The Policy Consequences of Direct versus Representative Democracy: A Regression-Discontinuity Approach*

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Abstract

This paper empirically compares policy outcomes under direct versus representative democracy. We make use of a regression-discontinuity design provided by a national law in Sweden which required local governments with a population size above a certain threshold to have representative democracy while those below could choose to have direct democracy in the form of town meetings. There was also a change in the population cut off which means that we can also implement a difference-in-difference approach. The results suggest that localities with direct democracy have much lower government spending as compared to those with representative democracy. The cross-sectional RD estimates are in the order of 30-50 percent while the difference-in-difference results are about 10-13 percent.

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1. Introduction

The debate on the virtues of direct democracy has been a recurrent topic ever since the democratic system of classical democracy, or direct democracy, was used in the Greek city-state of Athens. For example, Thomas Jefferson, the third President of the United States, was clearly an advocate of a democracy that should be as direct as possible. Jefferson called the town meeting form of government "the wisest invention ever devised by the wit of man for the perfect exercise of self-government, and for its preservation."[1] Today, the interest in this issue is still extremely large as emphasized by the fact that a Google search on the word "direct democracy" gives more than 900,000 hits.

The main contribution of this paper is to empirically study the policy consequences of adopting direct versus representative democracy. As a testing ground it uses historical data, i.e., before 1950, from Swedish local governments. This setting provides unusually credible sources of variations in these two political institutions since the choice of political institutions at that time was partly determined by population size. A local government with a population size over a certain threshold was required by national law to have representative democracy while those below could choose to have direct or representative democracy. As a result, a regression-discontinuity (RD) approach can be implemented to test whether the policy impact of direct democracy differs from the impact of representative democracy. The RD design is considered to produce unusually credible estimates of a causal effect as discussed by Hahn et al (2001) and Lee (2007), among others. Moreover, in 1938 the population threshold was lowered from 1501 to 701. This means that we can also implement a difference-in-difference approach, since we can construct treatment and control groups based on whether the towns were forced to change from direct democracy to representative democracy.

The results suggest that localities with direct democracy have considerably lower government spending as compared to those with representative democracy. The cross-sectional RD estimates are in the order of 30-50 percent while the difference-in-difference results are about 10-13 percent. We interpret the cross-sectional estimates as measuring the long-term policy effect while the difference-in-difference estimates are capturing the short-term effect of a switch from direct to representative democracy.

One reason for the strikingly large short- and long-term policy consequences of having representative rather direct democracy is that we compare direct democracy in its

[1] There are a number of books that discuss the town meeting form of government; see, e.g., Bryan (2004), Mansbridge (1980) and Zimmerman (1999)

purest form, i.e., citizens participate directly in policy making through regular town meetings, to representative democracy with proportional representation. In other words, we compare a form of government where all eligible citizens of a locality meet on a regular basis to decide on economic policy to one where elected representatives instead make policy choices. Thus, if the agency problem between voters and their agents is substantial,[2] the largest discrepancy in policy outcomes would be expected to be found when comparing these two polar cases. To our knowledge, a comparison of these two forms of government has never been made in the literature although policy making by assembled voters is still used in the Unites States and Switzerland. In the U.S., many towns in New England and a number of school districts have regular town meetings. In Switzerland, *the Landsgemeinde* is a body of lawmaking by assembled voters in a handful of cantons. Many Swiss municipalities also use town meetings.

This paper is related to literatures that investigate how political institutions shape economic policies. There is, for example, a literature that studies the policy consequences of representative democracy where citizens can directly participate in the decision-making process. The two most prominent institutions are *initiative* and *referendums*.[3] This literature shows that these institutions are related to various policy outcomes (e.g., Matsusaka 1995, 2004, 2005, Matsusaka and Feld 2003). Economic effects of other dimensions of political institutions have also been investigated.[4] For example, Persson and Tabellini (1999), Lizzeri and Persico (2001) and Milesi-Ferretti et al. (2002) theoretically compare how different electoral rules lead to different fiscal policies while Persson and Tabellini (2000) compare how different forms of government (parliamentary versus presidential) lead to different fiscal policy outcomes. Persson and Tabellini (2003, 2004) create a comprehensive data set on these political institutions and investigate how different constitutional arrangements shape fiscal policies.

While the empirical results from the research program of comparative political economy are very interesting, the program faces very challenging identification problems as discussed by Persson and Tabellini (2003), Besley and Case (2003) and Acemoglu (2005). In fact, Acemoglu "questions whether this research has successfully uncovered causal effects." However, this paper shows that it is possible to compellingly estimate causal effects of

[2] See Peltzman (1992) for a similar argument that voters are fiscal conservatives.
[3] The initiative allows ordinary citizens to propose a law, qualify their proposal for the ballot by collecting a predetermined number of signatures from fellow citizens, and the final decision is made by a vote of the electorate as a whole, while referenda are laws originating with the legislature before meeting the voters for approval.
[4] There are also a large number of empirical studies that use U.S. states to address how different political institutions shape economic policies, e.g., see Besley and Case (2003) and the references cited therein.

specific political institutions by exploiting credible quasi-experimental variations in unitary countries such as Sweden. The attractiveness of using a unitary country for estimating causal effects is that the central government sets the rules of the game, i.e., political institutions, for local governments but within these sets of rules, local governments have a large degree of fiscal freedom. We have previously successfully exploited quasi experimental sources of variations from the local level in Sweden and Finland to estimate the policy consequences of various political institutions. For example, we have used regression-discontinuity designs to investigate whether council size affects government policies (Pettersson-Lidbom 2003, 2006b) and whether parties are of importance for economic policy choices (Pettersson-Lidbom 2001a, 2006a and Tyrefors 2007).

The paper is organized as follows. Section 2 describes the institutional setting. In Section 3, we describe the data and the empirical design of the study, while section 4 presents the results. Section 5 discusses the findings and concludes the paper.

2. Institutional setting

In this section, we describe the institutional setting of Swedish local governments that will provide the opportunity to implement an RD design.

The period of study is 1930 to 1950. There existed about 2,500 local governments in Sweden until 1950. The local governments were divided into cities ("städer") and other localities ("landskommuner" and "köpingar"). These local governments were general purpose jurisdictions with independent taxation rights. The two largest programs were education, which constituted about 40 percent of total spending, and social welfare services which constituted about 30 percent of total spending.[5] The ratio of aggregated local government spending out of GDP was about 8 percent in this period.

From 1919 to 1938, Swedish local governments with a population of 1,501 or more were required by national law to have representative democracy ("kommunfullmäktige") while those below could choose to have direct democracy ("kommunalstämma").[6] The introduction of the rule was part of a major constitutional reform in Sweden in 1918. The Swedish parliament then passed the legislation of universal suffrage, which put an end to a franchise based on economic resources. All individuals aged above 23 were entitled to vote in the locality where they were registered. In the original government proposal of the constitutional reform, the population threshold was set to 3,001 but the proposal was later turned down in favor of a compromise.

