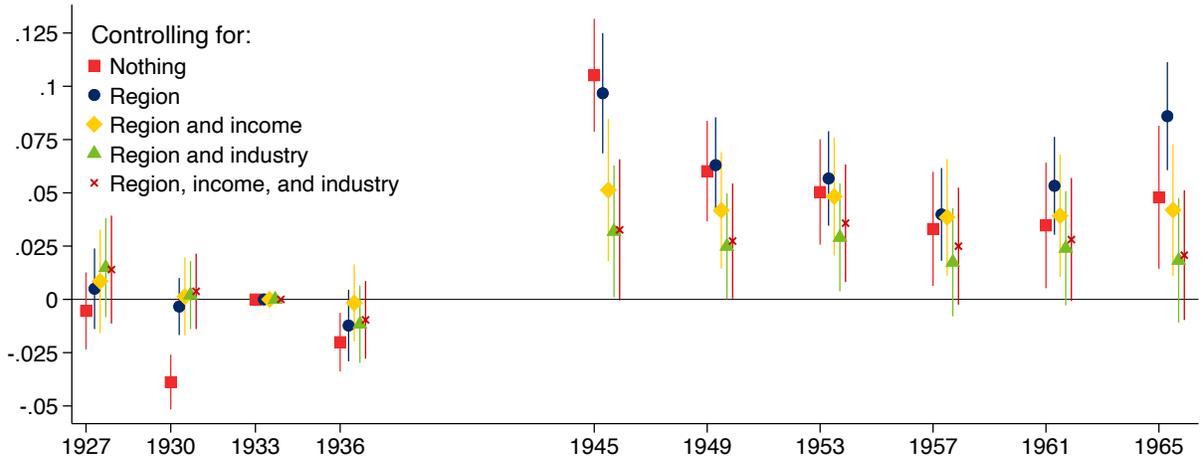
The point estimates vary between 0.06–0.11 standard deviations and all estimates are significant at the 10% level.

In contrast to the education and IQ results, we do not have precise results for income. In this case, income is measured for individuals when they are between 30 and 34, which may not give us enough power to distinguish between differential experience effects and income implications of greater education. In any case, the results are not statistically distinguishable from zero (our findings are very similar when we use treatment intensity at mother’s birth year and municipality to measure treatment intensity; see Appendix Table A1).

Interestingly, in contrast to the first-generation results, the effects are stronger for women than for men. While the estimates for the years of education of daughters is similar to those for sons, the estimates for their log income are large and statistically significant. The point estimates vary between 4.1 and 7.2 log points and only become larger when we allow for differential trends.

Overall, we interpret these findings showing that the 1936 school reform, consistent with the Labour Party’s objectives, impacted social mobility and educational opportunities more broadly within Norwegian society, and we can detect effects on second-generation Norwegians from the impacted municipalities. Nevertheless, the results are not precise enough to identify income effects for men on these cohorts.

Figure 6: Event-Study Estimates for the Vote Shares of the Labour Party



Note: This figure reports estimates for $y_{ptj} = Z_j\beta_t + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{ptj} is the vote share of the Norwegian Labour Party in year t at municipality j , Z_j 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 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. Appendix Figure A5 reports similar estimates for the other major parties.

5.3 Elections

Our results so far suggest that the reform created important economic benefits. We now turn to our main focus, the reform's political effects. We focus on the Norwegian Labour Party's success 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 6 present our event-study estimates for Labour Party's vote share in the national elections. We use 1933 as the reference category and report estimates without control variables and those allowing for differential trends by larger regions, pre-reform income quintiles, and pre-reform industrial structure of the municipality. We do not find any clear pre-reform trends. In contrast, though in line with our expectations, 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 increased between 5.5 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 37 percent

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

	Vote share				
	(1)	(2)	(3)	(4)	(5)
Labour	0.070 (0.013)	0.068 (0.010)	0.042 (0.013)	0.023 (0.012)	0.031 (0.013)
Communists	-0.012 (0.005)	-0.013 (0.004)	-0.008 (0.005)	-0.003 (0.005)	-0.006 (0.005)
Agrarian	-0.005 (0.010)	-0.041 (0.012)	-0.016 (0.014)	0.008 (0.012)	-0.003 (0.012)
Liberal	-0.089 (0.013)	-0.053 (0.013)	-0.022 (0.014)	-0.007 (0.014)	-0.007 (0.015)
Conservatives	-0.005 (0.012)	-0.027 (0.012)	-0.026 (0.014)	-0.042 (0.012)	-0.034 (0.013)
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 $y_{ptj} = \beta(Z_j \times \mathbb{1}[t \geq 1945]) + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, 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.

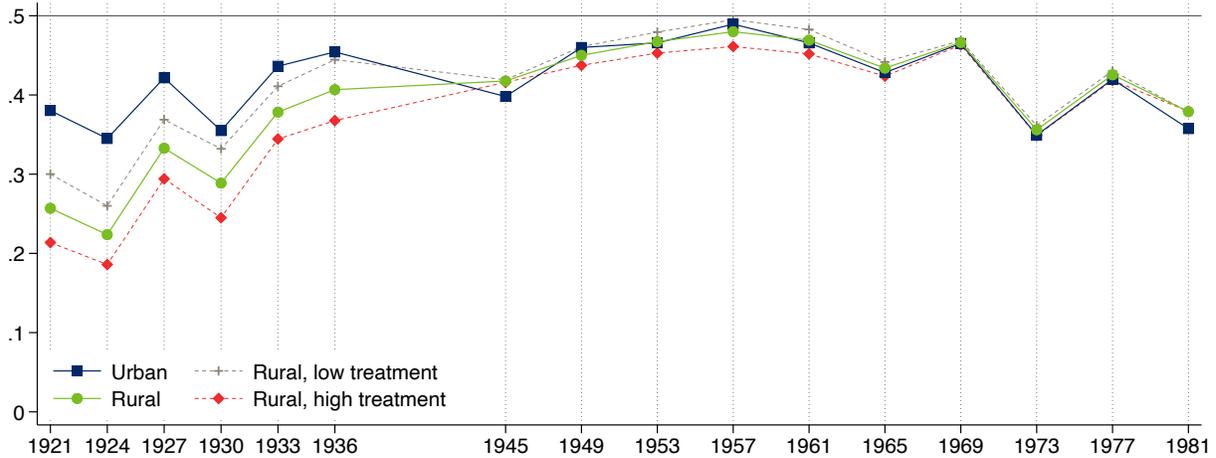
in 1933. While the estimates are slightly smaller for later elections, all specifications yield large, positive, and statistically significant estimates for every election until 1965.

The first row of Table 4 summarizes the effects for Labour Party’s vote share using standard differences-in-differences regressions (equation (6)). The point estimates for the Labour Party vary between 4.4 and 7.0 percentage points in municipalities that were fully exposed to the reform. These gains appear to arise primarily from losses of the Liberal Party. The estimates also suggest that Labour gained votes from the Conservatives and Communists, even if these effects are not as robustly significant as those for the Liberal Party. We do not find any consistent pattern for the Agrarian Party.

In order to put these results into a context, we ask what they imply for the overall vote share of the Labour party in the 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.¹⁸ For comparison, Figure 7 shows that the support for the Labour Party increased and caught up with the vote share in urban areas precisely after the school reform was enacted.

¹⁸We conduct this calculation by multiplying the event-study estimates for 1945 with the "bite" of the reform for municipality’s, Z_j , and then calculate population weighted averages of the implied effect over all rural municipalities.

Figure 7: 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.

