

# **Democracy, Redistribution, and Political Participation:**

## **Evidence from Sweden 1919-1950\***

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### **Abstract**

In this paper we empirically analyze how different forms of democracy affect redistributive spending programs, the size of government and political participation. Specifically, we compare representative democracy with direct democracy, i.e., town-meeting, using a very large data set on Swedish local government for the period 1919-1950. Due to the Swedish Local Government Act, we can implement two different design-based strategies: a regression-discontinuity design and a nonparametric instrumental variables approach. Our results indicate that going from direct democracy to a representative system dramatically increases political participation, redistributive spending, and the size of government. The estimated effects on public spending to the poor (poverty relief, child welfare and basic public education) are on the order of 35-70 percent while the effect on political participation is between 150-200 percent. We argue that these results most likely reflect that direct democracy is more prone to capture by (rich) local elites than representative democracy. We present further evidence that supports the theoretical framework developed by Acemoglu and Robinson (2006, 2008) based on the persistence of de facto political power.

Key words: income redistribution, forms of government, representative democracy, direct democracy, town meeting, elite capture, democratization, political participation, treatment effects, regression-discontinuity design, nonparametric instrumental variable design

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# 1. Introduction

There has been a recent interest in studying how (forms of) democracy and democratization shape economic and political outcomes in economics (e.g., Acemoglu and Robinson (2006), Persson (2005), Persson and Tabellini (2006)). Although that there are good theoretical reasons to expect that democracy and democratization would involve some income redistribution towards the relatively poor, there are few credible empirical studies that show that these political institutions have a *causal* impact on economic and policy outcomes (e.g., Acemoglu (2005), Robinson (2010)).

In this paper, we are making considerable progress on this important matter since we will compare redistributive policies and political participation in two distinct forms of democracy: representative democracy versus direct democracy.<sup>1</sup> To this end, we have constructed a comprehensive new data set with *yearly* observations on a large set of fiscal policies (e.g., poverty relief, child welfare, basic public education, public health, and public pensions) and political and economic characteristics for about 2,500 local governments in Sweden for the period 1919-1950. Most importantly, we are exploiting arguably exogenous sources of variation in forms of democracy at the local government level. In 1919, when universal and equal suffrage was introduced at the local level,<sup>2</sup> local governments with a population size above 1,500 were required by statutory law to have representative democracy while those below could choose either to keep their traditional form of government, i.e., town meetings, or switch to representative democracy. In 1938, the compulsory population threshold for a representative form of government was lowered to 700.

Thanks to the Swedish Local Government Act, it is thus possible to implement two regression-discontinuity (RD) designs: one before 1939 at the 1500 threshold and another after 1938 at the 700 threshold. The attractiveness of the RD design is that it can generate unusually credible causal estimates under very weak identification assumptions (e.g., Hahn et al. 2001 and Lee and Lemieux 2010). Moreover, in spite of the fact that our RD designs are fuzzy, they still share the same identification assumptions as in the sharp RD design since

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<sup>1</sup> Direct democracy is an umbrella term that covers a variety of political processes, all of which allow ordinary citizens to vote directly on laws rather than candidates for office (e.g., Matsusaka 2005). In this paper, we analyze the purest form of direct democracy, i.e., town meetings. However, many countries allow for other forms of political processes that provide limited direct democracy: e.g., initiative, referendum (plebiscite), and recall.

<sup>2</sup> At the local government level voting rights were graded according to taxable income until 1919. At the central government level, full male suffrage for those aged 25 and above was introduced in 1911 while for women it was implemented in 1921.

there is an eligibility rule that divides the population into eligibles and non-eligibles (see Battistin and Rettore (2008)). Thus, the estimates from our RD analyses correspond to an average treatment of the treated effect rather than a local average treatment effect LATE (Imbens and Angrist (1994)).

In addition to the RD analyzes, we also implement nonparametric instrumental variable (IV) approaches (Abadie (2003), Angrist and Imbens (1995)) which makes it possible to estimate the effects of forms of government away from the discontinuity points. In the nonparametric IV designs, we exploit the fact that the Swedish Local Government Act contained a set of rules governing the decision process of a switch from a representative system to a town meeting form of government. These rules made it difficult, or impossible, for a local government to switch from a representative system to a town meeting even if their population size were below the mandatory population threshold. The Swedish Parliament intentionally constructed these set of rules since they wanted to create a “status quo bias” for maintaining a representative system in a local government once such a system was in place. As a result, we can exploit these rules to construct instrumental variables estimates for local governments below the treatment threshold.<sup>3</sup> Consequently, we can estimate the effects of different forms of democracy across a larger set of local governments than in our RD approaches which should bolster external validity. In addition, a set of novel specification tests can now be constructed with the help of the nonparametric IV designs.

Results from both the RD designs and the non-parametric IV approaches clearly indicate that local governments with a representative system spend about 40-70 percent more on items target to the poor, i.e., basic public education, poverty relief and child welfare. Results also indicate that child welfare and poverty relief were the two individual spending programs which were the most affected. We also find that political participation increased by some 150-200 percent in the representative system.

We make a large number of validity checks of our RD designs. There is no evidence of sorting of local governments around the thresholds. We test whether local governments on either side of the cut-off are observationally similar by comparing baseline characteristics and find no evidence of a discontinuity in these baseline characteristics. We also conduct a number of falsification exercises, for example, by estimating RD designs when population

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<sup>3</sup> The instrumental variable can also be theoretically motivated from the Acemoglu and Robinson (2006, 2008) framework. As discussed below, citizens are likely to possess more overall political power than the elite in representative democracy than in direct democracy because citizens are able to solve their collective action problem in representative democracy. Thus, theory suggests that there should be considerably inertia in representative democracy once such a system is in place.

rule was not in place. Once more, we find no evidence of treatment effects suggesting that the RD designs are valid. Finally, our RD designs and IV approaches yield similar results, which lend credibility to both internal and external validity.

How do we explain our findings that redistributive spending that mainly benefits the poor is significantly larger in representative democracy than in direct democracy? We argue that the most likely explanation is that direct democracy is more prone to capture by (rich) local elites than representative democracy.<sup>4</sup> In direct democracy, local elites are able to deter the entry of political parties, in particular pro-poor left-wing parties. Moreover, poor people are often relatively isolated and less organized collectively. These reasons thus make it very hard for the voices of the poor to be heard (Robinson 2010). In addition, the typical absence of a secret ballot in town meetings means that the local elite can control the voters (Baland and Robinson 2008).

Some characteristics of the Swedish setting further support the notion of elite capture by local governments. At the time, a significant share of the Swedish rural population was dependent on agriculture and it is well established that agrarian economies are conducive to patron-client relationships. Specifically, and in contrast to most other European countries that had historically been feudal societies, Sweden did not implement any land reform in order to establish a more equitable distribution of farmland (e.g., Dovring 1965). Sweden had also a labor repressive agricultural system in the form of corvée obligations (“torparsystemet”) and a system with contract-workers (“statarsystemet”) that were mostly paid in kind until they were legally abolished in 1945 (e.g., Eriksson and Rogers 1978, Lund and Olsson 2005).<sup>5</sup> Moreover, Sweden was basically a nondemocracy until 1919 when universal and equal suffrage was introduced. Until then, voting rights were graded according to taxable income.<sup>6</sup> Only citizens with taxable income above a certain threshold were assigned any votes and there was no upper limit on the number of votes until 1900. Thus, in some rural governments one single individual could be a dictator, i.e., with more than 50 percent of the votes.<sup>7</sup> Historical case studies clearly show that the rich local elite had the political power in Swedish

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<sup>4</sup> An additional explanation for our results is that the cost of political participation is likely to be more costly in direct democracy than in representative democracy. Osborne et al. (2000) show that in meetings with costly participation (e.g., town meeting) the equilibrium number of participants will be small. Thus, our results of low participation rates at town meetings are consistent with the prediction from their model.

<sup>5</sup> The contract of the *statare* (married farmhand) was so formulated that he was subject to the “*tjänstejohnstadga*”, a law originally regulating the relationship between the master and those young, unmarried servants living in the household, until 1926. This law gave the employer a great deal of control of the private life of *statare*.

<sup>6</sup> In addition to wages or salary, interest on capital and possession of property were also counted as income.

<sup>7</sup> For example, in 1871, there were 54 local governments where one individual had a majority of votes

local governments when the weighted voting system was in place (e.g., Wigren 1988, Tiscornia 1992, Nyström 2003 and Malmström 2006). However, it seems likely that the local elite could still capture direct democracy (relatively to representative democracy) even after the introduction of equal and universal suffrage. Sweden's transition from a nondemocracy to a democracy may therefore be consistent with the idea in Acemoglu and Robinson (2008), namely that the elite has an incentive, and perhaps the ability to capture democracy.

Acemoglu and Robinson's framework (2006, 2008) suggests that the impact of institutions on economic and policy outcomes depends on the interaction between *de jure* political power, whose allocation is determined by political institutions, and *de facto* political power, which is possessed by groups as a result of their wealth or ability to solve the collective action problem.

In 1919, Sweden introduced equal and universal suffrage at the local level which moved *de jure* power away from the elite. At the same time, two different forms of democracy, direct and representative democracy, were simultaneously implemented at the local level. Arguably, citizens are allocated more *de jure* political power in a direct democracy than in representative democracy (e.g., Manin 1997). According to the Acemoglu and Robinson framework, however, the key issue for the determination of equilibrium economic policies is how the combination of the *de jure* and *de facto* political power was affected in these different forms of government. The evidence we present is consistent with the interpretation that the local elite had control of the local political system in direct democracy since they could block the entry of political parties (specifically pro-poor parties) and that there was an open vote. In sharp contrast, the existence of political parties in representative democracy helped the poor in solving their collective action problem and the secret ballot made it hard for the elite to control elections. Put differently, our evidence suggests that the political power of the (rich) elite persisted in direct democracy (relatively to representative democracy) after the introduction of universal and equal suffrage.

Some additional results also support an interpretation based on Acemoglu and Robinson (2006, 2008). In the RD approach we show that the redistribution to the poor were almost absent among local governments that were required to have representative democracy if their elections were *nonpartisan*. The nonpartisan elections also occurred almost exclusively in local governments whose economies were agrarian.<sup>8</sup> Also, we show that among those local governments who could choose their own form of government, the probability of having

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<sup>8</sup> For example, in 1938, 92 percent of all nonpartisan elections occurred in local governments where more than 50 percent were agriculture workers.

direct democracy was much larger for local governments with agrarian economies. Finally, in the nonparametric IV approach, we show that there is a strong first-stage relationship, i.e., once a local government has been forced to switch to representative democracy there is a large status-quo bias. These three set of results are all consistent with the hypothesis that the elite possesses more overall political power (de jure and de facto power) than the citizens in direct democracy compared to representative democracy, particularly if the economy is based on agriculture.

Our paper is related to several strands of literature. First, it is related to the voluminous literature on impact of political institutions on economic policy.<sup>9</sup> Second, our work is also related to research on comparative development as discussed above. Specifically, the change from nondemocracy to two different kinds of democracies (indirect and direct) at the local level in 1919 makes Sweden an attractive testing ground for theories about transition from nondemocracy to democracy and the persistence of de facto political power. Third, it is related to the literature on decentralization of governance and development.<sup>10</sup> This literature analyzes the function of local democracy in developing countries and to what extent local governments may be subject to capture by local elites. Our study may inform the current debate on the functioning of democracy at the local level in developing countries. Fourth, the paper is related to literature is on the determinants of the growth in the size of government or the size of redistributive spending programs.<sup>11</sup> The fifth strand of literature is on voluntary meetings with costly participation such as regulatory meetings in the U.S., school boards, and faculty meetings.<sup>12</sup> The sixth strand of related literature is on the determinants of voter participation and turnout.<sup>13</sup> Finally, the paper is related to the recent work on regression-discontinuity designs in political economics that implements RD designs.<sup>14</sup>

The rest of the paper is structured as follows. In section 2, we describe the institutional background and the data set. In section 3, we discuss RD designs and present the results. In

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<sup>9</sup> See, for example the surveys by Besley and Case (2003) and Persson and Tabellini (2003).

<sup>10</sup> See, for example, Bardhan (2002), Bardhan and Mookherje (2006), Foster and Rosenzweig (2004), Aragonès and Sánchez-Pagés (2008) and Olken (2010).

<sup>11</sup> See, for example, Meltzer and Richard (1981) and Lindert (2004).

<sup>12</sup> See, for example, Osborne et al. (2000) and Turner and Weninger (2005).

<sup>13</sup> See, for example, early work by Jackman (1987) and Powell (1986). Blais (2006) is a recent survey.

<sup>14</sup> For example, Pettersson-Lidbom (2001a, 2008) was the first study that exploited close elections to answer whether parties matter for policy choices while Lee (2008) was the first one to estimate the incumbency advantage. Pettersson-Lidbom (2001b, 2004) was the first study exploiting treatment rules based on local governments' population sizes. This literature also includes later work by, e.g., Bordignon et al. (2009), Brollo et al. (2009), Chamon et al. (2008), Ferreira and Gyourko (2009), Ferraz, and Finan (2009), Fujiwara (2008), Gagliarducci et al. (2008), Gagliarducci and Nannicini (2009), Litschig et al. (2009), Lee, Moretti and Butler (2004), and Warren, (2009).

section 4, we discuss the nonparametric IV approaches and present the results. Section 5 discusses additional evidence while section 6 concludes.

## 2. Institutional background and data

In this section, we describe the institutional background and the data set of Swedish local governments during the period of our study, i.e., 1919 to 1950. At that time, local governments were divided into three categories which originally were based on an urban-rural distinction. Urban local governments were called cities (“städer”), and semi-urban were called boroughs (“köpingar”) and rural were called rural local governments (“landskommuner”).<sup>15</sup> In 1951, there existed 133 cities, 84 boroughs and 2281 rural local governments. The first of two major boundary reforms reduced the number of local governments from 2,498 to 1,037 in 1952.<sup>16</sup> The second boundary reform completed in 1974 further reduced the number of local governments to 278. As of 2010, there exist 290 local governments.

Swedish local governments are economically important. In 1950, for example, the ratio of aggregate local government spending out of GDP in Sweden was about 10 percent, while today the ratio is more than 20 percent. Swedish local governments also have a large degree of fiscal autonomy. For example, they have independent taxation rights and decide on their own budgets. The bulk of revenues are raised through a local proportional income tax, while intergovernmental transfers constitute less than 20 percent of local revenues. During the period 1919-1950, the average local income tax rate was about 10 percent, while today the average local income tax rate is 31.5 percent.

### 2.1 Swedish Local Government Acts

The first local government acts were implemented on January 1, 1863.<sup>17</sup> There were two acts, one for the cities, and another one for the boroughs and rural local governments. Until 1918, it was voluntary for rural local governments and boroughs to have a representative form of government (“kommunfullmäktige”) while it was mandatory in cities with more than 3,000 inhabitants. However, virtually all rural local governments and boroughs did not switch to a representative system but choose to operate under the ancient town meeting (“kommunalstämma”) form of government instead. For example, in 1917 only 33 of a total of 2409 rural local governments and boroughs had voluntarily switched to a representative

<sup>15</sup> In 1935, 50 percent of the Swedish population of 6.2 million lived in rural areas.

<sup>16</sup> See, for example, Hinnerich (2009) for discussion and an analysis on the effect of Swedish boundary reforms.

<sup>17</sup> This section is based on the Swedish Code of Statutes (Svensk författningssamling, SFS). SFS 1862:13, SFS 1930:251, and SFS 1938:753.

system.<sup>18</sup> Due to changes in the Local Government Acts in 1918, all local governments with a population of more than 1,500 people were required by national law to have representative democracy, while those below this limit were allowed to keep their town meetings.

