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Political power, resistance to technological change and economic development: Evidence from the 19th century Sweden

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Political Power, Resistance to Technological Change and Economic Development: Evidence from the 19th century Sweden

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Comments are welcome

Björn Tyrefors Hinnerich[♦], Erik Lindgren[♥] and Per Pettersson-Lidbom[▲]

Abstract

This paper empirically tests the hypothesis that landed elites may block technological change and economic development if they fear that they will lose future political power (Acemoglu and Robinson (2002, 2006, and 2012)). It exploits a plausible exogenous change in the distribution of political power of the landed elites, i.e., a Swedish suffrage reform in 1862 which extended the voting rights to industrialists at the local level. Importantly, the votes were also weighted according to taxes paid. Thus, the higher taxes paid the more votes received. As a result, the landed elites had an incentive to block industrialization and technological progress since they otherwise would be “political losers”. We find that the change in political power from the landed elites to industrialists, through the extension of suffrage rights, lead to more investments in railways, faster structural change, and higher firm productivity. We also find that the change of political power affected both labor coercion and the adaption of labor-saving technologies within the agriculture sector along the lines suggested by Acemoglu and Wolitzky (2011) and Acemoglu (2010). Specifically, we find that is more labor coercion and less investments in labor-saving technologies in areas where landowners have more political power. We also provide evidence that many demographic outcomes were affected by the change in political power. Moreover, we find strong evidence of persistence in both extractive economic and political institutions even after the weighted voting system was abolished and universal suffrage introduced in 1919. Specifically, local governments that were previously political controlled by the landed elites were still using both extractive economic and political institutions (Acemoglu and Robinson (2008)).

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1. Introduction

This paper empirically tests whether politically powerful elites are able to block technological change and economic development using a newly constructed data set from the 19th and early 20th century Sweden.¹ The theoretical point of departure of the empirical analysis is based on the work by Acemoglu and Robinson (2000, 2006, 2012) that argue that elites may block technological change and economic development if the fear that they will lose their political power and future rents. As an exogenous change in the political power of elites, we exploit an extension of the franchise at the local government level in 1862. Historically, only landowners were entitled to vote at the local level in Sweden but after 1862 industrialists also received voting rights. Importantly, the voting system was weighted where voters received votes in proportion of their taxes paid. As a result, the relative strength of political power of the landed elites versus the industrialists was directly linked to economic development. Thus, according to the Acemoglu and Robinson hypothesis, landowners will try to block technological change and economic development since they are afraid of losing their political power if the society is industrialized.

Our empirical design—a combined differences-in-differences and instrumental variable approach—uses the initial change in the landed elites vote share due to the suffrage reform in 1862 as an instrumental variable. We argue that the initial change in political power is likely to be as good as randomly assigned across jurisdictions conditional on year and local government fixed effects. Consistent with this claim, we find that there is no correlation between the instrument and important development outcomes before 1862.

The causal chain that guides our empirical analysis is the following. First, the initial change in the suffrage reform should give rise to a more rapid increase in political power of the industrialists. Second, this change in political power should make it easier for the industrialist to redirect local government spending towards investments in railways. Third, the building of railways should, in turn, facilitate industrialization and structural change. Fourth, industrialization should also affect the demographic transition. Clearly, the causal chain is going to be endogenous except for the initial extension of suffrage rights. Thus, we can therefore only estimate the effect of political power, due to the suffrage reform, on various development outcomes.

¹ This paper is part of a larger project that investigates how political institutions have shaped Sweden's economic development in the 19th and 20th centuries.

To investigate the effect of political power on all these outcomes, we have collected, often on an annual basis, data for about 2,300 local governments for the period of investigation 1838-1920. Specifically, the data set consists of data on all local public investments in railways. It is noteworthy that 70% of all Swedish railway lines were financed by the local governments. Specifically, there were 800 openings of local railways during the period of study. We also have data on all local spending programs such as primary school spending and social welfare (poor relief). We have also gathered measures of structural change and productivity both for the manufacturing sector and the agricultural sector. In addition, we have collected data on labor coercion, wages, labor demand and labor-saving technologies in the agriculture sector. Moreover, we have data on large number of demographic variables such as mortality, infant mortality, fertility, out-migration, in-migration and population size.

We find that landowners are able to block local investments in railways if they have sufficient political power. We also find that local governments where the industrialists have more political power spends much more on railways investments but less on both primary education and social welfare. Thus, the finding about school spending does not lend support to that the process of industrialization raised the importance of human capital in the production process (e.g., Galor and Moav (2006), Galor et al. 2009) but rather that the technological change was unskilled-labor-biased during Sweden's industrialization until the First World War.²

We also find that the more political power the industrialists possess, the larger is the structural change of a society, i.e., there is a larger share of people employed in the manufacturing sector. Another finding is that manufacturing firms situated in societies politically controlled by the industrialists have higher labor productivity than firms located in areas controlled by the landed elite. We interpret this productivity differences as a consequence of barriers of migration caused by a repressive agriculture system. The agriculture productivity is also affected by the political power of the local elite since labor productivity is higher in areas controlled by the landed elite. We also find that real wages and investments in labor-saving technologies are lower in these areas. Moreover, agriculture workers also work longer days and there is a higher demand for unskilled agriculture workers in regions controlled by the landowners. We again interpret this findings as being a

² See also Ljungberg and Nilsson (2009) and Sandberg (1979) for Swedish studies that stress the importance of human capital in the Swedish economic development before the First World War. However, both these studies use aggregate time series data without any credible research design.

consequence of labor coercion since they are consistent with the results in Acemoglu and Wolitzky (2011), Acemoglu (2010), Allen (2009), Habakkuk (1962), Hornbeck and Suresh (2014), Naidu and Yuchtman (2013) and Naidu (2010). Specifically, we think it is the agricultural workers outside option that determines the degree of labor coercion in our data Acemoglu and Wolitzky (2011). Thus, agricultural workers have fewer outside options in areas controlled by landed elite than in places where industrialists have more political power.

When it comes to how the demographic transition is affected by the change in political power, we find that in places where industrialists have more political power the total mortality and infant mortality rates are higher. This finding is consistent with the evidence from the industrial revolution in England where both infant and adult mortality rates were positively correlated with industrialization (e.g., Huck (1995), Hanlon (2015)).³ Another finding is that out-migration in areas where landowners have more political power. However, we find little evidence that in-migration or fertility is affected by the relative political power between industrialists and landowners. Finally, and perhaps surprisingly, we find that population growth is higher in areas politically controlled by the landed elite. The reason for this is that mortality is so high in industrialized localities. Thus, this suggests that industrialization does not always lead to population growth (e.g., see Henderson (2005)) and can therefore create problems for studies that use urban population growth as a proxy for growth (e.g., Acemoglu et al. (2005), Dittmar (2011), Nunn and Qian (2011))

We also test for persistence in political and economic institutions after the abolishment of the weighted voting system in 1919 when Sweden introduced universal suffrage and the one person, one vote rule. We have data on labor market arrangements, i.e., whether the labor market contracts were based on feudal relationships (labor coercion) from 1924 and the type of local political system: direct versus representative democracy (e.g., see Hinnerich and Pettersson-Lidbom ((2014)). We find that local governments where the landed elite had most of the political power after the extension of the suffrage rights in 1862 (i.e., the initial change in voting power in 1862) still could exercise their political power even after Sweden was democratized in 1919. Specifically, they choose to keep the old feudal (repressive) labor contracts together with the archaic town meeting form of government. In sharp contrast, those local governments controlled by the industrialists instead switched to modern labor market contracts and the representative system.

³ Sandberg and Steckel (1997) argue that the Swedish industrialization was not bad for health. However, they do not have a credible research design.

Our paper is related to several strands of literature. First it is related to the voluminous literature of the political economy of growth and development as discussed by Acemoglu (2009), for example. This paper is also related to the literature on technological diffusion (e.g., Gerschenkron (1962), Nelson and Phelps's (1966), Griliches (1957), Krusell and Ríos-Rull (1996), Parente and Prescott (1994)) and the work on the direction and bias of technological change (e.g., Acemoglu 2002). It is also related to the work on the effects of transportation infrastructure (railways) on economic activity (e.g., Banerjee et al. (2012), Donaldson (2017), Donaldson and Hornbeck (2016)).⁴ Our paper also contributes to the debate of the role of human capital versus political institutions for economic development (e.g., Acemoglu et al. (2014), Glaser et al (2004)) Specifically it is related to the literature on the link between industrialization and investments in human capital (e.g., Allen (2003), Galor and Moav (2006), Galor et al. (2009), Goldin and Katz (2001), Mitch (1993), Mokyr (1990), Sandberg (1979), Squicciarini and Voigtländer (2015)). Our paper is also related to the literature on misallocation of resources within and across sectors (e.g., Rosenstein-Rodan (1943), Nurkse (1953), Lewis (1954), Rostow (1960), Banerjee and Duflo (2005), Hsieh and Klenow (1999, 2014), Restuccia and Rogerson (2008) and Gollin et al (2014)). Another related strand of literature is the work on fertility, mortality and the demographic transition (e.g., Becker (1981), Becker and Barro (1988), Preston (1975), Cutler et al. (2006), Galor (2005), Voth (2003)). It is also related to studies on migration, urbanization and the dual economy (e.g., Bairoch (1988), Lewis (1954)). Our work is also related to the literature on forced labor and labor coercion (e.g., Acemoglu and Wolitzky (2011), Dell (2010), Naidu (2010), Naidu and Yuchtman (2013)). Finally, our paper is related to studies of the economic development of Sweden until the First World War.⁵

The rest of the paper is structured as follows. Section 2 describes the historical background, local governments, the suffrage reform, and the data. Section 3 discusses the empirical design. Section 4 presents the results for local spending choices, i.e., railways and the other spending programs. Section 5 discusses the results for structural change and productivity. Section 6 present the results for the demographic transition while section 7 tests for persistence in extractive economic and political institutions. Finally, Section 8 concludes.

⁴ Berger and Enflo (2015) study the effect on railways on economic development. But they only analyze the effect on the main trunk lines on population growth for 81 very small towns. These towns have an average size of about 2,000 in 1855. Thus, they only make up about 6% of the Swedish total population.

⁵ This literature is large. For a collection of articles, see, Jonung and Ohlsson (1997). Most of the historical studies on Sweden are either based on highly aggregated statistics or on individual data from a few local governments. Most of these studies are either descriptive or uses correlation-based approaches. In contrast, this study uses an experimental design on the universe of all Swedish local governments.

2. Background

In this section, we provide a short description of the Swedish setting in the middle of the 19th century.⁶ We also describe the Swedish local governments, the suffrage reform in 1862 and the data we use in the analysis.

In 1850, Sweden was one of the poorest countries in Europe and was a predominantly rural and agricultural based society. For example, 78 % of the nearly 3.5 million inhabitants worked in the agriculture sector while only 9% worked in the manufacturing sector. Moreover, only 10% of the people lived in “cities”. Most of the 87 cities were also of small size with an average size of 4,000. The life expectancy was 41 years in 1855 and the infant mortality rate was 15%. In sharp contrast, in 1960, Sweden was one of the richest and most industrialized countries in the world. Other important facts about Sweden in 19th century were that a large share of the industrialization occurred in the rural areas (e.g., mining and timber industries) and there was a large demand for unskilled labor including women and children (e.g., Heckscher (1954), Schön (2012)).⁷ In 1850, Sweden was also to a large degree a feudal society. Specifically, it had a Four Estate Parliament consisting of nobles, clergy, burghers and peasants (landowning farmers).⁸ The nobility consisted of 1,200 male family heads, the clergy of 1,500 clergymen, the burghers of about 30,000 individuals and the peasants of 180,000-200,000 landowning farmers (Christensen 2006). In other words, only about 5 percent of the total population was entitled to vote. Another important fact was that Swedish farmers were not serfs but mostly freeholders in contrast to most other European feudal societies. Thus, the local elites in rural areas were made up by landowning farmers. Moreover, most farms were of relative small size since the average farm size was 9 hectare. There were also very few large farms, i.e., only 1% were larger than 100 hectare.⁹ The typical farm was a family farm with permanent hired labor, primarily unmarried farm-hands and maids, employed by the year and paid in the form of free lodging, food and small cash wages (e.g., Morell and Myrdal (2011, p. 174)).¹⁰ In other words, the Swedish agricultural economy was characterized by subsistence farming, production for the local market and a large supply of unfree and cheap labor.