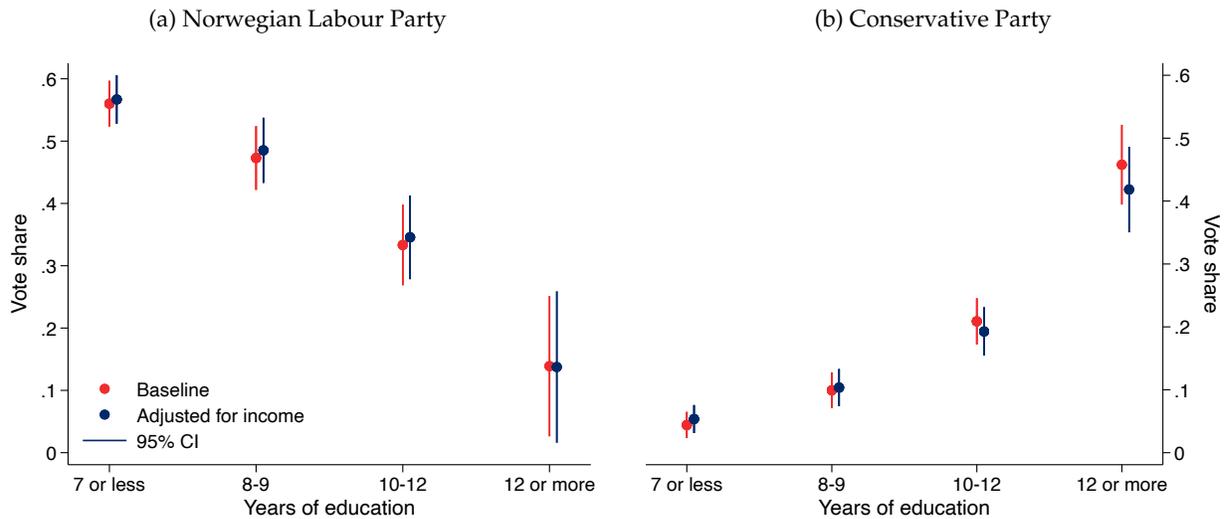
Specifically, between 1933 and 1945, the party gained 3.9 percentage points in rural areas, while it lost 3.8 percentage points of its support in the cities. As a consequence, the traditionally higher support the Labour Party enjoyed in cities disappeared and the party has hence been equally popular in rural and urban areas. Within rural areas, this increase in support for the Labour Party entirely came from municipalities that were more affected by the reform.

Overall, our results indicate that the major educational reform, promised and swiftly implemented in 1936 by the Norwegian Labour Party, had a significant impact on the political landscape of the country, raising the support for the party in the rural municipalities where it had a greater impact.

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 perceptions about the Labour Party. Specifically, we show that the impact on Labour Party vote share is substantially larger in municipalities that had no previous experience on their rule at 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

Figure 8: 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 the second specification, X_i is the respondent's income standardized so that the income adjusted shares refer to mean income.

well as their parents—were more likely to report voting Labour and to agree that the party had effectively implemented its program in the 1957 electoral survey.

6.1 Education and political preferences

We start with the hypothesis that the improved education induced by the reform directly increased support for the Norwegian Labour Party (for example, because the more educated are more likely to support social democratic parties). Our results do not find any support for this hypothesis. First, it is inconsistent with the fact that the largest effects on the Labour Party vote takes place in 1945, when the oldest cohort affected 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 were negatively correlated during the period we examine. Panel (a) of Figure 8 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. In fact, the more educated were substantially more likely to vote for the Conservative Party (panel (b)). Given this negative correlation, and the timing of the effects, it thus seems very unlikely that the electoral results would be driven by the effects of the reform on educational attainment.

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 may decide 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 9 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 9 examine whether the reform affected the composition of candidates. Panel (b) explores the possibility that the share of Labour candidates in the affected 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 a discouragement on potential candidates from other parties). In any case, 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 this case.

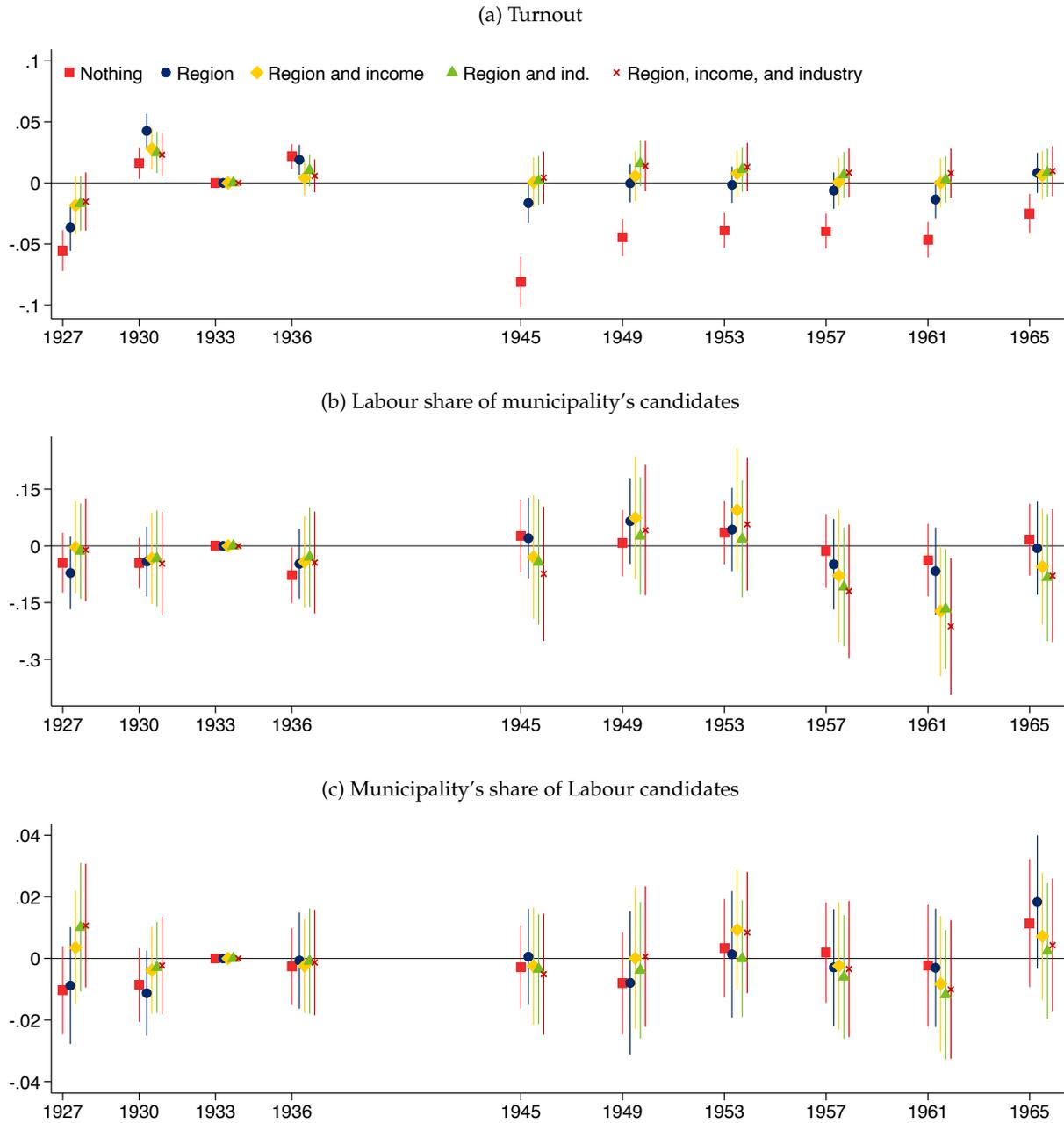
Finally, panel (c) explores the possibility that the Labour Party might have strategically allocated more candidates to heavily-affected municipalities at the expense of other areas, thus showing up as an increase in the share of candidates in the affected municipalities within the Labour Party's candidates. Once again, we do not find any evidence for such an impact.

Overall, we conclude that our estimated political effects are not working through participation channels.