The new Local Government Act of 1918 was part of a major constitutional reform in Sweden when the Swedish parliament passed a legislation of universal suffrage, which put an end to a franchise based on economic resources. From 1919, all individuals aged above 23 were entitled to vote in the local government where they were registered. In the original government proposal of the constitutional reform package, the mandatory population threshold for having representative democracy was set to more than 3,000 inhabitants but this proposal was turned down in favor of a compromise where the threshold instead was set to 1,500. In the debate surrounding the constitutional reform package, there were two main arguments for requiring rural local governments to switch to representative democracy rather than allowing them to keep their traditional town meeting. One argument was that representative democracy would better reflect the preferences of the majority of voters and the second argument was that politicians would make more informed decisions than voters. The previous argument was related that the attendance at town meetings was very low while political participation was much higher in the general elections to parliament. Despite the strong majority in favor of the representative form of democracy in Swedish Parliament, its members still took into account the very long tradition of having direct democracy at the local level and refrained from requiring that all local governments should have a representative system. Nevertheless, there were recurrent debates in Parliament to either lower the population cut-off to 1,000 or to make it compulsory for all local governments to have a representative system. The population cut-off was eventually reduced to 700 in 1938 and in 1953 it was decided that all local governments were required to have representative democracy.<sup>19</sup>

In the subsequent empirical analysis of the relationship between forms of governments and economic and political outcomes we are going to exploit the mandatory population thresholds using RD designs. We can implement two distinct RD designs: one for the period 1919-1938 when the population threshold for having representative democracy was 1500, and another for the period 1939-1951 when the threshold was 700.

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<sup>18</sup> Of these 33 local governments, 15 were rural local governments and 18 were boroughs.

<sup>19</sup> See Strömberg (1974) and Wallin (2007) for more extensive discussions of the debate in parliament for having representative democracy rather than direct democracy at the local level.



The Swedish Local Government Acts also contained decision rules governing the process of a switch from a town meeting form of government to a representative form system and back. For local governments below the population thresholds, the status quo form of government was the town meeting. However, if a local government had switched, either voluntary or involuntary, to a representative system, then a local government could not switch back within five-year period. Moreover, such a decision always required a new decision by the electorate. Thus, the Swedish Parliament intentionally created a strong “status quo bias” for maintaining a representative system in a local government once such a system had been put in place. This status-quo bias had important consequences for those local governments that were initially above the required population thresholds in the year of the introduction of the population threshold (1918 or 1938) but subsequently happened to be below the threshold. Despite the fact that they had the opportunity to do so after the end of the mandatory five-year period, relatively few switched back from the representative form of government to the town-meeting form of government.

As further discussed below, we will exploit this “status quo bias” for having representative democracy to construct an instrumental variable based on the initial assignment of treatment status, i.e., an indicator variable for being above the population threshold required for representative democracy in the year of introduction of the treatment rule.

Finally, and quite importantly, all local governments followed the statutory law without any exceptions. We know this fact from three independent sources. First, we have precise information about a local government’s treatment status (direct or representative forms of government) from the County Administrative Board, which is a government body responsible for overseeing national laws, laid down by the Swedish parliament and government. We possess this information since Swedish local governments were required by law to report any change in their forms of government to the County Administrative Board. Second, we have *yearly* information on the population size of each local government from administrative data. Third, we have the minutes from a large set of town meeting. There are never any inconsistencies between these three independent data sources.

Table 1 reports on the number of local governments with mandatory representative democracy, voluntary representative democracy and direct democracy for each election year.

### **2.2.1 Representative democracy**

As noted above, local governments with a population size over a certain threshold were required by law to have representative democracy while the local governments below the threshold could choose to have either direct democracy (town meeting) or representative democracy. There was a mandatory election every fourth year for all the local governments with representative democracy (i.e., 1922, 1926, 1930, 1934, 1938, 1942 and 1946). However, a local government was required to have an election during off mandatory election years if the population threshold was above the threshold as of January 1. The government term in office was until the next mandatory election year. The law required that election was to be held on a Sunday in the period September 13 to October 20 (SFS 1930:253). Elections were based on proportional representation with a closed party lists system in multi-member constituencies. Five traditional parties dominated the political arena: two left-wing parties (The Communists and The Social Democrats) and three right-wing parties (The Agrarian Party, The Liberal Party, and the Conservative Party). However, there were also a fairly large number of elections that were characterized by Statistics Sweden as nonpartisan, i.e., elections with a single non-political list of candidates or elections with two or more non-political lists. For example, in the 1938 election, 21 percent of all 1670 local government elections were nonpartisan, and 68 percent of these were elections with a single non-political list. These nonpartisan elections almost exclusively occurred in areas with large agrarian populations: out of the total of 345 nonpartisan local government elections, 316 occurred in local governments where agriculture workers constituted more than 50 percent of the population. As will be discussed below, we will exploit the difference between partisan and nonpartisan elections in order to test more refined hypothesis about the driving forces behind our results.

The local elected council was required to have at least three meetings. The first was to be held between March 16 and April 30 and should deal with the local government accounts from the previous year. The budget should be decided at the second meeting, which was to be held between October 1 and November 15, while the appointment of officials took place at a third mandatory meeting in December. The law required that a majority of the council members must be present at the council meetings to constitute a quorum. The national law also required that many economic decisions in the council had to be taken with supermajority. The chairman of the council and the vice-chairman were elected on a yearly basis. The executive agency of the local government (“kommunalnämnden”) was required to have 5 to 11 members and was elected by the council.

The number of council members ranged from 15 to 40 depending on the population size. Table 2 shows the number of required representatives in the local government council while Table 3 shows the average turnout rate for rural local governments for each election year.

### **2.2.3 Town meeting form of government**

The Local Government Act (SFS 1930: 251) was identical for local governments with a town meeting and a representative form of government except for the collective decision process, and the rule that the chairman and the vice-chairman of the town meeting had to be at least 25 years old and had to be elected for a four-year term (instead on yearly basis).

At the town meeting, the chairman made a proposal after the discussion that could be decided with a yes or no vote. The chairman then declared the outcome after voice vote of “yes” or “no”, unless somebody requested a secret vote.<sup>20</sup> Each eligible voter attending the town meeting was entitled to represent another voter provided that she had the power of attorney do so. However, each voter could only represent one additional eligible voter at most.

In contrast to the official election statistics, the attendance rate at town meetings was unfortunately not recorded by Statistics Sweden. However, the minutes of the town meetings contain such information when there was a secret vote. We have collected some of this information from the archives of the currently existing local governments. When going through a sample of the minutes, we discovered that only very few of them had secret ballots. We also found that the average attendance rates at meetings when there was a secret ballot was very low, about 7 percent of total population, which is based on a sample of 255 observations. The low attendance rate at town meetings is also confirmed by a previous study conducted by a Swedish Government Official Committee which reported a 9 percent attendance rates in the 1910-1914 period.<sup>21</sup> It seems likely that the attendance rate at meetings without a secret ballot would be even lower since the secret ballot was typically used when there were matters of debate and such meetings typically had more attendance rates.

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<sup>20</sup> The town meeting could also take action by show of hands responding to “yes” or “no” vote requests, or by division of the meeting into “yes” and “no” spaces.

<sup>21</sup> Förslag till kommunal rösträttsreform (1918). Stockholm: Nord. bokh.

## **2.2 Spending programs**

During the period of study, Swedish local governments were formally responsible for the following five main spending programs: basic compulsory education (“folkskola”), social assistance or poverty relief (“fattigvård”), child welfare (“barnavård”), basic social security (“folkpensionering”), and health care (“hälso och sjukvård”). Basic compulsory education was the largest spending program and with more than 40 percent of local government spending. Social assistance to the poor was the second largest program with about 20 percent of total spending. Child welfare was about 3 percent, pension about 5 and public health care about 1 percent of total local government spending.

The rise of the modern welfare state in Sweden mostly occurred after World War II. Thus, the development of social policy in Sweden differs little from international trends in the pre-war period (e.g., Lindert 2004 and Esping-Andersen and Korpi 1987). For example, aggregate public spending as a share of GDP was about 20 percent in 1950, which was slightly below the corresponding level of 24 percent in the U.S. At that time, the Swedish local government spending as a share of GDP was about 10 percent, exactly equal to the GDP share for state and local government expenditures in the US.

In the following we describe the local spending programs in more detail.<sup>22</sup>

### **2.2.1 Basic compulsory education**

As mentioned, basic compulsory education was the largest spending item with more than 40 percent of local government spending. The first basic public school system in Sweden was introduced in 1842 and was primarily intended for the lower classes since the upper classes had other options.<sup>23</sup> This type of two-tier system was a basic feature of the Swedish education system until 1972, when a compulsory nine-year comprehensive school was fully implemented in the schools. In 1882, basic public education was made compulsory for all children aged 7 to 14. However, a student that already had acquired the necessary knowledge before age 14 was allowed to leave. In practice, due to this exemption clause, most pupils only got 6 years of basic education. In 1936, the central government decided that the compulsory school should be 7 years, but local governments were given a transitional period of 12 years to implement the reform. After a nationwide evaluation experiment between 1949 and 1962, a nine-year compulsory comprehensive was introduced in 1962. The reason for this

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<sup>22</sup> The description of the individual programs is rather long because we wanted to be as clear as possible about the programs and to what extent they redistribute income from rich to poor. If needed, the description can be shortened considerably, and additional material can be made available on the web.

<sup>23</sup> This section is based on Richardson (1968).

school reform was partly to abolish selection based on grades into an academic and non-academic stream, and partly to impose a nationally unified curriculum.

In the old school system before 1928, most of the children from the upper classes did not go to basic public education but instead entered State (i.e., Federal) or Private Grammar Schools (“allmänna och enskilda läroverk”), both of which were part of the selective tier of the Swedish school system. From 1928, however, the central government made it compulsory for all children to go to basic public education for at least four years before they could enter a grammar school (SFS 1928:412). Pupils with better school achievement were then selected for grammars school, i.e., lower secondary school (“realskolan”) after they had completed either 4 or 6 years in basic compulsory school. In contrast, pupils from the lower classes could instead voluntarily enter a two-year municipal secondary school (“fortsättningsskolan”) after they had completed 6 years of public primary education.

Despite the fact that it was compulsory for local governments to provide basic public education, there were still large differences in basic education across local governments. For example, in 1941 more than 80 percent of the urban local governments had implemented the extension to 7 years of compulsory basic education, decided in 1936, but only 33 percent of the rural local governments. Moreover, most of the pupils in urban areas had 39 weeks of education per year while many of the pupils in rural areas only had 36 weeks. There were also large differences in the internal organization of schools among local governments. For example, many schools in rural areas were one-teacher or one-room schools while in urban areas primary education was provided in single-grade classrooms.

### **2.2.2 Poverty relief**

Social assistance or poverty relief was the second largest local government spending program with about 20 percent of total spending. Swedish local governments had been providing poverty relief for a long time, but it was not until the Poverty Relief Act of 1847 that social assistance was systematically regulated across the country. The Poverty Relief Act was changed in 1853 and 1871. These Acts only granted the poor a barely adequate support of their basic needs. In contrast, under the Poverty Relief Act of 1918, each local government was charged with the task of providing adequate care and relief to individuals in need. According to this Act, each local government was required to establish a public assistance Committee. The members of the Committee were at least three local appointees with at least one female member.

Two different classes of public assistance were established in the Act: “compulsory assistance” and “permissive assistance.” Compulsory assistance was given to any “destitute minor, aged, infirm, disabled or feeble-minded person incapable of maintaining herself by gainful employment” and not receiving adequate support from some other sources such as a relatives, employers, charity organization, insurance or aid society.<sup>24</sup> The public assistance committee was authorized to recover costs of compulsory public assistance from the recipient when his/her financial status improved, or from relatives legally liable for his/her maintenance. If the recipient had no relatives and could not pay then the local government was not reimbursed.

The public assistance committee was also empowered to provide permissive assistance, that is, aid beyond what was statutory required or to cases not eligible for compulsory assistance. For example, partially disabled persons who were poor and ineligible for pension disability payments, or individuals for whom the compulsory assistance payments were insufficient could be granted permissive assistance.

Social assistance was provided in three different forms by the local governments. These forms were (i) assistance to the recipients in their own homes, either cash allowances or assistance in kind, (ii) boarding out with a private family, and (iii) care in public institutions such as old-peoples, children’s or nursing homes and shelters. Most people received help in their own homes. On average, each year from 1919 to 1950, between 4 and 12 percent of the population received social assistance. According to a survey made in 1938 (SOU 1941:29), the primary causes of applying for social assistance were sickness and insufficient work capacity (60%), unemployment (17%), and insufficient income in relation to family size (13 %).

Finally, there were centrally mandated rules that governed the distribution of costs of the poor relief program across local governments for people that recently had moved in to the local government (“Hemortsrättsstadgarna). These rules made it possible for a local government to deny people moving in from other local governments’ poverty relief. For example, people over 60 could only get relief from the local government they lived in before they turned 60. This type of rule diminished much of the problems due to welfare migration and made sorting around the population treatment threshold in the RD analyses much less probable.

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<sup>24</sup> Inghe (1958, p. 11).

### **2.2.3 Child welfare**

Spending on child welfare and child protection was about 3 percent of total spending. The first Swedish legislation on child welfare was enacted in 1902, with one law calling for public care of maladjusted children (SFS 1902:67) and another concerning foster care of children (SFS 1902:63). The Child Welfare Act of 1924 (SFS 1924:361) centralized all measures of aid to a single agency, i.e., the child-welfare committee. All local governments were therefore required to establish a local child welfare committee to give special attention to the needs of children. As a result, the responsibility for care of children was transferred from the local public-assistance committees to the child-welfare committee. The public assistance committees, however, were still responsible for the provision of public relief to children living with their parents and for the payment of hospital and medical expenses for the needy children. The members of the child-welfare committee were local appointees.

The total number of children and adolescents for whom the child-welfare committee took some action (i.e., preventive action, protective education, and social protection) was about 40,000 each year.

### **2.2.4 Social security<sup>25</sup>**

Spending on pensions was about 5 percent of total spending. The National Pension Act of 1913 was the first national effort to provide social insurance for the entire population. The social insurance consisted of two parts. One part was almost universal pension insurance, financed by individual contributions based on taxable income. It guaranteed every citizen upon retirement or disability a pension related to previous earnings. The other part was a tax-financed, means-tested supplementary pension, to be paid to those with little or no contribution pension. However, it was found that most of the benefits from the contribution pension were inadequate. In the early 1930.s, for example, the yearly benefits corresponded to only a few hours' wages for the normal industrial worker. Consequently the retired or disabled had to rely to a large extent on benefits provided under the supplementary pension determined by an income or needs test administered at the local government level. The total costs for benefits paid by the supplementary pension were about nine times as high as the costs for the benefits from the pension insurance. Despite the existence of a social insurance system, many of the aged were still forced to supplement their pensions with poverty relief. In 1935 and 1937 the insurance system was reformed by making it more generous, i.e., less dependent on own contribution and taking into account cost-of living. Nonetheless, it was

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<sup>25</sup> This section is based on Edebalk (2009), Lundberg and Åmark (2001)

estimated that about 30 percent of retirees received poverty relief on an annual basis in the period 1939-47 (Elmér 1960). In 1948, the old-age pensions were reformed again. The insurance system from 1913 was abolished, and the pensions became universal, flat-rate and state financed. There were also various supplements to the pension, including a means-tested supplement linked to a cost-of-living index.

Local governments were required by the central government to finance part of the spending on social security. Each local government had to finance the administrations of pensions which were made by appointed members of the local pension committees where the head of the committee was appointed by the central government. They also had to finance a major part of the supplementary pension benefits since it was argued that this would decrease the amount of social assistance paid out to the elderly.

### **2.2.5 Public health<sup>26</sup>**

Spending on public health care constituted about 1 percent of total local government spending. The Public Health Act of 1919 (SFS 1919:444) regulated the local governments' obligations and powers regarding public health. The Act specified that the local governments should, for example, see to that the inhabitants should have drinking water of good quality, good housing, and good sewage systems. Health care spending includes, among other things, spending on wages of midwives, quality control of sanitation, quality control of food and water, quality control of accommodations, and measures to prevent spread of disease.

## **2.3 Data**

In order to evaluate the impact of the forms of democracy on local government spending and political participation, we have constructed a new comprehensive panel dataset for about 2500 local governments for the period 1919-1950. The data set consists of *yearly* observations on a large number of fiscal policies, political variables, and local government characteristics (e.g., population size, land area, structure of the economy). Table 4 contains descriptive statistics for the main variables that we use in this paper.<sup>27</sup>

Our data on budget items and other characteristics is mostly taken from two official printed (but non-digitalized) publications from Statistics Sweden, namely Local government finances: 1928-1952, and Statistical yearbook of administrative districts of Sweden: 1918-1952. However, for the budget items for the years 1919-1927, it was also necessary to collect

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<sup>26</sup> This section is based on Statens offentliga utredningar ("Swedish Government Official Reports") 1945:38.