⁶ See Koblik (1975) for a collection of articles describing Sweden’s economic development from 1750-1970.

⁷ See Bjurman and Olsson (1979) for a description of child labor in Sweden.

⁸ The Parliament Act of 1866 introduced a new system of representation namely a bicameral legislature.

⁹ The historical size distribution of farms where as follows: 25% below 2 hectare, 64% between 2 and 20 hectare, 10% between 20 and 100 hectare.

¹⁰ See also Schön (2008) for a description of the role agriculture in the Swedish economic development after 1870.

In fact, Sweden had a very labor repressive agrarian system. One component of this repressive system was the Master and Servant Act from 1833 which established that the farm servants (e.g., farm hands and maids) should be contracted for one-year at a time and required to do whatever work that the master (e.g., farmers) deemed necessary.¹¹ The institution of farm service was a crucial system for the supply of labor in agriculture in Sweden. For example, servants made up about 15% of the rural population in 1800. The Act also allowed for coercive measures such as corporal punishment and police fetching in the case the servants did not show up for work. Thus, a master had therefore almost complete control over his workers (e.g., Eklund (1974, p. 227)). A second component of the labor-repressive agrarian system was that the common people were required by law to be employed otherwise they could be sent to imprisonment for life (Eklund 1974, p. 211). A third component of the labor-repressive agrarian system was that a large share of tenant farmers was required by law to perform *corvée* duties, i.e., unpaid labor demanded by the landowner (typically 3 days per week). In 1860, for example, about 13% of the Swedish population lived in households with *corvée* obligations.¹²

The labor repressive element of the agrarian system was also reinforced by that labor mobility (domestic movements) was severely restricted since Sweden maintained a rigid systems of internal passport control until 1860.¹³ In addition, the freedom of trade was also heavily circumscribed until the mid-19th century when the craft guilds were abolished in 1846 and a more general freedom of trade Act for men and unmarried women was introduced in 1864.

Turning to a description of Swedish local governments, Sweden has a long history of local self-government in rural areas. Historically, there existed about 2,300 rural local governments and their decision-making body was the town meeting, i.e., a direct democratic form of government (e.g. see Mellquist (1974)).¹⁴ The town meeting regulation from 1817 stated that basically only landowners had voting rights at the meeting (Sörndal (1941)). Usually, the decisions at the meetings were taken by unanimity but sometimes, in the case of disagreement, a weighted voting scheme was being used where voters received votes in relation to their farm size as measured in terms of “*mantal*” which was the basic tax

¹¹ See Harnesk (1990) and Lundh (2010) for a description of the institution of farm service in the 18th and 19th centuries in Sweden.

¹² See Bäck (1992) for a description of the Swedish *corvée* system.

¹³ The poor relief law also restricted labour mobility since one could not get any social welfare benefits unless one had lived in the local government for at least 3 years (e.g., Hinnerich and Pettersson-Lidbom (2014))

¹⁴ Sweden also had between 87-94 cities in the latter part of the 19th century. The cities have a different political system than the rural local governments.

assessment unit of land in use since the 16th century. This type of weighted voting system gave landowners with larger farms somewhat more political power at the meetings since they received somewhat more votes for a farm with a large mantal.¹⁵

In 1862, the four Estate Parliament decided to extend the suffrage rights at the local level to other groups including industrialists in a new Local Government Act. The rationale behind this new law was the private property principle, i.e., all local taxpayers, including firms, should have voting rights in the local government (e.g., Norrlid (1970)). Moreover, one year later the Four Estate parliament also decided that all local taxpayers should receive votes in proportion to their taxes paid without putting any restrictions on the maximum number of votes a taxpayer could possess.¹⁶ Thus, a single taxpayer could therefore have the majority of votes.¹⁷ Importantly, the taxable income of a local taxpayer was determined by State regulations but the (proportional) tax rate was set independently by the local governments. Thus, a local government could therefore not directly affect the initial distribution of votes. The new suffrage reform in 1862 did not drastically increase the number of eligible voters at the local level since the share of taxpayers was still initially very low, i.e., only 10% of the population was entitled to vote.¹⁸

Here is natural to ask why the Peasant Estate would approve on such a radical change in the suffrage and the weighted voting system. To begin with, the Peasant Estate could not unitary block such a decision in Parliament since that requires a majority of votes among the Four Estates (Christensen 1997, p. 307). In addition, there was a disagreement between large and small landowners regarding many decisions which weakened the political power of the Peasant Estate relatively to the other Estates (Christensen 1997, p. 204).¹⁹ Here it also important to keep in mind at this time it was impossible to foresee the future profound change of the Swedish economy. For example, in 1860 there were only 154 firms with limited liability outside the Stockholm City while the number of firms had increased to 8,700 in 1917 (Norrlid (1970)). Moreover, the lack of industrialization before 1862 also meant that the

¹⁵ See Lagerroth (1928) for a description of the mantal and how it was being used in the local governments.

¹⁶ One firm (Ljusne Woxna AB, Söderala) had 87,974 votes in 1900, which can be compared with that the average number of votes per taxpayer which was about 50.

¹⁷ For example, a single taxpayer possessed the majority of votes in 54 local governments in 1871 and 44 in 1892.

¹⁸ There were some other restrictions on voter eligibility that kept the number of voters low such as an income threshold and absence of any tax liabilities to the local government (Mellquist 1970, p. 112).

¹⁹ In 1866, Sweden changed to a modern Parliamentary system with two chambers were the landowning farmers had almost full political control of the second chamber. The farmers were against any further extension of the franchise until 1909 when Sweden introduced male suffrage for those above the age 25 at the central level. However, this suffrage reform also included other changes which allowed the farmers to still retain their political power at the central level (e.g., Morell (2001, p 117))

landed elite did not lose much political power initially. Before the suffrage reform, landed elites had essentially complete political control of the local government, i.e., close to 100% vote share, while after the reform the average vote share for the landed elite was 72% and they also had a vote share of at least 76% in half of all local governments.²⁰ Moreover, landed elites only lacked the majority of the votes in only 298 out of all local governments. In other words, landed elites still had most of the political power in the lion share of local governments shortly after the suffrage reform.

The political power of landed elites however changed dramatically during the period of our study as can be seen by Figure 1 which plots the initial distribution of the vote share after the suffrage reform and the final distribution in 1908.²¹ In this last year, the average vote share of landed elites was 56% and there were now 825 local governments where landed elites had less than 50% of the votes. Another way of illustrating the dramatic change in the political power of landed elites can be seen in Figure 2 which plots the development in the average number of votes per local government for both landed elites and industrialists for the period 1878-1908. In the start of the period, the mean votes per local government unit were 4,666 for landed elites and 3,312 for industrialists. In the last year, the mean number of votes was 6,375 for landed elites 12,808 for industrialists. Thus, the average number of votes had only increased with 37% for landed elites while it had increased with 287% for industrialists.²²

The new Swedish Local Government Act of 1862 also included other important reforms.²³ Specifically, it explicitly gave local governments permission to deal with all economic matters of local importance. Previously, local governments had been in charge of primary education (about 30% of total spending), poor relief (about 30% of total spending) and matters related to the clergy (30% of total spending). However, now the Act also allowed the local government to spend on local infrastructure investment such as railways.

²⁰ There does not exist any statistical data on the weighted voting system until 1871 (BISOS R: 1874). However, these data are not reliable due to the very large number of errors (see the initial discussion in the publication) and cannot be used for constructing valid measures of vote shares of the landed elites. For this reason, we instead use the data published in BISOS U to construct the initial post-reform value of the vote share of the landed elites. Specifically, we take the mean value over the years 1881-1883 so as to minimize any measurement errors.

²¹ The reason for using 1908 as the end year is that was a large change in the weighted voting system in 1909 when the maximum of number of votes was capped at 40.

²² The reason for this large discrepancy is that the local agricultural property tax base only increased slowly during this period due to that the assessed property value increased slowly and that the common property tax rate was fixed at 3% by central regulations.

²³ Before 1862, Swedish local governments only had one town meeting for both clerical and secular matters. After 1862, there were two distinct town meetings; one for clerical matters including primary education and another one for secular matters (e.g., poor relief, railways). However, they both had identical weighted voting systems with identical voters.

A major contribution of this paper is that we exploit a newly assembled data set over local governments for the 19th century.²⁴ Our data includes, often on an annual basis, data on very large number of political, economic and demographic variables for about 2,300 rural local governments. We describe the variables used in the analysis in the following.

Our key explanatory variable of interest is the political power of the landed elite at the local government level, i.e., the ratio of the total number of votes of landowners to the total number of votes. We only have data on this variable for the period 1881-1908.²⁵ Thus, these data therefore dictates our empirical framework, a combined difference-in-difference and instrumental variable approach. Specifically, we construct our instrumental variable—the initial change in the political power of the landed elites due to the suffrage reform in 1862 based on this data. We use the mean over the years 81-83 so as to minimize any measurement errors in the instrumental variable. Although, that these data are measured nearly 20 years after the reform, we think this variable is a good measure of the initial change in political power for two reasons. First, industrialization was still in its infancy in the beginning of the 1880s. For example, in 1862 the share of worker in the manufacturing sector was 11% while it was 12% in 1879 (e.g., Edvinsson 2005). Second, we know from other sources that the vote share of the landed elite was 77% in 1865 while the average share is 72% based on the instrument. Thus, the political power of the landed elites has not changed much since 1865. More importantly, however, is that this instrument is uncorrelated with pre-reform development outcomes such as infant mortality, total mortality, fertility, and population growth as discussed below.

We will be analyzing a large number of outcomes related to technological change, local spending programs of local governments, and economic development. When it comes to technological change, we have yearly data on two variables that measures the adaption of a new technology, i.e., investments in local railways. The first variable is a dummy variable for whether a local government has a local railway line within its geographical boundary.²⁶ The second measure is an indicator variable for having taking a long term loan for investments in local railway constructions.²⁷ Thus, we therefore have two complementary measures of investments in local railways.

²⁴ The data collection is financed by an ERC consolidator grant.

²⁵ There does exist data for 1871 (BISOS R: 1874) but these data are not reliable due to the very large number of errors (see the initial discussion in the publication) and cannot be used for constructing valid measures of vote shares of the landed elites.

²⁶ This variable is constructed from yearly geocoded local railways networks.

²⁷ This variable is derived from information on all local governments' applications for long terms railway loans that was collected by hand from the National Archives (Civildepartementets arkiv: ingående diarer). We have

Turning to the other local government spending programs, we are going to analyze four outcomes: total per capita spending as well as the three major spending programs: (i) primary education (ii) poor relief and (iii) church spending.

When it comes to measures of structural change and productivity, we are going to use census data for 1910 to create measure of the share of people employed in the manufacturing and agricultural sector, respectively. We also have to measures for worker productivity at the plant level for 1914: output per worker and output per working days. Similarly, we have to measures of productivity in the agricultural sector for 1914: output per area and output per worker.

The demographic variables that will be used in the analysis are: (i) the mortality rate, (ii) the infant mortality rate, (iii) the fertility rate (iv), the population size, (v) in-migration and (vi) out-migration. These are measured on a yearly basis.

