



SCHOOL OF
ECONOMICS AND
MANAGEMENT

The Effect of Left-Wing Party Control on Economic Outcomes

Evidence from Swedish Local Governments

By David Sandberg

Abstract: A common belief is that left-wing parties tend to use their influence to expand the public sector by increasing taxes – but is this belief supported by data? This study aims to find the causal effect of having left-wing majority and left-wing rule on a set of economic and policy outcomes by using panel data of Swedish local governments between 1995 and 2018. A regression discontinuity design is applied to exploit the discontinuous party control change at 50 percent of the vote share making left-wing majority as good as randomly assigned to observations close to the threshold. The findings suggest that left-wing party control has a negative effect on income inequality and intergovernmental grants and a positive effect on employment and income tax rate where one part of the employment increase stems from public sector expansion.

Keywords: economic outcomes, fiscal policies, local governments, regression discontinuity design

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Supervisors: Andreas Bergh and Therese Nilsson

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1. Introduction

In the 1994 local election of the Swedish municipality of Falköping the right-wing parties just barely got the majority of the mandates. A few more votes on the left-wing parties would have given them an extra mandate resulting in left-wing majority and, thus, allowing them to realize their desired politics for the following term of office. But, does it matter if right-wing or left-wing parties end up with majority? Would Falköping be different in terms of economic outcomes if the left-wing parties instead got majority? By using panel data of all Swedish local governments between 1995 and 2018, this study examines the causal effect of having left-wing party control on a set of economic outcome variables.

A common problem with cross-country studies is that governments have different political and institutional settings making comparisons problematic. That is not the case when studying Swedish local governments since they are characterized as homogenous regarding political and institutional settings but still has a high degree of autonomy – making it appealing to study outcome differences originated from differences of ruling compositions within Sweden. But proving correlation between left-wing vote share and outcome variables does not necessarily imply causal relationship since there might be unmeasurable and underlying factors, such as voter preferences, making the correlation uninterpretable as causation. Randomization of left-wing party control would solve this endogeneity problem but would conflict with our concept of democracy. I will overcome this problem by using a regression discontinuity design (RDD) which exploits the fact that party control changes discontinuously at 50 percent of the vote share. Around this threshold, party control is “as good as” randomly assigned which allows one to distinguish an estimate of the causal effect.

This study can be viewed as two-folded – one part where I examine the effects of left-wing majority and one part where I examine the effects of having a left-wing government, that is, having a left-wing rule. Consequently, the terms “*majority effect*” and “*rule effect*” is frequently used. Unlike when estimating the rule effect, the majority effect does not say much about the effect of the actual ruling composition but should be seen more as a proxy for left-wing influence. The actual ruling composition can be difficult to categorize since parties may form coalitions in different political issues and specific coalitions can be arbitrarily defined as left-wing or not. Together, both effects will contribute to a deeper understanding of how left-wing influence affects economic outcomes.

The majority effect is of deterministic character meaning that once the left-wing mandate share reaches 50 percent, the treatment status shifts from 0 (having no left-wing majority) to 1 (having left-wing majority). The rule effect, on the other hand, is of probabilistic character meaning that the treatment status is not only affected by the 50 percent threshold. Still, I exploit the discontinuity change of having a left-wing rule which can be found where the left-wing parties get 50 percent of the vote share, although the probability does not go from 0 to 1. Thus, two different types of RDD are employed, namely “*sharp*” RDD when estimating the majority effect and “*fuzzy*” RDD when estimating the rule effect.

I use panel data of all 290 Swedish local governments, called municipalities, between 1995 and 2018 which generates enough data around the threshold which is important when using an RDD approach. The legislative bodies of the municipalities' have some attractive features for this study since they have the potential to affect economic outcomes. They are responsible for delivering key welfare services such as childcare, education up to upper secondary school, social services and elderly care. In total, the municipalities employ around 20 percent of the Swedish workforce which makes them a major part of the Swedish economy and labour market. The municipalities have strong rights of self-government which are established in the Swedish constitution and they have complete power over a large part of the income tax rate and where to allocate tax income.

Showing evidence of a large party control effect would conflict with the *median voter theorem* and *Tiebout sorting*. If the median voter theorem holds, it would not matter in terms of economic outcomes if the left-wing parties in the election of 1994 in Falköping would get those extra votes needed to attain majority since both ruling types, i.e. right-wing and left-wing governments, adjust their politics according to the preference of the voter in the centre of the left-right scale. Tiebout sorting states that local governments offer a certain level of services related to a corresponding tax rate and individuals' choice of where to live depends on their preferences of the trade-off between services and tax rate. If the model holds, all individuals have chosen their preferred municipality to live in and, thus, municipalities have no need to change their politics implying no party control effect.