6.3 Local Labour rule

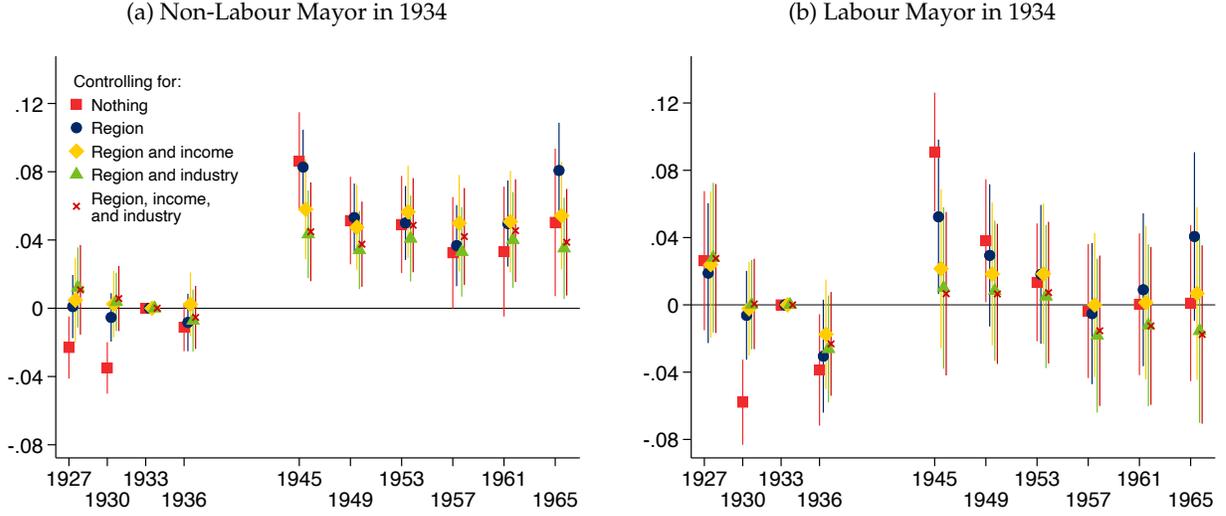
We now explore another potential mechanism behind our political results, by investigating whether the effects of the schooling reform were differential depending on recent experience of (local) Labour rule in the municipality. Specifically, we exploit the fact that more than a quarter of rural municipalities were governed by the Labour Party at the time of the reform. Not surprisingly, residents of municipalities where the Labour Party held local power were more likely to vote for Labour in national elections throughout our period ([Appendix Figure A6](#)). In addition, between the 1936 and 1945 national elections, support for Labour increased substantially in municipalities that were not governed by a mayor from the party, while the opposite happened in places that

Figure 9: Event-Study Estimates for Political Participation



Note: This figure reports estimates for $y_{ij} = Z_j\beta_t + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptij}$, where y_{ij} 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 Appendix Table A2 for a differences-in-differences version of these results.

Figure 10: Labor Vote Share Estimates by Earlier Exposure to Local Labour Rule



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 Appendix Table A3 for a differences-in-differences version of these results.

were already under local Labor rule at the time the party assumed national power.¹⁹

The event-study estimates reported in Figure 10 show that the effects of the 1936 school reform are significantly larger in non-Labour municipalities. We construct the figure by first estimating

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

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 10 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 10 reports estimates for $\hat{\beta} + \hat{\delta}$, corresponding to the effect of schooling reform on municipalities that had a Labour mayor in 1934.

¹⁹We define local Labour rule using data on the political affiliation of the mayor after the 1934 local elections. While some geographical clustering is evident (Appendix Figure A6), 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 (Appendix Figure A7).

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 (quasi-)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.²⁰

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

We finally examine whether those directly benefiting from the schooling reform and/or their parents were more likely to support Labour using individual-level data from the 1957 election survey (the first such survey conducted in Norway). Figure 11, panel (a), shows the distribution of votes for four groups defined by respondent’s age and type of residential municipality. We set the age threshold to 35 years, because the younger birth cohorts attended primary school during and after the implementation of the reform.

Unfortunately, this survey does not include information on municipality of residence and thus we cannot compute treatment intensity at the individual level in this data set. Nevertheless, as a crude proxy, we use information on whether a municipality had low or high population density in 1957 to approximate who were likely to live in affected areas. This cut roughly divides the data into urban and rural areas, with the latter having, on average, greater intensity of treatment, as we have seen so far

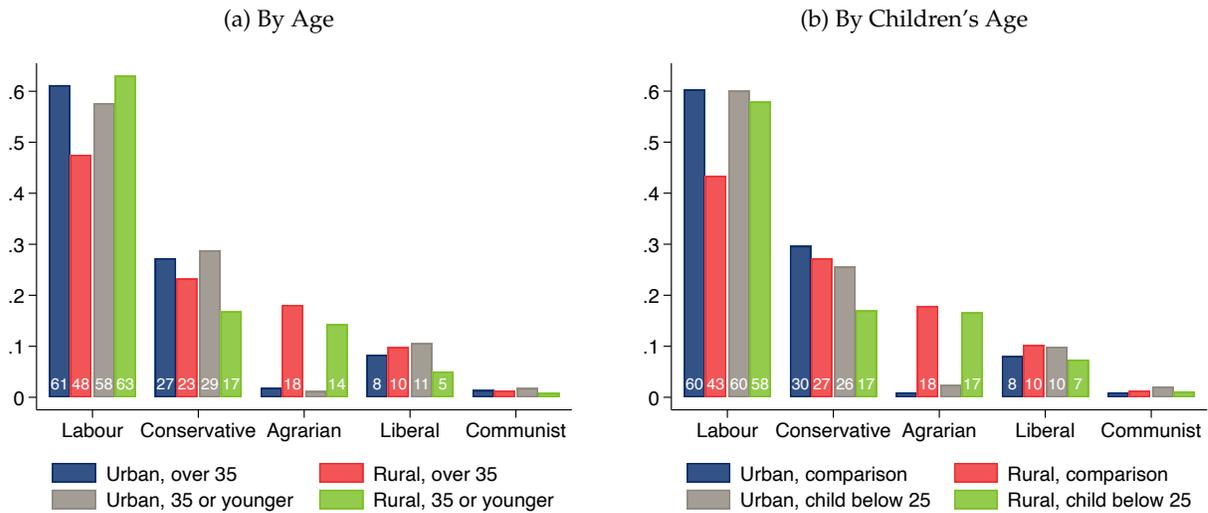
The survey data show that among older cohorts, those living in urban areas were 14 percentage points more likely to support the Labour Party than those living in rural areas. Remarkably, the gap is reversed for younger cohorts among whom the Labour Party has a 5 percentage point higher support in rural than in urban areas. This comparison could be interpreted as a differences-in-differences estimate, where (part of) the younger rural birth cohorts were directly affected by the reform, while the 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 (for one, there might be greater attenuation, and for another, other differential trends between rural and urban areas might confound this strategy). Nevertheless, Appendix Table A4 shows estimates using this strategy that are statistically significant (with the exception of the reduction of Labour support in urban areas across birth cohorts) and also confirms that the results are robust to controlling for region fixed effects.

²⁰In Appendix Figure A9 and Appendix Table A3, we show that these results are robust to using information on the affiliation of municipalities’ mayors in 1928 instead or in addition to 1934. (The records for the 1931 local elections are not available.) In addition, Appendix Figure A8 reports estimates for β from 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 the lagged dependent variables are from years 1927, 1930 and 1933. The estimates are similar as those reported in Figure 10 though somewhat smaller.

Figure 11: Vote Shares by Age and Residential Location in 1957



Note: This figure shows the distribution of answers to the question "Which party did you vote?" in the 1957 election survey. 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.