The two main arguments for forcing localities to have representative democracy rather than their traditional form of government, direct democracy, was that representative democracy would better reflect the preferences of the majority of voters and that politicians would make more informed decisions than voters. The first argument was related to voter turnout being rather low at "town meetings", while it was much higher in the general elections to parliament. Despite the strong majority in favor of the representative form of democracy in parliament, its members took into account the long tradition of direct democracy at the local level in Sweden and therefore, they refrained from requiring the less populous localities to have representative democracy. Nevertheless, there were recurrent debates in parliament to

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[5] Education is "folkskola", and social welfare services consist of three subprograms: "fattigvård", "barnavård" and "hälso och sjukvård" (poor relief, child care and health care).

[6] This section is based on the Swedish Code of Statutes SFS 1862:13, 1930:251, and SFS:753.

lower the population cut off and in 1938, it was reduced to 701. Finally, in 1953, all localities were required to have representative democracy.[7]

Table 1 shows the number of local governments that were required to have representative democracy, those that voluntary had it, and those that had direct democracy. There were typically rather few that chose to have representative democracy. During the period 1919 to 1946, only an average of 13 percent chose to have representative democracy.

2.1 Representative democracy

The localities with a population size over a certain threshold were required by law to have representative democracy with proportional representation, PR. The local governments below the threshold could choose to have either direct democracy in the form of a town meeting or representative democracy with PR. If they chose to have representative democracy, they were not allowed to change to direct democracy unless four years had passed since the decision to have representative democracy was taken. There was a mandatory election every fourth year for the localities with representative democracy. Thus, mandatory elections were held in 1930, 1934, 1938, 1942, 1946, and 1950. However, a locality 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). The number of council members ranged from 15 to 40 depending on the population size of a locality. Table 2 shows the number of required representatives in the local government council. Table 3 shows the voter turnout to the local government elections. The council was required to have at least three meetings. The first meeting was to be held between March 16 and April 30 and it 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 the 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 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") should consist of 5 to 11 members and it was elected by the council.

[7] See Strömberg (1974) and Wallin (2007) for a more extensive discussion of the debate in parliament for having representative democracy rather than direct democracy at the local level.

2.2 Direct democracy

The local governments that had direct democracy were required by law to have at least three town meetings ("kommunalstämma") per year. Similar to the council meetings, the first town meeting was to be held between March 16 and April 30 and it 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 the third mandatory town meeting in December. The chairman and the vice-chairman of the town meeting were elected for a four-year term. They had to be at least 25 years old. The decision process at the town meeting was that the chairman constructed 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 nobody requested a secret vote.[8] Each eligible voter attending the town meeting was entitled to represent another voter provided that she has the power of attorney do so. However, each voter could represent one eligible voter at the town meeting at most. Attendance at the town meeting was typically lower than at the local elections to the local government council. Similar to the council, the national law required a supermajority of the voters attending the meeting for many economic decisions at the town meeting. The executive body of the local government ("kommunalnämnd") was elected by the voters at the town meeting in December and it should consist of 5 to11 members.

[8] 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.

3. Data and empirical framework

In this section, we describe the data and empirical framework that will be used to estimate the impact of direct democracy and representative democracy on economic policy. We will use real spending per capita as our outcome measure. Figure 1 shows the development of spending over time. This figure shows that per capita spending remained roughly constant until 1942 when it began to increase sharply.

We will begin by discussing how to empirically identify the impact of direct democracy versus representative democracy using the cross-sectional information in the data set, which is then followed by a discussion about instead using time series information to identify the policy consequences of these two political institutions. If the results are consistent across these two approaches, this would provide unusually compelling evidence of the results from this paper having a causal interpretation since the methods make different identifying assumptions. Moreover, the results from the cross-sectional approach can be considered as capturing the long-term policy consequences of representative democracy rather than direct democracy since the equilibrium outcomes between localities with different forms of government are compared. On the other hand, the time series approach provides evidence of the short-term impact since it uses switches in the treatment status (representative and direct democracy) for identification of the treatment effect, i.e., it usually takes some time for a locality to move from one equilibrium outcome to another one.

3.1 Cross-sectional methods

The simplest way of estimating the policy consequences of having representative democracy rather than direct democracy is to run the following OLS regression on a cross section of local governments

$$SPEND_i = \alpha + \beta REPR_i + v_i \qquad (1)$$

where $REPR_i$ is an indicator variable taking the value of one if local government i has representative democracy and zero if it has direct democracy. Since we have a panel of jurisdictions over a twenty-year period, we can also estimate (1) using data for all years, i.e., pooled cross-sectional analysis. In a pooled OLS regression, it is important to take into account that there may be serial correlation within jurisdictions, which may seriously underestimate the standard errors as discussed by Case and Besley (2003). This is particularly important in the present context since both the outcome and the political institutions are very persistent over time. One of the most robust solutions (i.e., non-parametric) to the serial correlation problem is to cluster the standard errors at the town level, since the number of towns is very large (about 2,500) and the number of time series observations rather few (20) as discussed by Bertrand et al. (2004) and Wooldridge (2003). An alternative approach is to use a between OLS estimator, which effectively discards the time series information in the data set, since it only uses the variation between cross-section observations.

The parameter of interest is β which measures the mean difference in spending between the jurisdictions with representative democracy and those with direct democracy. As long as there is no correlation between the error term and *REPR*, the estimate of β will be an unbiased estimate of the average causal effect. However, this is not likely to be satisfied since there are omitted confounders such as scale effects or voter preferences. Nonetheless, we know from the previous section that the single most important factor that determines whether a locality has direct or representative democracy is its population size since localities above a certain population threshold (the threshold was 1,501 until 1938 and 701 thereafter) must have representative democracy while those below may choose between direct or representative democracy. On average, only 14 % of the localities below the population cut-off have chosen to have representative democracy (e.g., see Table 1). This suggests that if we control for population size in equation (1), the estimate of β may be unbiased. In other words, "selection on observables" or unconfoundedness,[9] i.e., $E[SPEND|REPR, POP] = E[SPEND|REPR]$ may be a reasonable assumption since we have good knowledge about the variable determining the treatment status, i.e., representative or direct democracy. A common estimating approach when unconfoundedness holds is to control for a flexible polynomial in the treatment determining variable population size, i.e.

$$SPEND_i = \alpha + \beta REPR_i + f(POP_i) + v_i \tag{2}$$

where $f(.)$ is some flexible polynomial function of population size. If $f(.)$ is correctly specified, this would give an unbiased estimate of β. However, selection on observables may not hold since some localities below the population threshold have chosen representative democracy. To solve this problem, we will make use of a fuzzy regression-discontinuity (RD) approach (e.g., see Hahn et al. 2001 or Imbens and Lemieux 2007).