<sup>27</sup> We are still in the process of collecting spending and political data for the years 1919-1922. We include this data in the final version of our paper.



data from unpublished material from Statistics Sweden kept at the Swedish National Archives (Arninge). Data on forms of democracy and voter turnout in elections were also collected from unpublished material at the Swedish National Archives while data on political participation at town meetings had to be collected directly from the archives of each individual, currently existing local government.

Since all our data is register-based except for political participation at town meetings, we have a truly comparable data set on the universe of all Swedish local governments. Specifically, we have *yearly* observations on the treatment determining variable, population size which means that the statistical analysis in our RD designs will be much more powerful than if the data were based on censuses, which is only conducted every tenth year. It is also noteworthy that the population register were not administered by the local government themselves, rather the keeping of vital statistics in Sweden was the duty of the established church until 1991.<sup>28</sup> Thus, a local government cannot strategically misreport their population size as to avoid having a certain type of form of government. However, a local government could still potentially try to control how people move in and out of their jurisdictions. If this is the case, then this could potentially make an RD analysis invalid, since local governments around the treatment thresholds would not be comparable. Below we show that there is no evidence of sorting around the threshold in the RD analyses. Sorting is not an issue in the non-parametric IV approaches; however, since the cut-off point was unknown for local governments at the time of their implementation as noted above.

We now turn to the outcomes of interest,<sup>29</sup> i.e., basic education, social assistance, child welfare, basic social security, health care and political participation. To facilitate the presentation of analyses, we will mostly focus on three outcomes: total spending, pro-poor spending and political participation. As a result, we aggregate spending on basic education, poverty relief and child welfare together since these policies are all, to a large extent, aimed at redistribution towards the poor. However, we will also briefly report separate results for these three spending programs.

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<sup>28</sup> Every parish in Sweden was required to maintain the records of its parishioners, even if some of them never set foot inside the church itself. Every birth, death, marriage, removal from the parish, or entry into it was carefully recorded by the clergyman of the parish or his assistant; or, if in a large city, by the clerical staff at his service. This system was put into effect in the latter half of the seventeenth century.

<sup>29</sup> We will restrict our analysis to rural local governments only, i.e. observations from cities and boroughs are dropped. This implies that 8 percent of the observations are dropped. This restriction does not impact on our analyses since very few of these observations are in a close neighborhood to the treatment thresholds. All nominal values are deflated with CPI with 1920 as base year

Although it is standard practice in a traditional regression analysis to express policy outcomes in per capita terms, we do not follow this practice here for two reasons. First, in the RD analysis there is absolutely no reason for transforming the outcome variable since we compare local governments that are expected to be identical in all dimensions (like in a true randomized experiment). Second, it would be highly controversial and possibly even harmful in a RD analysis to transform the outcome variable by dividing it with the treatment determining variable (in our case the population size). Nevertheless, to facilitate the interpretation of our results, we express all the outcome variables in logarithmic form such that the results can be interpreted as percentages. Importantly, all the results in the paper are unaffected by this transformation. In the nonparametric IV approach, it does not matter for the results whether the outcome variable is expressed in per capita terms or not since the model is saturated in population size (a full set of dummy variable for each population size).

### 3. Regression-discontinuity designs

In this section, we describe the RD design and present the results from the two RD designs, i.e., the estimated effects of having direct democracy rather than representative democracy at the treatment threshold on the three outcomes of interest: total spending, redistributive spending, and political participation.

#### 3.1 Identification issues

The RD designs are based on the RD setup analyzed by Battistin and Rettore (2008). They analyze an eligibility rule that splits the relevant population into eligible and ineligible subjects, and participation in the former group is determined according to rules potentially unknown to the researcher. The Swedish statutory law regulating the forms of government for local governments closely resembles their RD set-up but with one important exception further discussed below. The unit of analysis – a local government in our case – can therefore basically be grouped into one of three subgroups based on the classification used by Battistin and Rettore (2008), namely *ineligibles*, *participants* and *eligible non-participants*. *Ineligibles* are local governments with a population size above the threshold since they are forced to have representative form of government, *Participants* are local governments with a population size below the threshold with town meeting form of government, *Eligible non-participants* are local governments with a population size below the threshold that have endogenously switched from town meeting to representative form of government.

Battistin and Rettore (2008) formally show that the average effect of the treatment on the treated is identified locally at the threshold for eligibility despite the “fuzziness” of the

RD design. In a fuzzy RD design, the Wald estimand  $\tau^{rd}$  defined at the threshold  $c$  can be formally expressed as the following ratio

$$(1) \quad \tau^{rd} = \frac{\lim_{x \uparrow c} E[Y | X = x] - \lim_{x \downarrow c} E[Y | X = x]}{\lim_{x \uparrow c} E[D | X = x] - \lim_{x \downarrow c} E[D | X = x]}.$$

where  $Y$  is the outcome (e.g., redistributive spending or political participation),  $X$  is the treatment determining variable (i.e., population size), and  $D$  denotes the treatment received, with  $D=1$  if the unit was exposed to the treatment (i.e., town meetings), and  $D=0$  otherwise (i.e., representative democracy).

Typically, the ratio defined in (1) will identify a local average treatment effect (LATE) at the cut-off point (Hahn et al. 2000). However, in the RD set-up analyzed here, an eligibility rule divides the population into eligibles and non-eligibles according to a sharp RD design, and where eligible units self-select into treatment. Therefore, the ratio in (1) instead identifies the average treatment effect on the treated for those near the cut-off, as formally shown by Battistin and Rettore (2008). Their result rests on the fact that the probability of participation on the right-hand side of threshold  $c$  is zero by design, i.e., towns with a population size above the threshold  $c$  are not allowed to have town meetings,  $\lim_{x \downarrow c} E[D | X = x] = 0$ . The result of Battistin and Rettore (2008) require no restriction on the self-selecting behavior of eligibles, and is similar to the results by Bloom (1984) and Angrist and Imbens (1991).

For the analysis, presentation and estimation of the RD designs, we will basically follow the scheme outlined by Imbens and Lemieux (2008) and Lee and Lemieux (2010), that is: (i) show a histogram of the distribution of the assignment variable, (ii) present the results using binned local averages, (iii) graph a benchmark polynomial specification, (iv) explore the sensitivity of the results to a range of bandwidths, and a range of orders to the polynomials, (v) check whether there is a jump at the threshold for the baseline characteristics.

### **3.2 Results from the RD designs**

In this section, we will provide graphical analyses of the RD designs as well as results from regression specifications. We start with the RD design at the 1500-threshold for the period 1919-1938 followed by the RD design at the 700-threshold for the period 1939-1950.

### 3.2.1 The period 1919-1938

Figure 1 presents a histogram of the distribution of the assignment variable, using a fixed number of bins, for the period 1919-1938 when the treatment threshold was 1,500. The dots in the figure are local averages with a bin width of 5. This is the smallest bin width that makes the plots look smooth on either side of the cut-off value, while at the same time small enough to make the any jump around the cut-off value clear. Figure 1 reveals that the bin sizes is slightly above 50 at the 1500 threshold, which suggests that a non-parametric RD approach might have enough statistical power to detect a treatment effect. This plot shows no evidence of a discontinuity in the distribution of the forcing variable at the 1500 threshold. It is also noteworthy that Figure 1 shows that the distribution is heavily skewed since there are more than three times as many small local governments than larger ones, e.g., the bin size is 100 when population is around 1,000 and the bin size is 30 when population is about 2,000.

Next we present graphical evidence of three reduced-form relationships, namely (i) the relationship between the propensity of having the town-meeting form of government and population size (the “first-stage” relationship), (ii) the relationship between the two categories of spending (redistributive and total spending) and population size, and (iii) the relationship between political participation and population size. The binned local averages in all the plots are again based on a bin width of 5. We have also added a smoothed regression lines based on a fourth order polynomial regressions estimated separately on the two sides of the cut-off point to improve the visual clarity of the plots.

The relationship between the propensity of having a town meeting form of government and population size is shown in Figure 2. It shows clearly that the probability of treatment jumps down at the threshold. The jump is somewhat larger than 20 percentage points. Importantly, Figure 2 also shows that there are no other jumps in the probability of treatment of the same order of magnitude at any other value of the forcing variable, which lends credibility to the appropriateness of the RD designs.

The relationship between redistributive spending (i.e., spending on basic education, poverty relief and child welfare) and population size is displayed in Figure 3. The outcome is expressed in logarithms such that the impact estimate from the regressions can be interpreted as a percentage. There is clear evidence of a discontinuity at the cut-off point in the graph. The jump in spending is about 15 percent. Once again, there no other jumps of similar magnitudes in the graph. The estimated causal effect of having direct rather than representative democracy is the ratio between the reduced form estimate for the outcome and

the “first stage” estimate, which imply that redistributive spending is 75 ( $15/0.2$ ) percent lower for local governments with direct democracy.

The relationship between total spending and population size is plotted in Figure 4. Once again, this graph shows clear evidence of a discontinuity at the cut-off point and the jump at the threshold is about 10 percent, which is smaller than the jump for redistributive spending. This implies that the estimated treatment effect of direct democracy is around 50 percent.

Turning to the relationship between political participation and the population size, Figure 5 shows that the jump in political participation is more than 50 percent at the threshold. This means that the political participation in direct democracy will be 200 ( $50/0.2$ ) percent lower than in representative democracy.

In the statistical analyses of the RD design, we follow the approach suggested by Lee and Lemieux (2010), namely to explore whether the RD estimates are robust to the inclusion of higher-order polynomial terms (the series or polynomial estimation approach) and to changes in the window width around the cut-off point (the local linear regression approach). However, to save space, we only report the regression results based on a fourth order polynomial regressions estimated separately on the two sides of the cut-off point and where the window width is  $\pm 500$ , i.e., only observations within a population size within the interval 1,000-2,000 are included. An appendix shows results for smaller window widths around the 1,500 cutoff, combined with different polynomial functions of population size in the regression model. Table 5 shows the statistical results from the regressions which clearly confirm the results from Figures 2-5. Table 5 also provide additional information regarding statistical inference showing that all these estimates are statistically significantly different from zero at the 5 percent level or better.

### 3.2.2 The period 1939-1950

We now conduct an identical RD analysis for the period 1939-1950 when the threshold was 700 instead of 1,500.

Figure 6 displays a histogram of the assignment variable for the period 1939 to 1952 when the threshold was 700. The bin width is the same as in Figure 1, namely 5. Once more, there is little evidence of a discontinuity in the distribution at the threshold. Not surprisingly, there bin sizes at the 700 threshold are larger than at the 1500 threshold. On average, the bin size is around 90. Figure 7 shows the graphical relationship between the propensity of having the town meeting form of government and population size. Now the jump in the probability is much larger than in the previous period, around 60 percentage points. Again, there is no

discontinuity of this size anywhere else. Figure 8 shows that the jump in redistributive spending is 20 percent which means that redistributive spending is 33 ( $20/0.6$ ) percent lower for local governments with direct democracy compared to representative democracy. This estimate is thus lower than the corresponding estimate of 70 percent for the 1,500 cutoff. Figure 9 suggests that the jump in total spending is of the same size as the jump in redistributive spending. Figure 10 shows that the discontinuity in political participation rates is slightly above 100 percent which means that the political participation rate in direct democracy is about 170 ( $100/0.6$ ) percent lower than in representative democracy.

Table 6 displays the corresponding statistical estimates based on a fourth order polynomial regressions estimated separately on the two sides of the cut-off point with a window width of  $\pm 500$ , i.e., only observations within a population size within the interval 200-1,200 are included. The results in Table 6 accord with the graphical analyses. Table 6 also shows that all estimates are statistically significantly different from zero at the 1 percent level. An appendix shows results from smaller window widths around the 700 cut-off combined with different polynomial functions of population size in the regression model

## **4. Nonparametric IV designs**

In this section, we describe the identification assumptions behind the nonparametric IV designs and present the results, i.e., the estimated effects of having direct democracy as compared to representative democracy on the three outcomes of interest: total spending, redistributive spending, and political participation.

### **4.1 Identification issues**

As previously noted, some local governments do not unambiguously fit into any of three subgroups defined by Battistin and Rettore (2008). Those are the local governments that belong to the ineligible in the year when the population threshold for having mandatory representative democracy was introduced (1918) or changed (1938), but whose population sizes subsequently decreased below the mandatory population threshold. According to the Swedish statutory law, local governments belonging to this category were still required to have a representative form of government unless two conditions were fulfilled: (i) more than five years must have passed since the introduction of representative democracy, and (ii) more than 50 percent of the voters were in favor of switching from representative democracy to direct democracy.

As discussed above, these conditions imply a large status quo bias toward maintaining a representative form of government. As a result, we can implement a second identification strategy using the status quo bias as an instrumental variable. Due to the status quo bias, we can define an instrumental variable  $Z$  which is equal to one for all future time periods if a local government was below the threshold  $c$  in the year of the introduction of the mandatory population threshold, and  $Z=0$  otherwise. This instrument then exploits the fact that the choice of treatment  $D$  is unconstrained for those units with  $Z=1$  but heavily constrained for those with  $Z=0$ . We can now define a Wald estimand  $\tau^{iv}$  for each value of the treatment determining variable  $X$  below the cut-off  $c$

$$(2) \quad \tau^{iv}(X) = \frac{E[Y | Z = 1, X] - E[Y | Z = 0, X]}{E[D | Z = 1, X] - E[D | Z = 0, X]}.$$

It is now possible to estimate a weighted average of the covariate-specific LATEs,  $\tau^{iv}(X)$ , using 2SLS with a fully saturated first stage and a saturated model (a full set of dummies for all values of  $X$ ) for  $X$  in the second stage as shown by Angrist and Imbens (1995). However, a first-stage parameter for each value of  $X$  is likely to yield biased estimates due to many weak instruments. Nonetheless, as discussed by Angrist and Pischke (2008), 2SLS with a saturated model for covariates but with a restricted first stage with a constant parameter, produces the same, minimum mean-square-error approximation to an underlying average-treatment response function as in Abadie (2003). In other words, despite that 2SLS is used, identification is attained nonparametrically and does not depend on the choice of the parametric specification for the response function of interest. Moreover, since some local governments with  $Z=0$  are denied treatment,  $E[D | Z = 0, X] = 0$ , that is, for local governments that cannot switch from representative democracy to a town meeting within a five year period, the ratio in (2) will identify the effect of the treatment on the treated rather than LATE (Bloom 1984). The Wald estimand  $\tau^{iv}(X)$  will therefore have different interpretations depending on the range of  $E[D | Z = 1, X] - E[D | Z = 0, X]$  that is induced by the range of instrumental variables in the data. As a result, it is possible to estimate different weighted averages of covariate-specific LATEs by restricting the range of  $X$ . For example, if one restricts the  $X$  close to the threshold  $c$ , then this estimate from the nonparametric IV approach should be close to the treatment on the treated effect defined by the RD estimand in (1). There is also another reason for limiting the sample to observations that are close to the

treatment threshold, namely that the validity of the exclusion restriction is more likely to hold in this case since the comparison in the IV approach is explicitly based on the comparison of local governments that have identical population sizes but that some of them previously had a larger population size than the threshold. This type of IV approach therefore share the same logic with a RD design, namely to compare those subjects above the cutoff with those below but this comparison is conditional on identical values of the treatment determining variable.

The two design-based strategies therefore nicely complement each other: the RD design typically scores high on internal validity, while the IV design has more external validity in that it estimates the treatment effect for a broader population. The two designs also rely on different identifying assumptions. The RD design assumes smoothness in the expected potential outcomes at the discontinuity while the IV approach basically requires an exclusion restriction without any need for extrapolation.

## ***4.2 Results from the nonparametric IV approaches***

In this section, we report results from the nonparametric IV approaches. We start with the IV design at the 1500-threshold for the period 1919-1938 followed by the IV design at the 700-threshold for the period 1939-1950.

