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DEMOCRACY, REDISTRIBUTION, AND POLITICAL PARTICIPATION: EVIDENCE FROM SWEDEN 1919–1938

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In this paper, we compare how two different types of political regimes—direct versus representative democracy—redistribute income toward the relatively poor segments of society after the introduction of universal and equal suffrage. Swedish local governments are used as a testing ground since this setting offers a number of attractive features for a credible impact evaluation. Most importantly, we exploit the existence of a population threshold, which partly determined a local government's choice of democracy to implement a regression-discontinuity design. The results indicate that direct democracies spend 40–60 percent less on public welfare. Our interpretation is that direct democracy may be more prone to elite capture than representative democracy since the elite's potential to exercise de facto power is likely to be greater in direct democracy after democratization.

KEYWORDS: Elite capture, direct democracy, redistribution, regression discontinuity design.

1. INTRODUCTION

IN THIS PAPER, we empirically analyze how *different* forms of democracies shape redistributional policies after the introduction of universal and equal suffrage. For a number of reasons, Sweden's transition from a nondemocracy to a democracy in 1919 provides a unique opportunity to credibly evaluate the impact of different types of democracies on the redistribution of income toward the relatively poor. Most importantly, two forms of democracies were simultaneously introduced at the local level: representative democracy and direct democracy.[2] Representative democracies held regular elections every fourth year, where citizens voted for political parties. Direct democracies gathered citizens at town meetings—at least three times per year—to determine matters of economic importance.[3] Crucially, a population threshold (partly) determined a local government's choice of democracy: if the population was above

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[2]Until 1918, Sweden used a graded voting scale based on taxes paid at the local level.

[3]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, that is, town meetings.

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1,500, the local government was required by the Swedish Local Government Act to have a representative system. Below the threshold, a local government was free to choose one of the two systems, unless it had switched to representative democracy within the past five years. Consequently, we can implement two regression-discontinuity designs (RD), which generate credible causal estimates under quite weak identification assumptions (e.g., Hahn, Todd, and Van der Klaauw (2001), Lee and Lemieux (2010)).

The results from the two RD designs clearly indicate that local governments with direct democracy spent 40–60 percent less on social welfare for the relatively poor.[4] We make a large number of validity checks of the RD designs: local governments on either side of the cutoff point are observationally similar in baseline characteristics. There is no discontinuity in these baseline characteristics. There is no statistical evidence of sorting of local governments around the thresholds (McCrary (2008)). Finally, the two RD designs yield similar results, which lend credibility to their internal and external validity.

Why did representative democracy redistribute more income toward the relatively poor than direct democracy? We argue that direct democracy may be more prone to elite capture than representative democracy. As stressed by Acemoglu and Robinson (2008), the elite can capture democratic political process by exercising their de facto political power. The elite may have been more able to exercise more de facto political power in direct democracy than in representative democracy for several reasons. First, the lack of (propoor) political parties in direct democracy made it harder for the citizens to solve their collective action problems (e.g., Acemoglu and Robinson (2006)). Second, the chairman of the town meeting, often a member of the elite, had great agenda setting power. Third, many decisions at meetings were taken by an open vote, which made it easier for the elite to rely on intimidation,[5] even though (according to the Swedish Local Government Act) any attendants at the town meetings could always require a secret ballot (Baland and Robinson (2008)). Consistent with these arguments, we find that the political participation rates and the share of organized citizens were much higher in representative democracy than in direct democracy. We also show that the increase in public-welfare transfers in representative democracy was exclusively targeted to organized citizens (unemployed people and their families). In sharp contrast, unorganized citizens (e.g., elderly, disabled, and widows) did not receive

However, many countries allow for other forms of political processes that provide limited direct democracy, for example, initiative, referendum (plebiscite), and recall.

[4]Interestingly, Olken (2010) found no effect of the choice of public good between two types of decision mechanisms (referenda vs. a meeting-based process) using an experimental design.

[5]Also relevant is the fact that Sweden had a relatively repressive agricultural system in the form of corvée labor obligations ("torparsystemet") and a system with contract-workers ("statarsystemet") that were mostly paid in kind until it was legally abolished in 1945 (e.g., Eriksson and Rogers (1978), Lund and Olsson (2005)). As a result, farm workers earned less than 50% of the wages of unskilled manufacturing workers during most of the period up to World War II (Elmer (1963)).

any additional welfare transfers in the representative system.[6] We also provide evidence that the elite were sometimes able to block the entry of pro-poor political parties in the representative system. For example, about 19% of all local elections had a single-party system during the period 1919–1938. Comparing single-party system and multiparty systems, we find that the former had much lower welfare spending, political participations, and organized citizens than the latter. To summarize, the evidence presented in this paper suggests that the elite could capture direct democracy as well as representative democracy in the absence of multiparty competition by exercising their de facto political power. Other political mechanisms at work may be that the extended franchise to voters below median income increased the demand for redistribution (Romer (1975), Roberts (1977), Meltzer and Richard (1981)). Differential costs of political participation in meetings and elections could also be an additional mechanism. Nonetheless, although these other political mechanisms can potentially explain the differences in some, but not all, outcomes between direct and representative democracy, they cannot easily explain the difference between a one-party system and a multiparty system.

The paper focuses on a specific historical institution: direct democracy in the form of the town meeting. But our results contribute to a broader debate about whether decentralization (i.e., the devolution of political or fiscal powers to local governing bodies) enhances or diminishes local development. While it is often argued that decentralization increases the accountability of local governments and strengthens the voice of the poor, it may also enhance the influence of local elites. Our results suggest that political institutions that incorporate elements of direct democracy—such as town or village meetings in places from New England to India, California-style Ballot initiative, or Swiss referenda—may be more prone to elite capture. Consequently, it is important to take the problem of elite capture into account when designing democratic institutions to ensure a fair and efficient allocation of public funds.

Our paper is related to several strands of literature. It is related to the voluminous literature on the impact of political institutions on economic policy,[7] specifically to the work on direct democracy.[8] Our work is also related to research on comparative development in that the change from nondemocracy to two different kinds of democracies makes Sweden an attractive testing ground for theories about the transition from nondemocracy to democracy. It is related to the literature on decentralization of governance and development,[9]

[6]This makes eminent sense given that those who were permanently dependent on welfare did not have voting rights until 1945.

[7]See, for example, the surveys by Besley and Case (2003) and Persson and Tabellini (2003).

[8]See Matsusaka (1995, 2004, 2005) on the effect of the voter initiative in the United States and Funk and Gathmann (2011) and Feld, Fischer, and Kirchgässner (2010) on data from Swiss Cantons. There are a number of books that discuss the town meeting form of government in the U.S. context, such as Bryan (2003), Mansbridge (1980), and Zimmerman (1999).

[9]See, for example, Bardhan (2002), Bardhan and Mookherje (2006), Foster and Rosenzweig (2004), Aragonès and Sánches-Pagés (2009), and Olken (2010).

which analyzes the function of local democracy in developing countries. Our study may thus provide information to the current debate on the functioning of democracy at the local level in developing countries. Further, the paper is related to the literature analyzing the growth of government and redistributive spending programs.[10] Another strand of related research concerns voluntary meetings with costly participation, such as regulatory meetings in the United States, school boards, and faculty meetings.[11] Yet another related literature deals with the determinants of voter participation and turnout.[12] Seventh and finally, the paper expands the recent work on regression-discontinuity designs in political economics.[13]

The rest of the paper is structured as follows. In Section 2, we describe the institutional background and the data. In Section 3, we discuss the RD designs. In Section 4, we present the results. Section 5 discusses and presents evidence on the political mechanism, while Section 6 concludes the paper.

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: 1919–1938.[14]

2.1. *Swedish Local Governments*

Local governments have historically played an essential part in Swedish society. For example, the first Local Government Act of 1863 granted local governments independent income taxation rights.[15] As a result, the bulk of local

[10]See, for example, Meltzer and Richard (1981) and Lindert (2004).

[11]See, for example, Osborne, Rosenthal, and Turner (2000) and Turner and Weninger (2005).

[12]See, for example, early work by Jackman (1987) and Powell (1986). Blais (2006) is a recent survey.

[13]Pettersson-Lidbom (2001a, 2008) were the first studies that exploited close elections to answer whether parties matter for policy choices, while Lee (2008) was the first to estimate the incumbency advantage. Pettersson-Lidbom (2001b, 2004, 2012) were the first studies exploiting treatment rules based on local governments' population sizes. This literature also includes later work by, for example, Bordignon, Nannicini, and Tabellini (2010), Brollo, Nannicini, Perotti, and Tabellini (2013), Ferreira and Gyourko (2009), Ferraz and Finan (2009), Fujiwara (2011), Gagliarducci, Nannicini, and Naticchioni (2011), Gagliarducci and Nannicini (2013), Litschig and Morrison (2013), and Lee, Moretti, and Butler (2004).