We will also use a number of pretreatment control variables, i.e., they are measured before the instrumental variable. We will control for the following eight variables in the analysis: (i) the share of eligible voters, (ii) the total number of votes,²⁸ (iii) the inequality in the vote share distribution (Gini coefficient), (iv) population size, (v) the share of people working in the agricultural sector, (vi) the share of people working in the manufacturing sector, (vii) the share of people below 15, and (viii) the share of people in the working force.²⁹

In the empirical analysis, we will use two different samples due to jurisdictional changes and that some local governments sometimes share common responsibilities for education and poor relief. The full sample consists of 2,218 local governments while the restricted sample consists of 1,624. In the restricted sample we have dropped all those local governments with inconsistencies in their boundaries or those who have joint responsibilities for the period 1838-1909. We also have differential rates of missing observation across the various outcomes since they come from different sources. See the Appendix for discussion of these issues.

Table 1 shows summary statistics of the variables used in the analysis for the restricted sample. We have divided the table into four panels. Panel A shows the summary statistics for the instrument, Panel B, for the explanatory variable, Panel C for the outcome variables and Panel D for the pretreatment control variables.

treated an application as being identical to a long term loan since the central government almost always granted the applications (Oredsson (1989)).

²⁸ The total number of votes is also a proxy for income since the number of votes is proportional to taxes paid.

²⁹ Some of the pretreatment control variables (v-viii) are taken from the North Atlantic Population Project (NAPP) database. See link <https://www.nappdata.org/napp/participants.shtml>.

3. The Empirical Design

As we discussed above, our theoretical point of departure is the models by Acemoglu and Robinson (2000, 2006, 2012), which suggest that there exists a causal relationship between political institutions— the strength of the political power of landed elites— and technological change and other growth-related outcomes. Thus the population regression equation of interest is therefore

$$(1) \quad Y_{it} = \alpha + \beta X_{it} + v_{it}$$

where the index i denotes a local government at time period t . The outcome Y_{it} is either a measure of investments in railways, other types of local government policy (education, social welfare), a measure of structural change or productivity, or a demographic outcome (fertility, mortality). The explanatory variable, X_{it} , is a measure of the contemporaneous relatively *de jure* political power of the landed elites. In our context of a weighted voting system with basically two opposing elites (landowners and industrialists), X_{it} is defined as the present vote share of landed elites. The hypothesis is that the more *de jure* political power the landed elite possess currently, the more likely is that technological change and economic development can be blocked, i.e., the parameter β is negative if Y_{it} is defined as a positive development outcome.

Clearly, we need to find some exogenous source of variation in the political power of landed elites to be able to estimate a causal relationship. We argue that the initial distribution of the political power of landed elites due to the suffrage reform in 1862 will be as good as randomly assigned, conditional on local government and time-fixed effects. Thus, our empirical design is a combined difference-in-differences and instrumental variable approach where the structural relationship (1) now also includes local government fixed effects, α_i , and time-fixed effects λ_t , i.e.,

$$(2) \quad Y_{it} = \alpha_i + \lambda_t + \beta X_{it} + v_{it}$$

and where the instrumental variable for current political power of the landed elite, X_{it} , is the initial value of the vote share of landed elites, X_{i0} . In other words, and analogous to a randomized controlled trial (RCT) with partial compliance, we use the *assigned* treatment level (X_{i0}) as an instrumental variable for the *actual* treatment level (X_{it}).

We are going to estimate the parameter β in equation (2) in two different ways. The first estimator is long-difference instrumental variable estimator (LD-IV) which basically uses only observations from two cross-sections in the panel data set, i.e., the initial period, $t=0$, and the last period, $t=T$. More specifically, the LD-IV approach uses the following reduced form (RF) and first-stage (FS) equations:

$$(3) \quad \Delta Y_i = a + bX_{i0} + \Delta \varepsilon_i$$

$$(4) \quad \Delta X_i = c + dX_{i0} + \Delta \omega_i$$

where equation (3) is the RF and equation (4) is FS in the LD-IV approach and $\Delta Y_i = Y_{iT} - Y_{i0}$ and $\Delta X_i = X_{iT} - X_{i0}$ are the long differences, respectively.³⁰ However, as discussed by McKenzie (2012), having repeated measures of outcomes both at baseline ($t=0$) and at endpoint ($t=T$) reduces measurement errors and increases the power of the analysis. Thus, we will therefore use multiple measurements of the outcomes in the LD-IV analysis. Here it is important to stress that the instrument is also based on repeated measurements as discussed previously. Obviously, it does not matter whether the instrumental variable X_{i0} is differenced or not in equations 3 and 4 since the suffrage “experiment” starts in period $t=0$ for all i .

The second estimating method is a fixed-effects instrumental variables (FE-IV) estimator which uses all available time periods in the panel data set. Specifically, the FE-IV approach uses the following RF and FS equations:

$$(5) \quad Y_{it} = \alpha_i + \lambda_t + \sum_{t=1}^{t=T-1} (X_{i0} \times D_t) \pi_t + v_{it}$$

$$(6) \quad X_{it} = \alpha_i + \lambda_t + \sum_{t=1}^{t=T-1} (X_{i0} \times D_t) \rho_t + e_{it}$$

where D_t is an indicator variable for time period t .³¹ Thus, the parameters π_t and ρ_t are therefore time-varying reduced form and first-stage coefficients.

The two estimating approaches nicely complement each other since they have different strength and weaknesses. For example, the LD-IV estimator is likely to be more robust to dynamic misspecifications and measurement errors than the FE-IV estimator since it relies on one long-difference rather than year-to-year variation in the outcomes (Griliches and Hausman (1986), McKinnish (2008)). On the other hand, the FE-IV is likely to be more

³⁰ Equations (3) and (4) are both standard panel data differences-in-differences with only two time periods. For a textbook treatment see for example, Chapter 11 (Section3) in Stock and Watson (2003).

³¹ Equations (5) and (6) are both standard panel data differences-in-difference regressions with multiple time periods. For a textbook treatment see for example Chapter 11 (Appendix 2) in Stock and Watson (2003).

statistically powerful since it uses much more data than the LD-IV. Moreover, the result from FE-IV is also likely to be more externally valid than the LD-IV since it uses many more time periods. The FE-IV, however, might suffer from many instruments problems since there are a large number of over-identifying restrictions (e.g., Hansen et al. (2008)). Taken together, this discussion suggests that finding similar results from both approaches would bolster both internal and external validity of the results.

We will also include a number of pretreatment variables to address concerns about any potential violations of the exclusion restriction. In other words, although that the instrument (the suffrage reform) is likely to be as good as randomly assigned conditional on local government and time fixed effects, the instrument could potentially affect the outcome via other channels than the relative distribution of political power between the two local elites: industrialists and landowners. For example, an unequal distribution of wealth and income may also have an effect on economic development (e.g., Sokoloff and Engerman (2000)). Thus, the initial inequality in the vote share distribution may therefore have an independent effect on the outcome. We will therefore control for the following pretreatment variables: (i) the inequality in the vote share distribution (Gini coefficient) (ii) the share of eligible voters (iii) the total number of votes (iv) the share of industry workers (v) the share of people working in the agricultural sector (vi) population size (vii) the share of people below 15 and (viii) the share of people in the working force. Perhaps, the most important control variables are (i)-(iii) since they explicitly control for other characteristics of the initial distribution of de jure political power that might have a direct effect on the outcome independently from the vote share of landed elites. In practice, all these pre-treatment variables will they will be directly included in levels in both FS and RF equations in the LD-IV while they will be interacted with a full set of time indicators and included in both FS and RF equations in the FE-IV approach.

Turning to the results from the first stage of the LD-IV and FE-IV approaches, Table 2 shows the results from the first-stage of the LD-IV approach i.e., equations 4. Table 2 presents these results for three specifications: Column 1 and 2 show the results for the restricted sample, with and without control variables while Column 3 displays the result for the full sample with control variables. The estimated first-stage effect varies between -0.09 and -0.15 . The negative effect means that there is a larger decrease in vote share of the landowners in areas where the landed elite has a higher initial vote share. This means that there is “beta convergence” across local governments (e.g., Barro and Sala-i-Martin (1992),

Mankiw et al. (1992)).³² In other words, the political power of industrialist, which is basically a measure of income,³³ grows faster in places where landowners initially had a high vote share. This make perfect sense since areas initially controlled by the landed elite was poorer than areas initially controlled by proto-industrialists.³⁴ Thus, agricultural societies (poor local governments) are therefore catching up with industrial societies (rich local governments) in terms of voting power of the industrialists. The first-stage results from FE-IV approach, equation 6, also corroborates this finding. The result is displayed in Figure 3 where we have plotted the point estimate together with a 95% confidence interval. The excluded instrumental interaction is 1908. Thus, the point estimate should be interpreted relatively to 1908. For example, the point estimates for 1884 is 0.12 which means that the vote share of the landed elite was 12 percentage points higher in 1884 than in 1908. Consequently, the decrease in the point estimates from 1884 to 1908 means that there increase in the de jure political power of the industrialists over time.

Turning to the strength of the instruments, Table 2 shows that the cluster-robust first-stage F-statistic in the LD-IV approach is large since it varies between 26 and 56. Thus, the single instrument is therefore strong. In the FE-IV there are 24 instruments and almost all of them are highly statistically significant different from zero with t -values above 5. However, the F-test is still only about 10 which is the threshold value for weak instruments suggested by Stock and Yogo (2005).

We continue by testing whether the instrument— the initial distribution of the political power of landed elites due to the suffrage reform in 1862—is as good as randomly assigned conditional on local government and time fixed effects using data for the pretreatment period 1838-1859. To be able to illustrate the statistical test graphically, we first create a binary variable where those local governments with a higher value of X_{i0} than the median value form one group (e.g., the treatment group) and those with a value below the median make up the other group (e.g., the comparison group). Thus, the set-up is now identical to a standard difference-in-differences approach and we can therefore graphically illustrate whether the two groups have similar trends in development outcomes before the policy change in 1862. Figures 4-7 show the trends for these two groups for a number of different development outcomes for the period 1838-1859. The pretreatment outcomes that we have data on are the

³² Beta convergence is when a poor economy tends to grow faster than a rich one.

³³ As discussed previously, the industrialist receives votes in proportion to their income.

³⁴ See Schön (1982) for a discussion of the Swedish proto-industry.

mortality rate, the infant mortality rate, the fertility rate, and population size.³⁵ All variables are all expressed in logarithmic form. These figures reveal that the two groups have parallel trends in these outcomes during the period before the suffrage reform in 1862. We can also perform statistical tests of whether the instrument is correlated with any of the four pretreatment outcomes. The first test is based a long-difference specification between 1839 and 1859, i.e., $\Delta Y_i = \alpha + \beta X_{i0} + v_i$. Panel A of Table 3 presents these results for the restricted sample, both with and without the control variables. The estimates are all small and not statistically significant at conventional levels except for estimate for population size without control variables in Column 7.³⁶ However, a much more statistically powerful test is to pool together all the differences-in-differences estimates for the whole period and estimating $\Delta Y_{it} = \alpha + \beta X_{i0} + v_{it}$. Panel B in Table 3 shows the results from this test. Now all the point estimates are very small. Nonetheless, some of the point estimates are still statistically significant but this is not surprising since the sample size is very large. Thus, these estimates are therefore precisely estimated zeroes. Taken together, these tests strongly suggest that the instrument is as good as randomly assigned conditional on local government fixed effects and time-fixed effects.

³⁵ Unfortunately, there only exists demographic data before 1860. This data comes from the predecessor of Statistics Sweden, Tabellverket. This data were downloaded from the Demographic Database, Cedar, Umeå University (see link <http://www.cedar.umu.se/english/ddb/public-access-databases/tabverk-on-the-web/>)

³⁶ The estimates in Table 2 can also be compared with the corresponding reduced form estimates displayed below. For example, the reduced form effect for mortality in panel A of Table 11 is in the range 0.24-0.40 while they are 0.03 in Table 2.