[9] This assumption also goes by the name "conditional mean independence" or "ignorability of treatment".

In the fuzzy design, the ratio of the jump in the outcome, *SPEND*, at the treatment threshold to the jump in the probability of treatment, *REPR*, at the same cut-off is interpreted as the average causal effect of the treatment at the point of discontinuity. One way of estimating the average causal effect in the fuzzy RD design is to use an instrumental variable (IV) method. In this case, the treatment rule is used as an instrumental variable, i.e., $Z_i = 1[POP \geq c]$ where c is the population cut-off (either 1,501 or 701) for the requirement of a locality to have representative democracy. The IV approach can be expressed as two reduced form relations. One relationship is between the treatment and the instrument, i.e., the "first stage"

(3) $$REPR_i = b + \theta Z_i + h(POP) + n_i,$$

while the other relationship is between the outcome and the instrument; i.e.,

(4) $$SPEND_i = d + \pi Z_i + g(POP) + r_i.$$

As shown by Hahn et al. (2001), the estimate of the average effect at the point of discontinuity will be the ratio between the jump in of the outcome π to the jump in the probability of treatment θ, i.e., $\hat{\beta}^{IV} = \frac{\hat{\pi}}{\hat{\theta}}$.[10] In other words, the IV estimator is a Wald estimator. Since there are two treatments cut-offs: POP=1,501 and POP=701, two different IV estimates of the effect of direct versus representative democracy on spending can be obtained.

An important concern in an RD design is that the number of observations should be sufficiently large close to the treatment threshold. Figure 2 shows the distribution of the population size for the year 1931 for the localities with less than 2,500 inhabitants (82 % of the local governments have a population size of less than 2,500). This figure shows that at the 701 threshold, there are more than 100 jurisdictions slightly "below" and also 100 localities slightly "above". In contrast, there are only 50 jurisdictions slightly "below" and 50 slightly "above" at the 1501 threshold. This means that the statistical power will be much higher for the RD approach at the 701 threshold. Figure 2 also reveals that there is no indication of

[10] A useful way of thinking about this particular way of constructing an instrumental variable is to make a comparison with a randomized experiment where there is only partial compliance with the treatment protocol. Since the political institution can be partly chosen by the towns, there is only going to be partial compliance with the treatment protocol. In such a case, the assigned treatment level can serve as an instrumental variable for the actual treatment level, which is precisely the reason why the law can be used to construct an instrumental variable for the political institutions.

manipulation of the treatment determining variable, since the number of jurisdictions on either side of the treatment threshold is about the same (e.g., McCrary2005). This is also to be expected since the population figures that determine the treatment status are based on the administrative records collected by the central government.

3.2 Time series methods

In this section, we describe how the time-series information in the data can be used to identify the impact of representative democracy versus direct democracy on spending. As previously noted, in 1938 the threshold that determines whether a locality must have representative democracy was lowered from 1,501 to 701. This change in the rule forced 505 jurisdictions to switch from direct democracy to representative democracy (see Table 1). In addition, there are also 36 jurisdictions that increased their population size during the periods 1931 to 1938 or 1939 to 1950 so that they were required to change their treatment status from direct to representative democracy. Moreover, there are also 15 localities that changed from representative democracy to direct democracy since their population size has decreased below the treatment threshold. We can use this variation in political institutions across time to estimate their impact on policy. A standard approach is to use a fixed effect or a difference-in difference approach, i.e.,

$$(5) \qquad SPEND_{it} = \mu_i + \lambda_t + \delta REPR_{it} + r_{it}$$

where i indexes localities and t time, μ_i is a locality fixed effect and λ_t is a time fixed effect. The inclusion of these fixed effects means that the identification of the parameter δ – the effect of direct versus representative democracy – will only be based on the within jurisdictional variation. Similar to the cross-sectional approach, it might be necessary to control for population size in (5) for the estimate to be unbiased since the variation in these political institutions will largely depend on the population size. Once again, a number of jurisdictions have endogenously chosen to change their treatment status which may introduce a bias in the estimates. To solve this problem, we can once more use an instrumental variable approach where we define the instrument as Z_{it} =1 if a local government was required to have representative democracy during the period 1931 to 1950, and 0 otherwise. The first-stage regression will therefore be

$$(6) \qquad REPR_{it} = \mu_i + \lambda_t + \psi Z_{it} + h_{it}.$$

The inclusion of fixed locality and time effects implies that the instrument will only capture rule triggered changes in treatment status: i.e., the change in the rule in 1938 and all other mandatory changes in treatment status before and after 1938 due to population changes. Therefore, we will also include controls for population size in equation (6). As is standard in many difference-in-difference set ups, the control variable will be interacted with a full set of time-fixed effects, thereby allowing the treatment determining variable population size to have a different impact on the outcome each year.

4. Results

In this section, we report the results from the cross-sectional and time series methods as discussed in the previous section. We start by reporting the results from the cross-sectional methods, followed by the results from the time series methods.

4.1 Cross-sectional results

Table 4 shows these results from using OLS on pooled cross-sectional data over the period 1931 to 1950. The total number of localities is 2,566. To address serial correlation problems in the pooled OLS regression, the standard errors are clustered at the locality level as previously discussed. The outcome variable is real per capita spending and it is expressed in logarithmic form, which means that the estimated effect of having representative democracy versus direct democracy will have a percentage interpretation. The estimated effect without any controls in Column 1 is strikingly large, 0.31, i.e., the estimated effect is therefore 100*[exp(0.31)-1]=37 percent. However, the effect is reduced to about 0.138 (Column 2), i.e. about 13.8 percent, when we control for a third-order polynomial in population size and a full set of time-fixed effects. Nevertheless, the estimated effect does not change to any large extent when we control for a fourth- or a fifth-order polynomial population size in Columns 3 and 4, respectively. The insensitivity of the estimated effect to the functional form specifications of the relation between population size and the outcome suggests that the unconfoundedness assumption might hold in the data, i.e. conditional on population size there is no correlation between error term and treatments: representative or direct democracy. To further probe the functional form issues of the treatment determining variable, we restricted the data to the localities with a smaller population size than 2,500 but a larger size than 400 to ensure that the estimated effect is not due to localities with a relatively large or small population. We still include a fifth-order polynomial in population size and a full set of time effects in the specification. The estimate effect of 11.3 percent in Column 5 is once more quite similar to the previous estimates. As a final robustness check, we present an OLS estimate only based on the between variation in the data, i.e., we estimate an OLS regression on time-averaged data. The result is displayed in Column 6 and it shows that the estimated effect is 12.7; once more, not different from the previous point estimates. However, the standard error of 0.019 is almost twice as large as the clustered ones in Columns 1 to 5, which shows there to be some statistical efficiency to be gained by using pooled OLS rather than the between estimator. To sum up, the results from the OLS specifications suggest that a local government with representative democracy spends, on average, about 12 percent more than a local government with direct democracy. The estimated effect is robust to functional form issues of the treatment determining variable, i.e., the relation between spending and population size. Nevertheless, since a local government can partly decide to have representative democracy rather than direct democracy, we turn to the fuzzy RD approach.