The findings of this study suggest that left-wing party control has a positive effect on employment and income tax rate where a part of the increase of employment comes from an expanding public sector. The Gini coefficient, i.e. income inequality, and intergovernmental grants are negatively affected. Consequently, there is a party control effect implying that I can reject the existence of the median voter theorem and Tiebout sorting. I find no evidence suggesting any significant effects on municipalities' income statements.

The outline of the paper is the following: in the next part I summarize previous research, section 2 describes the data used, section 3 describes the method applied, section 4 presents the results and in section 5 I discuss the findings.

1.1 Previous research

There are several studies examining the party control effect on economic outcomes and policies using an RDD approach. Most of this research focuses on local governments in the United States where the two-party system is appealing for an RDD approach. David Lee (2001) pioneered this field as he applied RDD on voting outcomes and found evidence suggesting that incumbency has a positive effect on the probability of winning the subsequent election.¹ Later, Ferreira & Gyourko (2009) applied an RDD on

¹ Lee (2008) did also in a similar study more carefully explore the mechanisms of RDD and its advantages.

the mayoral elections in cities of the United States and found that the political positioning of the mayor does not have any effect on the size of local government, the composition of public spending or the crime rate. However, they stress that for some types of cities, the reason for not finding any effects could arise from Tiebout sorting.

Caughey & Sekhon (2011) suggest that there is covariate imbalance around the threshold which conflicts with the important RDD assumption that treatment status changes discontinuously at the threshold but the potential outcome does not. The authors argue that previous studies about voting outcomes, particularly Lee (2008), does not take in to account the fact that relevant actors act differently when the election results are expected to be close to the threshold and, thus, one should not test for covariate discontinuity on data far away from the threshold. Their criticism of previous RDD studies' naïve assumptions is heavily based on that incumbency generates more money compared to the opposition which would be used in the subsequent election campaign if the election was expected to be tight. I find it unlikely that the same results would be found in Sweden where, within my understanding, the success of politicians is less dependent on campaign money.

Going to multi-party systems, Freier & Odendahl (2015) find that political power does matter for economic outcomes such as the income tax rate by using election outcomes of municipalities in Germany. In order to handle the non-dichotomous party control, which often is the case in a multi-party system since different coalitions can be formed in different questions, the authors construct a weighted index for political power which is not only dependent on whether the party in question is one of the majority ruling parties. Consequently, they focus on political power rather than mandate share which makes their paper to some extent unique.

Another way to handle issues related to multi-party systems is what Folke (2014) does when capturing the effect of one additional mandate of an individual single-issue party. Thus, he does not exploit the discontinuity around 50 percent but instead on each mandate assignment. He uses data of Swedish municipalities and finds that anti-immigration parties and the Green Party do influence policies within their respective areas. However, when looking at all Swedish parties, no significant effects on income tax rates were found.

The most influential paper on the Swedish context is Pettersson-Lidbom (2008) who applied an RDD on Swedish municipalities between 1974 and 1994 to examine the causal effect of left-wing majority on a set of economic outcomes. Unlike Folke (2014), Pettersson-Lidbom finds a significant (positive) effect of left-wing majority on income tax rate. Their different findings indicate that majority power constitutes tax rate policies rather than individual party representation. Pettersson-Lidbom also finds that public expenditures and employment are positively affected by left-wing majority but around half of the increased employment is a result of an expanded public sector. Recently, Fiva et al. (2018) estimated both the majority effect and individual party effect, i.e. a combination of Folke (2014) and Pettersson-

Lidbom (2008) but focusing on municipalities of Norway. They focus on income tax rate and public spending allocation and find that left-wing influence increases income tax rate and public spending but right-wing parties tend to spend more on elderly care whereas left-wing parties spend more on childcare.

The part when I estimate the majority effect is to some extent a reproduction of Pettersson-Lidbom's but with a more current period of time and some different outcome variables. Pettersson-Lidbom did not take into account that a left-wing majority does not necessarily imply left-wing rule – a gap which this study will fill by estimating the rule effect.