4. Local government spending on railways and other fiscal choices

In this section, we analyze the effect of the distribution of political power between the landed elite and the industrialist on local governments spending on railways, education, social welfare and church. As noted previously: spending on education, social welfare and church is the main local spending programs and typically constitute more than 90% of total local government spending.

Sweden started to build railways in 1856 which was very late from a European perspective. In 1853, it was decided by the Four Estate Parliament that only the trunk lines should be built and operated by the State while all other local railways should be operated and financed by private interests in the form of limited liability companies that was to a large extent owned by local governments.³⁷ Since only 30% of the total Swedish railway network, consisting of a maximum of 16,886 km, were constructed by the State this meant 70% of all Swedish railways were to a considerable extent financed by local governments (Oredsson (1989)).³⁸ Figure 8 shows the annual size of the railway network up to 1910 for both state and privately owned railways. It reveals that only before 1874 was the size of the state owned railway network was larger than the privately owned. Another way of illustrating the extent of the privately owned Swedish railway network is provided by Figure 9, which shows a map of the railway network in 1910 where the lines colored in red are state owned. Specifically, it shows how dense the privately owned railway system was in 1910.

Figure 10 shows the yearly number local governments with local railways for our sample period 1880-1908. It reveals that about 300 local governments already had a local railway in 1880 but this number had increased to 800 in 1908. There were, however, more than 800 local governments that invested in local railways via public holdings of stocks in railway firms. These local public investments were typically financed via long-term loans, typically with 40 years maturity, which required approval from the central government. Figure 11 shows the annual number of local governments that have a long term investment loan for local railways.

The local financing of railways was highly controversial during this period. For example, Mellquist (1974) describes a very large number of cases where landowners were in fierce conflict with industrialist over the building and financing of local railways and where the industrialist had exercised their higher voting power at the town meetings. In those cases,

³⁷ For the financing and construction of Swedish railways see Hedin (1967), Nicander (1980), Modig (1971) and Oredsson (1989).

³⁸ Before the First World War, Sweden had 25 km of railroad for every 10,000 people which was more than twice of any other European country (Hedin (1967))

the landowners appealed directly to the central government arguing that the local financing of railways was unconstitutional or economically impossible due to the stressful fiscal situation of the local government. However, the original town meeting decision was very seldom overturned since local governments were entitled by the local government act in 1862 to finance the building of local railways.

Turning to the results from our LD-IV and FE-IV approaches, Table 4 shows the results for local public investments in railways, i.e., a binary variable indicating whether a local government has a railway line or not.³⁹ Panel A shows the reduced form effect from the LD-IV approach, i.e., equation 3. Panel B presents the IV estimate from the LD-IV approach while Panel C displays the results from the FE-IV approach. Again, we present results from three specifications: (i) restricted sample without controls (Column 1)), (ii) restricted sample with controls (Column 2) and (iii) the full sample with controls (Column 3). Panel A shows that the reduced form effect is about 0.1 and statistically significantly different from zero at the 5% level for all three specifications. To compute the IV estimate, we need to scale the estimated reduced form effect with the corresponding first-stage effect in Table 2. Note here that the first-stage estimate is negative, which implies that the higher the initial vote share of the landed elite the higher is the growth rate in political power of the industrialist. Thus, the estimated effect from the LD-IV method in Panel B is therefore between -0.7 and -1.7 which imply that a 10 percentage points *increase* in the current vote share of the landed elite *reduces* the probability of building a local railway with 7-17 percentage points. In other words, local governments controlled by landowners built fewer railways than those ruled by industrialists. The estimated effect from the FE-IV in Panel C is also in the same range as in the LD-IV approach. Thus, it reassuring that we get highly similar results from both IV estimators because that bolsters both internal and external validity as discussed previously.

Table 5 shows the results for our second measure of local public investments in railways, i.e., a binary variable indicating whether a local government has taken a long-term railway loan. Again, we present the results in exactly the same way as we did in Table 4. The reduced form effect in the LD-IV approach is in the range 0.18-0.32. Thus, the LD-IV estimate is between -1.2 and -2.5 . In other words, a 10 percentage *increase* in the current vote share of the landed elites *decrease* the probability of taking a long-term railway loan with 12-25 percentage points. Thus, local governments controlled by industrialists have more

³⁹ Angrist and Pischke (2009) discuss the problem when the outcome variable is binary when estimating causal effects. They recommend using a linear probability model.

railway loans. Once more, the results from the FE-IV specifications in Panel C are similar to the result from the LD-IV approach.

We now turn to the results from the other local government spending programs, namely primary education (Table 6), social welfare (Table 7) and spending on the clergy (Table 8). We also report results from total spending (Table 9). All the spending measures are expressed in logarithmic form.

Starting with primary education, which is measured as expenditure per child for children aged 7-14, Table 6 shows that the estimated reduced form effect from the LD-IV approach ranges from -0.65 to -0.80 . Thus, the corresponding range of IV estimate is therefore in the range 5.2-9.6. However, the IV estimates with control variables are both around 5, which means that a 10 percentage *increase* in the current vote share of the landed elites *increase* spending on primary education with 50 percent.⁴⁰ Consequently, local governments controlled by the industrialists therefore spend much less on primary education. Once again, the findings from the FE-IV specifications in Panel C in table 6 are similar to the result from the LD-IV approach.

Continuing with per capita spending on social welfare (poor relief), Table 7 displays that the estimated reduced form effect in the LD-IV method is about -0.5 . The estimated IV effect is therefore in the range 3.0-6.2. The IV estimates with controls are both however about 3. Thus, a 10 percentage *increase* in the current vote share of the landed elites *increase* welfare spending with 30 percent. As a result, local governments where industrialists have more political power spend less on social welfare.⁴¹ Once more, the results from the FE-IV specifications are similar to findings from the LD-IV approach.

Table 8 presents the results on per capita church spending. All the estimates are small and not statistically significant. Thus, the distribution of political power between landowners and industrialists does therefore not affect church spending. This finding is not surprising since there was no clear conflict of interest between the landed elites and

⁴⁰ This may seem as a very large effect. However, spending on primary education includes very large spending items such as school buildings. The denominator is also often very small, i.e., numbers of children in the age range 7-14. Thus, these two points is reflected by the fact that coefficient of variation is larger than 100% for educational spending per child. Moreover, expressing spending in per capita terms instead reduces the estimated IV effect to less than 2.

⁴¹ Interestingly, when we measures welfare spending in terms of number of paupers rather than population size, the estimated effect switches sign. This has to do with that there are more paupers in local governments controlled by landowners than industrialists. Thus, a pauper therefore gets higher welfare benefits in local governments controlled by the industrialists. This result makes sense since industrial workers must receive a higher compensation, if they become unemployed or sick, than agricultural workers due to compensating wage differentials in the industrial sector.

industrialists regarding church matters. Consequently, that church spending is not related to the distribution of political power strengthen the causal interpretation of the previous results.

Turning to the results from total per capita spending, Table 9, reveals mixed results. In the LD-IV approach, there is little evidence that total spending is affected, at least when control variables are included. In the FE-IV method, however, the estimated effect is statistically significant but rather small, less than 1, when controls are included. Thus, taken together, there is weak evidence that total spending is affected.

To summarize the findings about fiscal spending choices, we find that when industrialist gain more political power in the local government, due to the suffrage reform in 1862, they start investing in railways but at the same time they reduce spending on both primary education and social welfare.

5. Structural change and productivity

In this section, we test whether the change in the political power also had an effect on the structural change. i.e., the shift of employment from agriculture to manufacturing, and productivity within both the manufacturing and agriculture sectors. At the aggregate level, the employment in manufacturing sector had increased from 11% to 23% from 1862 to 1908 (Edvinsson 2005).

To estimate the long-run effect of political power on the structural change, we are going to use the LD-IV approach, i.e., equations 3 and 4. Importantly, and in contrast to the previous analyses, the baseline outcome Y_0 will here be assumed to be zero (or equivalently, a fixed common constant). In other words, we argue that is sensible to define the baseline outcome as being approximately zero since Sweden was almost a completely agrarian society before the suffrage reform 1862 as discussed previously. As a result, both the reduced form (equation 3) and the first-stage (equation 4) in the LD-IV approach can now be expressed in levels rather than in differences, i.e.,

$$(7) \quad Y_i = \alpha_0 + \alpha_1 X_{i0} + \varepsilon_i,$$

$$(8) \quad X_i = \beta_0 + \beta_1 X_{i0} + \omega_i.$$

where the IV estimate is the ratio of α_1 and β_1 .⁴²

To measure the structural change at the local level, we are going to use data from the 1910 Swedish census.⁴³ Specifically, this data allows for the constructing of a rather crude measure of the employment shares in both the agricultural and manufacturing sectors.⁴⁴ Table 10 presents the results from the LD-IV approach. Panel A shows the first-stage results while panel B display the reduced form effects and Panel C present the instrumental variable estimates. Column 1 and 2 show the results from the share of employment in manufacturing and Columns 3 and 4 for the share of employment in agriculture. Column 1 and 3 show the result for the restricted sample while Column 2 and 4 display the results for the full sample. Starting with the first-stage estimates in Panel A, we see that it is 0.9 for both the restricted sample and for the full sample. Turning to the reduced form results in panel B, the estimated

⁴² In the empirical specification of the LD-IV approach for having built a railway where the baseline outcome is zero, the estimated reduced form effect is -0.16 (s.e. = 0.07) for the restricted sample and -0.22 (s.e.= 0.05) for the full sample. Thus, the LD-IV estimate in the current approach is therefore negative for railways and therefore completely consistent with the finding of a negative effect for railways in the previous LD-IV approach in Table 4 where the baseline outcome was different from zero.

⁴³ The Swedish 1910 census data was provided by the Swedish National Archives.

⁴⁴ The 1910 census is the first Swedish census with reasonably good occupational statistics.

for the share of employment in manufacturing, expressed in logarithmic form, is -1.5 for both the restricted sample and for the full sample. The corresponding IV estimate is therefore -1.7 . Thus, the negative IV estimates imply that the more political power the landed elite possess the more it can block industrialization and structural change. The estimated effect is also large, i.e., a 0.1 increase in the political power of the landed elite leads to a 17% reduction the share of manufacturing workers. Turning to the result for the share of agricultural workers in Columns 3 and 4, we see exactly the opposite result, namely that that the more political power the landed elite possess the larger is the share of agricultural workers. The magnitude of the IV estimates is however smaller for agriculture than manufacturing since a 10 percentage point increase the vote share of the landed elite leads to a 10 % increase in the share of agriculture workers.

Having found that the political power affects structural change and economic development, it is also interesting to investigate if productivity within the agriculture and the manufacturing sectors is affected by the political power. For example, there is some previous work in economics that argues that increases in agricultural productivity are an essential condition for economic development (e.g., Rosenstein-Rodan (1943), Nurkse (1953), Lewis (1954), Rostow (1960)). Thus, the change in suffrage rights in 1862 could perhaps have led to an increase in agriculture productivity, which in turn started the industrialization process. The ruling elite could also potentially affect labor productivity in the both the manufacturing and agricultural sectors. For example, there are many policies and institutions constraints that can cause misallocation of resources across firms as discussed by Banerjee and Duflo (2005), Hsieh and Klenow (1999, 2014), Restuccia and Rogerson (2008) and Gollin et al (2014). Perhaps the most relevant factor for misallocation of resources in the Swedish setting was that there were severe barriers to moving out of agriculture due to the Swedish labor repressive system described previously. Thus, the local landed elite could therefore enforce a strict control of the movements of their agriculture workers.

To measure labor productivity in the manufacturing sector, we rely on yearly data from 1914 on all Swedish manufacturing plants with a more than 10 workers and a production value of at least 10,000 kr. Thus, there are 9,695 plants that meet these sample restrictions. We have digitized data on total sales value, total number of workers as total number of working days.⁴⁵ As a result, we can construct two measures of labor productivity at the plant level: output per worker and output per working days.