Tables 5 to 7 show the results from the fuzzy RD design for the treatment threshold at population size 701 for the period 1939 to 1950. These tables differ depending on the sample around the point of discontinuity that is being used and how the functional form of the treatment determining variable, population size, is specified. In Table 5, the sample is ±30, i.e., the localities with a population size between 671 and 731, while the sample is increased to ±100 in Table 6. In table 7, the sample is further increased to ±700 from the discontinuity point 701, i.e., the localities with less than 1401 inhabitants. In the ±30-sample there are 222 different localities, in the ±100 there are different 403 localities and there are 1605 different localities in the ±700 sample. We allow the treatment determining variable, population size, to have different functional forms to the left and right of the discontinuity, i.e., the polynomial in population size is interacted with the treatment indicator (e.g., see Lee 2007).

Starting with the smallest sample around the discontinuity, Column 1 shows the reduced form (equation 4), namely regressing log per capita spending on the instrument 1[POP≥701] without any control variables except fixed year effects. The time-fixed effects are included since we are using repeated (yearly) observations on the outcome for each locality. For this reason, we also cluster the standard errors at the jurisdictional level. The estimated effect is 7.1 percent and it is highly statistically significant from zero. The corresponding first-stage regression (equation 3) is displayed in column 3. The estimated probability of treatment is 25 percentage points and it is highly statistically significant from zero. Thus, there is a significant jump in both spending and the probability of treatment at the point of discontinuity. As previously explained, to compute the casual effect of having representative rather than direct democracy on spending, one needs to take the ratio between the estimates of the reduced form (column 1) with the probability of treatment (column 3). Column 5 shows that this IV/Wald estimate is 0.28, i.e. 32.7 percent. When we control for a linear spline in population size, the reduced form estimate increases slightly to 8.3 percent while the first-stage estimate decreases somewhat to 0.217 relative to the corresponding specifications without population controls. Consequently, the IV/Wald estimate becomes larger, 0.38 (47 percent), when we control for the treatment determining variable. Nevertheless, both Wald estimates in columns 5 and 6 are statistically significantly different from zero at the 1% level and economically very large. Here, it is important to point out that it does not matter for the results whether we use log(per capita spending) or log(total spending) as our outcome measure. For example, if we use total spending with a linear spline in population size, we will get a reduced form estimate of 0.083 with a standard error of 0.022 which is identical to the estimate in Column 2. This similarity should not come as a surprise since we are comparing localities with essentially the same population size at the point of discontinuity, i.e., 700 to 701.

To further probe the appropriateness of the fuzzy RD design, the RD graphs with the predicted regression line from a fourth-order polynomial spline for spending and the probability of treatment are displayed in Figures 3 and 4, respectively. These graphs have been constructed by ranking the localities on both sides of the threshold based on their population distance from the threshold. Every group is cut by the 500th rank value, giving us approximately 500 observations in each group. Consequently, the distance along the x-axis between each group will not be equal.[11] Then, we display the average value of spending for each group. These figures clearly show that there is a marked jump in both spending and the probability of treatment at the point of discontinuity. Moreover, the size in the jumps in the graphs corresponds to the estimated size of the jumps in Table 5. Thus, the RD graphs give further support for a causal interpretation of the RD results.

In Tables 6 and 7, we have also used the fuzzy RD design for larger samples around the point of discontinuity and we also use a more flexible specification of the treatment determining variable, namely a fourth-order spline in the population size. These two tables show that the reduced form estimate is not particularly sensitive to the sample nor to the functional form of the treatment determining variable since the estimate is in the range 6.9 to 8.5 percent. The estimate of the probability of treatment is also fairly robust since it ranges from 19.6 to 27.5. As a result, the IV/Wald estimate is still highly statistically significant and substantial. The estimate effect is in the range from 0.249 in the largest sample (Table 7) to 0.429 in the smaller sample (Table 6).

As a final check on the RD design at this treatment threshold, we have constructed a "falsification" exercise, namely we have constructed an RD graph for spending for the previous period when the treatment cut off was 1,501, i.e., until 1938. We would not

[11] Another way of constructing an RD graph is to fix the bin widths on the horizontal axis. However, as depicted in figure 2, the number of local governments is not evenly distributed. Thus, creating groups based on their ranking value and holding group size roughly constant yields a more accurate measure of average spending in a group, since it is not based on one or few observations which could be the case if we fix the bin widths. Naturally, if the distribution were uniform, the two methods would give the same result.

expect to see a jump in spending at the population size 701 during this period since the rule did not apply. Indeed, there is no evidence of a jump in spending as can be seen from Figure 5. Thus the absence of a jump in Figure 5 is therefore in sharp relief to the results from Figure 3. Moreover, when we regress log per capita spending on the indicator 1[$POP \geq 701$], the estimate is 0.013 with a standard error of 0.028. Thus, this falsification exercise provides additional evidence for a causal interpretation of our findings.

We now turn to the results from the other treatment rule that was in force until 1938, i.e., the population cut off at 1,501. We will follow the same procedure as with the previous fuzzy RD analysis, i.e., we report results from three different samples and with different functional forms of the treatment determining covariate. It is important to keep in mind that the number of jurisdictions that are close to this threshold is much lower than at the 701 threshold (see Figure 2). As a result, to have as many localities in the sample as in the previous RD, the interval around the point of discontinuity must be much larger. For example, there were 222 different localities in the smallest sample, ±30, in the previous RD analysis, while we need to increase the interval to ±100 in order to get about the same number of localities, i.e., 211. Thus, this interval is more than three times as large. Table 8 presents the results from the ±100 sample, i.e., the localities with a population size between 1,401 and 1,601, Table 9 displays the results from the ±250-sample, and Table 10 shows the results from the ±1000 sample. As expected, the estimated effects are not very precisely measured. Specifically, the reduced form outcome is never statistically different from zero in any of these tables. There is, however, a significant jump in the probability of treatment. The evidence provided by the RD graphs gives similar results.[12] In future work, we will extend the data back to 1919 when the rule was introduced to get a more precise estimate of the treatment effect. Moreover, this makes it possible to construct a similar refutability exercise as discussed above, namely there should be no jump in spending at the 1,501 threshold before the rule came into place in 1919.