### **4.2.2 The period 1919-1938**

We start by showing graphical evidence for the IV approach. All these graphs are identical to the RD graphs with the exception that we now also include observations for those local governments that were affected by the status quo bias, i.e., those local governments that were originally above the threshold in year of the change in the rule and subsequently below. These local averages are marked with a triangle and colored blue in the figures.

Figure 11 shows the probability of treatment, i.e., town meetings rather than representative democracy, for period 1919-1938. This figure illustrates two important points. It shows clear evidence of a status quo bias since the probability of having direct democracy is consistently much lower for the observations that were required to have representative democracy in 1919 than for the comparable observations not affected by the status quo bias. Also, there is no evidence of a jump in the probability of treatment at the treatment threshold 1500 for observations affected by the status quo bias. These two observations from Figure 11 imply two additional specifications checks that test the validity of the IV approach. The first specification check is that there should be no jump in any of the other outcomes (redistributive spending, total spending, and political participation) at the treatment cut-offs since there is no jump in the probability of treatment at the threshold for these observations.



The second test is that the outcomes for the observations affected by the status quo bias should also be consistently larger than the corresponding outcomes for the observations not affected by the status quo bias. Indeed, Figures 13-15 all shows that there is no jump in the outcomes at the threshold and that the outcomes affected by the status quo bias are consistently larger than the outcomes for the observations not affected by the status quo bias.

Table 7 presents the statistical results from the nonparametric IV approach, i.e., all specifications are saturated with a full set of dummy variables for all values of population size as previously discussed. Here, the sample is restricted to population sizes in the range 1,300-1,500. There are two reasons for limiting the sample to observations close to the treatment threshold as noted above. The first reason is that the validity of the exclusion restriction is more likely to hold while the second reason is that in a world of heterogeneous treatment effects, the treatment effect identified in the IV approach is likely to be similar to the treatment effect in RD design the closer the IV sample is to the threshold. As a result, to test for the validity of the instrument, we can compare the estimate from the IV approach with the estimate from the RD design. If these two estimates differ then this might be because (i) the instrument is not valid, or (ii) the treatment effect is heterogeneous. Thus, comparing the results in Panel A in Table 7 with those in Table 5 show that reduced form effect estimates are highly similar except for the estimate of the probability of having direct democracy, which is somewhat larger in the IV approach (0.315 vs. 0.229). This difference in the probability of treatment thus makes estimates of the treatment effects displayed in Panel B somewhat smaller in the IV approach than in the RD design. Importantly, the estimates are however not statistically significantly different from each other. Moreover, restricting the sample even closer to threshold does not change these conclusions as can be seen from Table 9 where the sample has been restricted to observations with a population size in the range 1,450 to 1,500.

#### **4.2.2 The period 1939-1950**

Here we present an identical analysis as in the previous subsection for the period 1939 to 1950 when the threshold was 700. The graphical evidence in Figure 13 allows us to draw the same conclusions as in the previous analysis, i.e., that there is no jump in the probability of treatment at the treatment threshold for those local governments that were required to have representative democracy in 1938, but subsequently whose populations later fell below the threshold. However, the status quo bias is much larger for the later period 1939-1950, which is not surprising since they had less time to change back to direct democracy (6 years) than in

the previous period 1919-1938 (12 years). Moreover, the boundary reform eventually implemented in 1952 was already discussed in 1946, suggesting that most local governments expected that they anyway had to implement representative democracy in the near future. Looking at the graphs for the reduced form outcomes, Figures 14-16 reveal that (i) there is no jump in the outcomes, (ii) the outcomes for the local governments with a status quo bias is consistently larger than for the unconstrained local governments. Once again, this supports the validity of the IV approach.

Turning to the statistical analysis, Table 10 reports the reduced form results in Panel A and the results for treatment effects in Panel B where the sample is limited to the 500-700 population bracket. The results in Table 10 are broadly similar to the results from the RD design in Table 6, although that the treatment effects for redistributive spending and total spending are somewhat larger than in the RD design. However, the results are almost identical if the sample is further limited. Table 9 presents results for the 650-700 population bracket. As noted above, this finding might suggest that the exclusion restriction is violated or that there the treatment effect is heterogeneous.

## **5. Additional results**

In this section we provide results from individual spending programs, various robustness checks as well as tests of the likely mechanism behind the treatment effect.

### ***5.1 Individual spending programs***

Here we provide some additional results regarding the individual spending programs that make up redistributive spending. The first set of results is from the individual spending programs that make up redistributive spending, i.e., spending on basic education, poverty relief and child welfare for the two periods. Table 11 shows that spending on poor relief and child welfare is typically more affected than spending on basic education, particularly in the period 1919-1938.

### ***5.2 Robustness checks***

In this subsection, we discuss three robustness checks but to save space their results are presented in an Appendix. First, we conduct a falsification exercise by estimating whether there was a discontinuity at the 700 threshold before the law was implemented in 1939. We find no evidence of a jump in any of the outcomes. Second, we test whether the baseline characteristics are similar around the treatment threshold. We cannot reject that the baseline characteristics are similar. Finally, we test whether a local government spending program that

should not be affected by the treatment status at the threshold was affected. Spending on police was not decided by the local governments themselves but by police districts, which were made up of several local governments. We find no evidence that police spending is affected by treatment status. All these tests support a causal interpretation of our findings.

### **5.3 Mechanism**

In this section, we provide further evidence in support of the Acemoglu and Robinson framework (2006, 2008).

The first test concerns the relationship between the choice of democracy and the structure of the economy, i.e., agrarian versus industrialized. Table 12 shows results from regressing an indicator for direct democracy, measured in 1938 or 1950, on a measure of the importance of the agriculture sector for the local government tax base, for the periods 1919-1938 and 1939-1950, respectively. The samples only include local governments that are free to choose their own form of democracy (i.e., local governments below the population cutoff). The estimated effect in the earlier period is 0.0055, which means that if the economy goes from being completely industrialized to completely agricultural, the probability of having direct democracy increases by 55 percentage points. Our results therefore clearly indicate that direct democracy is mostly used in agrarian societies rather than industrialized which is consistent with the notion that the elite has more to lose from income redistribution to the poor if the economy is agrarian (e.g., Sweden had some labor repressive institutions until in 1945). The ability of elite to exercise de facto power is also likely to be larger in an agrarian economy (e.g., due to client-patron relationships) which provides a complementary explanation for the above correlation between forms of democracy and the structure of the economy.

The second test is based on whether elections in representative democracy were partisan or non-partisan. Non-partisan elections are elections with only a single non-political list (e.g., 14% of all elections are nonpartisan in 1938). Non-partisan elections also almost exclusively occurred in agrarian societies (92% in 1938). To perform this test, we decompose the treatment effect from the RD analysis based on partisan versus nonpartisan elections. Of course, we realize that such sample restrictions may be debatable since whether an election will be partisan or not may itself be an endogenous outcome. Nevertheless, we might still learn something about the mechanism behind the treatment effect. We expect to find that the largest treatment effects for pro-poor public spending in partisan elections, specifically if government is controlled by a left-wing party. By contrast, we expect to find much smaller

effects in nonpartisan elections. Table 13 reveals that treatment effect is largest among local governments with left-wing governments (Panel A), about half the size in local governments with right-wing governments (Panel B) and smallest among those with non-partisan elections (Panel C). Table 10 also reveals that the political participation rate is larger in partisan elections (Panel A and Panel B), compared to nonpartisan elections (Panel C). As noted above, almost all of the nonpartisan elections occurred in local governments with many workers in agriculture. These results are consistent with the landed elite using non-partisan elections as a political strategy to limit income redistribution in representative democracy. Thus, it seems that the elite could exercise its de facto political power to block the creation of pro-poor political parties if the economy was agrarian.

Finally, we ask what happened when local governments were first required to have representative democracy but later become eligible to switch back to direct democracy. The answer to this question comes from the “first-stage” relationship in the nonparametric IV approach. Results indicate that there was a strong inertia in representative democracy once it had been implemented. This finding is consistent with the elite having a problem of replacing a pro-poor political institution (representative democracy) once the introduction of political parties had helped the citizens to solve their collective action problem, since the citizens had considerably de facto political power.

## 6. Conclusions

We have analyzed the difference between direct and representative democracy with respect to public spending programs (poverty relief, basic public education, public health, child welfare) and political participation. To this end, we have constructed a truly unique data set from about 2,500 Swedish local governments with yearly observations during the period 1919-1950. We exploit variation in the form of democracy government using statutory local government laws in Sweden. Specifically, we implement regression-discontinuity (RD) designs based on the Local Government Act of 1918 which required local governments with a larger population size than 1,500 to have a representative form of government. Local governments with a population size below the threshold could choose either to keep their traditional town meeting form of government or switch to the representative form. In 1938, the compulsory population threshold for representative form of government was lowered to 700. In addition to the RD designs, we also implement nonparametric IV approaches by exploiting the fact that central legislation formally created a “status quo bias” for maintaining a representative system in a local government once such a system was in place.

Results from both identification strategies strongly indicate that public spending to the poor increases with 40-70 percent when local governments have representative democracy rather than direct democracy. Political participation in representative democracy is also about 150-200 percent larger.

We can interpret our results in the context of Acemoglu and Robinson's (2006, 2008) game-theoretic framework on the transition from non-democracy to democracy. Their framework is based on a redistributive conflict between the elite (the rich) and the citizens (the poor). The model suggests that the impact of institutions on economic and policy outcomes depends on the interaction between *de jure* political power determined by political institutions and *de facto* political power possessed by groups as a result of their wealth or ability to solve the collective action problem. Equilibrium economic policies depend on the net effect of these two sources of power. In a nondemocratic society, the elite monopolizes *de jure* political power. Upon democratization the elite loses this privilege, but has an incentive to capture democracy by investing more in *de facto* power, i.e., increasing the intensity of its collective action. The evidence presented in this paper nicely fit this framework.

Sweden made a transition from non-democracy to democracy in 1919. At the same time, direct or representative democracy, were basically *randomly* assigned in a certain specific interval at the local level. In representative democracy, there were regular elections with political parties which made it relatively hard for the elite to control the political process since the existence of left-wing parties helped poor citizens to solve their collective action problems. The citizens are therefore likely to possess more overall political power than the elite in representative democracy. By contrast, direct local democracies had no political parties and the voting was open, which made it relatively easy for the elite to control the political process. We provide further statistical evidence in support of the Acemoglu and Robinson's framework.

In future work, we plan to investigate how the two forms of government affect health outcomes. If income distribution to the poor is so much larger in representative democracy it should potentially also lead to better health outcomes. We also plan to explicitly analyze the persistence in the identity and power of political elites. The mechanism in this paper is based on the persistence of *de facto* political power and emphasizes how the same elites are able to shape politics in the wake of institutional change. We therefore plan to collect data on the identity of the political elites, before and after the transition to democracy in 1919.

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Table 1. Number of local governments with representative or direct democracy

Election year	Representative democracy		Direct democracy
	Mandatory	Voluntary	
1919	870	67	1469
1922	889	117	1398
1926	887	147	1377
1930	873	192	1354
1934	867	274	1273
1938	1617	53	739
1942	1576	135	668
1946	1524	199	662

Table 2. The council size law

Population size in the range	Mandatory council sizes
0-1,999	15-20
2,000-4,999	15-25
5,000-9,999	20-30
10,000-	25-40

Table 3. Voter turnout in the rural local government elections

Election year	Total (%)
1919	52
1922	28
1926	42
1930	51
1934	58
1938	62
1942	64
1946	69

Table 4. Descriptive Statistics

Variables	N	Mean	S.D.	Years covered
Spending categories				
Basic public education	68,117	90,385	160,058	1923-1950
Poverty relief	61,255	37,440	65,553	1923-1948
Child welfare	61,257	5,492	11,288	1923-1948
Redistributive spending (The sum of above 3 items)	68,109	134,207	225,625	1923-1950
Pensions	56,262	8,704	13,810	1928-1950
Public health	49,404	3,683	13,308	1928-1948
Police	49,401	3,715	8,738	1928-1948
Total spending	78,972	185,291	352,510	1915-1919, 1923-1951
Other variables				
Population size	103,602	1654	1953	1908-1951
Direct democracy (town meeting) =1	65,952	0.451	0.498	1923-1950
Attendance in town meetings if secret ballot	255	60	69	1921-1951
Participants in regular elections if representative democracy	31,692	819	834	1922, 1926, 1934, 1938, 1942, 1946

Table 5. RD design at the population threshold 1,500 for the period 1919 to 1938

	Probability of having direct democracy	Redistributive spending	Total spending	Political participation
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**Panel A:** Reduced-form effect estimates

Indicator variable =1 if population size below 1,501	0.229 (0.091)	-0.167 (0.061)	-0.099 (0.056)	-0.600 (0.228)
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**Panel B:** Treatment effects estimates from the RD design

The effect of having direct versus indirect democracy	-	-0.728 (0.314)	-0.433 (0.247)	-2.098 (0.940)
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Notes: The standard errors are clustered at the locality level. All specifications include a fourth-order polynomial model for population size that is allowed to be different on both sides of the cutoff. The window width around the cut-off point is  $\pm 500$ . The number of observations in columns 1-3 is 10,334 while it is 8,894 in column 4.

Table 6. RD design at the population threshold 700 for the period 1939 to 1950

	Probability of having direct democracy	Redistributive spending	Total spending	Political participation
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**Panel A:** Reduced-form effect estimates

Indicator variable =1 if population size below 701	0.580 (0.082)	-0.202 (0.062)	-0.198 (0.057)	-1.132 (0.187)
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**Panel B:** Treatment effects estimates from the RD design

The effect of having direct versus indirect democracy	-	-0.348 (0.114)	-0.341 (0.101)	-1.892 (0.129)
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Notes: The standard errors are clustered at the locality level. All specifications include a fourth-order polynomial model for population size that is allowed to be different on both sides of the cutoff. The window width around the cut-off point is  $\pm 500$ . The number of observations in columns 1-3 is 15,031 while it is 14,863 in column 4.

Table 7. The non-parametric IV approach for the period 1919 to 1938

	Probability of having direct democracy (1)	Redistributive spending (2)	Total spending (3)	Political participation (4)
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**Panel A:** Reduced-form effect estimates

Indicator variable =1 if population size below 1501 in 1918	0.315 (0.064)	-0.177 (0.033)	-0.110 (0.036)	-0.504 (0.124)
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**Panel B:** Treatment effect estimates from the IV design

The effect of having direct versus indirect democracy	-	-0.571 (0.127)	-0.348 (0.113)	-1.620 (0.340)
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Notes: Notes: The standard errors are clustered at the locality level. The data consists of observations on population size within the range 1,300-1,500 for the years 1919-1938. All specifications are saturated, i.e., a full set of dummy variables for all values of population size. The number of observations in columns 1-3 is 2,376 while it is 1,457 in column 4.

Table 8. The non-parametric IV approach for the period 1919 to 1938

	Probability of having direct democracy	Redistributive spending	Total spending	Political participation
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**Panel A:** Reduced-form effect estimates

Indicator variable =1 if population size below 1,501 in 1918	0.231 (0.065)	-0.145 (0.046)	-0.051 (0.062)	-0.455 (0.170)
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**Panel B:** Treatment effect estimates from the IV design

The effect of having direct versus indirect democracy	-	-0.626 (0.201)	-0.218 (0.253)	-1.856 (0.822)
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Notes: Notes: The standard errors are clustered at the locality level. The data consists of observations on population size within the range 1,450-1,500 for the years 1919-1938. All specifications are saturated, i.e., a full set of dummy variables for all values of population size. The number of observations in columns 1-3 is 527 while it is 379 in column 4.

Table 9. The non-parametric instrumental variable approach for the period 1939 to 1950

	Probability of having direct democracy	Redistributive spending	Total spending	Political participation
<b>Panel A:</b> Reduced-form effect estimates				
Indicator variable =1 if population size below 701 in 1938	0.640 (0.039)	-0.298 (0.033)	-0.326 (0.031)	-1.029 (0.088)
<b>Panel B:</b> Treatment effect estimates from the IV design				
The effect of having direct versus indirect democracy	-	-0.465 (0.058)	-0.510 (0.055)	-1.673 (0.074)
Notes: Notes: The standard errors are clustered at the locality level. The data consists of observations on population size within the range 500-700 for the years 1939-1950. All specifications are saturated, i.e., a full set of dummy variables for all values of population size. The number of observations in columns 1-3 is 3,596 while it is 3,253 in column 4.				