⁴⁵ This data comes from the National Archives. We have digitized all yearly plant level data for the period 1913-1952.

Table 11 shows the results for the two measures of labor productivity. Columns 1 and 2 present the results for output per worker (in logarithmic form) while Columns 3 and 4 show the results from output per working days (in logarithmic form). Column 1 and 3 shows the results for the restricted sample and Columns 2 and 4 for the full sample. The estimated reduced form and IV effects for both labor productivity measures are negative. Thus, a manufacturing firm based in a local government politically controlled by the landed elite has lower labor productivity than firms in situated in local governments controlled by the industrialists. This result suggests that there are misallocations of workers due to barriers of movements out of the agriculture sector. The estimated effects are also fairly large since an 10 percentage points increase in the vote share of the landed elite reduced the labor productivity of a manufacturing firm with 4-5 percent.

Turning to the measure of agriculture productivity, we have digitized annual data on the yield from 16 different crops and their prices at the local government level for the period 1913-1952.⁴⁶ We can therefore construct an average measure, which is based on the average over the period 1913-1919, of total output per unit of land for all local governments as well as a measure of output per agriculture worker. The results are presented in Table 12. Perhaps surprisingly, political power has no effect on output per area unit. However, it is important to keep in mind that biological and chemical technology was little developed during the 19th century. Thus, there were little farmers could do to increase land productivity. In contrast, labor productivity was affected by political power. Specifically, in areas where the landed elite has more political power had higher labor productivity.

How should we think about that the political power of the local elite is related to labor productivity both within the manufacturing and agriculture sectors? One possible interpretation is based on that there are restrictions on labor mobility due to the labor repressive agriculture, i.e., the Master and Servant Act, as discussed previously. Thus, the findings that in areas where the landed elite has more political power the labor productivity is lower in the manufacturing sector, but higher in the agriculture sector, can both be rationalized by that there are restriction on labor mobility from the agriculture sector. In other words, in places politically controlled by the landed elite, the manufacturing firms do not get the best workers and farm workers must work harder due to barriers on labor mobility. Moreover, our finding that the agriculture productivity is higher in areas controlled by the landed elite is also consistent with labor coercion in the agricultural sector, since the model by

⁴⁶ This data comes from the National Archives.

Acemoglu and Wolitzky (2011) imply that labor coercion always increases the effort of the agent.⁴⁷

To further investigate the role of labor coercion in the process of economic development and technology adoption in the agricultural sector,⁴⁸ we have gathered additional information on (i) the real wage of unskilled agriculture workers, (ii) working hours, (iii) labor demand, and (iv) labor-saving technological innovations,⁴⁹ i.e., number of horses per arable land.⁵⁰ Table 13 shows the reduced form results, i.e., equation 7. All the outcomes are expressed in logarithmic form except for labor demand, which is a zero-one variable. Table 13 displays that in places politically controlled by the landed elite: the real wage is lower (Columns 1 and 2), the workers have longer working days (Column 3 and 4), there is higher labor demand for agriculture workers (Column 5 and 6) and there is less investments in labor-saving technological innovations (Columns 7 and 8). Some of these estimated effects are also large. For example, the maximum effect on wages is larger than 20% while the effect on labor saving technologies is in the order of 40-50%.

These findings are potentially consistent with the three hypotheses in the economics literature, namely that (i) low-wage agricultural labor discourages labor-saving technological innovation (e.g., Habakkuk (1962), Allen (2009), Acemoglu (2010)) (ii) labor coercion increases the effort of workers (Acemoglu and Wolitzky (2011), and (iii) labor coercion is more prevalent in regions that experience larger unskilled labor demand (e.g., Domar (1970), Acemoglu and Wolitzky (2011)).⁵¹ However, for our argument to hold water we also need to convincingly establish that labor coercion is much higher in regions controlled by the landed elite. In Section 7, we show that the prevalence of labor repression is much higher in areas politically controlled by the landed elite after democratization in 1919.

⁴⁷ For example, Fogel and Engerman (1974) also argue that Southern slavery was productive.

⁴⁸ Hornbeck and Naidu (2014) study the relationship between restriction of labor mobility and agriculture development in the historical context of the United States.

⁴⁹ Morell (2001, p. 288) argue that the power that the horses provided was a crucial component in the mechanization of the Swedish agriculture sector. He writes “Hästen slog inte bara ut oxen som dragare i den tunga åkerbearbetningen utan drog också slåtter-, skörde- och såmaskiner”. Thus, number of horse per area should be a good measure of mechanization in the agriculture sector.

⁵⁰ This data comes from the National Archives. Data on agriculture wages, working time and labor demand is available on a yearly basis from 1911-1928. Data on cost of living is from 1916, data on number of horses is from 1919 while data on arable land is from 1917.

⁵¹ In the next section, we also show that out-migration is higher in areas politically controlled by the landowners during the sample period 1884-1908. Thus, labor scarcity in 1913 is therefore due to previous out-migration of agricultural workers from these areas.

6. Demographic transition

In this section, we test whether the change in the political power also had an effect on the demographic transition. As noted in the introduction, there is a large literature about the demographic transition. In this paper, we will analyze the following six demographic variables: total mortality, infant mortality, fertility, out-migration, in-migration and population. All these variables are expressed in logarithmic form. We are again going to use the FE-IV and LD-IV approaches described in section 3.

Starting with per capita mortality, Panel A of Table 14 shows that the reduced form effect from the LD-IV approach ranges from 0.22 to 0.29. Hence, the estimated IV effect in Panel B ranges from -1.9 to -2.6 but for the specifications with control variables is about -1.9 . Thus, the mortality rate is higher for local governments controlled by the industrialists. Specifically, 10 percentage *decrease* in the current vote share of the landed elites *increase* mortality with almost 20 percent. The estimated effect from the FE-IV approach is similar to the estimate from the LD-IV.

Table 15 presents the results for infant mortality, the ratio of the number of deaths of children under one year of age and number of births. The estimated reduced form effect in Panel A ranges from 0.20-0.40. Thus, the estimated IV effect ranges from -1.9 to -4.4 , but for the specifications with control variables it is about -2 . Again, we find that 10 percentage *decrease* in the current vote share of the landed elites *increase* infant mortality with about 20 percent. The estimated effect from the FE-IV approach is similar to the estimate from the LD-IV.

Turning to fertility, i.e., the ratio of number of births to population size, Table 16 reveals that there is little or no evidence for a relationship between the distribution of political power and fertility. Specifically, very few of the estimates are statistically significant and the estimated effects in the LD-IV and FE-IV are of opposite signs.

Table 17 presents the results for per capita out-migration. The estimated reduced form effect in Panel A is about -0.2 . Hence, the estimated IV effect is in the range 1.3-2.4. Thus outmigration is higher in local governments controlled by the landed elite. For example, 10 percentage *increase* in the current vote share of the landed elites *increase* outmigration with roughly 10-20 percent. The FE-IV approach also suggests that effect on outmigration is of similar magnitude, namely about 10%.

Table 18 present the results for per capita in-migration. All the effects are imprecisely estimated. However, all the IV estimates have the same sign and the most of the estimates are

about -1 . Thus, we therefore conclude that there is some, albeit weak, evidence that the in-migration is higher in local governments controlled by the industrialists.

Finally, Table 19 presents the results for population. The estimated reduced form effect is about -0.6 . Thus the estimated IV effect from the LD-IV approach is about 4 for the specifications including control variables. Thus, and perhaps surprisingly given the conventional wisdom (e.g., Acemoglu et al (2005), Dittmar (2011), Nunn and Qian (2011)), local governments controlled by landowners have higher population growth than those controlled by industrialists. The estimated effect is also large since 10 percentage *increase* in the current vote share of the landed elites *increase* the population with roughly 40 percent. The results from the FE-IV also suggests an effect of similar magnitude, namely 30 percent. Nonetheless, these results regarding population growth is not surprising given the previous results that mortality is much higher in areas where the industrialists has more political power and that there is no effect on fertility and in-migration.

7. Persistence in extractive economic and political institutions

In this section, we empirically test for persistence in both extractive economic and political institutions.

Acemoglu and Robinson (2008, 2012) have argued that repressive economic institutions might persist despite a significant change in political institutions. Thus, we therefore test whether extractive economic institution persisted at the local level even after the weighted voting system was abolished and Sweden introduced universal suffrage in 1919. To construct such a test we rely on a survey conducted by a Swedish Agency (Socialstyrelsen). The survey were sent to all local governments in October 1924 and asked what type of labor market contracts that was being used. Specifically, the survey asked whether the traditional (feudal) labor market contracts based on labor coercion, i.e., the Master and Servant Act discussed previously, were still used in the agriculture sector. It is noteworthy that the Master and Servant Act was abolished in 1926 but that the Farmers Party voted against this decision in Parliament (Eklund 1974, p. 236). However, despite the abolishment of the Master and Servant Act, Sweden still kept a labor repressive contract-work system and corvée duties until the mid-1940s.⁵²

We have digitized the information in the survey. The Agency received information from 1,708 out of 2,345 local governments. We use a binary coding, i.e., those local governments that predominately are using feudal labor market contracts is coded as using an extractive economic institution while the others are coded as having inclusive economic institutions. We also have information on whether the feudal contracts where used on small and large farms.

To test whether there is persistence in extractive economic institutions after Sweden was democratized we can use the same type of empirical design as in section 5. Specifically we can use equation 7, the reduced form in the LD-IV approach. Thus, the empirical design is therefore based on a long differences-in-differences approach where all local governments have the same baseline outcome. In other words, we only regress an indicator variable for having an extractive economic institution in 1924 on our instrument, the initial change in the vote share of the landed elite since all local governments had exactly the same labor repressive economic institutions before the suffrage reform in 1862.

Table 20 shows the results from this regression. Panel A displays the results from the survey based on large farms while Panel B shows the results from small farms. In Column 1 we present the result for the restricted sample while Column 2 shows the results for the full

⁵² A contract-worker was a married farmhand who was employed on a yearly basis and received a wage consisting of money, food and housing. See Lund and Olsson (2011) for a description of this system

sample variables. In Panel A, the estimated effect varies between 0.56 and 0.66. Thus, a 0.1 increase in the initial vote share of the landed elites will increase the probability of having an extractive economic institution after democratization with about 6 percentage points. Panel B shows the corresponding results for small farms. The estimated effects are now about half the size of the previous ones, suggesting that larger firms are using labor repressive contracts to a larger extent than smaller ones.

We can also test for whether extractive political institutions also persist in Sweden after democratization in 1919. This test builds on the results from Hinnerich and Pettersson-Lidbom (2014, 2017) which compare the development outcomes between two types of democracies at the local level. After 1919, local government could choose to keep their old type of town meeting form of government or switch to a representative form of government. However, if the population was above 1,500 the local government was required to have representative democracy. Hinnerich and Pettersson-Lidbom exploit this population threshold by using a regression-discontinuity design to test whether these two types of democracies have different policies and development outcomes. They find that representative democracy spend more on social welfare than direct democracy (Hinnerich and Pettersson-Lidbom (2014)). In Hinnerich and Pettersson-Lidbom (2017), they also find that the representative system is much more industrialized and has higher agriculture productivity. Consistent with the finding of this paper that industrialization is bad for health, they also find that representative democracy has higher deaths rates among adults which is mostly due to higher incidence of Tuberculosis (TB)). They interpret these results as evidence for that the local landowning elites can capture the political process in town meetings and therefore block industrialization and economic development. Thus, this suggests that direct democracy is an extractive political institution as compared to the representative system. Consequently, we would expect that a local government that was previously politically controlled by the landed elites should stick to the town meeting form of government after 1919. Hence, we can again use equation (7) to test whether there is persistence in extractive political institutions where the outcome is now defined as having the direct democracy after 1919. Obviously, for this test to make sense we need to restrict the data to those local government that could choose their own type of democracy, i.e., those local governments with a population size below 1,500. We perform this test for two different years: 1924 and 1938. We choose 1924 since the labor market survey was conducted this year and 1938 since this is the final year before population threshold was changed from 1,500 to 700. Table 21 presents these results. Panel A display the results for 1924 while Panel B shows the result for 1938. Again, we present the same two

specifications as in Table 20. All the results in Table 20 suggest that repressive political institutions persist since they are all positive and statistically significant. The estimated reduced form effect in in Panel A is about 0.15. Thus, a 0.1 higher initial vote share of the landed elites imply that the probability of having a repressive political institution increase with 1.5 percentage points in 1924. The estimated effects are of twice as large in 1938 (Panel B). Thus, there is considerably degree of persistence in extractive political institutions over time.