4.2 Time-series results

In this section, we present the results from the time series methods described in section 3, i.e., we use a fixed effect or a difference-in-difference approach combined with an instrumental variable approach. As previously noted, the major switch from direct to representative

[12] Once more, we have ranked the localities on both sides of the threshold, based on their distance from 1,501. Every group is cut by the 500th rank value, giving us approximately 500 observations in each group. We then display the average value of spending for each group.

democracy came from the change in the treatment rule in 1938 when the requirement for representative democracy was lowered from the population level of 1501 to 701. This suggests that the sample can be divided into treatment and control groups. The localities with a population size between 701 and 1501 constitute the treatment group, since they were forced by law to switch from direct to representative democracy in 1939. Consequently, the localities with a population size above 1501 or below 701 constitute the control group since they were not forced to switch treatment status, i.e. direct or representative democracy. Therefore, we will define two different control groups: the localities with a population size between 0 and 700, and the localities with a population size between 1501 and 2500. To take into account that some localities in both the treatment and the control groups have endogenously chosen to have representative democracy, we will use an IV approach, that is, we use the rule which required a locality to have representative democracy as an instrumental variable (equations 5 and 6). Table 11 shows the results from the sample including both control groups and treatment group, i.e., all localities with a population size below 2,500. Table 12 shows the results from the sample where only the control group with less than 701 inhabitants is being used, i.e., the sample consists of all localities with a population of less than 1501, while Table 13 displays the results for the control group with a population size between 1501 and 2500. All reported standard errors in these tables are also clustered at the locality level.

Starting with the sample where both control groups are included, the first column in Table 11 shows the reduced form estimate for spending per capita on the instrument. This specification only includes a full set of locality and time fixed effects. The estimated effect is 2.1 percent and it is highly statistically significant from zero. When we control for the treatment determining variable by the interaction of a fifth-order polynomial in population size interacted with a full set of time fixed effects (the impact of population size is allowed to vary across years), the estimated effect becomes slightly larger, i.e., 2.6 percent (Column 2). The first-stage estimate without any controls is displayed in Column 3. This estimate is also highly statistically significant and it suggests that the probability of having representative democracy increases by 57 percentage points. However, when we control for a fifth-order polynomial in population size interacted with the time fixed effects, the first-stage effect is reduced to 26 percentage points. When we scale the reduced form estimates with the first-stage estimates, the IV/Wald estimates become 3.7 percent for the specification without population controls while the estimate is 10.2 percent with controls for population size. Thus, it seems to be of importance to take into account the effect of the treatment terming variable [fackterm?] on the outcome in the fixed-effect analysis.

To check whether the choice of control group is of importance for the results, Table 12 shows the results when we only use the control group with localities with 700 inhabitants at most, while Table 13 presents the results from the other control group with a population size between 1501 and 2500. Both these tables show a statistically significant reduced form effect of similar size (2.7 vs. 3.0 percent) and a strong first stage of roughly similar magnitude (0.240 vs. 0.217). These results are similar to the results in Table 11 (columns 2 and 4) and therefore the IV/Wald estimates in Tables 12 and 13 are of similar magnitude (12.7 and 12.0 percent). Thus, the results are robust to the choice of control group, which supports a causal interpretation of the results.

5. Conclusions

In this paper, we investigate the policy consequences of direct versus representative democracy using data from Swedish local governments during the period 1930 to 1950. Thanks to a rule which required the localities with a population size above a certain threshold to have representative democracy while those below could choose to have direct democracy regression, it is possible to implement a regression discontinuity (RD) approach. Moreover, we can also use a difference-in-difference approach since the threshold was changed during the period of study. The results suggest that localities with direct democracy have considerably lower government spending as compared to those with representative democracy. The cross-sectional RD estimates are in the order of 30-50 percent while the difference-in-difference results are about 10-13 percent. We interpret the cross-sectional estimates as measuring the long-term policy effect, while the difference-in-difference estimates capture the short-term effect of a switch from direct to representative democracy.

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

Election year	Representative democracy		Direct democracy	
	Mandatory	Voluntary		Proportion (%) of localities below the cut-off with direct democracy
1919	870	67	1469	95
1922	889	117	1398	92
1926	887	147	1377	90
1930	873	192	1354	87
1934	867	274	1273	82
1938	1617	53	739	93
1942	1576	135	668	83
1946	1524	199	662	77

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 local elections 1919 to 1946.

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

Table 4. OLS estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Representative democracy =1	0.312***	0.138***	0.127***	0.114***	0.113***	0.127***
	(0.011)	(0.012)	(0.012)	(0.012)	(0.013)	(0.019)
Time fixed effects	No	Yes	Yes	Yes	Yes	-
Polynomial in population size	No	Third order	Fourth order	Fifth order	Fifth order	Fifth order
Sample	Full	Full	Full	Full	400<POP<2,500	Full
R^2	0.117	0.474	0.478	0.480	0.401	0.411
Observations	48,619	48,619	48,619	48,619	32,718	2,566

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level

Table 5. RD estimates at the treatment threshold 701 for the period 1939-1950: the ±30-sample.

	Reduced form		First stage		IV/Wald estimates	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy =1 if pop≥701	0.071***	0.083***	0.252***	0.217***		
	(0.020)	(0.022)	(0.035)	(0.038)		
Representative democracy =1					0.283***	0.383***
					(0.083)	(0.113)
Polynomial in population size	No	Linear spline	No	Linear spline	No	Linear spline
Number of localities in sample	222	222	222	222	222	222
Observations	986	986	986	986	986	986

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population between 671 and 731.* Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level

Table 6. RD estimates at the treatment threshold 701 for the period 1939-1950: the ±100-sample.

	Reduced form		First stage		IV/Wald estimates	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy =1 if pop≥701	0.084***	0.085***	0.196***	0.216***		
	(0.022)	(0.025)	(0.039)	(0.039)		
Representative democracy =1					0.429***	0.397***
					(0.130)	(0.130)
Polynomial in population size	Linear spline	4^{th} order spline	Linear spline	4^{th} order spline	Linear spline	4^{th} order spline
Number of localities in sample	403	403	403	403	403	403
Observations	3,262	3,262	3,262	3,262	3,262	3,262

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population between 601 and 801. All specifications include a fourth-order spline in the treatment determining variable population size.* Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level

Table 7. RD estimates at the treatment threshold 701 for the period 1939-1950: the ±700-sample.

	Reduced form	First stage	IV/Wald estimates
	(1)	(2)	(3)
Dummy =1 if pop≥701	0.069***	0.275***	
	(0.024)	(0.039)	
Representative democracy =1			0.249***
			(0.091)
Number of localities in sample	1,605	1,605	1,605
Observations	18,512	18,512	18,512

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population of less than 1,401. All specifications include a fourth-order spline in the treatment determining variable population size.* Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 8. RD estimates at the treatment threshold 1501 for the period 1931-1938: the ±100-sample.