Table 10. The non-parametric instrumental variable approach for the period 1939 to 1950

	Probability of having direct democracy	Redistributive spending	Total spending	Political participation
<b>Panel A:</b> Reduced-form effect estimates				
Indicator variable =1 if population size below 701 in 1938	0.592 (0.053)	-0.215 (0.037)	-0.237 (0.037)	-0.912 (0.122)
<b>Panel B:</b> Treatment effect estimates from the IV design				
The effect of having direct versus indirect democracy	-	-0.364 (0.069)	-0.401 (0.066)	-1.654 (0.094)
Notes: Notes: The standard errors are clustered at the locality level. The data consists of observations on population size within the range 650-700 for the years 1939-1950. All specifications are saturated, i.e., a full set of dummy variables for all values of population size. The number of observations in columns 1-3 is 941 while it is 857 in column 4.				

Table 11. Results from individual spending programs

	Basic education	Poor relief	Child welfare
<b>Panel A: RD for the period 1919-1938</b>			
The effect of having direct versus indirect democracy	-0.404 (0.314)	-1.272 (0.508)	-4.614 (3.740)
<b>Panel B: Non-parametric IV for the period 1919-1938</b>			
The effect of having direct versus indirect democracy	-0.214 (0.125)	-1.075 (0.228)	-2.642 (0.704)
<b>Panel C: RD for the period 1939-1950</b>			
The effect of having direct versus indirect democracy	-0.309 (0.128)	-0.381 (0.187)	-0.260 (0.350)
<b>Panel D: Non-parametric IV for the period 1919-1938</b>			
The effect of having direct versus indirect democracy	-0.426 (0.062)	-0.550 (0.111)	-0.525 (0.215)

Table 12. The relationship between direct democracy and the importance of the agriculture sector for the sample of local governments that can choose their own treatment status

	Indicator for having direct democracy in 1938	Indicator for having direct democracy in 1950
Percentage of the local government tax base that is related to agriculture measured at the start of the period (in 1918 or 1938)	0.0053 (0.0007)	0.0044 (0.0012)
Number of observation	1,465	772



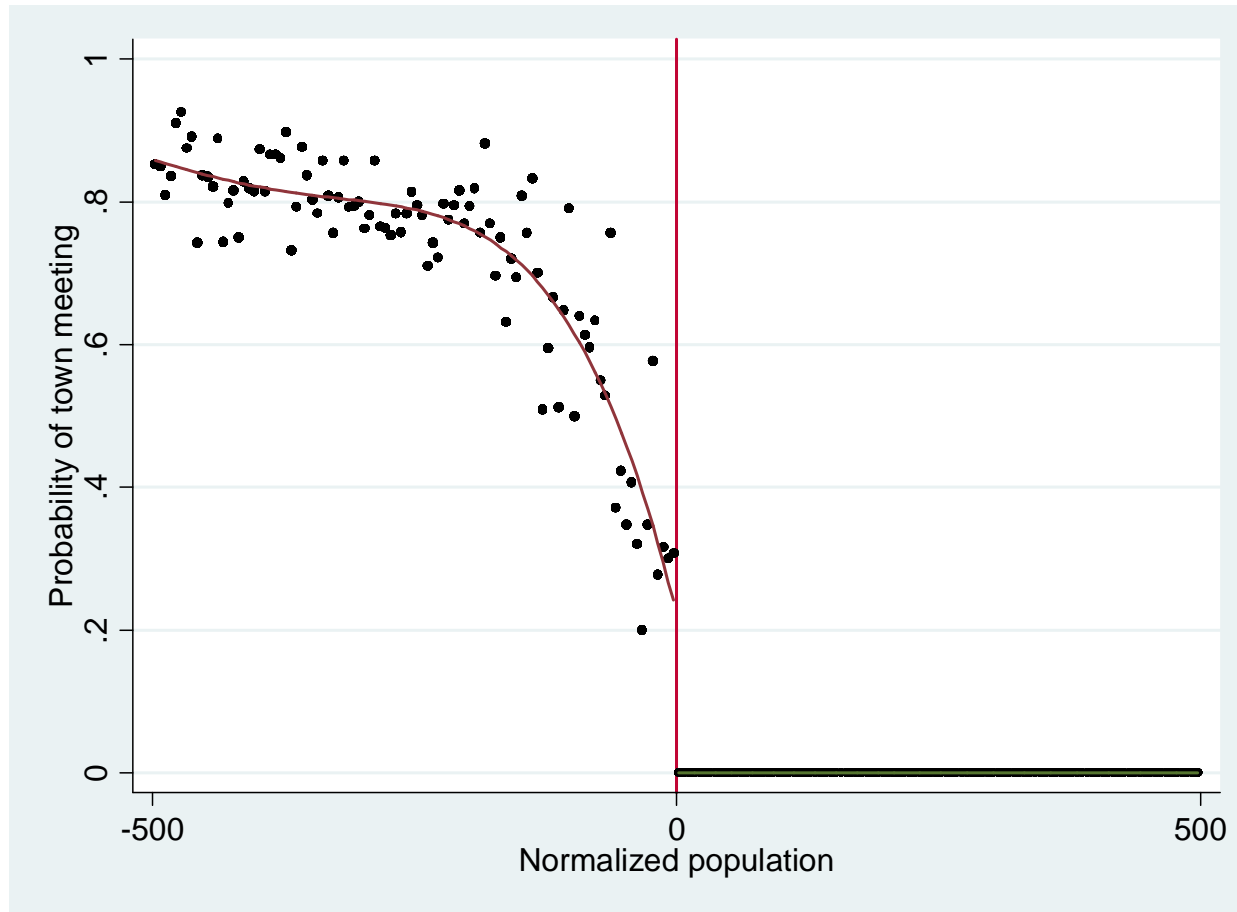
Table 13. Conditional RD estimates based on partisan versus nonpartisan elections

	Redistributive spending	Total spending	Political participation
<b>Panel A: Partisan elections: Sample with left-wing majorities</b>			
Indicator variable =1 if population size below 701	-0.440 (0.102)	-0.453 (0.093)	-1.432 (0.185)
The effect of having direct versus indirect democracy	-0.758 (0.201)	-0.781 (0.183)	-2.393 (0.122)
<b>Panel B: Partisan elections: Sample with right-wing majorities</b>			
Indicator variable =1 if population size below 701	-0.244 (0.075)	-0.251 (0.068)	-1.512 (0.189)
The effect of having direct versus indirect democracy	-0.378 (0.122)	-0.389 (0.109)	-2.259 (0.073)
<b>Panel C: Nonpartisan elections</b>			
Indicator variable =1 if population size below 701	-0.148 (0.071)	-0.155 (0.067)	-0.956 (0.201)
The effect of having direct versus indirect democracy	-0.256 (0.125)	-0.267 (0.116)	-1.598 (0.187)

Notes: The standard errors are clustered at the locality level. All specifications include a fourth-order polynomial model for population size that is allowed to be different on both sides of the cutoff. The window width around the cut-off point is  $\pm 500$ . In panel A, we include only local governments with left-wing majorities on the right side of the threshold. In panel B, we include only local governments with right-wing majorities on the right side of the threshold. In panel C, we include only local governments with non-partisan elections on the right side of the threshold.

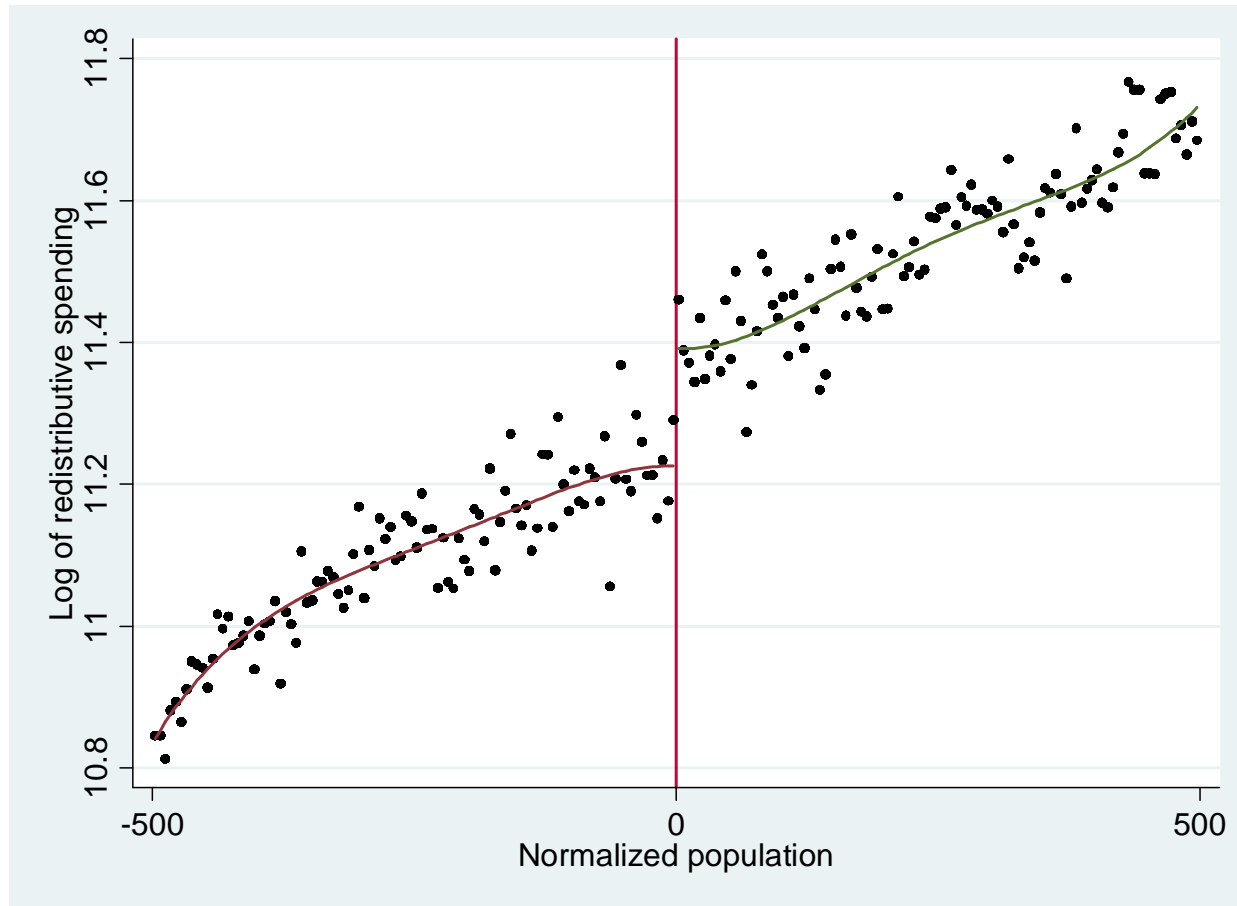


Figure 2. Relationship between the probability of having town meeting and population size: 1939-1950



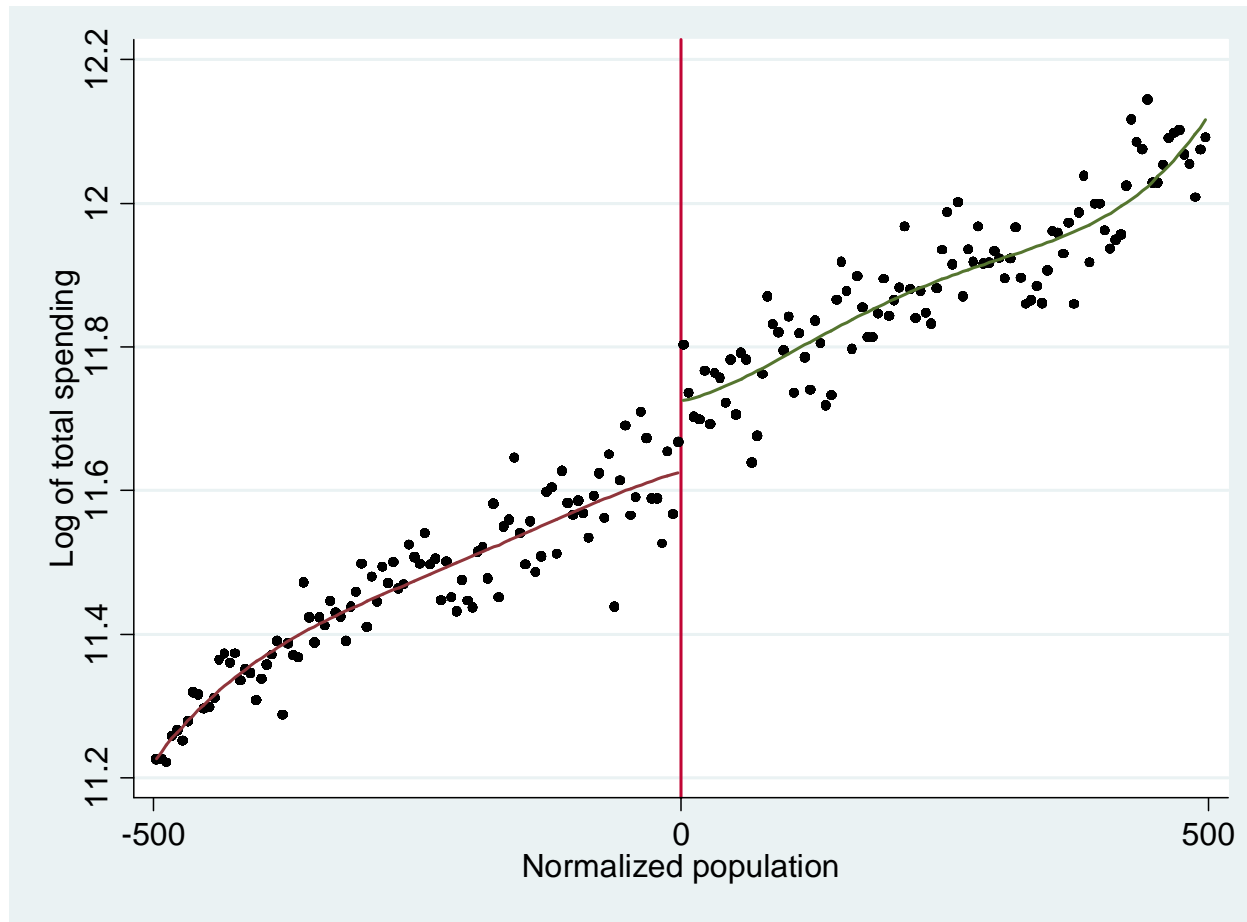
Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline.