8. Conclusion

This paper provides a novel explanation of Sweden's long-term economic development based on a political economics framework developed by Acemoglu and Robinson (2000, 2006, 2008, 2012).⁵³ The point of departure is that the Sweden was a feudal society in the beginning of the 19th century and were landed elites controlled all political power. Thus, their argument is that landed elites should oppose industrialization and economic growth due to the fear of losing their future political power. We test this hypothesis by exploiting a suffrage reform in 1862 which extended the voting rights to industrialists at the local government level. Consistent with the political economics framework, we find that landed elites blocked technological change and economic development in those local governments where they still could retain their political control after the suffrage reform.⁵⁴ We also find that there was persistence in both extractive economic and political institutions in those local governments previously politically controlled by landed elites even after Sweden was democratized and introduced universal suffrage in 1919.

Thus, based on the results of this paper and our other work (Hinnerich and Pettersson-Lidbom (2014, 2017)), we therefore argue that changes in local political institutions played a key role in the Swedish growth miracle, i.e., transforming Sweden from one of the poorest countries in Europe in the mid-19th century to one of the richest countries in the world in the 1960s.

⁵³ A complementary way of thinking about Sweden's long-term economic development is provided by an economic growth framework developed by Rodrik (2013). His framework combines the dual economy approach (e.g., Lewis (1954)) with the standard neoclassical growth model. We can therefore think of the Swedish economic development as consisting of two distinct phases where the first phase is best explained by the dual economy approach and the second phase is more consistent with neoclassical model. The first phase starts in 1862 when industrialists gained political power at the local level and the second phase starts in 1919 when workers gained political power at the local level.

⁵⁴ Here it is important to point out that the early Swedish industrialization took place in the rural areas.

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Figure 1. Distribution of the vote share of landed elites

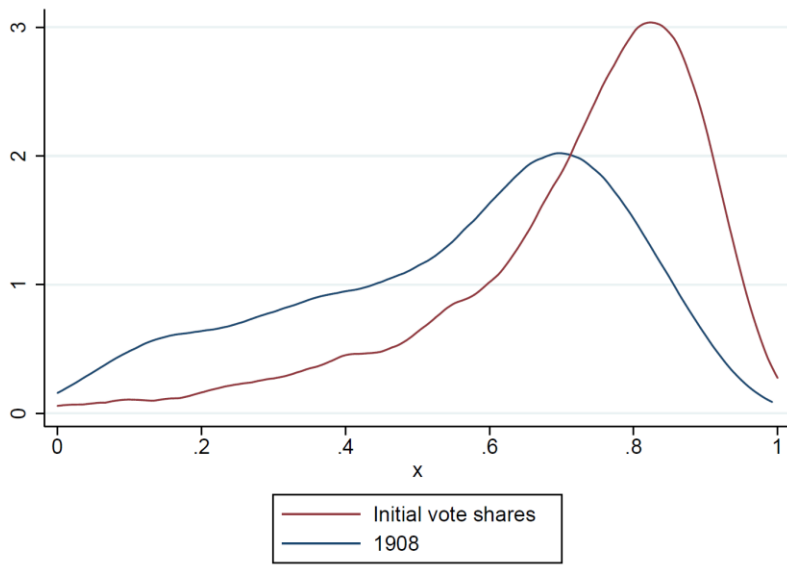


Figure 2. Average number of votes per local government for landowners and industrialists

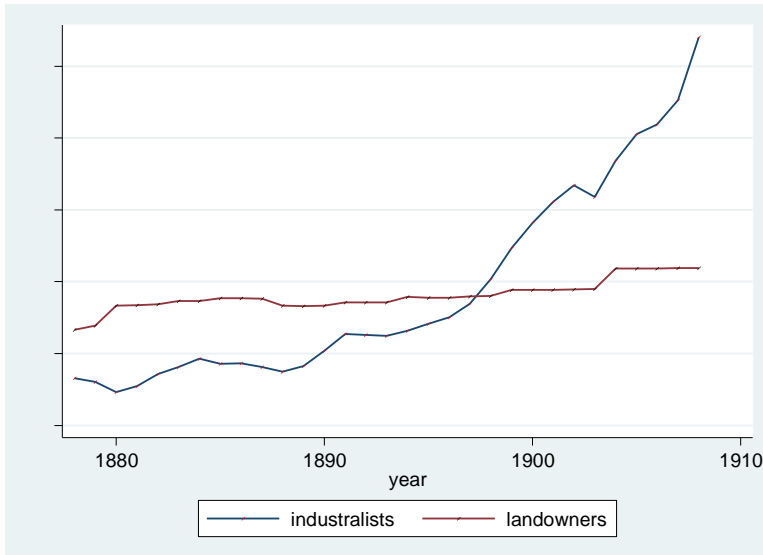


Figure 3. First stage estimates from the FE-IV approach

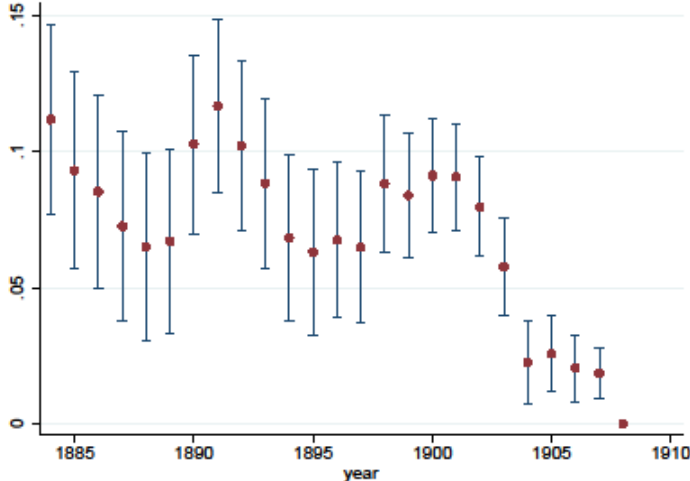


Figure 4. Test of parallel trends for the period 1839-1859: log mortality rate



Figure 5. Test of parallel trends for the period 1839-1859: log infant mortality rate