[14]It is noteworthy that at the beginning of the 20th century, Sweden was only 20 years into its industrialization and still predominantly an agrarian and rural society, for example, 75 percent of the total population lived in rural areas and 69 percent of those were directly or indirectly dependent on agriculture for subsistence. Sweden was also among the poorest countries in Europe at that time: per-capita GDP was about half that of the United Kingdom and the United States in 1901 (Maddison (1995)). More information about the Swedish historical context was provided by Scott (1988).

[15]This section is based on the Swedish Code of Statutes (Svensk författningssamling, SFS). SFS 1862:13, SFS 1918:1026, and SFS 1930:251.

government revenues was (and still is today) raised through a local proportional income tax, with intergovernmental transfers making up a small part (typically less than 20 percent) of local revenues. Moreover, the average local income tax rate was about 10 percent during the period of investigation, 1919–1938 (while today the average local income tax rate is higher than 30 percent). Local governments were economically important because they provided many important public services such as education and social welfare. Consequently, the ratio of aggregate local government spending out of GDP in Sweden is high from an international perspective. During the period of our investigation, local governments were divided into three categories, which were originally based on an urban-rural distinction.[16] As discussed further below, this paper focuses on rural local governments.

Historically, Swedish local governments had direct democracy in the form of town meetings ("kommunalstämma"), where all eligible voters were gathered on a regular basis to decide on matters of economic importance. Until 1918, it was voluntary for rural local governments to choose representative democracy ("kommunfullmäktige"), while this was mandatory in cities with more than 3,000 inhabitants. However, very few local governments switched to a representative system. For example, in 1917, only 33 of a total of 2,409 local governments had voluntarily switched to a representative system.[17] Due to a change in the Local Government Act in December 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 given a choice between direct democracy and representative democracy. The new Local Government Act was part of a major constitutional reform in Sweden in which the Swedish Parliament passed equal and universal suffrage in 1918. Almost all individuals aged above 23 were now entitled to vote in the local government where they were registered.[18] In the original proposed constitutional reform package, the mandatory population threshold for having representative democracy was set to 3,000 inhabitants, but this proposal was turned down in favor of a compromise with a threshold of 1,500. In the debate surrounding the constitutional reform package, one main argument was advanced for requiring rural local governments to switch to representative democracy:[19] the locality would be governed by people more responsible than the average attendant at a town meeting. This argument reflected the fact of attendance at town

[16]In 1950, there existed 133 cities, 84 boroughs, and 2,281 rural local governments. The first of two major boundary reforms reduced the number of local governments from 2,498 to 1,037 in 1952. The second boundary reform, which was completed in 1974, further reduced the number of local governments to 278. As of 2010, there are 290 local governments.

[17]Of these 33 local governments, 15 were rural local governments and 18 were boroughs.

[18]There were still some people with no voting rights after 1919, namely, (i) people with foreign citizenship, (ii) people with unpaid taxes, (iii) recipients of permanent welfare, (iv) prisoners, (v) people with cognitive disabilities, and (vi) people who had been made bankrupt.

[19]See Strömberg (1974) and Wallin (2007) for more extensive discussions of the debate in Parliament about representative or direct democracy at the local level.

meetings being very low—12 percent on average—and much higher political participation in elections to Parliament. For this reason, members of Parliament argued that direct democracy may be vulnerable to shocks to meetings' attendance rates. Nonetheless, despite the strong majority in favor of the representative form of democracy, Swedish Parliament still took into account the very long tradition of direct democracy at the local level and refrained from requiring all local governments to have a representative system.

The new Swedish Local Government Act also spelled out decision rules for the process of the switch. For local governments below the population thresholds, the status-quo form was direct democracy. However, if a local government had switched to a representative system, it could not switch back within a five-year period. Thus, the Swedish Parliament intentionally created a strong "status-quo bias" for maintaining representative democracy in a local government once such a system had been put in place. As a result, we have two forcing variables instead of only one: namely, the population size in year $t-1$ for the period 1919–1938 and the population size in 1918 until 1925. The two-dimensional RD design will be further discussed below.

Table I shows the number of local governments with representative democracy (voluntary or mandatory) and direct democracy for the regular election years 1919, 1922, 1926, 1930, and 1934. Clearly, the bulk of local governments with a population size below the 1,500 threshold chose to keep direct democracy, while some voluntarily switched to representative democracy. Table I also shows an increasing trend in the number of local governments that voluntarily switched to representative democracy, from 52 to 274, during this period.

Local governments with representative democracy held a mandatory election every fourth year. However, a local government was required to have an election in the coming year when the population threshold of 1,500 was crossed by January 1, with the new government's term in office until the next mandatory election year. The Local Government Act required that elections be held on a Sunday in the period September 13 to October 20. Elections were based on a proportional representation formula with closed party lists in multimem-

TABLE I

NUMBER OF LOCAL GOVERNMENTS WITH REPRESENTATIVE AND DIRECT DEMOCRACY[a]

	Representative Democracy			
Election Year	Mandatory	Voluntary	Direct Democracy	Voter Turnout
1919	884	52	1,466	52
1922	889	124	1,389	28
1926	888	149	1,375	42
1930	875	193	1,350	51
1934	867	274	1,273	58

[a]Source: Archives of Statistics Sweden.

ber constituencies. At the time, five traditional parties dominated the political arena: two left-wing parties (the Communists and the Social Democrats) and three center-right parties (the Agrarian Party, the Liberal Party, and the Conservative Party). However, a fairly large number of elections were nonpartisan as characterized by Statistics Sweden, that is, with a single nonpolitical list of candidates or with two or more nonpolitical lists.

Councils elected in the representative democracy were required to have at least three meetings per year. The first, to be held between March 16 and April 30, was to deal with the local government accounts from the previous year. The budget should be determined at the second mandatory meeting, to be held between October 1 and November 15, while a third mandatory meeting in December was to take care of the appointment of officials. The national law also required that many economic decisions in the council be taken with a supermajority. The chairman and the vice-chairman of the council were elected on a yearly basis. The executive agency of the local government ("kommunalnämnden") was required to have 5 to 11 members elected by the council. The law required that a majority of the council members be present at the council meetings to constitute a quorum. The number of council members ranged from 15 to 40 depending on population size. Importantly, these population thresholds do not coincide with the 1,500 threshold for representative democracy, with the closest one at 2,000. Table I shows the average turnout rate for rural local governments in all regular elections between 1919 and 1938. Most of these elections had a turnout rate above 50%.

The Local Government Act 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 of a single year).

The Local Government Act mandated the following decision-making procedure at the town meeting. After discussion of an item on the agenda, the chairman makes a proposal that can be decided with a yes or no vote. The chairman then declares the outcome after a voice vote of "yes" or "no," unless somebody requires a second vote. This vote can be either open (a roll-call vote) or closed depending on the request. Thus, any attendant at town meetings always had the option of requiring a secret ballot. Each eligible voter attending the town meeting was also entitled to represent at most one other voter provided that he had the power of attorney to do so.

The attendance rate at town meetings was unfortunately not recorded by Statistics Sweden, in contrast to the election statistics. However, we have collected the minutes of the town meetings for a large set of local governments both before and after democratization in 1919. These minutes typically contain information about attendance rates if somebody requested a second vote (open or closed). We have a representative sample of 195 local governments for the period 1912–1916.[20] There were a total of 567 meetings with a second vote. Consequently, there was a second vote in 19% of all required meetings.[21] The average turnout at these meetings was 12%. For the period 1919–1938, we have a slightly more selected sample of 74 local governments. There were a total of 608 meetings with a secondary vote.[22] Thus, there was a second vote in 14% of all required meetings.[23] Closed ballots were used in 241 (i.e., 40%) of these meetings. Overall, the average turnout was 14%, but in meetings with closed votes it was 18%.

2.2. *Public Spending Programs*

During the period of our study, 1919–1938, Swedish local governments were formally responsible for the following five spending programs: (i) basic compulsory education, (ii) social welfare or poor relief,[24] (iii) child welfare, (iv) basic pensions, and (v) health care. Basic compulsory education was the largest spending program, constituting more than 40 percent of total spending, while social assistance to the poor was the second largest program, with about 20 percent of total spending. In this study, we will use social welfare spending as the policy outcome of interest since this is indisputably the most redistributive program. It is noteworthy that the development of social policies in Sweden differed little from the international trends before World War II (e.g., Lindert (2004), Esping-Andersen and Korpi (1986)).

Swedish local governments had been providing public relief or social welfare for a long period of time,[25] but it was not until the Poor Law of 1847 that social assistance was systematically regulated across the country. The Poor Law was changed in 1853 and 1871. These laws only granted the poor a barely adequate support of their basic needs. In contrast, under the Poor Law of 1918 (SFS 1918:422), each local government was charged with the task of providing adequate care and relief to all individuals in need. According to this law, each local government was required to establish a public-assistance committee with

[20]These data are taken from the publication "Förslag till kommunalrösträttsreform avgivet (1918)."

[21]The total number of required meetings is 2,925 ($3 \times 5 \times 195$).