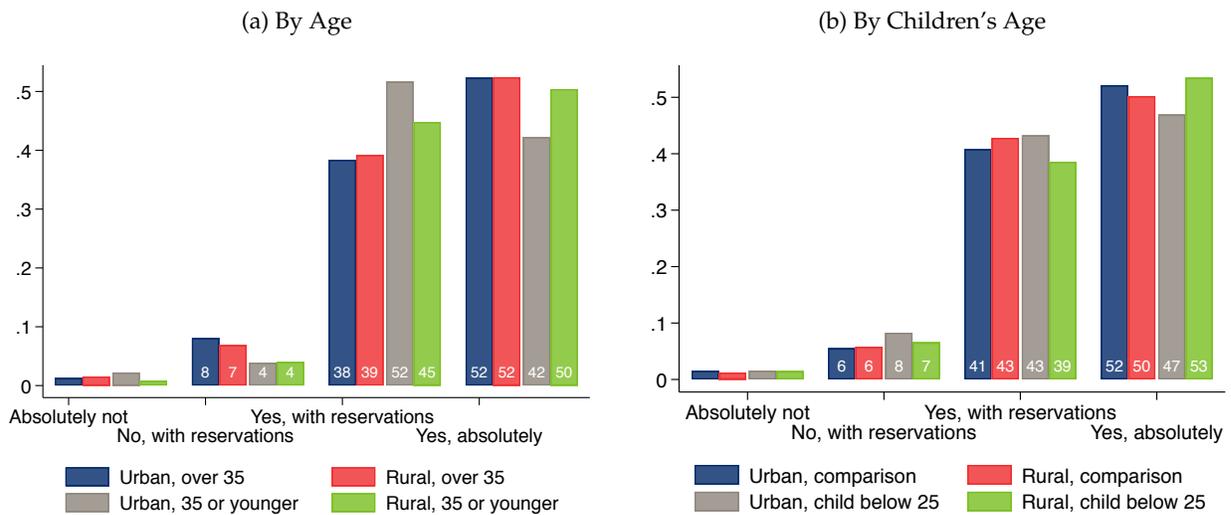
The survey additionally contains information on the respondent's children, and we can conduct a similar analysis for respondents whose children were affected by the reform. Specifically, we split the sample between those who had children younger than 25 years old in 1957 and the rest.²¹ Panel (b) of Figure 11 presents the raw averages. It shows a 17 percentage point rural-urban gap in support for the Labour Party among voters who either had no children or whose children were too old to have been directly affected by the reform. By contrast, the rural-urban difference is very small among voters who had children young enough to be affected by the reform. The implied differences-in-differences estimates in columns (3) and (4) of Appendix Table A4 document that these gaps are statistically significant and robust to controlling 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 importantly, the 1957 election survey contains questions concerning the reasons behind respondents' voting decisions. In Figure 12, we present the answers to 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

²¹Age categories used in the survey do not allow us to use the same age categories for the affected respondents and the children of respondents. The category "Younger than 25 years old" is the closest we can get to the affected children in this survey.

Figure 12: 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.

rural-urban status.

Figure 12 shows that 90% or more of respondents in all groups felt that the Norwegian Labour Party had been willing and able to implement its agenda. 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. Tellingly, however, rural respondents who were either young enough to have been directly affected or who had children young enough to have been impacted were 8 percentage points 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". A similar pattern is present in panel (b) of Figure 12, where we split the sample by the age of respondents' children. Nevertheless, these results should be interpreted with greater caution, since the implied differences-in-differences estimates reported in columns (5)-(8) of Appendix Table A4 are not statistically significant.

Overall, we cautiously interpret these results as being consistent with the hypothesis that a major contributor to the shift in the support towards the Norwegian Labour Party was driven by directly-affected individuals and their children, who rewarded the party for having delivered on its major campaign promise and improved schooling significantly.

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 reform 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—significantly increased their education. There is also evidence that their subsequent labor income rose as well, 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.

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 evidence 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.

Several economists and commentators are presently calling for major institutional reforms in Western nations (e.g. [Esping-Andersen et al. 2002](#); [Rajan 2019](#); [Stiglitz 2019](#)). 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 interesting area for future research.

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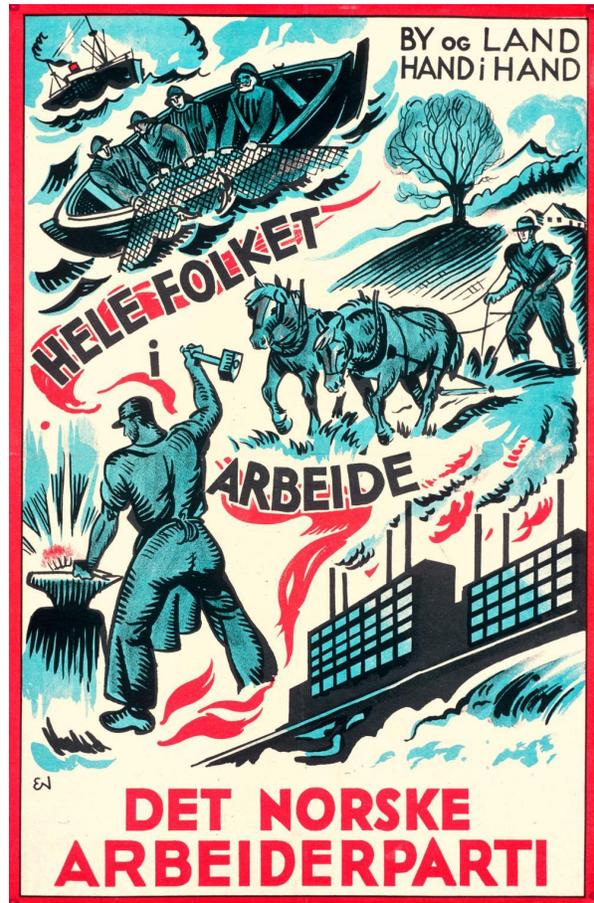
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Figure A1: Norwegian Labour Party's Election Posters