Figure 3. Relationship between redistributive spending and population size: 1919-1938



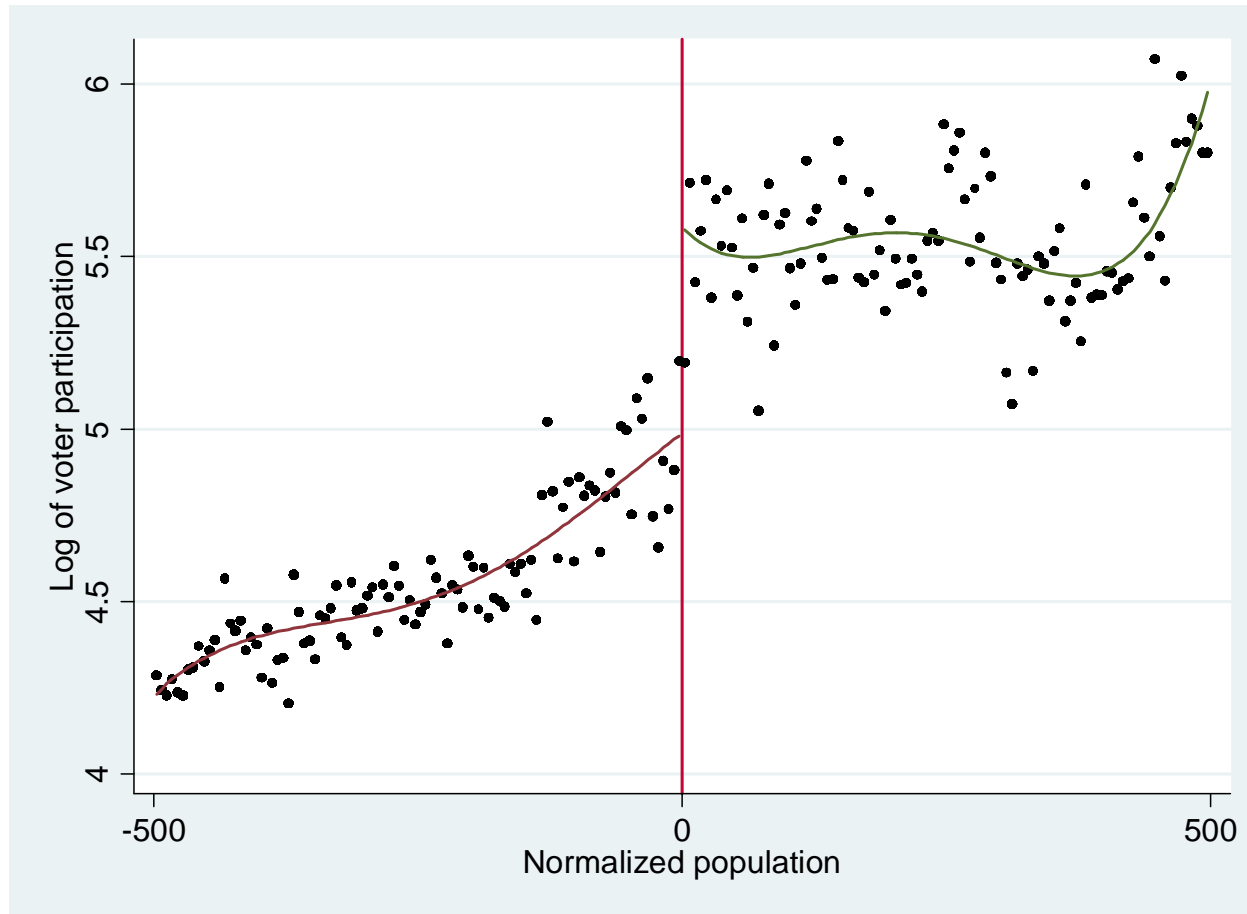
Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline. Redistributive spending includes spending on basic public education, poverty relief and child welfare.

Figure 4. Relationship between total spending and population size: 1919-1938



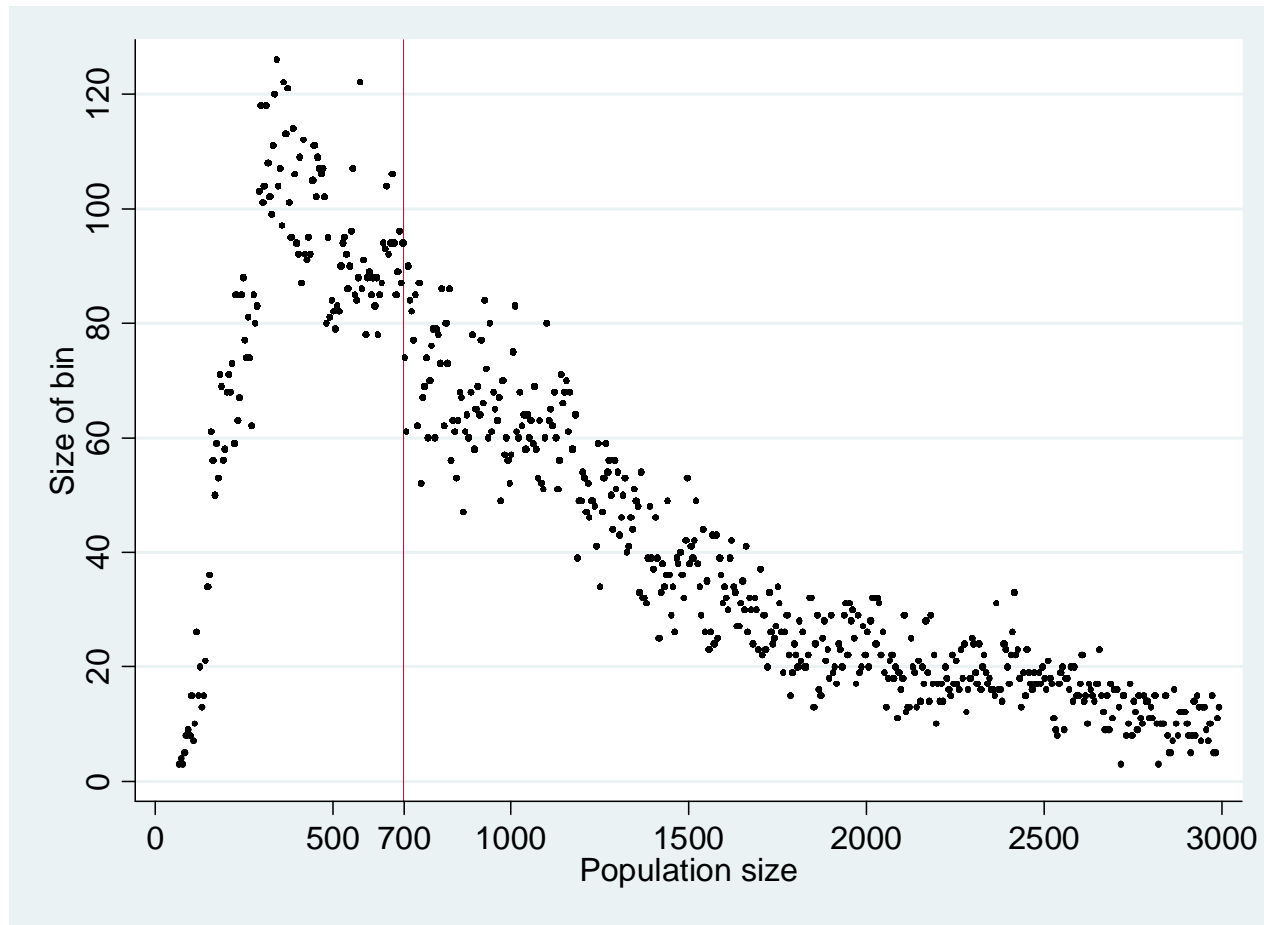
Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline

Figure 5. Relationship between political participation and population size: 1919-1938



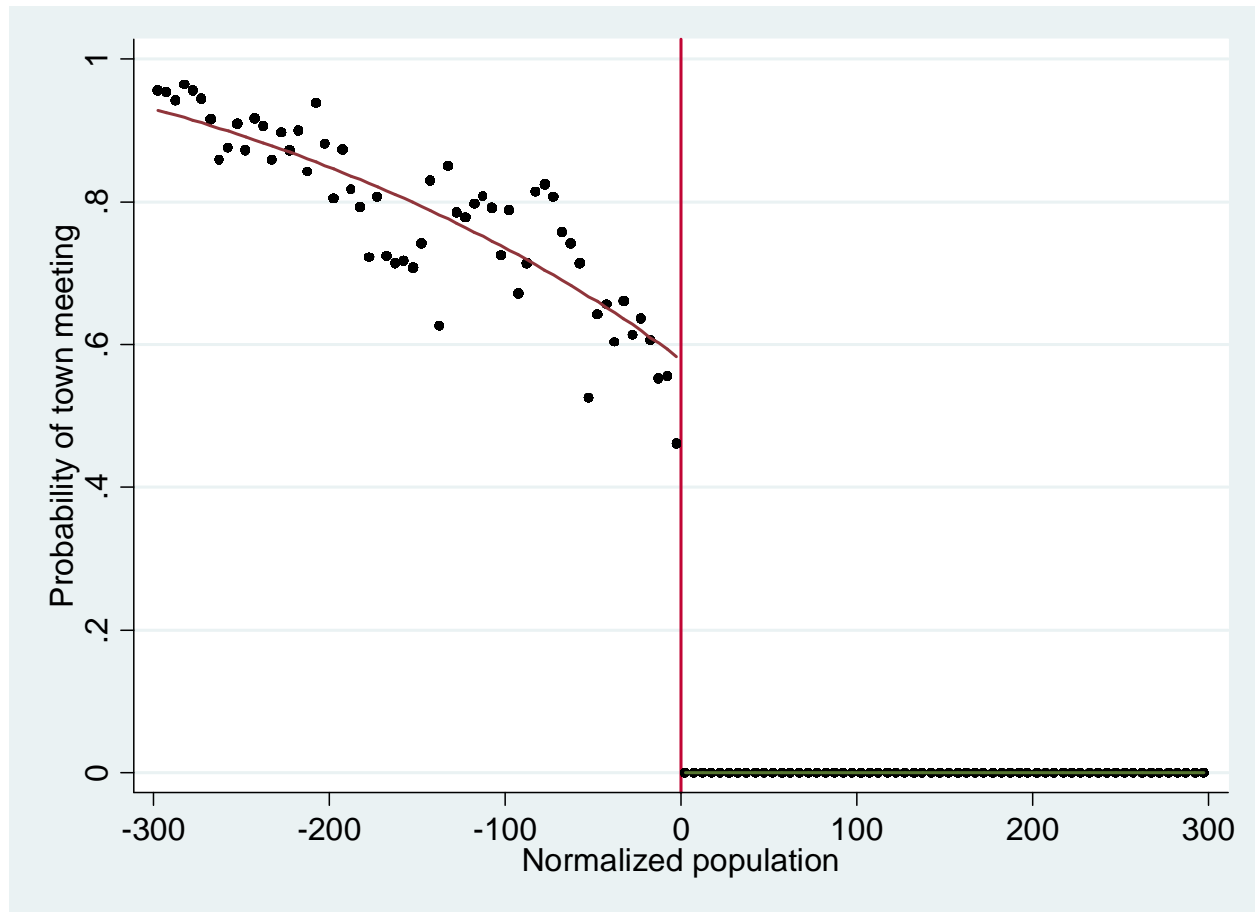
Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline

Figure 6. Density plot of treatment determining variable–population size: 1939-1950



Note. Local averages based on a binwidth of 5.

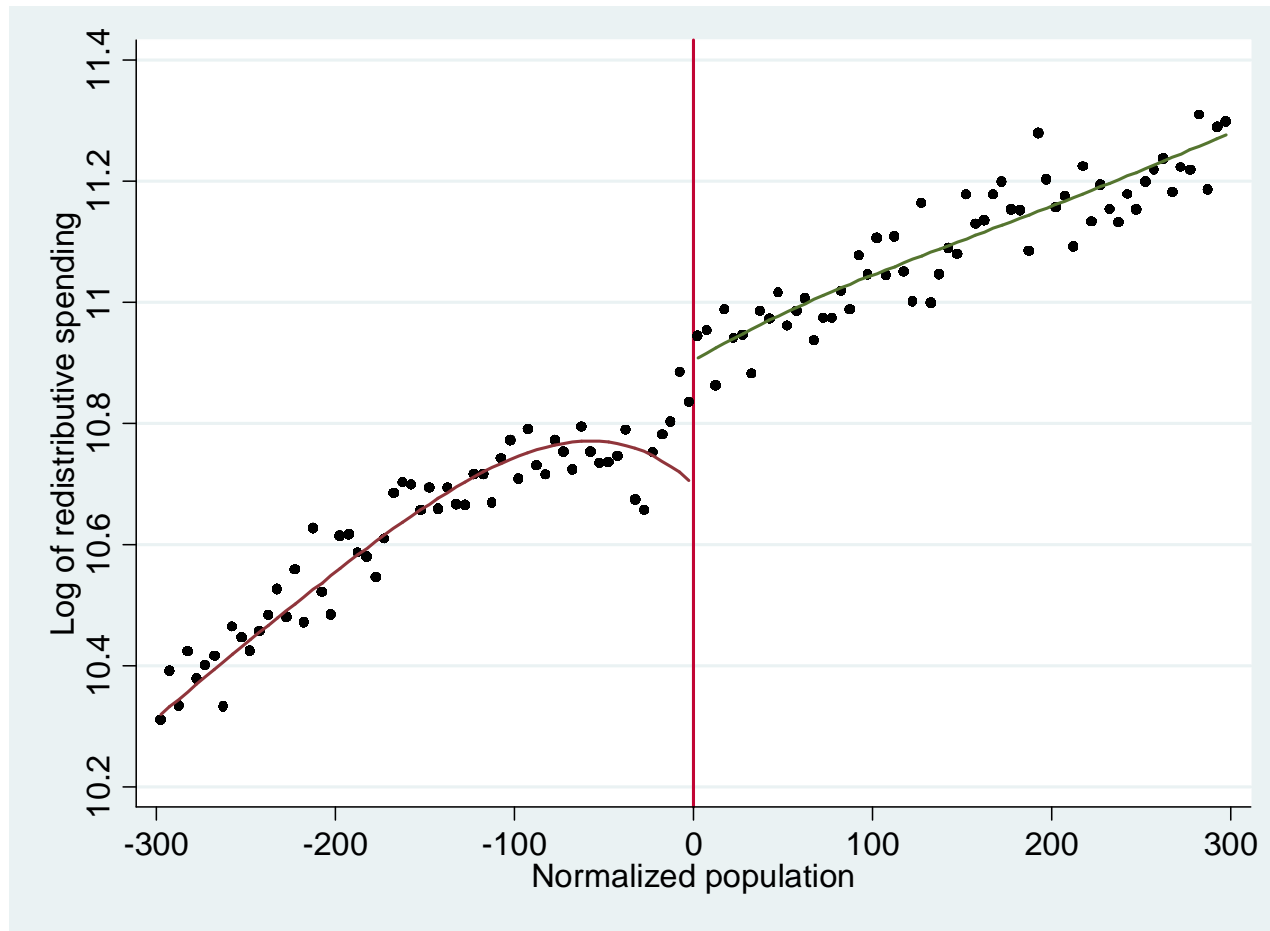
Figure 7. Relationship between the probability of having town meeting and population size: 1939-1950



Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline.

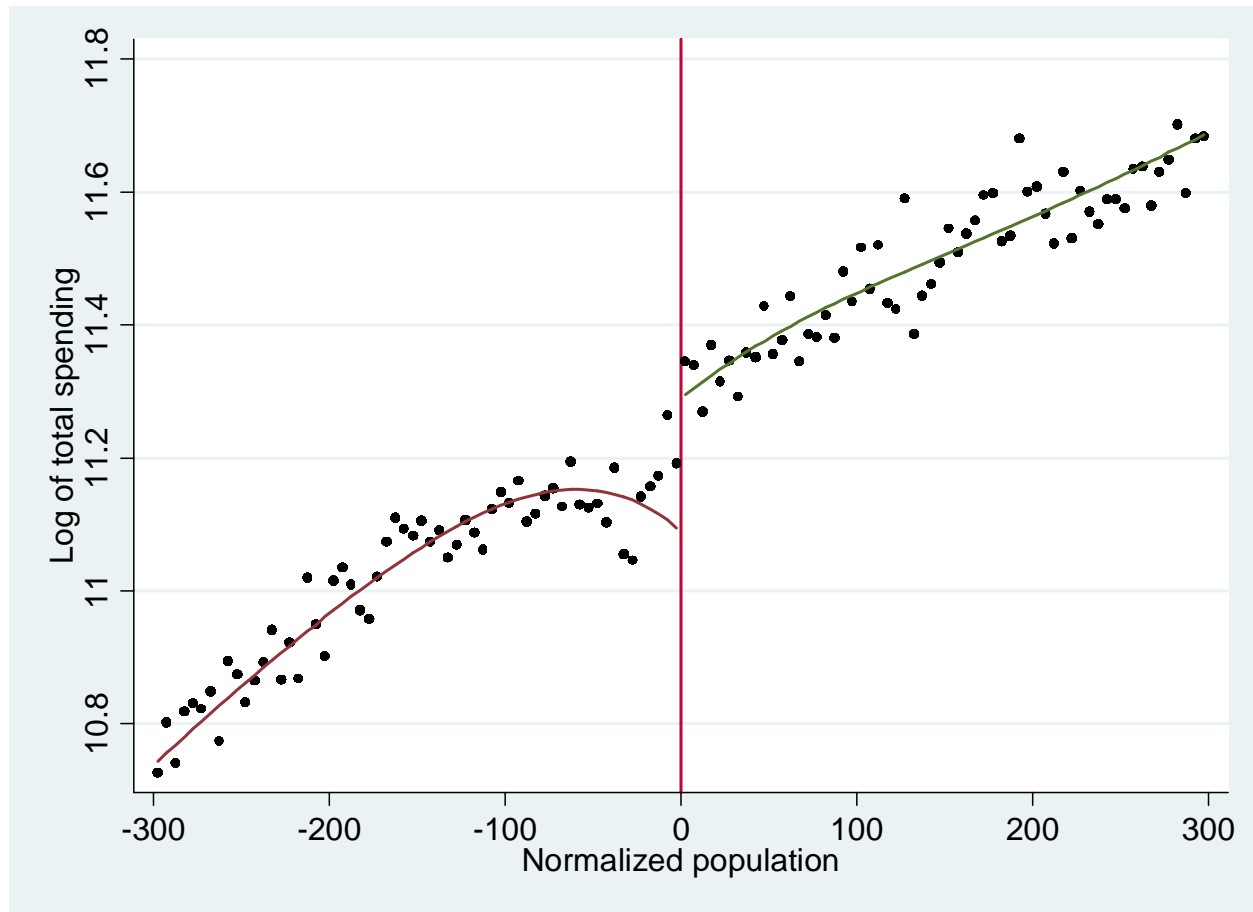


Figure 8. Relationship between redistributive spending and population size: 1939-1950