[22]The data are extracted from Svensk Lokalhistorisk Databas, a database that covers digitized minutes from local governments from 6 out of 24 counties in Sweden. See www.lokalhistoria.nu.

[23]The total number of required meetings is 4,440 ($3 \times 20 \times 74$).

[24]The term poor relief refers to any actions taken by either governmental or ecclesiastical bodies to relieve poverty. Poor relief is often used to discuss how European countries (e.g., English Poor Law) dealt with poverty until modern time. The Swedish Poor Law system was in existence until the emergence of the modern welfare state, that is, it was not formally abolished until the Social Assistance Act in 1957. See Rosenthal (1967) for an historical overview of Swedish welfare programs. For the U.S. transition from the poor law system to social welfare, see Trattner (1998) or Katz (1986).

[25]See Edebalk (2009), Lundberg and Åmark (2001), Rosenthal (1967) for overviews of the development of poor relief and social insurance in Sweden.

at least three local appointees, one of whom should be a woman. Two different classes of public assistance were established in the new law: compulsory and voluntary, that is, aid beyond the statutory requirements or to persons not eligible for compulsory assistance.

Public assistance was provided in three different forms by the local governments: (i) assistance to the recipients in their own homes, either as cash allowances or in kind, (ii) boarding out with a private family, and (iii) care in public institutions such as a workhouse or a poorhouse. About 60–70 percent of the recipients received assistance in their homes during the period 1919–1938, while 25 percent received care in a public institution. Each year, as much as between 4 and 10 percent of the total population received social welfare in some form. This number also includes dependents, that is, children whose parents received support. A much higher number of adult females than males were directly dependent on support. In 1919, the adult women-to-men ratio was 1.9, but this dropped to 1.2 in 1938. The recipients of social assistance were classified as being on either permanent or temporary assistance. Those on permanent support were mainly disabled, elderly, or widows who could not support themselves, while those on temporary support were mostly unemployed. At the beginning of the period, about 15 percent of all adult welfare recipients were being classified as temporary recipients, while this figure had increased to 40 percent at the end of the period. In other words, the number of temporary welfare recipients nearly tripled over the period 1919–1938. Taken together, the decrease in the female-to-male ratio and the sharp increase in temporary recipients suggest that after democratization, poor relief was increasingly given to unemployed male workers.

Finally, it is important to stress that welfare migration was severely restricted by the Poor Law ("Hemortsrättsstadgarna"). If people moved after the age of 60, they were not eligible for public assistance from the new local government. People below 60 could not get any social welfare during a period of two years if they decided to move. Moreover, a local government could expel people that were not eligible for social welfare. These types of rules make sorting around the population treatment threshold in the RD analyses much less likely. Indeed, we find no statistical evidence of sorting around the threshold, as further discussed below.

2.3. *Data*

In order to evaluate the impact of the form of democracy on local government spending, we have constructed a new comprehensive panel data set for about 2,500 local governments for the period 1918–1938. The main data set consists of *yearly* observations on a large number of fiscal policies, political variables, and local government characteristics. Our data come from both published and unpublished material produced by Statistics Sweden.[26] The unpublished material is kept in the National Archives of Sweden and was collected by hand. For the published material, we have digitized it by using data-entry services in India. Table II contains descriptive statistics for the variables that we use in this paper.

As the main outcome variable of interest, we use per-capita social-welfare spending.[27] We also use three other outcome variables in the analysis of the political mechanisms. One concerns how well citizens are organized at the local level:[28] the percentage of citizens belonging to one of the major social movements: labor unions, temperance lodges, and free churches.[29] Panel A of Table II shows that about 9 percent of the people were organized during the period 1919–1938. The two other outcomes relate to disaggregated social welfare spending, namely, the part of welfare spending that went to public outdoor and indoor relief, respectively.[30] Outdoor relief was poor relief in the form of money, food, clothing, or goods, given without the requirement that the recipient enters a public institution. In contrast, recipients of indoor relief were required to enter a public institution such as a workhouse or poorhouse. With the disaggregated welfare data, it is possible to evaluate how much welfare spending was distributed to recipients on temporary rather than permanent support, because indoor relief was only given to recipients classified as permanently poor. Panel A shows that spending on outdoor relief was about twice as large as spending on indoor relief.

The forcing variable in the RD analysis is population size: either in year $t-1$ or in 1918. It is noteworthy that the population registers were not administered by the local governments themselves; the keeping of vital statistics was, rather, the duty of the Swedish State church until 1991.[31] Thus, a local government could not strategically misreport its population size so as to avoid having a certain form of government. However, a local government could still potentially

[26]Our data on budget items and other characteristics are mostly taken from two official publications from Statistics Sweden, namely, Local Government Finances and Statistical Yearbook of Administrative Districts of Sweden. However, for the budget items for the years 1918–1927, it was also necessary to collect data from unpublished material from Statistics Sweden kept at the Swedish National Archives. Data on forms of democracy and voter turnout in elections were also collected from unpublished material at the Swedish National Archives.

[27]All nominal values are deflated with CPI with 1914 as the base year.

[28]The primary data on labor unions were collected by Carl Göran Andrea and Sven Lundkvist at the Department of History, Uppsala University and made available to us by the Swedish National Data Service (SND) at University of Gothenburg.

[29]For an overview of the social movements in Sweden, see Lundkvist (1980).

[30]This data is only available for 1918–1937.

[31]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.

TABLE II

DESCRIPTIVE STATISTICS[a]

Variables	Mean	St. Dev.	Min	Max	Obs.
Panel A. Outcome variables 1919–1938					
Per capita social welfare spending	6.31	4.06	0	59.96	48,128
Per capita spending on indoor relief	2.07	2.72	0	52.24	45,724
Per capita spending on outdoor relief	4.16	2.83	0	29.76	45,728
Percentage of organized citizens	9.04	18.0	0	198	48,152
Panel B: Forcing variables					
Population size at time $t-1$	1,717	2,004	91	26,491	48,164
Population in 1918	1,715	1,988	110	21,648	2,400
Panel C: Baseline or pre-treatment characteristics as measured in 1917 or 1918					
Per capita social welfare spending, 1918	2.48	2.10	0	41.41	2,398
Per capita spending on indoor relief, 1918	1.25	2.05	0	40.35	2,398
Per capita spending on outdoor relief, 1918	1.22	0.81	0	6.41	2,400
Percentage of organized citizens, 1917	7.59	12.3	0	270	2,380
Number of total recipients including children, 1917	58	104	0	1,714	2,400
Number of adult recipients, 1917	38	59	0	1,090	2,370
Number of children directly supported, 1917	7	15	0	289	2,370
Number of children indirectly supported, 1917	14	38	0	581	2,370
Number of people receiving full support, 1917	21	28	0	295	2,400
Number of people boarded out, 1917	8	13	0	139	2,370
Number of people in public institutions, 1917	13	20	0	196	2,370
Number of public institutions, 1917	0.76	0.58	0	8	2,400
Number of slots available in public institutions, 1917	19	24	0	200	2,400
Total area (km^2), 1918	18,160	81,181	0	1.95e+06	2,371
Land area (km^2), 1918	17,025	75,530	15	1.81e+06	2,371
Arable land (km^2), 1918	1,566	1,213	0	13,524	2,400
Total income tax base, 1918	195,656	452,911	786	6.10e+06	2,400
Economic structure (% agriculture), 1918	49.5	22.1	0	98.5	2,370
Number of eligible male voters at the parliamentary elections, 1917	359	371	0	4,373	2,400
Number of voters at the parliamentary elections, 1917	229	233	0	3,003	2,387
Proportion of left-wing voters at the parliamentary elections, 1917	0.30	0.20	0	1.00	2,380

[a]All nominal values are in SEK and deflated with CPI with 1914 as the base year.

try to control how people moved in and out of its jurisdictions. If that were the case, this could potentially invalidate an RD analysis, since local governments around the treatment thresholds would not be comparable. Below, we find no evidence of sorting around the threshold in the RD analyses.

Finally, we have collected 22 baseline or pre-treatment characteristics, that is, variables dated before the introduction of the two treatments—direct or representative democracy—in 1919. One set of variables consists of the four baseline outcomes. Another set of variables consists of characteristics of the social-welfare program: the number of total recipients including children, the number of adults, the number of children directly supported, the number of children indirectly supported, the number of people receiving full support, the number of people boarded out, the number of people in public institutions (i.e., poorhouses), the number of public institutions, and the number of slots available in the public institutions. The other set of variables consists of two geographic variables: total area and land area, three economic variables: arable land, income tax-base, and economic structure (percent of the economy based on agriculture), population size, and four variables capturing the political characteristics of the community: the number of eligible voters at the parliamentary elections in 1917, the turnout at the parliamentary elections in 1917, and the proportion of left-wing voters at the parliamentary elections in 1917. We use these 22 pre-treatment variables to test a key implication of the RD, namely, that these covariates should be balanced around the population threshold in the RD design.