(a) 1930

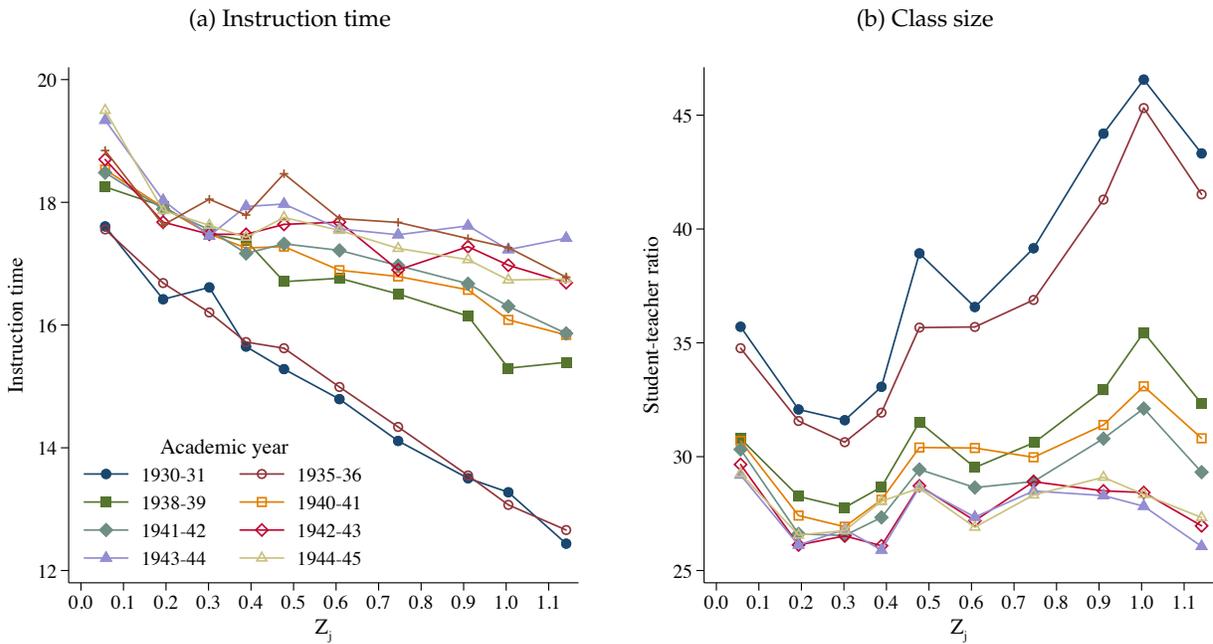


(b) 1933



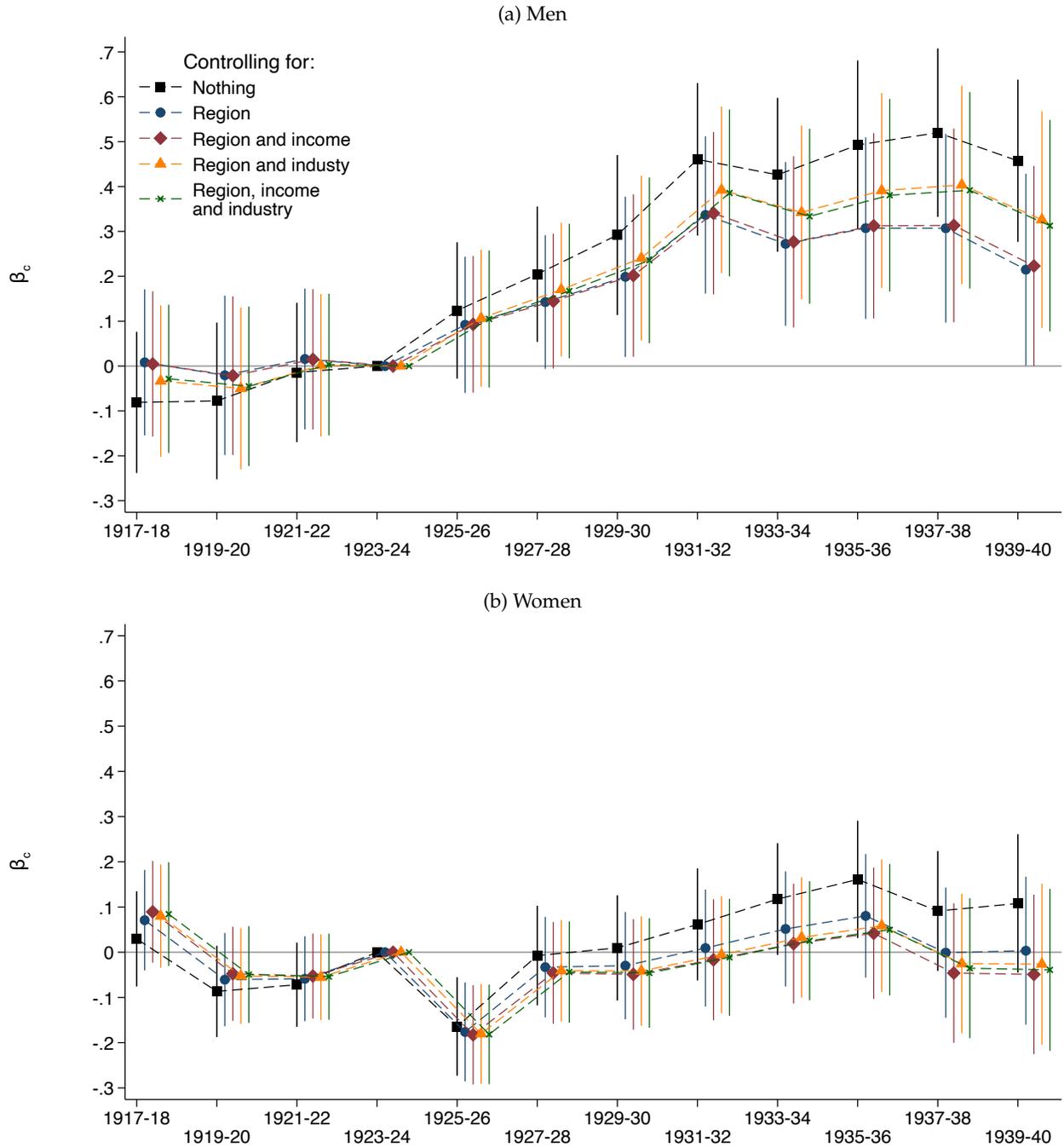
Note: The change in the rhetoric of the Norwegian Labour Party as illustrated by election posters. Both are designed by the same artist, Erling Nielsen. The 1930 campaign focused on urban industrial workers and the poster design is heavily influenced by Soviet art. The 1933 campaign led with a message 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](#) (GOF00005767 and P8), © Arbeiderpartiet/Arbark Arbeiderbevegelsens.

Figure A2: Changes in instruction time and student-teacher ratio by treatment intensity



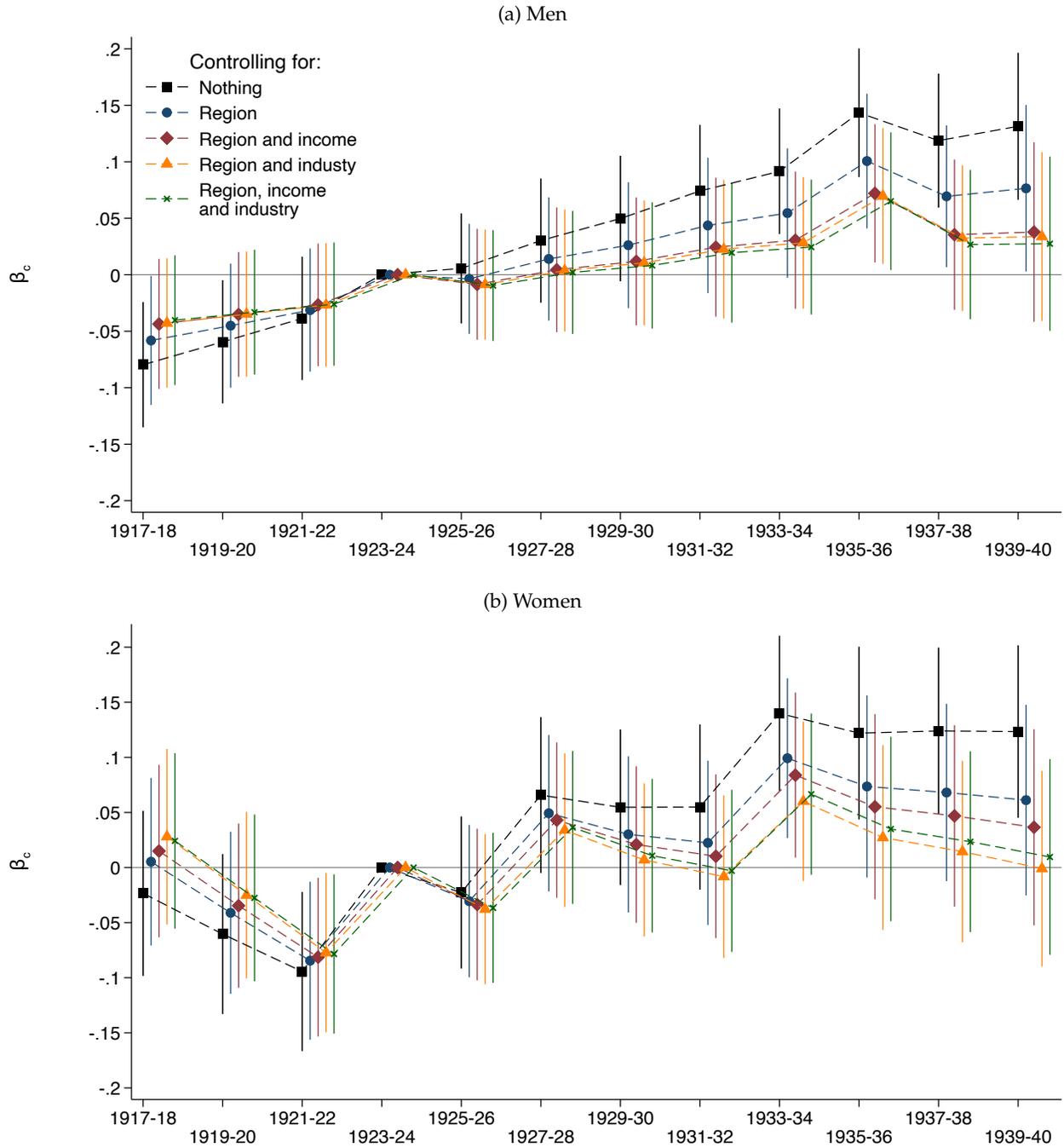
Note: Panel (a) shows average instruction time (vertical axis) by deciles of our treatment intensity measure (horizontal axis) in years 1930, 1935, 1938 and 1940-1944. Average instruction time in 1935-36 and our treatment intensity measure are closely connected by construction (see equation (1)). Panel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

Figure A3: Event-study estimates for years of education, alternative specifications



Note: Estimates for β from regression $y_{ijc} = Z_j\beta_c + \mu_c + \mu_j + \epsilon_{ijc}$.