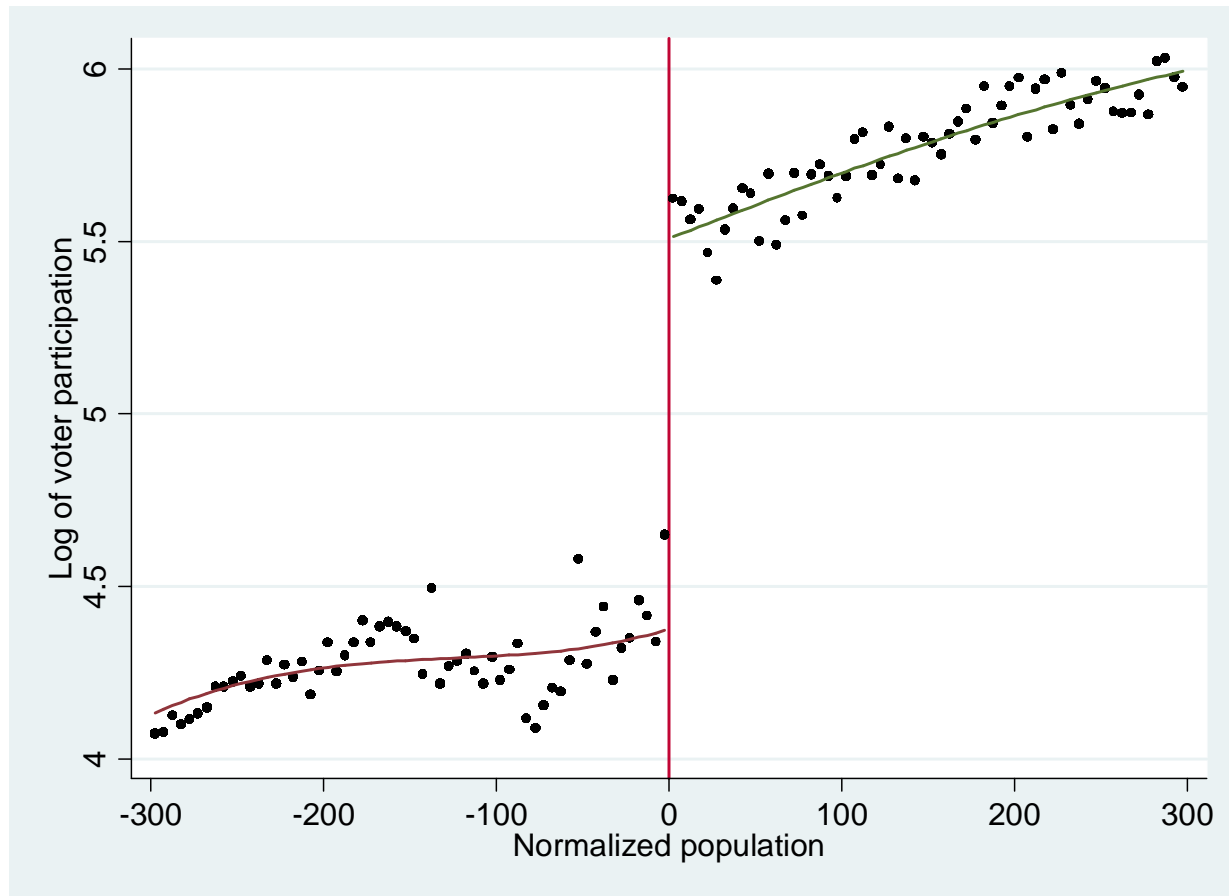
Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline. Redistributive spending includes spending on basic public education, poverty relief and child welfare.

Figure 9. Relationship between total spending and population size: 1939-1950



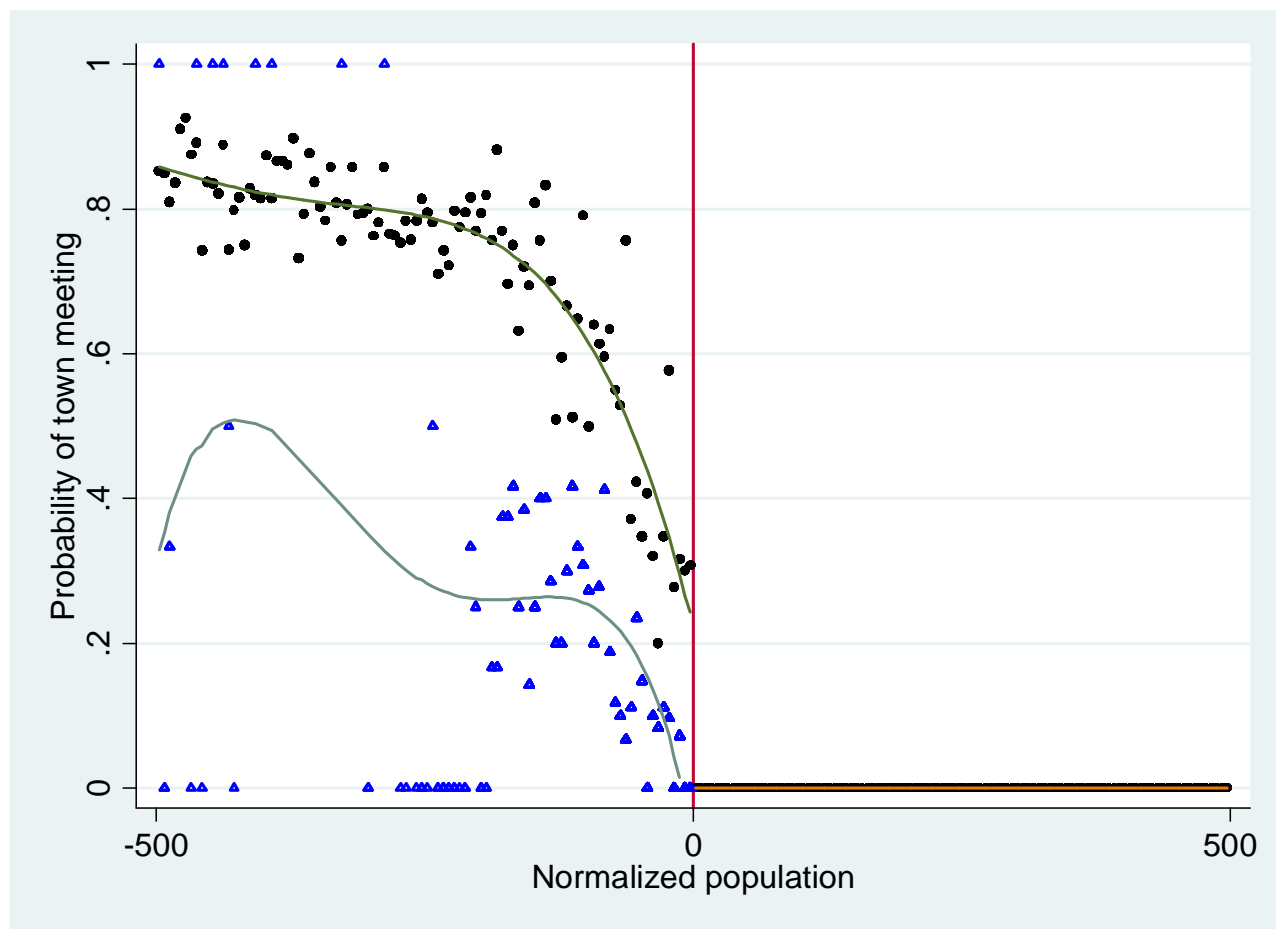
Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline

Figure 10. Relationship between political participation and population size: 1939-1950



Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline

Figure 11. Relationship between the probability of having town meeting and population size: 1939-1950

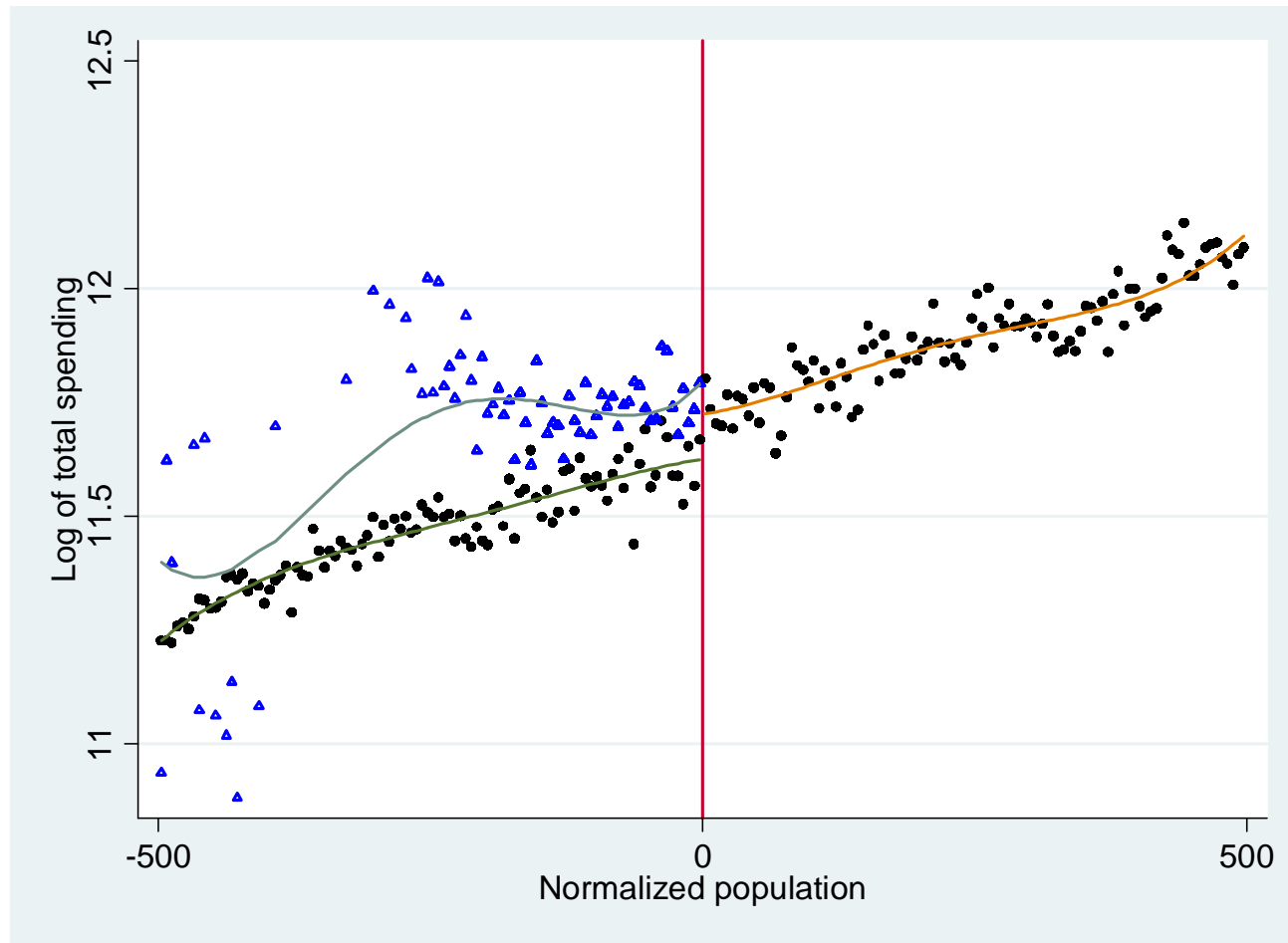


Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline.

Figure 12. Relationship between redistributive spending and population size: 1919-1938