3. REGRESSION-DISCONTINUITY DESIGNS

In this section, we discuss the implementation of the regression-discontinuity (RD) design. As noted above, local governments were required to have representative democracy if their population size was larger than 1,500, but could choose to have representative or direct democracy below this cutoff point. Thus, our RD approach is a fuzzy design, but since we only have a one-sided compliance problem, that is, the treatment rule is binding for those above the cutoff point, the estimated treatment effect corresponds to the treatment-on-the-treated effect (Bloom (1984), Battistin and Rettore (2008)). In other words, the regularity conditions required for the identification of the mean counterfactual outcome in our (fuzzy) RD design are essentially the same as in a sharp RD design (Battistin and Rettore (2008)).

Moreover, the design is also a multidimensional RD or a boundary RD design because a municipality must keep representative democracy for at least five years after its introduction in 1919 even if its population size were to fall below the mandatory cutoff point. As a result, there are two forcing variables, namely, the population size in year $t - 1$ for the period 1919–1938 and the population size in 1918 for the period 1919–1925.

As discussed by Imbens and Zajonc (2011), Reardon and Robinson (2010), Wong, Steiner, and Cook (2010), and Papay, Willett, and Murnane (2011), a multidimensional RD design can be analyzed in several different ways, for example, as separate scalar RD designs or reduced to a scalar design with “distance to the nearest boundary,” or any other monotone function as the unitary forcing variable. Each of these approaches estimates a well-defined average causal effect for a specific subpopulation.

In this paper, we will analyze the multidimensional RD design as two separate scalar RD designs.[32] Thus, we will estimate *standard* (cross-sectional) RD specifications of the form

$$Y_i = a + \beta D_i + f(W_i) + u_i, \tag{1}$$

where Y_i is the outcome variable, for example, the logarithm of per capita social welfare spending, W_i is the forcing variable, either population size in year $t-1$ or population size in 1918, and D_i is an indicator variable taking the value of 1 if a local government has direct democracy and 0 if it has representative democracy. The parameter of interest is β, which is the treatment effect of having direct rather than representative democracy. As noted above, representative democracy is mandatory if the population is above 1,500, while there is a choice between direct and representative democracy for those local governments with a population below the threshold. Thus, our RD approach is fuzzy and we will therefore use the eligibility rule $Z_i = 1[W_i \leq 1{,}500]$ as an instrument for treatment status D (e.g., Hahn, Todd, and Van der Klaauw (2001), Imbens and Lemieux (2008)).

We note that one of the RD designs is embedded in a panel context, whereby the treatment is determined according to the realization of the forcing variable population size year by year. However, we still conduct the RD analysis for the entire pooled cross-section data set, following the recommendation of Lee and Lemieux (2010). They argued that it is unnecessary for identification in an RD analysis to exploit the panel feature of the data, since the "source of identification is a comparison between those just below and above the threshold, and can be carried out with a single cross-section." In fact, including local government fixed effects would introduce more restrictions without any gain in identification.[33] Nonetheless, we include a full set of time fixed effects, since this makes it clear that a number of cross-sectional experiments are pooled together across time. Moreover, we clustered standard errors to account for any dependence within the municipalities over time (e.g., Arellano (1987), Bertrand, Duflo, and Mullainathan (2004)). We also cluster the standard errors on one additional dimension since the forcing variable, population size, is discrete (Lee and Card (2008)). Thus, we make use of Cameron, Gelbach, and Miller's (2011) multi-clustering approach.

Because of the two scalar RD designs, there will also be two estimates of the treatment effect β, where both estimates correspond to the treatment-on-the-treated effect, as noted earlier. This makes it possible to test whether the

[32]In a previous version, we used the function, max(population in year $t-1$, population in 1918), as the unitary forcing variable. This RD design produced similar results.

[33]Cellini, Ferreira, and Rothstein (2010) developed a dynamic RD design where they made use of additional restrictions for identifying dynamic "treatment-on-treated" effects. The implementation of their RD approach was based on a global approach, that is, it used all data in the sample with flexible controls for the forcing variable, rather than a local approach, that is, local linear regressions.

treatment effect varies across the two subpopulations: those local governments near the population threshold of 1,500 in year $t-1$ and those local governments near the population threshold in 1918. As discussed below, comparing these two estimates is useful since there is only a limited overlap (at most 23%) between the two subpopulations. Nonetheless, the precision of the two estimates will most likely differ. This is related to the fact that one of the forcing variables, population in 1918, does not vary across time. Consequently, there will be many fewer observations around the 1,500 threshold in the RD analysis based on population size in 1918, since it basically only uses variation from one single cross-section to identify the parameter of interest.[34]

Equation (1) is estimated by nonparametric local linear regressions (LLR), as suggested by Hahn, Todd, and Van der Klaauw (2001) and Porter (2003). The bandwidth is selected by different procedures, namely, those suggested by Imbens and Kalyanaraman (2012), Calonico, Cattaneo, and Titiunik (2013), Ludwig and Miller (2007), and Almond, Doyle, Kowalski, and Williams (2010).[35] To deal with the problem that the selected bandwidth may be too "large" for the usual distributional approximations invoked in the literature to be valid (e.g., Calonico, Cattaneo, and Titiunik (2013)), we "undersmooth" the LLR estimator, that is, we choose a "small" enough bandwidth so that the bias is likely to be negligible. In other words, we display the results from smaller bandwidths than the optimal ones according to the selections procedures.

Following the suggestions of Imbens and Lemieux (2008) and Lee and Lemieux (2010), we use a rectangular kernel, which is equivalent to estimating a standard linear regression over the interval of the selected bandwidth on both sides of the cutoff point.[36]

4. RESULTS

In this section, we present the results regarding the effect of direct democracy and representative democracy on per capita social welfare spending.[37] We

[34]Since the outcome variable varies across time, one can think of two ways of estimating the treatment effect when the forcing variable is population in 1918. One approach is to collapse all the data to a single cross-section, while another method is to estimate the treatment effect in the same way as with the other RD analysis, that is, as repeated cross-sections with time fixed effects. In practice, the two approaches yield almost identical results, as discussed below.

[35]We thank Douglas Almond, Joseph Doyle, Amanda Kowalski, and Heidi Williams for sharing their Stata code, which implements the cross-validation procedure. For the other bandwidth selection methods, we use the Stata code developed by Calonico, Cattaneo, and Titiunik (2012).

[36]Imbens and Lemieux (2008) wrote, "From a practical point of view, one may just focus on the simple rectangular kernel, but verify the robustness of the results to different choices of bandwidth," while Lee and Lemieux (2010) argued that it is "more transparent to just estimate standard linear regressions (rectangular kernel) with a variety of bandwidths, instead of trying out different kernels corresponding to particular weighted regressions that are more difficult to interpret."

[37]In a previous version of this paper, we argued that one should not express the outcome in per capita terms because population size is the forcing variable. In the Supplemental Material

begin with results when the forcing variable in the RD design is defined as the population in year $t-1$, followed by the results when the forcing variable is defined as the population in 1918.

4.1. *Forcing Variable: Population in Year $t-1$*

We present our RD results in three ways: the reduced-form effect, the first-stage effect, and the instrumental variable or Wald estimate, that is, the ratio between the estimates of the reduced-form effect and the first-stage effect. We also show the results with and without the additional 22 pre-treatment characteristics. However, we always include a full set of time fixed effects in the baseline specification. Regarding the choice of bandwidth,[38] we find that three of the bandwidth selectors yield a bandwidth in the range 77–120,[39] while Imbens and Kalyanaraman (2012) gave a much larger bandwidth of 202.[40] In order to avoid that the data-driven confidence intervals may be severely biased, we follow the suggestion of Calonico, Cattaneo, and Titiunik (2013) to report results from bandwidths smaller than the optimal ones. Therefore, we report results for bandwidths in the range of 20–120. In the Supplemental Material, we also report results for larger bandwidths (up to 300) and a different order of the polynomial (first–third) (Table A.I) and specifications where the RD slope does not differ across the threshold (Table A.III). It is reassuring that none of these additional specification checks alters any of the results presented below for the LLR with bandwidths smaller than 120.

It is noteworthy that there are 158 different local governments in the smallest bandwidth (20), while there are 296 in the largest bandwidth (120). The number of observations is larger, however, namely, 520 and 3,113, respectively. Panel A of Table III shows the reduced-form estimates, Panel B the first-stage estimates, and Panel C the corresponding Wald estimates.

The estimated reduced-form effect on social-welfare spending ranges from -7.5 to -11.7 percent without any covariates and from -8.3 to -11.4 percent with covariates. The estimated effects are thus quite insensitive to the choice of bandwidths and the inclusion of control variables. Nonetheless, the effects are still much more precisely estimated when covariates are included: the standard errors are 26–64 percent smaller. Importantly, the estimates with the smallest

(Hinnerich and Pettersson-Lidbom (2014)), we show that the results are completely unchanged if one uses total spending instead of per capita spending.

[38]For the bandwidth selection procedures, we only use data within the population interval {1,200, 1,800}.