Figure A4: Event-study estimates for log income, alternative specifications



Note: Estimates for β from regression $y_{ijc} = Z_j\beta_c + \mu_c + \mu_j + \epsilon_{ijc}$.

Appendix tables and figures

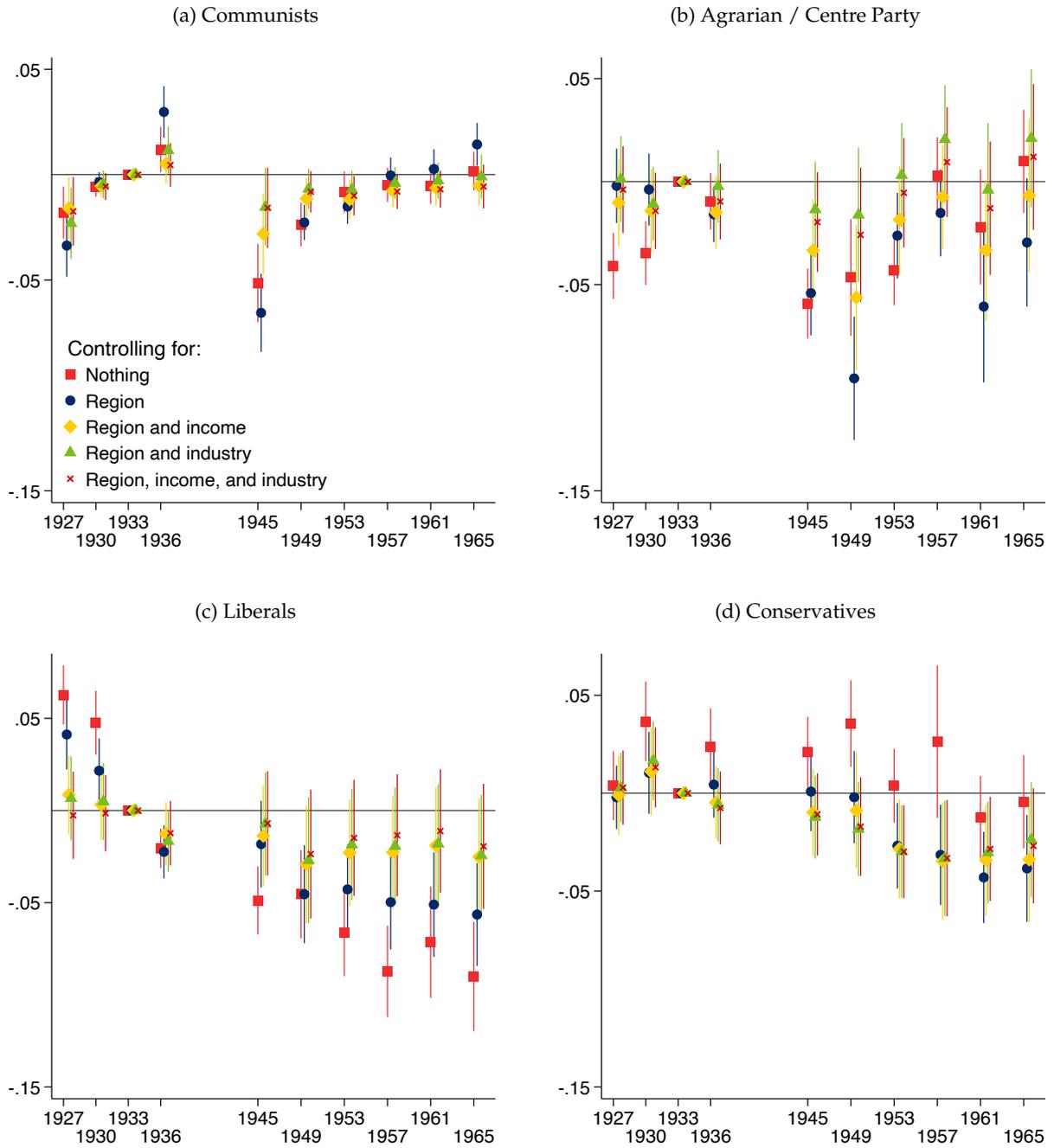
Table A1: Differences-in-Differences Estimates for the Second Generation by Mother's Exposure

	Men					Women				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years of education	0.207 (0.048)	0.124 (0.066)	0.112 (0.068)	0.090 (0.064)	0.099 (0.067)	0.278 (0.052)	0.113 (0.062)	0.093 (0.069)	0.114 (0.062)	0.098 (0.068)
IQ	0.087 (0.023)	0.011 (0.029)	0.022 (0.031)	0.008 (0.030)	0.018 (0.031)
Log income	0.028 (0.011)	-0.001 (0.015)	-0.015 (0.017)	-0.003 (0.016)	-0.014 (0.017)	0.024 (0.017)	0.071 (0.022)	0.076 (0.024)	0.076 (0.023)	0.082 (0.024)
Linear time trends by:										
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 $y_{ijc} = \beta Z_{jc}^M + \mu_c^M + \mu_j^M + \epsilon_{ijc}$, where Z_j^M is the treatment intensity of the reform for mother's municipality of birth j , μ_c^M is a vector of fixed-effects for mother's year of birth, and μ_j^M is a vector of fixed-effects for mother's municipality of birth. Standard errors are clustered at municipality of birth level.

Appendix tables and figures

Figure A5: Vote shares of other parties



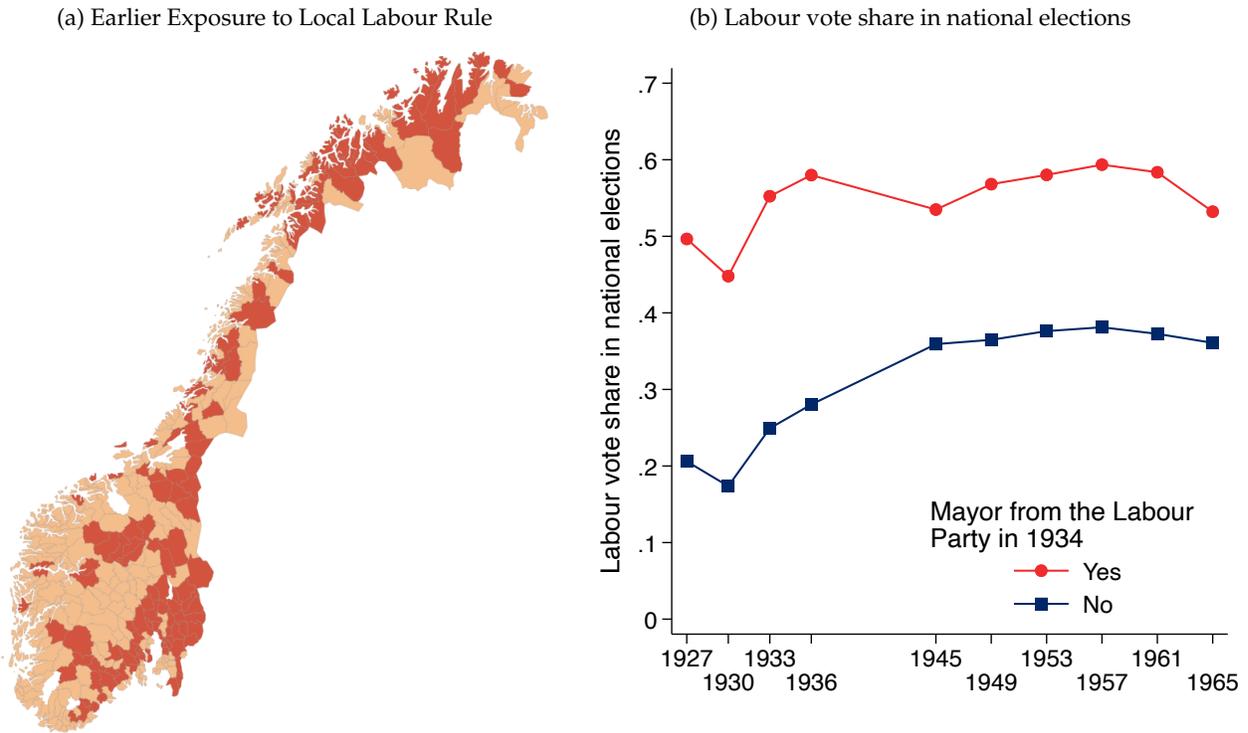
Note: This figure reports estimates for $y_{ptj} = Z_j\beta_t + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{ptj} is the vote share of part p in year t at municipality j , Z_j is our treatment intensity measure and X_{j0} is a vector of pre-reform observable characteristics that vary between 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 A2: Differences-in-Differences Estimates on Political Participation