	Reduced form		First stage		IV/Wald estimates	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy =1 if pop≥1501	-0.026	-0.078	0.231	0.238		
	(0.032)	(0.058)	(0.034)	(0.054)		
Representative democracy =1					-0.113	-0.326
					(0.142)	(0.270)
Polynomial in population size	No	Linear spline	No	Linear spline	No	Linear spline
Number of localities in sample	211	211	211	211	211	211
Observations	1,073	1,073	1,073	1,073	1,073	1,073

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population between 1,401 and 1,601. Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level

Table 9. RD estimates at the treatment threshold 1501 for the period 1931-1938: the ±250-sample.

	Reduced form		First stage		IV/Wald estimates	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy =1 if pop≥1501	-0.056	-0.074	0.123	0.249		
	(0.041)	(0.058)	(0.047)	(0.056)		
Representative democracy =1					-0.452	-0.297
					(0.400)	(0.260)
Polynomial in population size	Linear spline	4^{th} order spline	Linear spline	4^{th} order spline	Linear spline	4^{th} order spline
Number of localities in sample	420	420	420	420	420	420
Observations	2,778	2,778	2,778	2,778	2,778	2,778

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population between 1251 and 1851.* Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level

Table 10. RD estimates at the treatment threshold 1501 for the period 1931-1938: the ±1000-sample.

	Reduced form	First stage	IV/Wald estimates
	(1)	(2)	(3)
Dummy =1 if pop≥1501	-0.023	0.250	
	(0.036)	(0.046)	
Representative democracy =1			-0.089
			(0.148)
Number of localities in sample	1,578	1,578	1,578
Observations	12,056	12,056	12,056

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population between 501 and 2,501. All specifications include a fourth-order spline in the treatment determining variable population size * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 11. Fixed effect instrumental IV estimates: localities with a population size below 2,500.

	Reduced form		First stage		IV/Wald estimates	
	(1)	(2)	(3)	(4)	(5)	(6)
Dummy =1 if required to have representative democracy	0.021***	0.026***	0.572***	0.258***		
	(0.006)	(0.009)	(0.016)	(0.022)		
Representative democracy					0.037***	0.102***
					(0.010)	(0.037)
Population	No	5^{th} order×year	No	5^{th} order×year	No	5^{th} order×year
Number of localities in sample	2081	2081	2081	2081	2081	2081
Observations	39,706	39,706	39,706	39,706	39,706	39,706

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population below 2,500. Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 12. Fixed effect IV estimates: localities with a population size below 1,501.

	Reduced form	First stage	IV/Wald estimates
	(1)	(2)	(3)
Dummy =1 if required to have	0.030***	0.240***	
representative democracy	(0.011)	(0.025)	
Representative democracy			0.127***
			(0.044)
Number of localities in sample	1,683	1,683	1,683
Observations	31,676	31676	31,676

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population below 1,500. All specifications include a fifth-order spline in the treatment determining variable population size interacted with a full set of time fixed effects. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 13. Fixed effect IV estimates: localities with a population size between 701 and 2,500.

	Reduced form	First stage	IV/Wald estimates
	(1)	(2)	(3)
Dummy =1 if required to have	0.027**	0.217***	
representative democracy	(0.013)	(0.032)	
Representative democracy			0.120**
			(0.059)
Number of localities in sample	1,359	1,359	1,359
Observations	16,536	16,536	16,536

Note- Huber-White standard errors allowing for clustering at the local government level are in parentheses. The sample is restricted to localities with a population between 701 and 2,500. All specifications include a fifth-order spline in the treatment determining variable population size interacted with a full set of time fixed effects. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Figure 1. The development of spending per capita 1931 to 1950

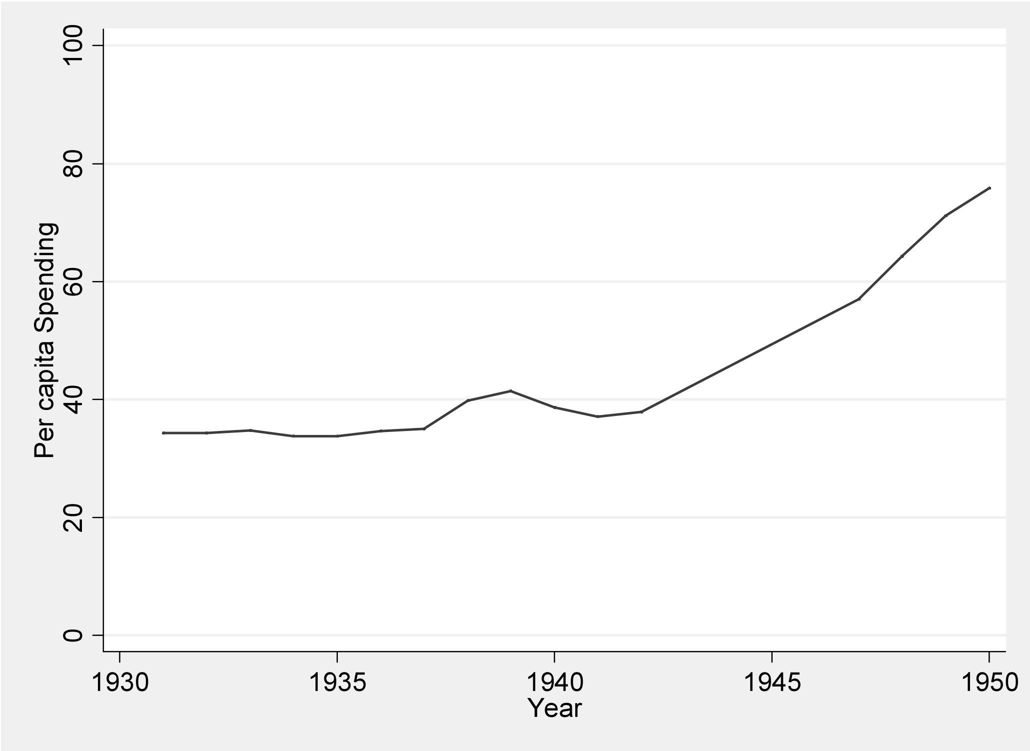

Figure 2. The distribution of population size for local governments with a smaller population size than 2,500.