[39]The Calonico, Cattaneo, and Titiunik (2012) method gives a bandwidth of 77, Ludwig and Miller (2007) a bandwidth of 120, and Almond et al. (2010) a bandwidth of 76.

[40]That the Imbens and Kalyanaraman (2012) approach gives such a large bandwidth is perhaps not surprising given that Calonico, Cattaneo, and Titiunik (2013) noted that, “Unfortunately, most (if not all) of these approaches lead to bandwidths that are too “large” because they do not satisfy the bias-condition,” that is, $nh_n^5 \to 0$.

TABLE III

LOCAL LINEAR ESTIMATES FROM THE REGRESSION-DISCONTINUITY DESIGN WHEN THE FORCING VARIABLE IS POPULATION IN YEAR $t-1$[a]

Bandwidths:	20	40	60	80	100	120
Panel A: Reduced-form relationship						
Reduced-form effect	−0.107*	−0.075	−0.093**	−0.117***	−0.084**	−0.078**
(no covariates)	(0.058)	(0.046)	(0.042)	(0.037)	(0.034)	(0.034)
Reduced-form effect	−0.092**	−0.093***	−0.101***	−0.114***	−0.089***	−0.083***
(including pre-treatment covariates)	(0.036)	(0.028)	(0.031)	(0.029)	(0.027)	(0.025)
Panel B: First-stage relationship						
First-stage effect	0.140***	0.168***	0.165***	0.143***	0.154***	0.169***
(no covariates)	(0.030)	(0.036)	(0.037)	(0.034)	(0.034)	(0.036)
First-stage effect	0.161***	0.183***	0.167***	0.148***	0.155***	0.168***
(including pre-treatment covariates)	(0.038)	(0.039)	(0.038)	(0.034)	(0.034)	(0.035)
Panel C: Wald or IV estimates						
Treatment effect	−0.768*	−0.445	−0.565**	−0.819***	−0.549**	−0.461**
(no covariates)	(0.446)	(0.284)	(0.260)	(0.291)	(0.229)	(0.209)
Treatment effect	−0.574**	−0.511***	−0.604***	−0.771***	−0.572***	−0.492***
(including pre-treatment covariates)	(0.230)	(0.173)	(0.207)	(0.241)	(0.191)	(0.165)
Number of local governments	158	193	232	252	274	296
Number of observations	520	1,021	1,535	2,074	2,608	3,113

[a]Each entry is a separate local linear regression with a uniform kernel. All specifications allow for the RD slope to differ across the threshold and include a full set of time fixed effects. The dependent variable in Panels A and C is per capita welfare spending in logarithmic form. The dependent variable in Panel B is an indicator for having direct democracy rather than representative democracy. Panel C is the Wald estimator, the ratio between the reduced-form effect and the first-stage estimate. The forcing variable is population in year $t-1$. See the text for a description of included pre-treatment covariates. Standard errors, clustered at both the municipality level and the running variable, are within parentheses (Cameron, Gelbach, and Miller (2011)). Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

bandwidths—where the bias of the standard errors is likely to be negligible—are still rather precisely estimated, yielding significant estimates even when the LLR estimator is greatly undersmoothed (Calonico, Cattaneo, and Titiunik (2013)).

We next turn to the first-stage estimates as displayed in Panel B of Table III. The estimated "jump" in the probability of treatment at the threshold ranges from 14.0 to 16.9 percentage points without covariates and from 14.8 to 18.3 with control variables. Once more, the estimated effects are quite stable and precise across bandwidths and with and without control variables.

Panel C of Table III shows the IV (Wald) estimates, that is, the effect of having direct democracy rather than representative democracy on per capita social-welfare spending. To obtain the correct percentage interpretation of the estimated treatment effect (when the estimate is large), it is necessary to use the transformation $100 * [\exp(\text{estimated effect}) - 1]$, as discussed by Halvorsen and Palmquist (1980). Thus, the estimated treatment effect in Table III varies between -36 and -56 percent in the specifications without covariates and between -39 and -53 percent with covariates. These are highly statistically significant in all specifications with covariates.

We now turn to other specification checks of the RD designs suggested in the literature. Figure 1 displays the reduced-form relationship between per capita social-welfare spending and the instrument once the pre-treatment character-

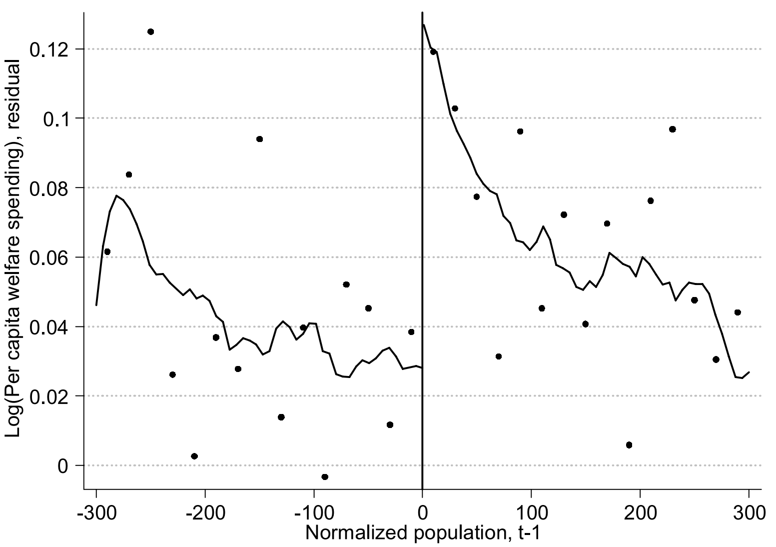

FIGURE 1.—Reduced-form relationship using population in year $t - 1$ as the forcing variable. The dependent variable is the residual from a regression of per capita welfare spending on 21 covariates. Plotted points are conditional means with a binwidth of 20. The solid line is the predicted values of a local linear smoother with a rectangualar kernel and a bandwidth of 60.

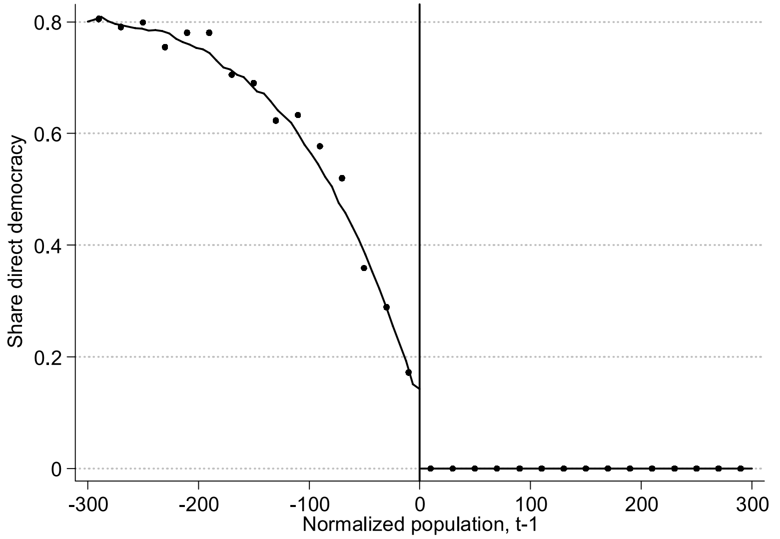

FIGURE 2.—First-stage relationship using population in year $t-1$ as the forcing variable. The dependent variable is an indicator variable for having direct democracy. Plotted points are conditional means with a binwidth of 20. The solid line is the predicted values of a local linear smoother with a rectangular kernel and a bandwidth of 60.

istics have been partialled out. The plotted points are conditional means of the residual with a bin size of 20, and the width around the population threshold is ±300. The solid line is the predicted values of a local linear smoother.[41] Figure 1 reveals a clear discontinuity at the population threshold of 1,500, while the relationship looks rather smooth elsewhere. The size of the jump at the threshold lines up well with the reduced-form estimates in Table III, that is, about 10 percent.

Figure 2 displays the first-stage relationship for the same window size as in Figure 1. Once more, we see a clear discontinuity at the threshold while the relationship appears to be smooth elsewhere. The jump in the probability of treatment at the threshold is about 16 percentage points, which is similar to the first-stage estimates in Table III. Moreover, Figure 2 also clearly reveals the one-sided compliance problem.

Next, we investigate whether the baseline characteristics are balanced, that is, those variables determined before 1919. As noted before, we use three sets of baseline or pre-treatment characteristics (see Panel C of Table II): one set of variables consists of the four baseline outcomes, the second set consists of nine characteristics of the social-welfare program, while the other set has two geographic variables, three economic variables, and three variables capturing the political behavior of citizens. Columns 1 and 2 of Table IV show the re-

[41] We use a rectangular kernel with a bandwidth of 60.