	(1)	(2)	(3)	(4)	(5)
Turnout	-0.042 (0.006)	-0.012 (0.006)	0.000 (0.008)	0.000 (0.008)	0.004 (0.008)
Labour candidate share (out of all municipality's candidates)	0.048 (0.028)	0.040 (0.036)	-0.007 (0.052)	-0.038 (0.049)	-0.037 (0.054)
Candidate share (out of all districts Labour candidates)	0.006 (0.005)	0.006 (0.005)	0.001 (0.006)	-0.005 (0.006)	-0.002 (0.006)
Time trends by:					
Region	no	yes	yes	yes	yes
Income	no	no	yes	no	yes
Industry	no	no	no	yes	yes

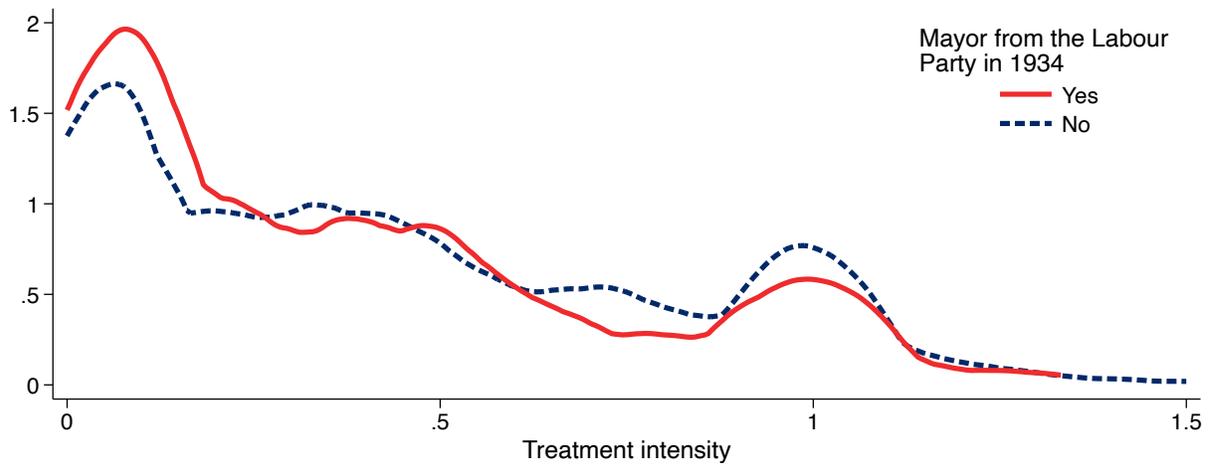
Note: Point estimates and standard errors (in parentheses) for β from regression $y_{tj} = \beta(Z_j \times \mathbb{1}[t \geq 1945]) + X_{j0}\theta_t + \mu_t + \mu_j + \epsilon_{ptj}$, where y_{tj} is either the share of Labour candidates out of all candidates from municipality j (panel a) or the share 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 A6: Local Politics



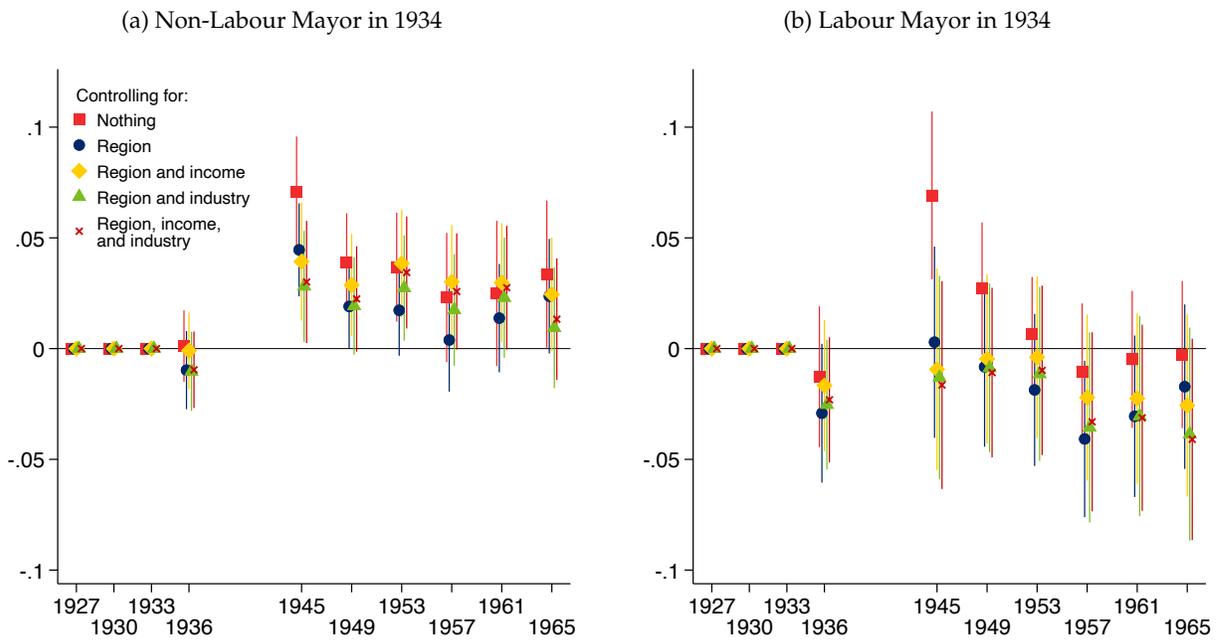
Note: In panel (a), municipalities that had experienced local labor in 1934 are marked with the darker color. Panel (b) shows the support for the Norwegian Labour Party in national elections over time for these two types of municipalities.

Figure A7: 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 A8: Labor Vote Share Estimates by Earlier Exposure to Local Labour Rule, Lagged Dependent Variable Specification



Note: This figure corresponds to Figure 10 but using lagged dependent variable specification, i.e., cross-section regressions controlling for Labour vote share (and its square) in 1928, 1930 and 1933.