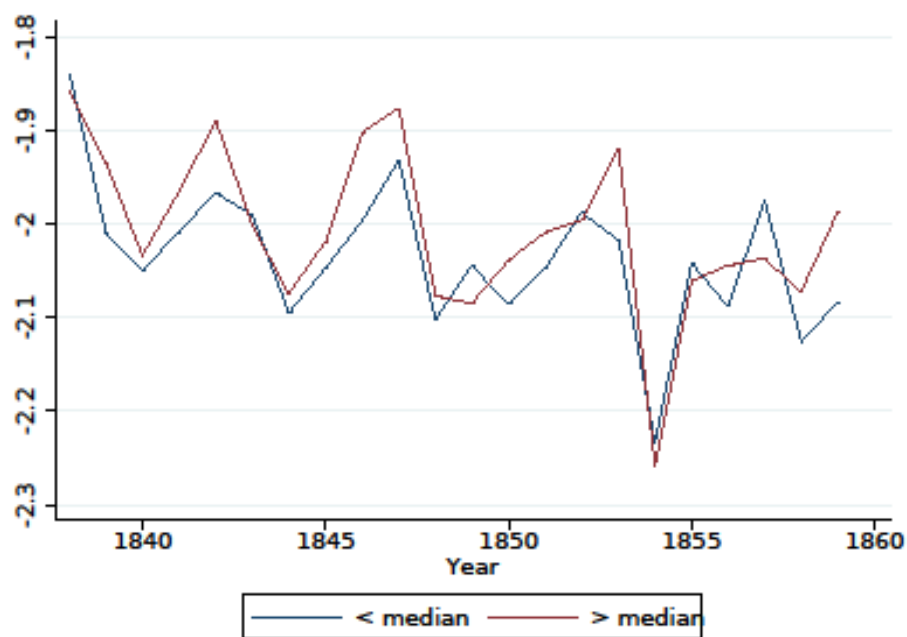


Figure 6. Test of parallel trends for the period 1839-1859: log fertility rate

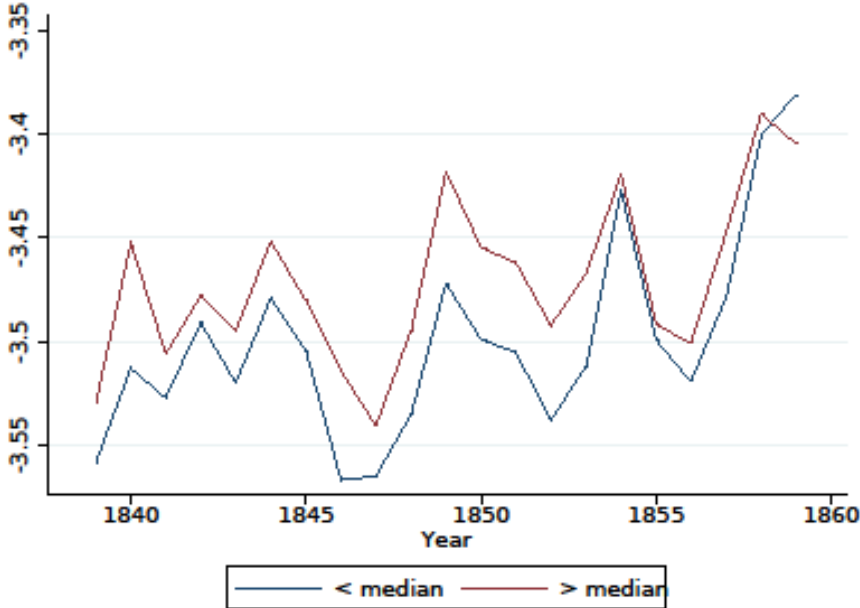


Figure 7. Test of parallel trends for the period 1839-1859: log population

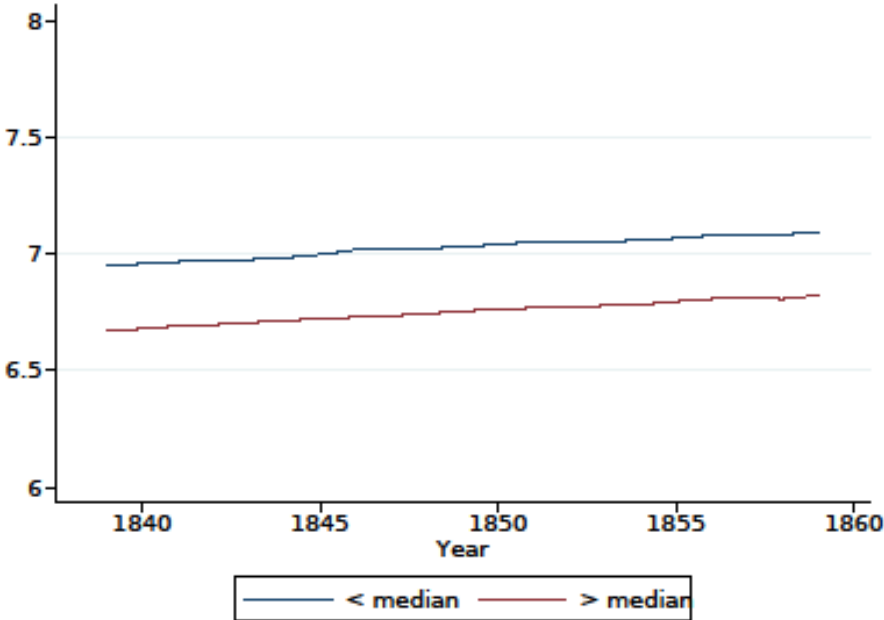


Figure 8. Length of the railway network

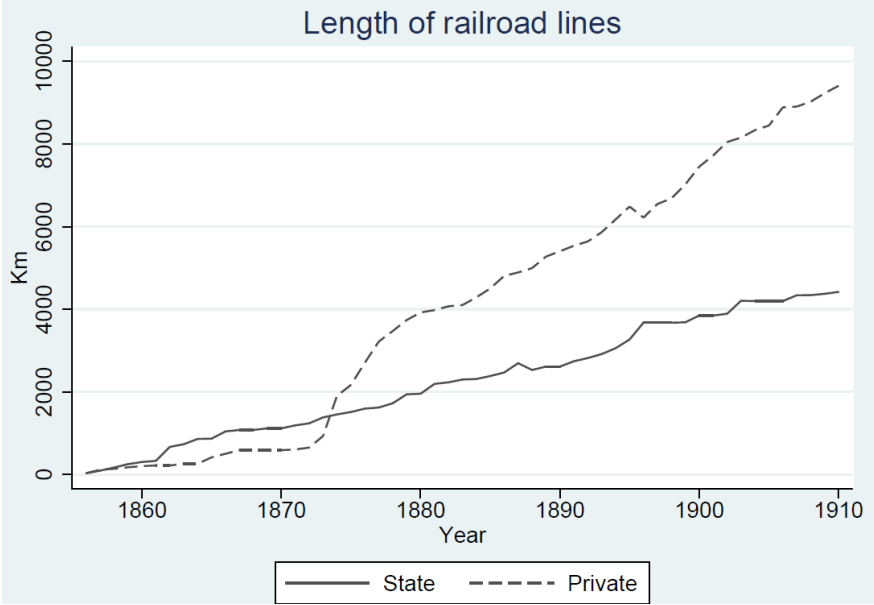


Figure 9. The Swedish railway network in 1910



Figure 10. Number of local government with a local railway line 1880-1908

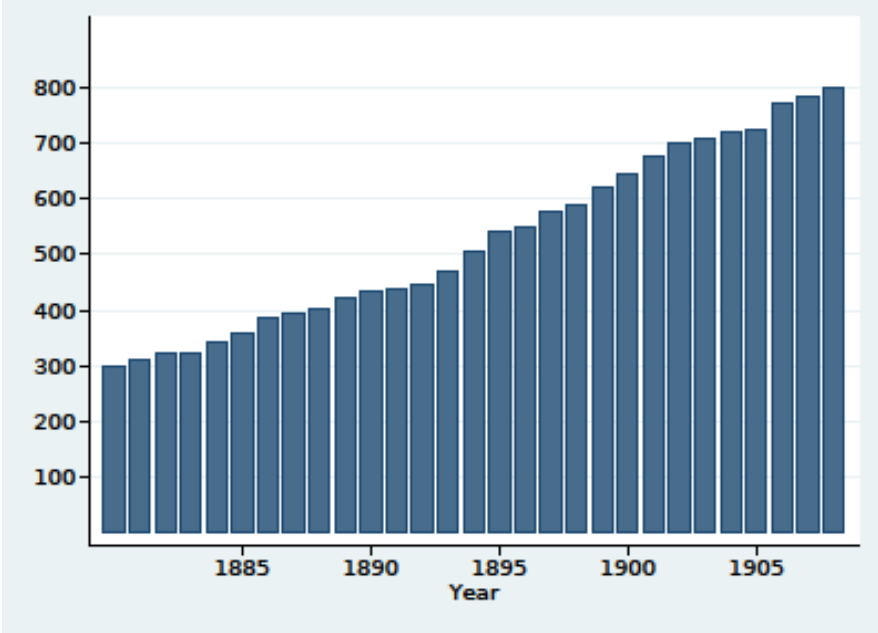


Figure 11. Number of local governments with a railway loan

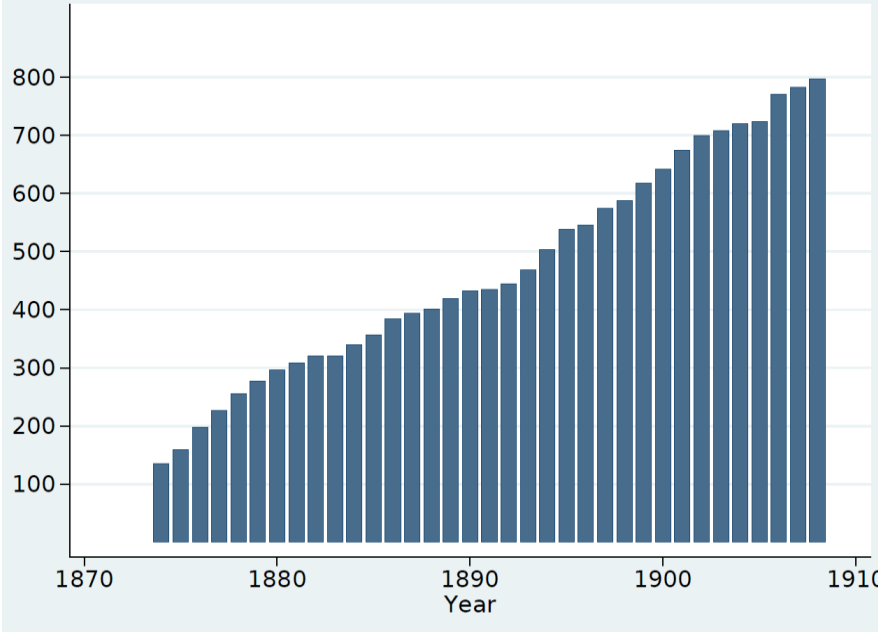


Table 1. Summary statistics for the restricted sample for the period 1881-1908.

Variables	Mean	St. Dev.	Min	Max	Obs.
<u>Panel A. Instrumental variable</u>					
Initial vote share of landed elites	0.72	0.18	0	0.99	1,629
<u>Panel B. Explanatory variable</u>					
Present vote share of landed elites	0.67	0.21	0	1	40,720
<u>Panel C. Outcome variables</u>					
Railways lines	0.23	0.42	0	1	40,725
Railway loans	0.17	0.37	0	1	40,725
Total spending per capita	6.98	4.74	1.34	167	40,451
Education spending per child	16.1	17.6	0.02	613	40,458
Welfare spending per capita	1.50	1.60	0	98	40,717
Church spending per capita	2.21	2.51	0	132	40,522
Share of people working in the agriculture sector 1910	0.64	0.16	0.02	0.96	1,615
Share of people working in the manufacturing sector 1910	0.12	0.10	0	0.65	1,615
Output per worker: manufacturing firms 1914	7,066	11,516	207	156,088	887
Output per working days: manufacturing firms 1914	25	36	1	621	851
Output per area: agriculture	513	1876	0	65,875	1613
Output per worker: agriculture	4024	3238	0	22,834	1613
Mortality rate	0.0158	0.0050	0	0.0679	40,525
Infant mortality rate	0.0864	0.0729	0	1	40,394
Fertility rate	0.049	0.0134	0	0.32	40,078
Population size	1537	1280	102	14,454	40,721
In-migration rate	0.0632	0.0448	0	0.46	40,525
Out-migration rate	0.0737	0.0434	0	0.55	40,525
<u>Panel D. Pretreatment control variables determined before 1884</u>					
Share of eligible voters	0.10	0.04	0.02	0.26	1,629
Total votes	8,356	7,807	582	73,052	1,629
Inequality in the vote share distribution (Gini coefficient)	0.72	0.08	0.41	0.94	1,629
Population size	1,548	1,187	124	11,538	1,629
Share of people below 15 in 1880	0.32	0.03	0.19	0.44	1,629
Labor force participation rate in 1880	0.28	0.04	0.15	0.45	1,629
Share of people working in the agriculture sector in 1880	0.67	0.11	0.15	0.93	1,629
Share of people working in the manufacturing sector in 1880	0.05	0.04	0	0.46	1,629

Notes: All nominal values are in SEK and deflated

Table 2. Results from the first-stage of the LD-IV approach

	Change in vote shares: 1881-1908 (1)	Change in vote shares: 1881-1908 (2)	Change in vote shares: 1881-1908 (3)
Initial vote share of landed elites	-0.09*** (0.02)	-0.13*** (0.02)	-0.15*** (0.02)
Controls	No	Yes	Yes
First stage F statistics	26	33	56
Observations	1,624	1,624	2,218

Notes: Each entry is a separate regression. This table shows first-stage results from the LD-IV approach. Column 1 shows the results from the restricted sample of 1629 local governments without any pretreatment control variables. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample of 2,224 local governments including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 3. Testing whether the instrument, initial vote share of landed elites, is correlated with outcomes determined before the suffrage reform

	Mortality rate		Infant mortality rate		Fertility rate		Population	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Long-difference regression								
Initial vote share of landed elites	-0.02 (0.08)	-0.12 (0.10)	0.03 (0.13)	0.03 (0.17)	-0.03 (0.06)	-0.03 (0.07)	-0.06*** (0.02)	0.02 (0.03)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	1,359	1,359	1,078	1,078	1,360	1,360	1,361	1,361
Panel B. Differences-in-differences regression								
Initial vote share of landed elites	0.00 (0.00)	-0.00 (0.01)	0.01 (0.01)	0.02** (0.01)	0.00 (0.00)	0.00 (0.00)	-0.00*** (0.00)	0.00 (0.00)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	28,313	28,313	21,705	21,705	28,498	28,498	28,714	28,714

Notes: Each entry is a separate regression. All the outcome variables are expressed in logarithmic form. Panel A shows the results from a long-difference specification from 1839 to 1859, while Panel B shows the results from a first-difference specification for the period 1839-59. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 4. Results for investments in railways: whether a local government has built a railway line

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	0.12** (0.05)	0.22*** (0.06)	0.11** (0.05)
Controls	No	Yes	Yes
Observations	1,624	1,624	2,218
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	-1.41** (0.59)	-1.66*** (0.48)	-0.73** (0.34)
First-stage F-statistics	25	33	55
Controls	No	Yes	Yes
Observations	1,624	1,624	2,218
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	-0.85** (0.42)	-1.45*** (0.59)	-0.77** (0.38)
Controls	No	Yes	Yes
First-stage F-statistics	10.3	7.4	9.9
Observations	40,595	40,595	55,420

Notes: Each entry is a separate regression. The dependent variable is a dummy variable for whether a local government has a local railway line within its geographical boundary. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 5. Results for investments in railways: whether a local government has taken a long-term railway loan

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	0.19*** (0.05)	0.32*** (0.06)	0.18*** (0.05)
Controls	No	Yes	Yes
Observations	1,624	1,624	2,218
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	-2.19*** (0.70)	-2.46*** (0.61)	-1.19*** (0.40)
First-stage F-statistics	26	33	56
Controls	No	Yes	Yes
Observations	1,624	1,624	2,218
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	-0.88** (0.40)	-2.00*** (0.57)	-1.02*** (0.40)
Controls	No	Yes	Yes
First-stage F-statistics	10.3	7.4	9.9
Observations	40,595	40,595	55,420

Notes: Each entry is a separate regression. The dependent variable is a dummy variable for whether a local government has a long term railway loan. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 6. Results for spending on primary education.