TABLE IV

TEST OF BALANCE OF PRE-TREATMENT CHARACTERISTICS[a]

Forcing Variable:	Population in Year $t-1$		Population in 1918	
Bandwidths:	70	80	50	60
Panel A: Baseline outcomes				
Log per capita social welfare spending in 1918	0.051	0.016	−0.081	−0.030
	(0.074)	(0.056)	(0.299)	(0.295)
Log per capita outdoor spending in 1918	0.054	−0.047	−0.295	−0.080
	(0.101)	(0.105)	(0.382)	(0.456)
Log per capita indoor spending in 1918	0.508	0.525	−0.165	−0.459
	(0.365)	(0.324)	(2.008)	(1.782)
Share of organized citizens in 1917	−0.003	−0.000	−0.054*	−0.024
	(0.008)	(0.008)	(0.029)	(0.030)
Panel B: Characteristics of the social welfare spending program and its recipients				
Number of total recipients including children in 1917	0.9	0.2	−2.9	−5.7
	(2.0)	(1.9)	(8.9)	(7.9)
Number of adults in 1917	0.8	0.4	−0.1	−1.5
	(1.6)	(1.5)	(6.8)	(6.1)
Number of children directly supported in 1917	−0.2	−0.1	−2.7*	−2.6*
	(0.3)	(0.3)	(1.5)	(1.3)
Number of children indirectly supported in 1917	0.3	−0.1	−0.1	−1.5
	(0.7)	(0.6)	(3.5)	(3.0)
Number of people receiving full support in 1917	−0.1	−0.5	−2.6	−3.7
	(1.0)	(0.9)	(5.2)	(4.6)
Number of people boarded out in 1917	−0.2	−0.4	2.2	0.6
	(0.5)	(0.5)	(3.0)	(2.7)
Number of people in public institutions in 1917	0.1	0.3	−2.5	−1.8
	(0.8)	(0.7)	(5.0)	(4.3)

(*Continues*)

TABLE IV—*Continued*

Forcing Variable:	Population in Year $t-1$		Population in 1918	
Bandwidths:	70	80	50	60
Number of public institutions in 1917	0.0	0.01	0.1	0.1
	(0.0)	(0.05)	(0.4)	(0.3)
Number of slots available in public institutions in 1917	0.4	0.7	−2.9	−1.4
	(1.5)	(1.3)	(9.5)	(8.3)
Panel C: Characteristics of local governments				
Total area (m^2) in 1918	390	273	−11,158	−11,748
	(2,134)	(2,100)	(11,505)	(9,497)
Land area (m^2) in 1918	330	194	−10,559	−11,120
	(2,058)	(2,025)	(11,254)	(9,282)
Arable land (m^2) in 1918	16	29	363	458
	(71)	(66)	(405)	(358)
Income tax base in 1918	5,914	5,002	−14,177	−7,220
	(7,106)	(5,941)	(32,631)	(31,245)
Economic structure (share agriculture) in 1917	0.0	0.0	0.1	0.0
	(0.0)	(0.0)	(0.1)	(0.1)
Population size in 1918	21	12	n.a.	n.a.
	(18)	(14)		
Number of eligible male voters at parliamentary elections in 1917	19**	13	7	12
	(10)	(8)	(22)	(20)
Number of voters at parliamentary elections in 1917	10	7	−8	−0.1
	(7)	(6)	(18)	(17)
Proportion left-wing voters at parliamentary elections in 1917	−0.0	−0.0	−0.0	−0.0
	(0.0)	(0.0)	(0.1)	(0.1)

[a]Each entry is a separate local linear regression with a uniform kernel. All specifications allow for the RD slope to differ across the threshold and include a full set of time fixed effects. Standard errors, clustered at both the municipality level and the forcing variable, are within parentheses (Cameron, Gelbach, and Miller (2011)). Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

sults from testing whether these 22 baseline characteristics are balanced at the treatment threshold. (In the Supplemental Material, we show the corresponding graphical analyses.) We report estimates from two bandwidths: 70 and 80. Only one of the 44 estimates is significant at the 5 percent level. However, that is to be expected since, if 100 specifications are tested, it is likely that five will be statistically significant by chance, and this should not raise any substantial concerns about the validity of the design. Moreover, these significant specifications are not very credible anyway, since they are all highly sensitive to the choice of bandwidth. Thus, we have no statistical evidence of a discontinuous effect at the threshold for the baseline covariates. These results provide strong support that the RD design is likely to be valid.

We also test for direct evidence of sorting around the threshold by searching for a sharp break in the distribution of the assignment variable, population size in $t-1$, at the cutoff. For sorting to undermine the causal interpretation of the RD approach, agents (i.e., local governments) need to be able to sort *precisely* around the treatment threshold in the RD design. For this test, we use the McCrary (2008) test, which is a test of whether the density of the forcing variable, the population size in year $t-1$, is continuous at the population threshold 1,500.[42] Figure 3 displays the result from the McCrary test graphically. The graphs show little or no evidence of a discontinuity in the distribution of the forcing variable at the threshold. In addition, the estimate from the McCrary density test is also small and statistically insignificant.[43] To sum up, all specification tests suggest that the RD design using the population in year $t-1$ as the forcing variable is compelling.

4.2. *Forcing Variable: Population in 1918*

In this subsection, we report results from the RD design when the forcing variable is defined in 1918. There are some important differences between this RD design and the previous one, as previously noted. First, the treatment assignment rule was only in place from 1919 to 1925, which implies that the number of observations on the outcomes is much smaller with this RD design. Second, the forcing variable in 1918 does not vary across time. As a result, the problem with a discrete forcing variable is more severe in this design than the other because the forcing variable will not have more continuous support when we pool the data over time. As an illustration of the problem, the number of

[42]In the Supplemental Material, we also display a histogram (see Figure A.9) over the forcing variable which is a more informal test of sorting. This graph does not show any evidence of a discontinuity at the threshold, either.

[43]According to the McCrary test, the default bin size is 18 and the default bandwidth is 1,187. The estimate from this test is 0.0002 with a standard error of 0.025. Thus, we find no evidence of sorting.

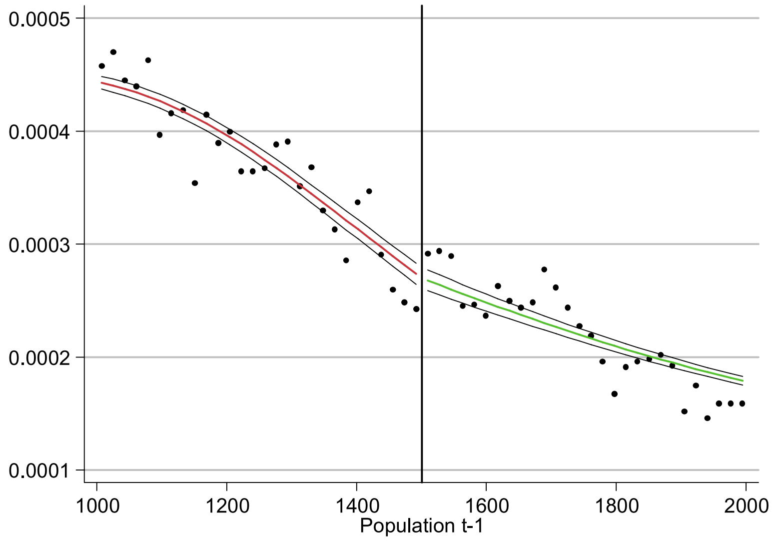

FIGURE 3.—The McCrary density test: population in year $t-1$ as the forcing variable. McCrary (2008) is a test of whether the density of the forcing variable, the population size in year $t-1$, is continuous at the population threshold. The point estimate is 0.0002 with a standard error of 0.0254.

local governments is only 35 in a window width of 20 around the population threshold, while the corresponding number of local governments is 158 in the previous RD design. On the other hand, sorting around the threshold is not an issue in this RD design, as the population treatment threshold was unknown to local governments at the time of their implementation, as noted above. Below, we conduct the same type of RD analysis as in the previous section.

Starting with the choice of bandwidth: the data-driven bandwidths range from 53 (Calonico, Cattaneo, and Titiunik (2013)) to 163 (Imbens and Kalyanaraman (2012)).[44] Almond et al. (2010) produced a bandwidth of 62, while Ludwig and Miller (2007) gave a bandwidths of 91. Once again, to avoid bias in the standard errors (Calonico, Cattaneo, and Titiunik (2013)), we report results from bandwidths up to 60. However, in the Supplemental Material, we also report results for larger bandwidths and a different order of the polynomial (Table A.II) and specifications where the RD slope is constrained to be the same across the threshold (Table A.IV). It is reassuring that these additional specification checks mostly confirm the results presented below for the LLR with a bandwidth of less than 60.

[44]For the bandwidth selection procedures, we only use data within the population interval of 1,200 and 1,800.