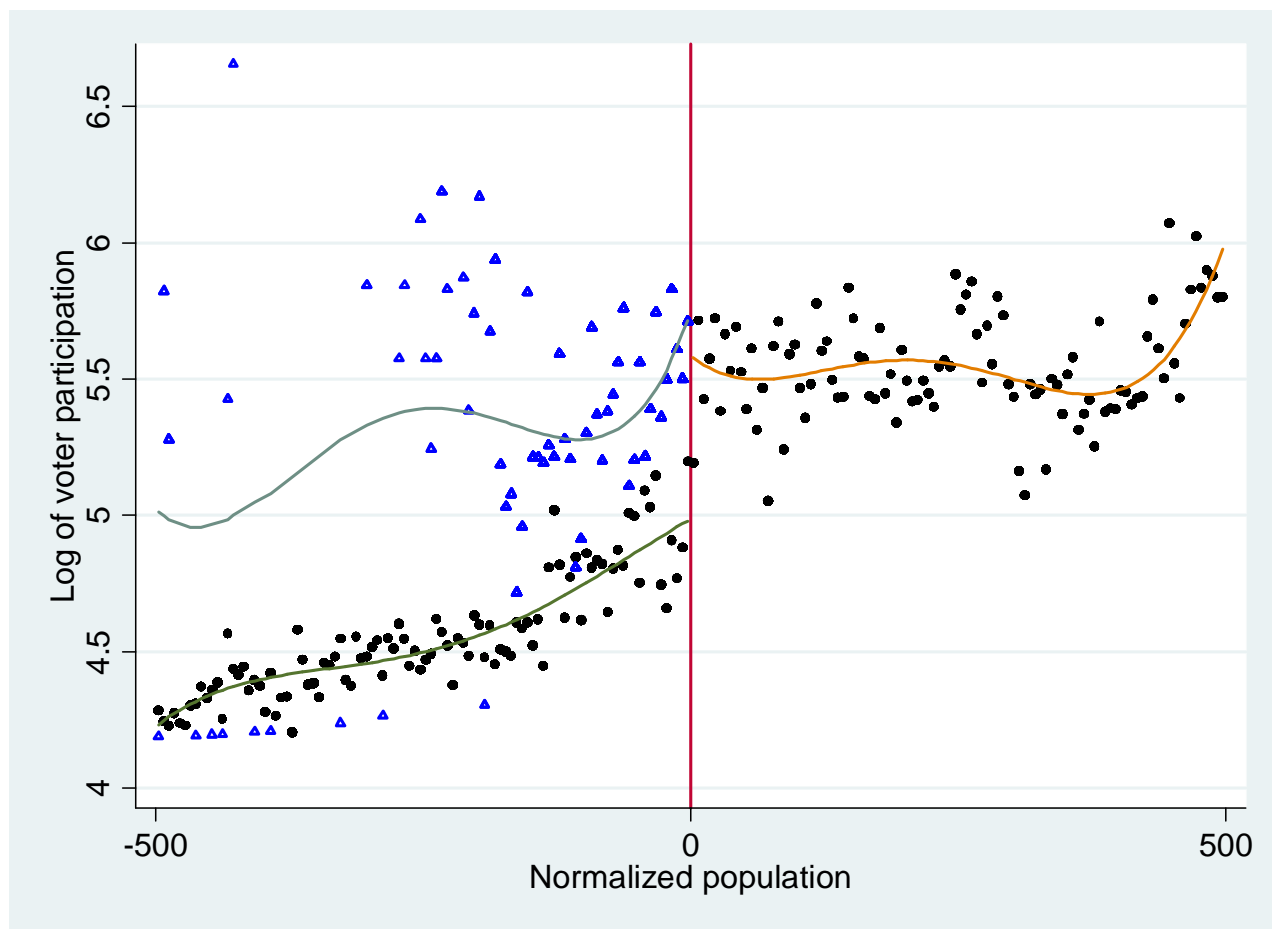


Figure 13. Relationship between total spending and population size: 1919-1938



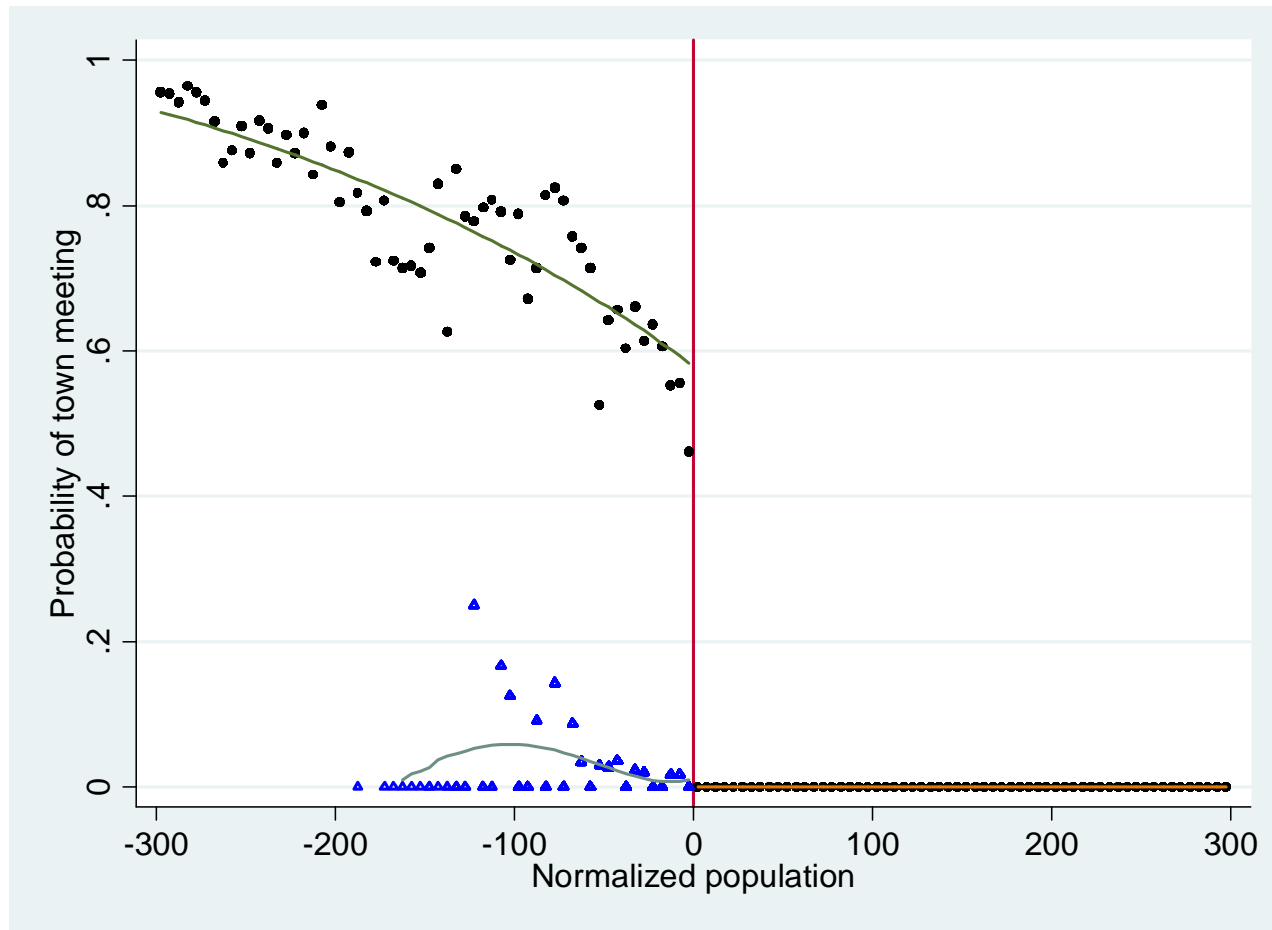
Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline

Figure 14. Relationship between political participation and population size: 1919-1938



Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline

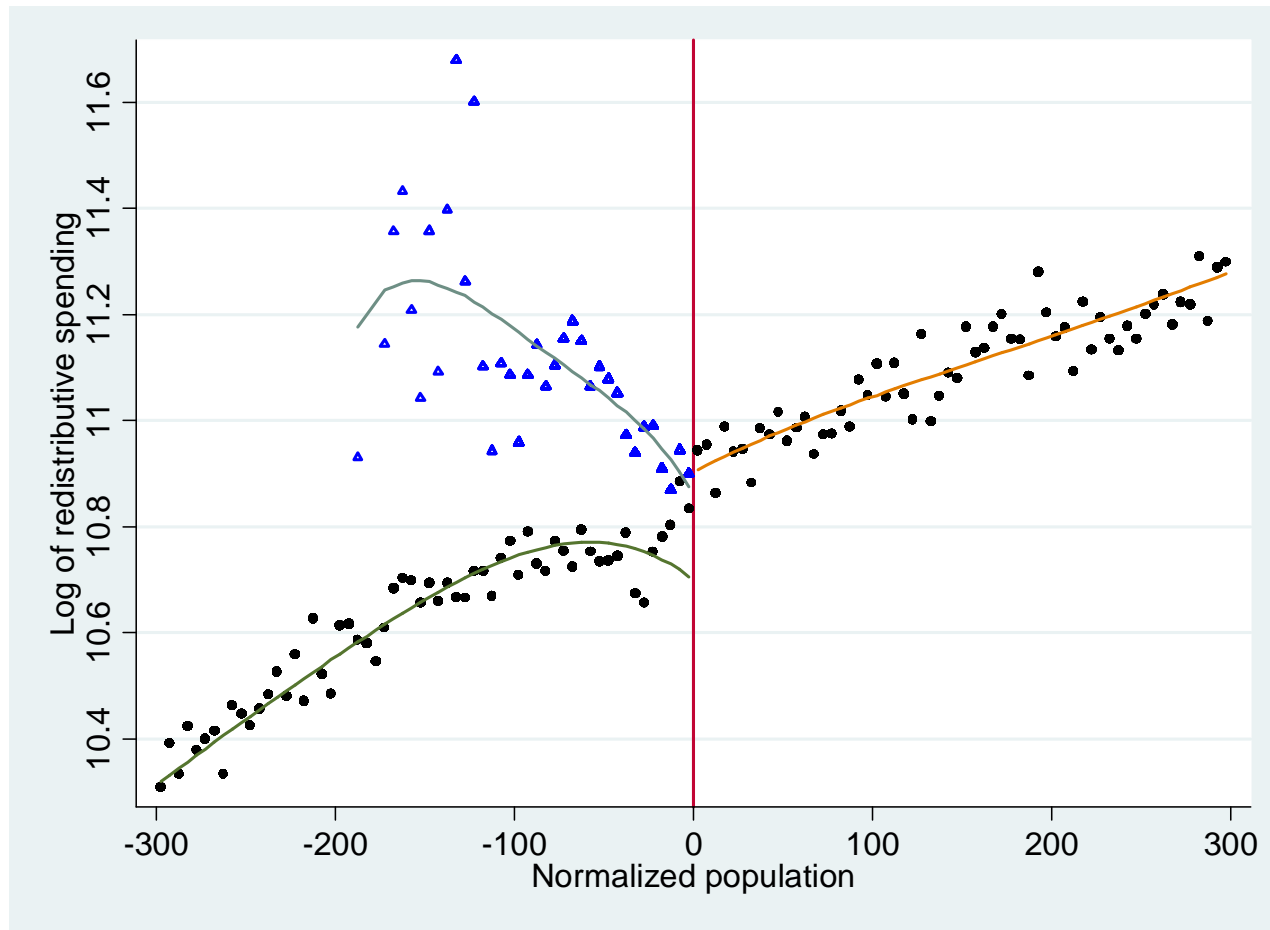
Figure 15. Relationship between the probability of having town meeting and population size: 1939-1950



Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline.

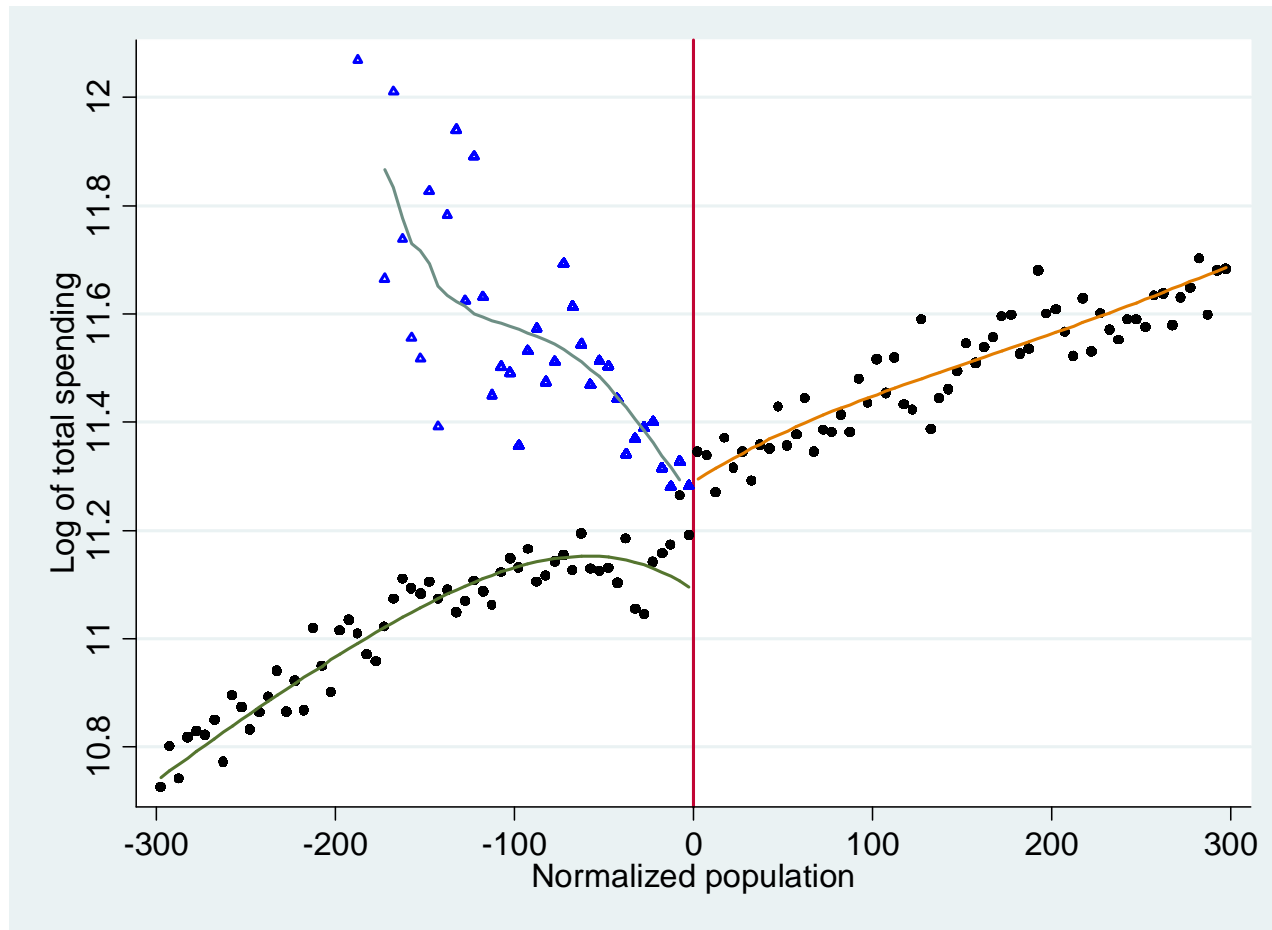


Figure 16. Relationship between redistributive spending and population size: 1939-1950



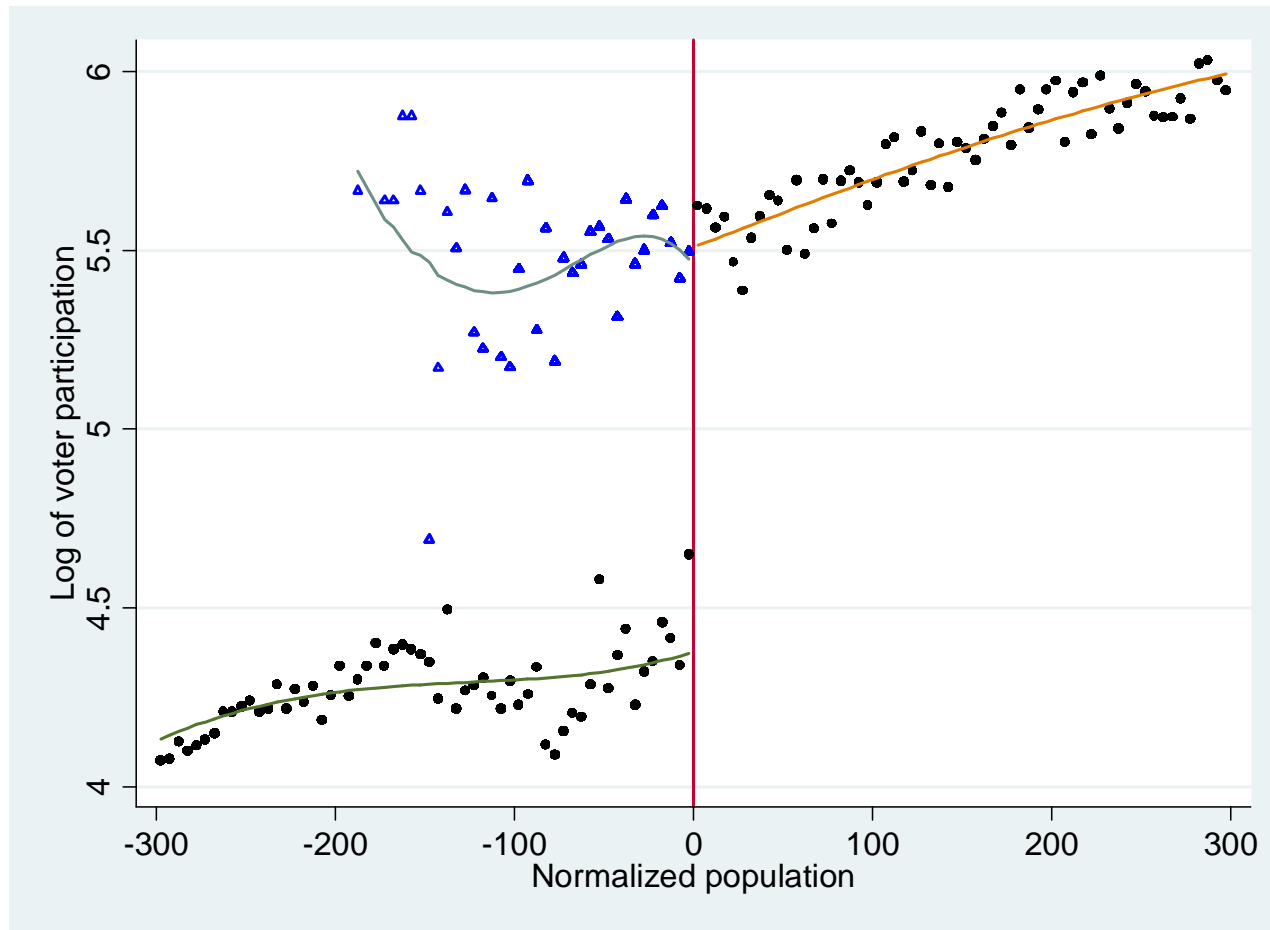
Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline. Redistributive spending includes spending on basic public education, poverty relief and child welfare.

Figure 17. Relationship between total spending and population size: 1939-1950



Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline

Figure 18. Relationship between political participation and population size: 1939-1950



Note. Local averages based on a binwidth of 5. Polynomial fit based on a fourth order spline