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	-0.81*** (0.08)	-0.65*** (0.09)	-0.78*** (0.08)
Controls	No	Yes	Yes
Observations	1,609	1,609	2,023
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	9.64*** (2.50)	5.15*** (1.40)	5.46*** (1.15)
Controls	No	Yes	Yes
First-stage F-statistics	23	28	44
Observations	1,609	1,609	2,023
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	5.64*** (1.17)	4.45*** (1.22)	4.44*** (0.97)
Controls	No	Yes	Yes
First-stage F-statistics	10.4	7.4	10.1
Observations	40,330	40,330	51,211

Notes: Each entry is a separate regression. The dependent variable is per child (7-14) spending on primary education in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 7. Results for spending on social welfare

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	-0.54*** (0.09)	-0.47*** (0.12)	-0.45*** (0.10)
Controls	No	Yes	Yes
Observations	1,620	1,620	2,211
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	6.27*** (1.67)	3.65*** (1.10)	3.03*** (0.76)
Controls	No	Yes	Yes
First-stage F-statistics	25	32	54
Observations	1,620	1,620	2,211
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	2.67*** (0.82)	2.21** (0.94)	2.01*** (0.71)
Controls	No	Yes	Yes
First-stage F-statistics	10.4	7.5	9.9
Observations	40,483	40,483	55,240

Notes: Notes: Each entry is a separate regression. The dependent variable is per capita spending on social welfare in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 8. Results for church spending

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	-0.02 (0.07)	-0.02 (0.10)	0.01 (0.09)
Controls	No	Yes	Yes
Observations	1,615	1,615	2,085
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	0.20 (0.82)	0.13 (0.75)	-0.5 (0.60)
Controls	No	Yes	Yes
First-stage F-statistics	23	30	49
Observations	1,615	1,615	2,085
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	0.17 (0.54)	0.67 (0.61)	0.32 (0.49)
Controls	No	Yes	Yes
First-stage F-statistics	10.3	7.4	10.1
Observations	40,394	40,394	52,321

Notes: Each entry is a separate regression. The dependent variable is per capita church spending in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 9. Results for total spending

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	-0.19*** (0.05)	-0.05 (0.06)	-0.07 (0.05)
Controls	No	Yes	Yes
Observations	1,608	1,608	2,010
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	2.24*** (0.76)	0.39 (0.45)	0.48 (0.36)
Controls	No	Yes	Yes
First-stage F-statistics	23	31	52
Observations	1,608	1,608	2,010
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	1.59*** (0.46)	0.95** (0.45)	0.79** (0.35)
Controls	No	Yes	Yes
First-stage F statistics	10.4	7.4	10.0
Observations	40,323	40,323	50,992

Notes: Each entry is a separate regression. The dependent variable is per capita total spending in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 10. Results for structural change

	Employment shares in the manufacturing sector		Employment shares in the agricultural sector	
	(1)	(2)	(3)	(4)
Panel A. First-stage estimates				
Initial vote share of landed elites	0.90*** (0.02)	0.89*** (0.02)	0.90*** (0.02)	0.89*** (0.02)
Panel B: Reduced form estimates				
Current vote share of landed elites	-1.57*** (0.11)	-1.54*** (0.09)	0.90*** (0.07)	0.97*** (0.06)
Panel C. Instrumental variable estimates				
Current vote share of landed elites	-1.75*** (0.12)	-1.72*** (0.10)	1.00*** (0.07)	1.07*** (0.06)
Observations	1,612	2,233	1,614	2,243

Notes: Each entry is a separate regression. The dependent variable is expressed in logarithmic form. Columns 1 and 3 show the results from the restricted sample while Column 2 and 4 display the results for the full sample. Robust standard errors are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 11. Productivity in the manufacturing sector

	Output per worker		Output per working days	
	(1)	(2)	(3)	(4)
Panel A. First-stage estimates				
Initial vote share of landed elites	0.80*** (0.02)	0.78*** (0.02)	0.79*** (0.02)	0.78*** (0.02)
Panel B: Reduced form estimates				
Current vote share of landed elites	-0.28* (0.15)	-0.27** (0.13)	-0.35*** (0.13)	-0.38*** (0.10)
Panel C. Instrumental variable estimates				
Current vote share of landed elites	-0.35* (0.19)	-0.34** (0.16)	-0.45*** (0.16)	-0.49*** (0.13)
Observations	884	1,195	848	1,147

Notes: Each entry is a separate regression. The dependent variable is expressed in logarithmic form. Columns 1 and 3 show the results from the restricted sample while Column 2 and 4 display the results for the full sample. Robust standard errors are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 12. Productivity in the agricultural sector

	Output per area unit		Output per worker	
	(1)	(2)	(3)	(4)
Panel A. First-stage estimates				
Initial vote share of landed elites	0.90*** (0.02)	0.90*** (0.02)	0.90*** (0.02)	0.88*** (0.02)
Panel B: Reduced form estimates				
Current vote share of landed elites	-0.09 (0.17)	0.03 (0.14)	1.11*** (0.12)	1.41*** (0.10)
Panel C. Instrumental variable estimates				
Current vote share of landed elites	-0.11 (0.18)	0.07 (0.15)	1.24*** (0.13)	1.58*** (0.12)
Observations	1,608	2,278	1,599	2,218

Notes: Each entry is a separate regression. The dependent variable is expressed in logarithmic form. Columns 1 and 3 show the results from the restricted sample while Column 2 and 4 display the results for the full sample. Robust standard errors are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 13. Factors related to agricultural development

	Real wage		Number of working hours per day		Labor demand		Number of horses per area	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. First-stage estimates								
Initial vote share of landed elites	0.91*** (0.02)	0.90*** (0.02)	0.91*** (0.02)	0.90*** (0.02)	0.91*** (0.02)	0.90*** (0.02)	0.92*** (0.02)	0.93*** (0.02)
Panel B: Reduced form estimates								
Current vote share of landed elites	-0.19*** (0.03)	-0.20*** (0.02)	0.06*** (0.01)	0.05*** (0.01)	0.00 (0.07)	0.07 (0.06)	-0.34*** (0.06)	-0.45*** (0.07)
Panel C. Instrumental variable estimates								
Current vote share of landed elites	-0.21*** (0.03)	-0.23*** (0.03)	0.07*** (0.02)	0.06*** (0.01)	0.00 (0.08)	0.08 (0.07)	-0.37*** (0.07)	-0.48*** (0.07)
Observations	1,343	1,877	1,491	2,099	1,469	2,073	1,545	2,164

Notes: Each entry is a separate regression. The dependent variable is expressed in logarithmic form. Columns 1, 3, 5 and 7 show the results from the restricted sample while Column 2, 4, 6 and 8 display the results for the full sample. Robust standard errors are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 14. Results for mortality

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	0.22*** (0.04)	0.25*** (0.05)	0.29*** (0.04)
Controls	No	Yes	Yes
Observations	1,621	1,621	2,209
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	-2.64*** (0.74)	-1.91*** (0.52)	-1.92*** (0.39)
Controls	No	Yes	Yes
First-stage F-statistics	24	31	51
Observations	1,621	1,621	2,209
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	-1.67*** (0.42)	-1.24*** (0.43)	-1.53*** (0.37)
Controls	No	Yes	Yes
First-stage F-statistics	10.1	7.4	9.9
Observations	40,337	40,337	54,929

Notes: Each entry is a separate regression. The dependent variable is per capita mortality in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 15. Results for infant mortality

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	0.40*** (0.06)	0.24*** (0.09)	0.36*** (0.07)
Controls	No	Yes	Yes
Observations	1,556	1,556	2,088
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	-4.45*** (1.18)	-1.93*** (0.80)	-2.51*** (0.65)
Controls	No	Yes	Yes
First-stage F-statistics	26	27	46
Observations	1,556	1,556	2,088
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	-2.86*** (0.61)	-1.68*** (0.63)	-2.11*** (0.52)
Controls	No	Yes	Yes
First-stage F-statistics	10.0	6.4	8.3
Observations	33,114	33,114	44,346

Notes: Each entry is a separate regression. The dependent variable is infant mortality in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 16. Results for fertility

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	-0.03 (0.03)	-0.04 (0.04)	-0.05 (0.04)
Controls	No	Yes	Yes
Observations	1,621	1,621	2,209
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	0.39 (0.42)	0.28 (0.34)	0.36 (0.26)
Controls	No	Yes	Yes
First-stage F-statistics	24	31	54
Observations	1,621	1,621	2,209
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	-0.43 (0.31)	-0.67* (0.35)	-0.46* (0.27)
Controls	No	Yes	Yes
First-stage F-statistics	9.6	7.3	9.8
Observations	39,945	39,945	54,446

Notes: Each entry is a separate regression. The dependent variable is per capita fertility in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 17. Results for out-migration

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	-0.20*** (0.04)	-0.21*** (0.06)	-0.19*** (0.05)
Controls	No	Yes	Yes
Observations	1,621	1,621	2,209
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	2.40*** (0.71)	1.62*** (0.52)	1.29*** (0.35)
Controls	No	Yes	Yes
First-stage F-statistics	24	31	54
Observations	1,621	1,621	2,209
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	1.04*** (0.39)	0.97** (0.45)	0.95*** (0.35)
Controls	No	Yes	Yes
First-stage F-statistics	10.1	7.4	9.9
Observations	40,390	40,390	55,036

Notes: Each entry is a separate regression. The dependent variable is per capita out-migration in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 18. Results for in-migration

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	0.09 (0.06)	0.12* (0.07)	0.12* (0.06)
Controls	No	Yes	Yes
Observations	1,621	1,621	2,209
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	-1.02 (0.70)	-0.96 (0.59)	-0.77* (0.42)
Controls	No	Yes	Yes
First-stage F-statistics	24	31	54
Observations	1,621	1,621	2,209
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	-0.83* (0.48)	-1.01* (0.61)	-1.08** (0.45)
Controls	No	Yes	Yes
First-stage F-statistics	10.2	7.4	9.9
Observations	40,382	40,382	55,023

Notes: Each entry is a separate regression. The dependent variable is per capita in-migration in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 19. Results for population

	(1)	(2)	(3)
Panel A. Reduced form estimates from the LD-IV approach			
Initial vote share of landed elites	-0.58*** (0.04)	-0.56*** (0.04)	-0.56*** (0.04)
Controls	No	Yes	Yes
Observations	1,624	1,624	2,218
Panel B: Results from the LD-IV approach			
Current vote share of landed elites	6.74*** (1.56)	4.26*** (0.93)	3.79*** (0.67)
Controls	No	Yes	Yes
First-stage F-statistics	26	33	56
Observations	1,624	1,624	2,218
Panel C. Results from the FE-IV approach			
Current vote share of landed elites	3.57*** (0.66)	3.08*** (0.70)	2.92*** (0.56)
Controls	No	Yes	Yes
First-stage F-statistics	10.4	7.5	10.0
Observations	40,591	40,591	55,416

Notes: Each entry is a separate regression. The dependent variable is population size in logarithmic form. Column 1 shows the results from the restricted sample. Column 2 displays the results for the restricted sample with control variables. Column 3 presents the results from the full sample including control variables. The pretreatment control variables are: the share of eligible voters, the total number of votes, the inequality in the vote share distribution, population size, the share of people working in the agricultural sector, the share of people working in the manufacturing sector, the share of people below 15 and the share of people in the working force. Standard errors, clustered at the local government level are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 20. Testing for persistence in extractive economic institutions

	(1)	(2)
Panel A. Labor repressive contracts at large farms in 1924		
Initial vote share of landed elites	0.66*** (0.09)	0.56*** (0.07)
Observations	861	1,223
Panel B: Labor repressive contracts at small farms in 1924		
Current votes share of landed elites	0.28*** (0.09)	0.30*** (0.07)
Observations	1,103	1,571

Notes: Robust standard errors are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 21. Testing for persistence in extractive political institutions

	(1)	(2)
Panel A. Town meetings for of government in 1924		
Initial vote share of landed elites	0.17*** (0.07)	0.15*** (0.05)
Observations	1,016	1,469
Panel B: Town meetings form of government in 1938		
Current votes share of landed elites	0.26*** (0.08)	0.29*** (0.07)
Observations	1,016	1,442

Notes: Robust standard errors are within parentheses. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.