TABLE V

LOCAL LINEAR ESTIMATES FROM THE REGRESSION-DISCONTINUITY DESIGN WHEN THE FORCING VARIABLE IS POPULATION IN 1918[a]

Bandwidths:	20	30	40	50	60
Panel A: Reduced-form relationship					
Reduced-form effect	−0.536	−0.434*	−0.350	−0.287	−0.292
(no covariates)	(0.334)	(0.255)	(0.216)	(0.203)	(0.188)
Reduced-form effect	−0.461**	−0.412***	−0.422***	−0.379***	−0.272***
(including pre-treatment covariates)	(0.216)	(0.145)	(0.109)	(0.102)	(0.097)
Panel B: First-stage relationship					
First-stage effect	0.420***	0.319***	0.421***	0.392***	0.452***
(no covariates)	(0.129)	(0.114)	(0.116)	(0.108)	(0.106)
First-stage effect	0.453***	0.430***	0.422***	0.427***	0.472***
(including pre-treatment covariates)	(0.130)	(0.099)	(0.102)	(0.102)	(0.094)
Panel C: Wald or IV estimates					
Treatment effect	−1.274	−1.362*	−0.831	−0.732	−0.645
(no covariates)	(0.868)	(0.796)	(0.508)	(0.509)	(0.411)
Treatment effect	−1.017	−0.958**	−1.000***	−0.886***	−0.577**
(including pre-treatment covariates)	(0.630)	(0.453)	(0.370)	(0.315)	(0.233)
Number of municipalities	35	43	54	64	79
Number of observations	239	295	372	439	544

[a]Each entry is a separate local linear regression with a uniform kernel. All specifications allow for the RD slope to differ across the threshold and include a full set of time fixed effects. The dependent variable in Panels A and C is per capita welfare spending in logarithmic form. The dependent variable in Panel B is an indicator for having direct democracy rather than representative democracy. Panel C is the Wald estimator, the ratio between the reduced-form effect and the first-stage estimate. The forcing variable is population in year 1918. See the text for a description of included pre-treatment covariates. Standard errors, clustered at both the municipality level and the running variable, are within parentheses (Cameron, Gelbach, and Miller (2011)). Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Panel A of Table V shows the reduced-form estimates, Panel B the first-stage estimates, and Panel C the corresponding Wald estimates.[45] The reduced-form effect ranges from -24 percent (i.e., $100 * [\exp(-0.272) - 1]$) to -41 percent (i.e., $100 * [\exp(-0.536) - 1]$). These reduced effects are larger but also much less precisely estimated than the corresponding estimates in Panel A of Table III (e.g., the standard errors are 4–6 times larger). Nonetheless, all estimates are significant at the 5 percent level or better in the specifications with covariates.

Turning to the first-stage estimates in Panel B, they are all in the range from 32 to 47 percentage points. Specifically, the estimates with covariates are in a

[45]We report results from RD specifications treating the data as repeated cross-sections with time fixed effects. However, in the Supplemental Material, we show the corresponding results when we collapse the data into one single cross-section (Table A.IX). The results are strikingly similar.

more narrow range (42–47 percentage points) and precise. Compared to the previous RD design, the first-stage estimates are almost three times larger.

The IV estimates are displayed in Panel C. All estimated effects are in the range from −44 to −74 percent. Particularly, in the specifications with covariates, all IV estimates lie between −44 and −64 percent, which is in the same range as in the previous RD analysis (see Panel C of Table III). In other words, it seems that the treatment effect of most interest does not differ across the two RD designs. This suggests that the estimated effect may be generalized even to a larger population than the two subpopulations. It is important to stress that the two RD populations are not the same. Indeed, the overlap between the observations in the two RD designs is at most 23 percent.

As in the previous subsection, the other specification checks of the RD design do not indicate any problems. Figures 4 and 5 show the reduced-form relationship and the first-stage relationship and both display a clear discontinuity at the threshold, where the size of the jump closely corresponds to the estimated effects in Table VII.

Columns 3 and 4 of Table IV show the test of balance of the pre-treatment characteristics. (In the Supplemental Material, we show the corresponding graphical analyses.) We report estimates from two bandwidths: 50 and 60. Few of the specifications show any significant effect (3 out of 42 at the 10% level).

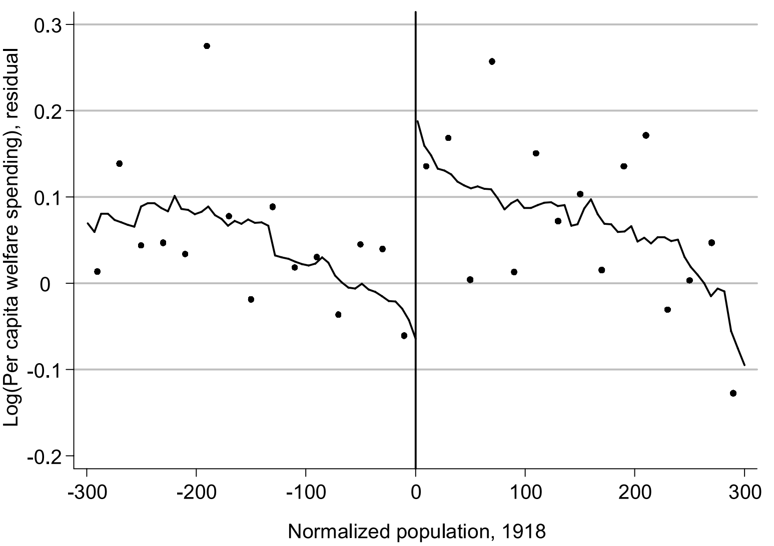

FIGURE 4.—Reduced-form relationship using population in 1918 as the forcing variable. The dependent variable is the residual from a regression of per capita welfare spending on 20 covariates. Plotted points are conditional means with a binwidth of 20. The solid line is the predicted values of a local linear smoother with a rectangular kernel and a bandwidth of 60.

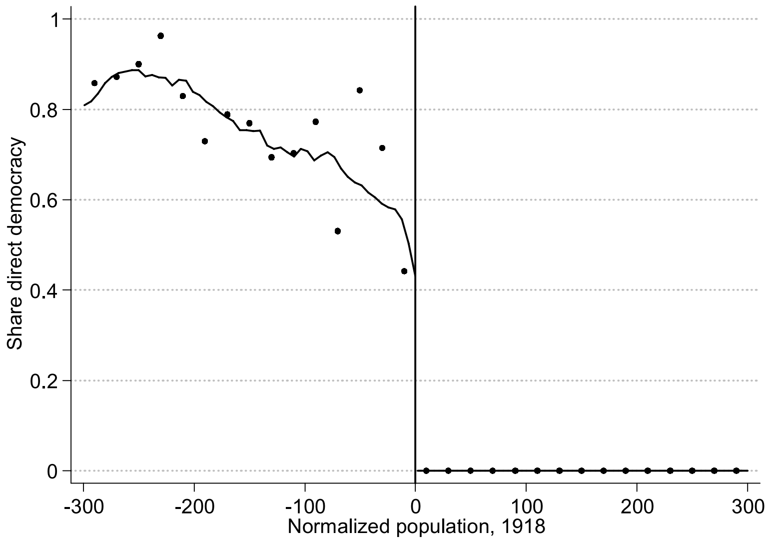

FIGURE 5.—First-stage relationship using population in 1918 as the forcing variable. The dependent variable is an indicator variable for having direct democracy. Plotted points are conditional means with a binwidth of 20. The solid line is the predicted values of a local linear smoother with a rectangualar kernel and a bandwidth of 60.

Finally, the McCrary density test does not indicate any sorting around the treatment threshold since the discontinuity estimate is very small and insignificant (−0.012 with a standard error of 0.101). Moreover, the graphical result displayed in Figure 6 does not indicate any jump in the distribution of the forcing variable at the discontinuity point. To sum up, all specification tests suggest that the RD design using population in 1918 as the forcing variable is credible.

5. MECHANISMS

In this section, we discuss—and present statistical evidence on—some of the potential mechanisms that could explain our main result, that is, that per capita social-welfare spending is much higher in representative than in direct democracy.

The ability of different groups in society to solve their collective-action problems may be influenced by existing institutions, as stressed by Acemoglu and Robinson (2006, 2008). If citizens can solve their collective-action problem, their argument goes, they can exercise additional de facto political power and therefore get more redistribution. Moreover, if citizens are well organized, this makes it more difficult for elites to exercise their de facto power (e.g., labor repression). In this perspective, social-welfare spending may be higher in representative democracy because it allows the large majority of the poor citizens

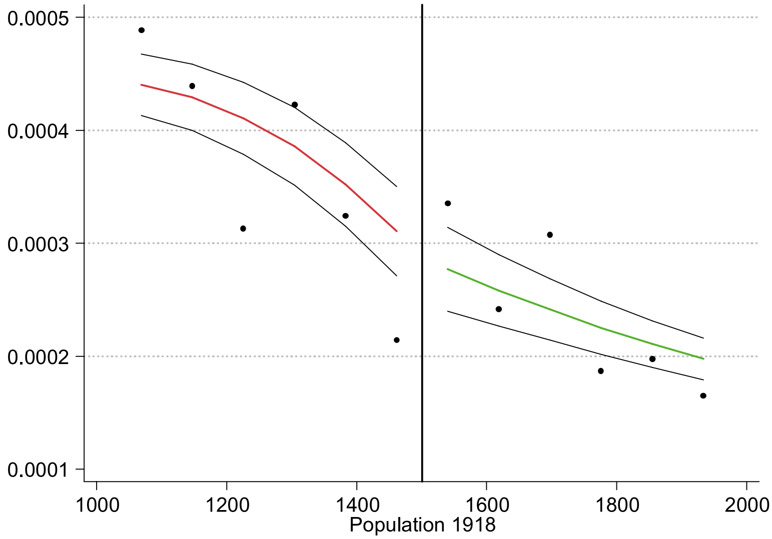

FIGURE 6.—McCrary density test: population in 1918 as the forcing variable. McCrary (2008) is a test of whether the density of the forcing variable, the population size in 1918, is continuous at the population threshold The point estimate is −0.0119 with a standard error of 0.1015.

to solve their collective-action problem via the existence of pro-citizen parties, political competition, and elections.