Table A3: Impact on Vote Shares by Local Politics

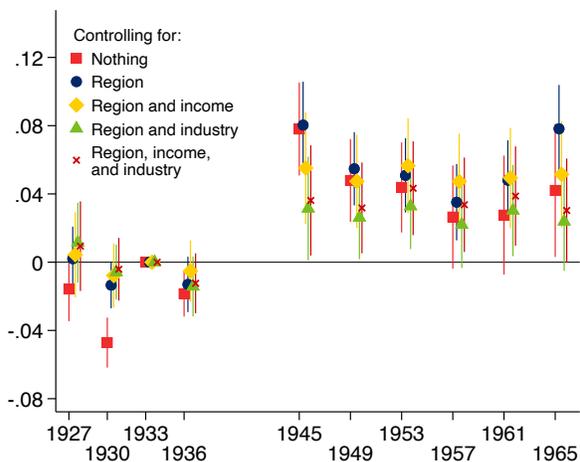
	Labour Party Vote Share				
	(1)	(2)	(3)	(4)	(5)
<i>A: Labour Mayor in 1934</i>					
Main effect (β)	0.067 (0.017)	0.062 (0.010)	0.051 (0.012)	0.036 (0.011)	0.041 (0.012)
Interaction (γ)	-0.027 (0.024)	-0.033 (0.021)	-0.040 (0.020)	-0.040 (0.020)	-0.046 (0.020)
<i>B: Labour Mayor in 1928</i>					
Main effect (β)	0.064 (0.015)	0.064 (0.010)	0.053 (0.012)	0.030 (0.011)	0.038 (0.012)
Interaction (γ)	-0.045 (0.027)	-0.050 (0.022)	-0.062 (0.022)	-0.047 (0.022)	-0.056 (0.022)
<i>C: Labour Mayor in 1928 or 1934 (or both)</i>					
Main effect (β)	0.065 (0.017)	0.060 (0.011)	0.046 (0.012)	0.033 (0.011)	0.036 (0.012)
Interaction (γ)	-0.021 (0.024)	-0.028 (0.021)	-0.035 (0.020)	-0.034 (0.020)	-0.038 (0.020)
<i>D: Labour Mayor in 1928 and 1934</i>					
Main effect (β)	0.065 (0.015)	0.066 (0.010)	0.058 (0.012)	0.034 (0.011)	0.043 (0.012)
Interaction (γ)	-0.063 (0.026)	-0.068 (0.021)	-0.079 (0.021)	-0.062 (0.022)	-0.072 (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 $\hat{\delta}$.

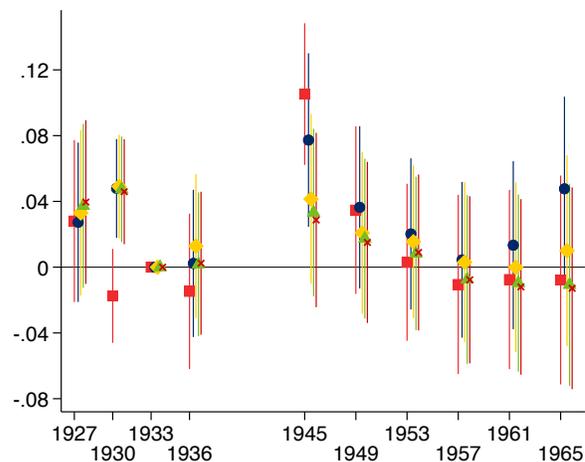
Appendix tables and figures

Figure A9: Alternative definitions for local Labour rule

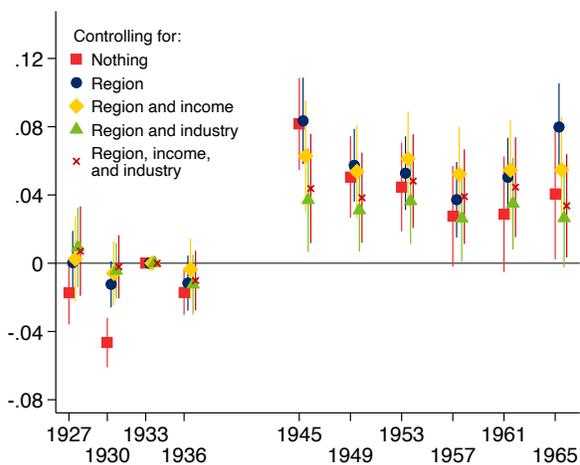
(a) Non-Labour Mayor in 1928



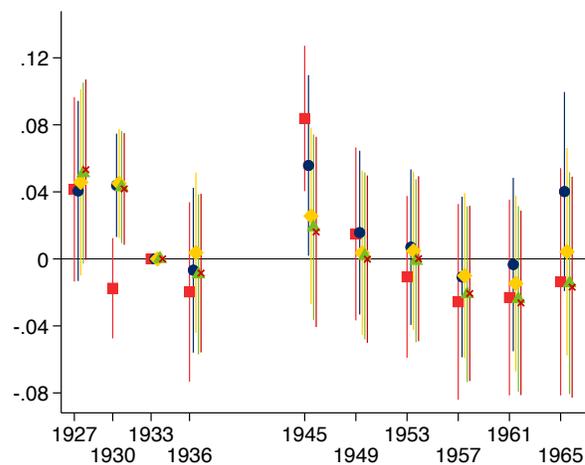
(b) Labour Mayor in 1928



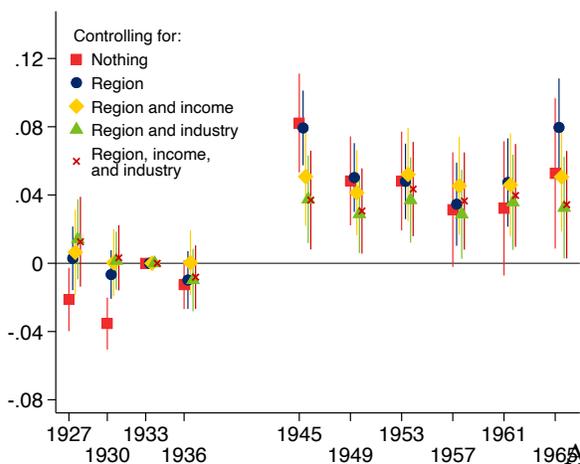
(c) Non-Labour Mayor in 1928 or 1934



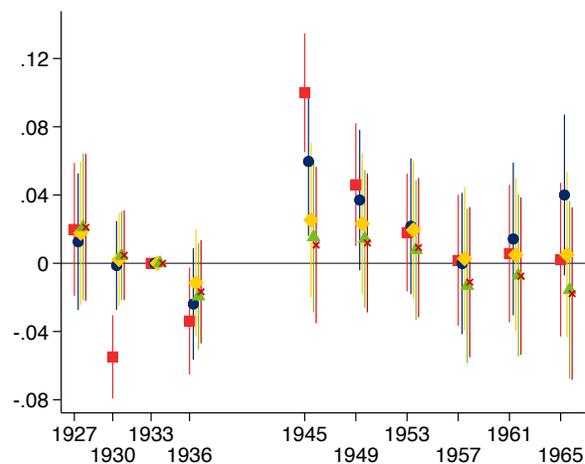
(d) Labour Mayor in 1928 and 1934



(e) Non-Labour Mayor in 1928 and 1934



(f) Labour Mayor in 1928 or 1934



Appendix tables and figures

Table A4: Differences-in-Differences Estimates for the 1957 Survey Data

	Voted the Labour Party				Strongly agrees that the Labour Party has been willing and able to implement its agenda			
	Children		Parents		Children		Parents	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rural	-0.137 (0.035)	-0.166 (0.035)	-0.169 (0.043)	-0.194 (0.044)	0.001 (0.034)	-0.014 (0.036)	-0.019 (0.042)	-0.046 (0.044)
Affected	-0.036 (0.045)	-0.034 (0.044)	-0.003 (0.040)	-0.009 (0.040)	-0.101 (0.043)	-0.094 (0.044)	-0.052 (0.039)	-0.062 (0.039)
Rural × Affected	0.190 (0.068)	0.199 (0.067)	0.148 (0.060)	0.147 (0.058)	0.081 (0.067)	0.068 (0.068)	0.085 (0.059)	0.101 (0.059)
Regional FEs	no	yes	no	yes	no	yes	no	yes
Observations	1,109	1,107	1,109	1,107	1,170	1,166	1,170	1,166

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)–(4), the outcome is an indicator variable for person i voting the Norwegian Labour Party. In columns (5)–(8), 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 columns (1) and (2), A_i is one for individuals, who were 35 years old or younger in 1957 and zero for other respondents. In columns (3) and (4), A_i takes value one if the respondents youngest child is 25 or younger and zero otherwise. In columns (2) and (4), we control for respondent's region of residence.