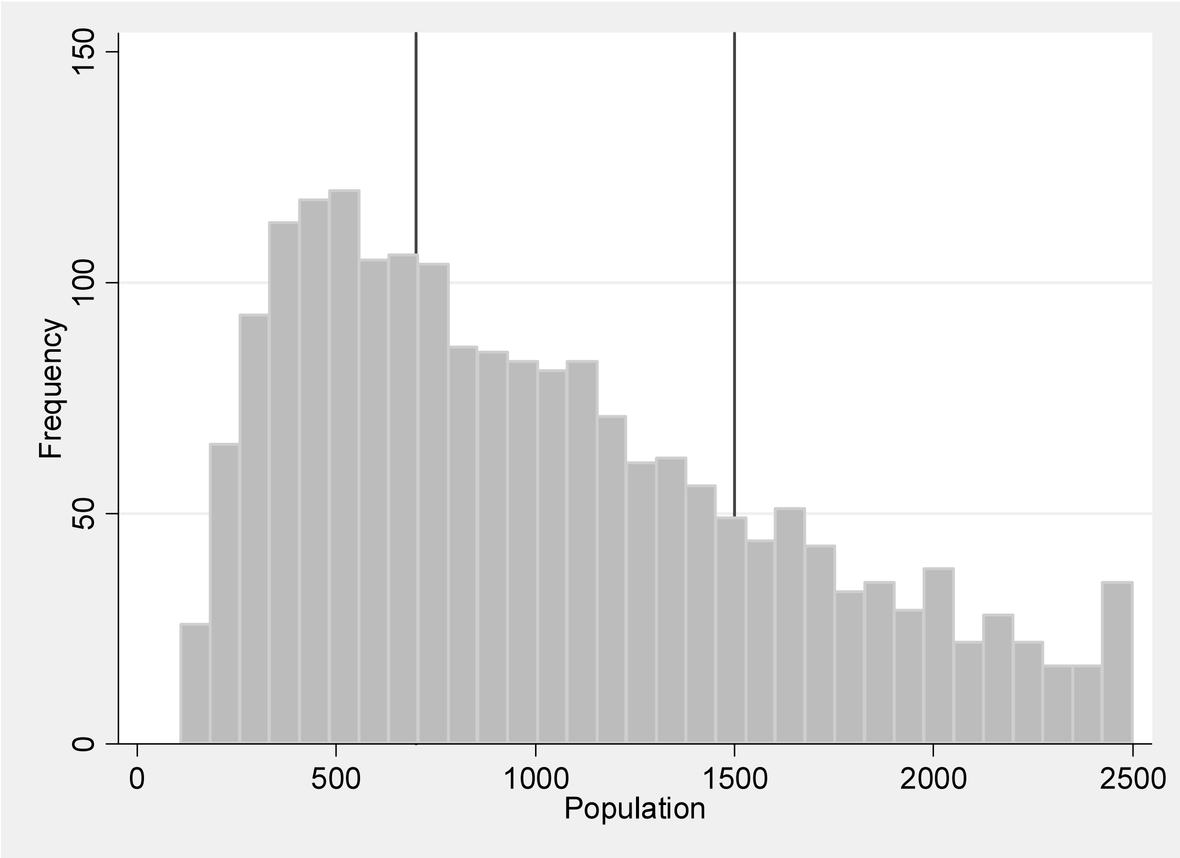

Figure 3. RD graph: treatment threshold 701 after 1938.

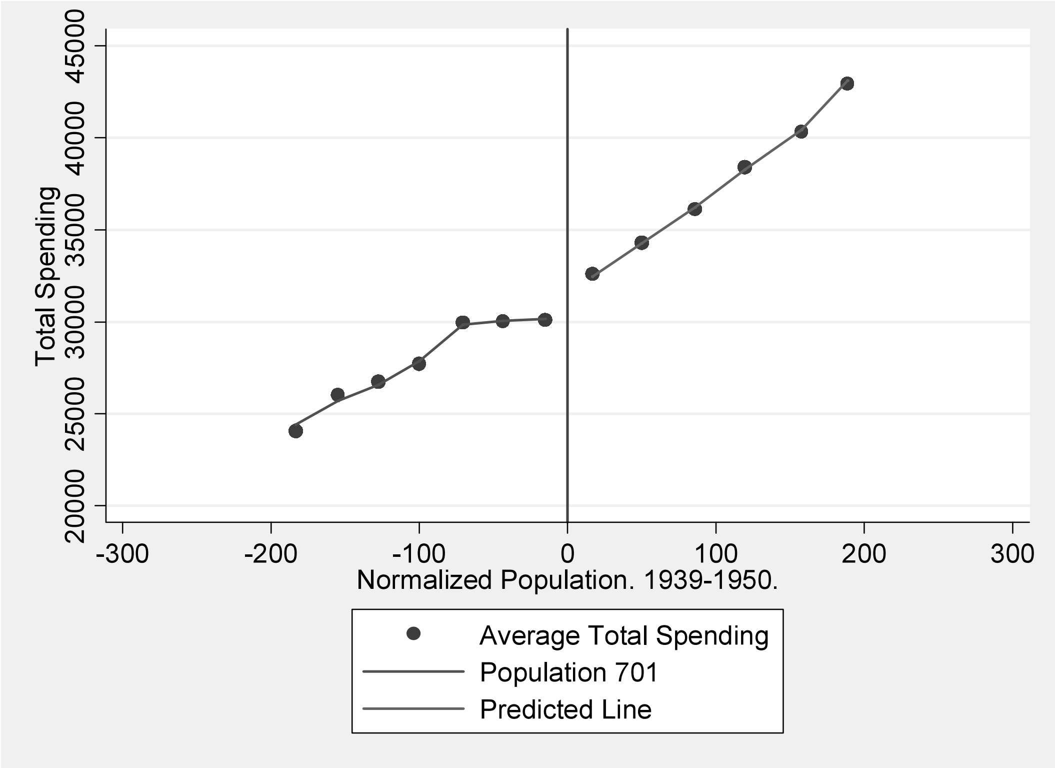

Figure 4. RD graph: probability of treatment at the 701 threshold.