To test this hypothesis, we first need to find a measure of how well citizens are organized. To this end, we have put together a data set on the membership rates of the most important Swedish social movements: labor unions, temperance lodges, and free churches. These all shared the common goal of universal and equal suffrage, that is, they were all pro-citizen organizations. These data contain disaggregated information on the number of members at the end of each year for the period 1880–1945.[46] Using these data, we can measure how well citizens are organized at the local government level by the share of the population that belongs to a social movement. According to this measure, the percentage of organized citizens is nearly 9 percent during the period 1919–1938.

We can now use the same RD designs with the share of organized citizens as the outcome variable. Panel A of Table VI shows the estimated treatment effects for organized citizens. In Columns 1 and 2, the forcing variable is population in year $t-1$, and in Columns 3 and 4, the forcing variable is population in 1918. Once again, we show the results from multiple bandwidths: 70 and 80 for population in year $t-1$ and 50 and 60 for population in 1918. The estimated treatment effects range from −4.7 and −8.3 percentage points,

[46]There is, however, a great deal of missing data.

TABLE VI

TEST OF MECHANISMS[a]

Forcing Variable:	Population in Year $t-1$		Population in 1918	
Bandwidths:	70	80	50	60
	Panel A: Organized citizens			
Treatment effect	−0.060**	−0.061**	−0.083**	−0.047*
	(0.028)	(0.027)	(0.036)	(0.025)
	Panel B: Outdoor welfare spending			
Treatment effect	−1.134***	−1.294***	−0.739**	−0.754**
	(0.414)	(0.419)	(0.369)	(0.305)
	Panel C: Indoor welfare spending			
Treatment effect	−1.089	−1.048	−2.082	−0.489
	(1.758)	(1.696)	(2.358)	(1.661)

[a] Each entry is a separate local linear regression with a uniform kernel. All specifications allow for the RD slope to differ across the threshold, and include all pre-treatment covariates and a full set of time fixed effects. The dependent variable in Panel A is the share of organized citizens. The dependent variable in Panel B is per capita welfare spending on outdoor relief in logarithmic form. The dependent variable in Panel C is per capita welfare spending on indoor relief in logarithmic form. Coefficients significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

respectively, and are similar across the two RD designs. In other words, representative democracy has between 50 and 90 percent more organized citizens than direct democracy, since the mean is almost 9 percent.

To further probe the collective-action hypothesis, we use the disaggregated data for welfare spending (outdoor versus indoor welfare spending) mentioned in Section 2. Are better organized citizens (those living in representative democracy) able to exercise additional de facto political power to get more income redistribution? To answer this question, we evaluate how much of welfare spending was distributed as indoor relief and outdoor relief, respectively. We expect that organized citizens should mostly demand outdoor relief, since those receiving indoor relief (the permanently poor) had no political rights until 1945. Panel B of Table VI shows the results from using outdoor relief as the dependent variable in the two RD designs, while Panel C shows the results from using indoor relief as the outcome. Strikingly, the treatment effects are only statistically significant for outdoor relief. The estimated effects for outdoor relief correspond to an effect of 50 to 70 percent. These results strongly suggest that better organized citizens get more social welfare spending in a representative democracy, if they become unemployed, while unorganized citizens—the permanently poor—do not receive any additional welfare spending in a representative democracy.

So far, we have presented evidence that suggests that better organized citizens in the representative system are able to exercise their de facto political power to get more redistribution. Why cannot the local elite exercise their de facto political power to limit redistribution? Put differently, can the local elite capture the political process even after democratization? To probe this question, we would ideally like to have data on the identity of the local political elite before and after democratization in both representative and direct democracy. With such data, one could test whether the persistence in the identity of the local elite differs between the two political regimes after democratization (Acemoglu and Robinson (2008)). Since we currently lack such data, we have instead relied on information from some case studies made by Swedish historians (e.g., Wigren (1988), Tiscornia (1992), Nyström (2003), Nydahl (2010), Malmström (2006)). According to these studies, there is clear evidence of a strong persistence in the identity and power of the local political elites, at least before democratization. The most salient reason for this strong persistence was the graded voting scale based on income, property, and wealth. In fact, there were no restrictions on the maximum number of votes in the graded voting system until 1900. As a result, one individual had a majority of votes in a substantial number of local governments.[47] In 1900, the number of votes was capped to 5,000 and was further reduced to 40 in 1909. Some studies (e.g., Wigren (1988) and Nydahl (2010)) show that certain local elites could still maintain their key political positions even after the one-person–one-vote system had been introduced in 1919. In fact, in some municipalities (e.g., Ramsele, Ådalsliden, Torsåker, and Stigsjö) only one (male) person turned up to vote at the first election after democratization because the local elite had already determined the outcome of the election (Nydahl (2010)). Moreover, in many of these municipalities, there was no electoral competition until the mid 1930s.

We also have other suggestive evidence of local elite-capture after democratization based on the idea that the elite can limit redistribution if they are able to curb electoral competition. Specifically, redistribution should be particularly small if the elite can entirely block the entry of pro-poor parties in the election. To test this hypothesis, we have collected data on the number of parties participating in the election. Perhaps surprisingly, a single-party system is observed in a fairly large number of local elections. For example, in the first election in 1919, 30 percent of the local governments had a one-party system. On average, during 1919–1938, 19 percent of all elections had a single party running uncontested.

The simple idea is to compare the average outcomes—welfare spending, outdoor relief, indoor relief, voter turnout, and share of organized citizens—in single-party systems and multiparty systems. Naturally, the number of parties is potentially endogenous. To somewhat mitigate this concern, we limit the comparison to local governments that were forced to have representative democracy, that is, those with a population size above 1,500.

Table VII shows that per capita welfare spending is 18 percent larger in multiparty systems than in one-party systems. While multiparty systems have 33

[47] For example, there were 54 such local governments in 1871.

TABLE VII

ONE-PARTY SYSTEM VERSUS MULTIPARTY SYSTEM[a]

	Welfare Spending	Outdoor Relief	Indoor Relief	Organized Citizens	Voter Turnout
One-party system	6.329	3.517	2.774	0.092	0.235
Multiparty system	7.468	4.679	2.693	0.127	0.488
Difference in means	1.139***	1.162***	−0.081	0.035***	0.253***

[a] Welfare spending, outdoor relief, and indoor relief are all expressed in real per capita terms. Organized citizens and voter turnout are expressed as shares. Only local governments that are required to have representative democracy are included.

percent higher per capita outdoor relief, there is no difference in indoor relief. In addition, voter turnout is more than twice as high in multiparty systems, and the share of organized citizens is 40 percent higher. Thus, it seems that representative democracy with a one-party system can be characterized as a dysfunctional democracy since it does not create political equality through the free entry of parties.

The results in Table VII are strikingly similar to the previous RD results comparing representative democracy with direct democracy. Thus, the same mechanism may be at work, namely, the local elite captures representative democracy with a one-party system and direct democracy. It is important to point out that the difference in outcomes between a one-party system and a multiparty system is hard to explain with other models that could potentially explain the difference between direct and representative democracy, such as the median-voter explanation (Meltzer and Richard (1981)), the difference between open and closed ballots (Baland and Robinson (2008)), or differential costs of political participation in meetings and elections.

6. CONCLUSIONS

We compare how two political regimes—direct versus representative democracy—redistribute income toward the poor segments of society after the introduction of universal suffrage in Swedish local governments. For this purpose, we exploit a population threshold, which partly determined a local government's choice of democracy. Our regression-discontinuity design generates credible causal estimates under very weak identification assumptions. The results indicate that direct democracies spend 40–60 percent less on public welfare than representative democracies. We also find that citizens are much better organized collectively in representative democracies after democratization, and that unemployed workers tend to get more welfare support in those democracies than the permanently poor. These results are consistent with Acemoglu and Robinson's (2006, 2008) framework of democratization, which stressed how political regimes shape the ability of different groups in society to solve collective-action problems.

In future work, we hope to investigate how the two political regimes—direct versus representative democracy—affect long-run economic development outcomes such as health, structural shifts of employment and production from agriculture to manufacturing, and economic growth. We also plan to systematically analyze the persistence in the identity and power of the local political elites before and after democratization.

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