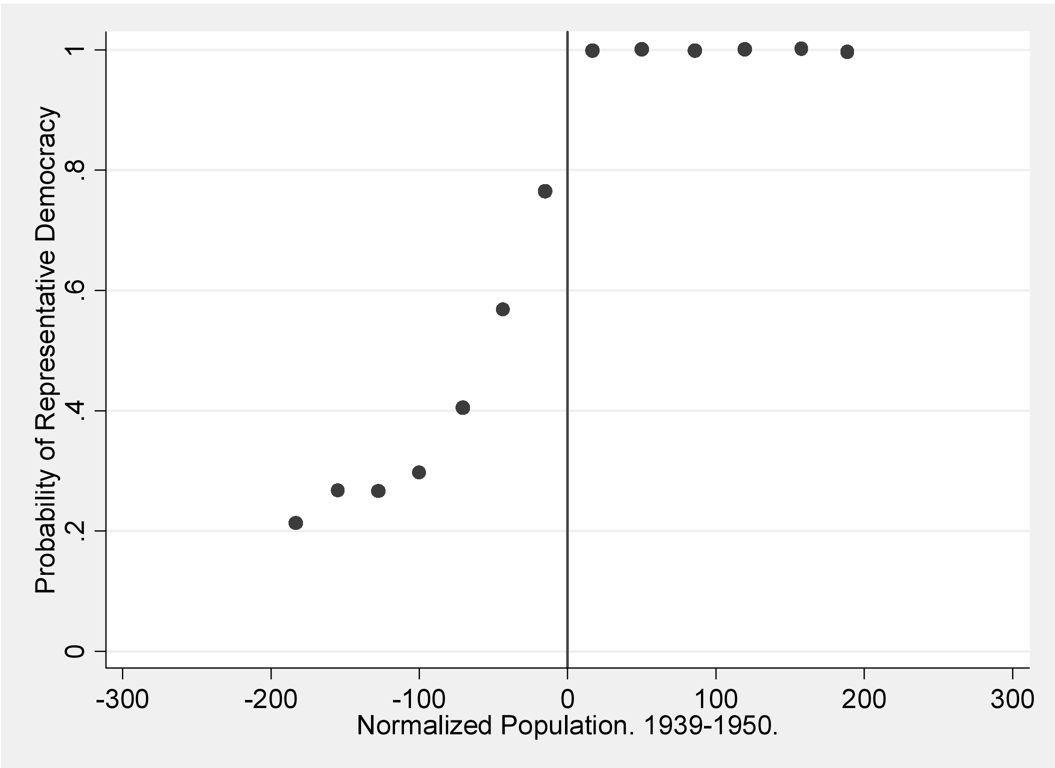

Figure 5. RD graph: treatment threshold at 701 before 1939

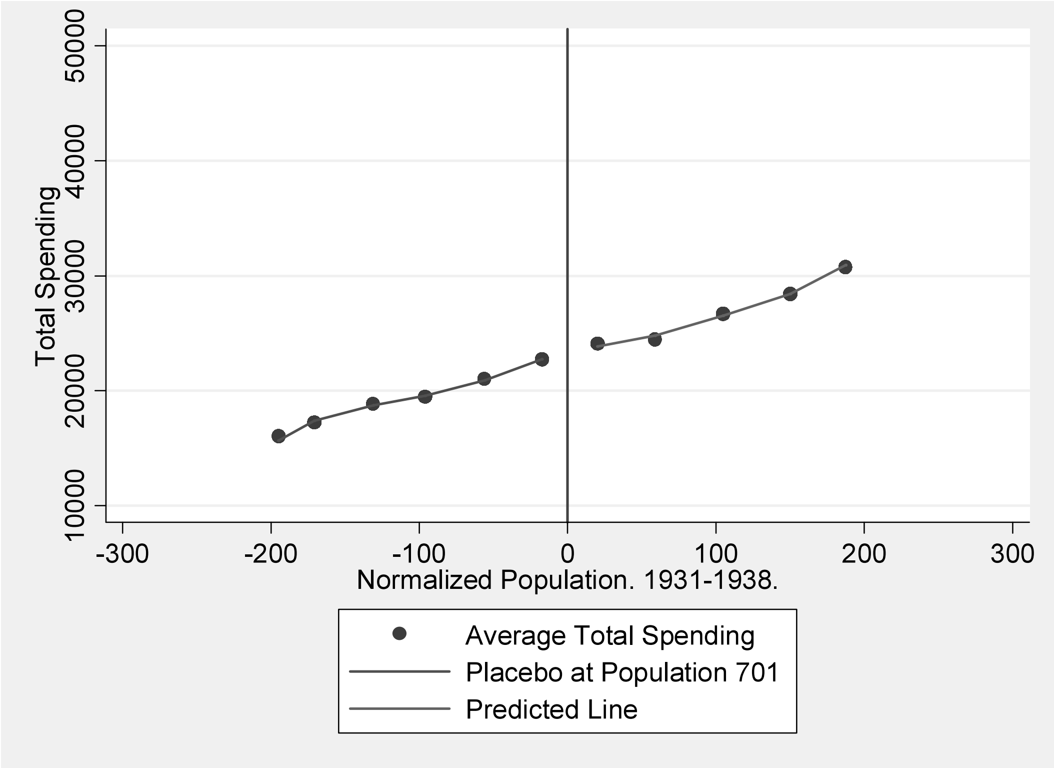

Figure 6. RD graph: treatment threshold at 1,501 before 1939

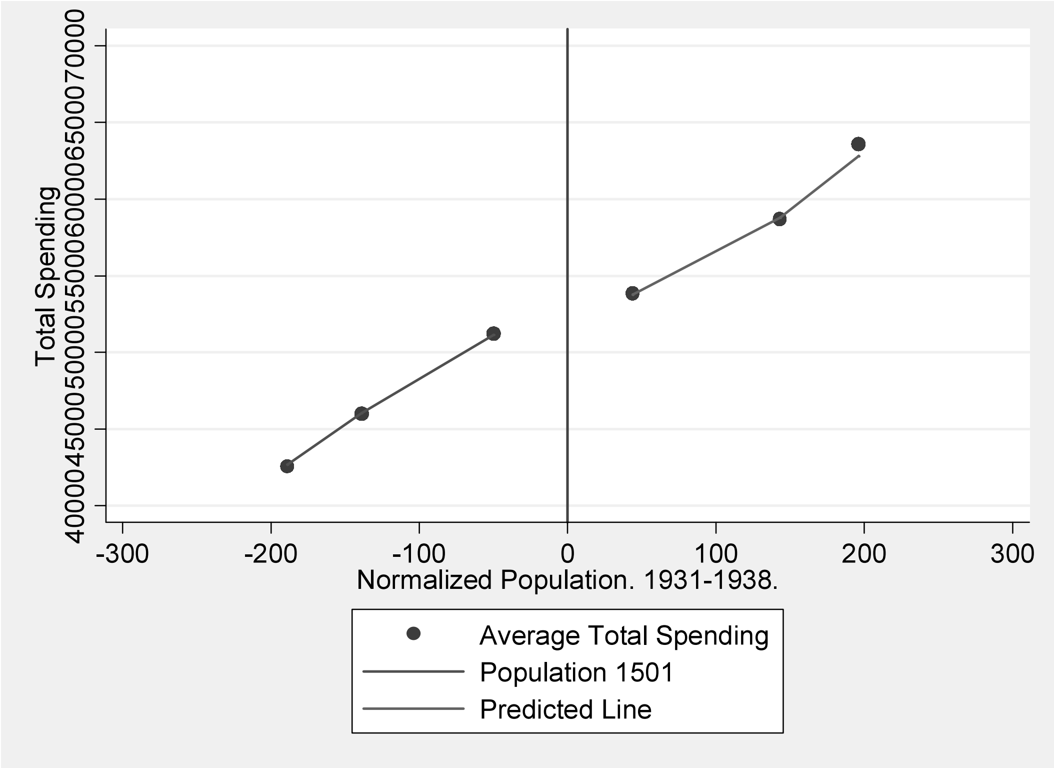

Figure 7. Probability of treatment at 1,501 before 1939