2. Data

I use a panel data set of all 290 Swedish municipalities between 1995 and 2018.² All data has been gathered from Statistics Sweden (Statistiska Centralbyrån) and Swedish Association of Local Authorities and Regions (SALAR) (Sveriges kommuner och regioner) where the data is publicly available. Before going into details about the characteristics of the data I provide some necessary information about the workings of Swedish municipalities.

2.1 The workings of Swedish municipalities

Once every fourth year in September all 290 municipalities hold elections for their municipal councils (Kommunfullmäktige) which is the decision-making body at the local level.³ The councils are required by the Swedish Local Government Act and consist of an odd number of seats between 21 and 101, depending on the population size of the municipality, which are all distributed to parties proportionally to the election results.⁴ The responsibilities of the municipalities, and indirectly the elected representatives in the councils, include, e.g. childcare, education up to upper secondary school, social services and elderly care making the municipalities a major factor in the Swedish economy. There is no binding coalition discipline in Swedish councils meaning that parties of the governing majority are not required to vote the same in all matters. In other words, there can be different coalitions for different political topics. Still, as Bäck (2008) suggests, the Swedish municipalities are characterized as “quasi-parliamentary” since the ruling parties appoint committee leaders and full-time posts.⁵

Between 1995 and 2018 it has been six terms of office with an average voter turnout of 81 percent. Since the data is conducted on an annual basis and the fact that elections are held in September, I choose to assign election results to the year after the election is held and onwards to the next election year. For instance, the election results of 1994 are used to the years 1995, 1996, 1997 and 1998. Consequently, the results from the election of 2018 are not used in this study. This choice is strengthened even more by the fact that new governments are in most cases formed in late September or mid-October and, thus, will not have any substantial impact on the election year in question. Moreover, municipal councils’ actions may not have a direct impact on economic outcomes also suggesting that this choice is convenient.

² Two municipalities have been established since 1995. Nykvarn in 1999 and Knivsta in 2003.

³ The elections for the regional council and national parliament take place at the same date.

⁴ Unlike at the national level, there is no lower electoral threshold for parties to get a seat in the municipal council. Although, as of 2018, a threshold has been introduced also at municipality level.

⁵ Bäck (2008) also thoroughly analyses of how party coalitions take place in Swedish municipalities.

2.2 Categorization of parties

The Swedish municipalities are multiparty systems and consist both of parties that have a national party represented in the national parliament, and local parties that are only active in one or a few municipalities. In this study, left-wing majority means that the Social Democrats, the Green Party and the Left Party have a majority of the mandates. Consequently, local parties that are left-wing parties will not be defined as that when estimating the majority effect.

When defining the categorization of left, right or mixed rule, I follow SALAR's categorization which is more flexible and does not restrict a left-wing government to only include the three left-wing parties. This is relevant when finding the rule effect and then I only look at the difference between left-wing and right-wing governments, i.e. not including mixed governments. Table 1 shows the majority and rule distribution. Columns (1)-(2) are based on the mandate distribution between the blocks and do not take the actual ruling composition into account. Columns (3)-(5) are based on SALAR's categorization. In total, the data set consists of 6,948 observations, where one observation represents a municipality at a given year.

Table 1. Party majority and party rule in Swedish municipal councils between 1995 and 2018

Election period	Left-wing majority (1)	Nonleft-wing majority (2)	Left-wing rule (3)	Right-wing rule (4)	Mixed rule (5)
1995-1998	776	376	700	240	212
1999-2002	564	592	508	420	228
2003-2006	600	560	503	416	241
2007-2010	404	756	365	628	167
2011-2014	432	728	439	564	157
2015-2018	364	796	396	356	408
Sum	3,140	3,808	2,911	2,624	1,413

2.3 Definitions and characteristics of variables

Table 2 presents some characteristics of the variables used in this study and is divided into outcome variables, covariates and forcing variable. Total employment measures the share of workers of the population who lives in the municipality (night-time population). However, the public and private employment variables measure the share of people working in the municipality and not necessarily live there (day-time population). Thus, one should be careful when comparing the effects on these three employment variables. One reason might be that total employment will, most likely, be positively affected if a neighbouring municipality starts to employ more people whereas public and private employment may be negatively affected – due to the difference in measuring night-time or day-time population. Public employment represents the share of the population who works for a municipality's

administration and municipal-owned companies and organizations. That means that employees of the regional government and the national government are excluded from this variable. Thus, the public sector employment variable reflects public jobs which the municipal council to some extent can affect. The sum of the public and private employment does *not* add up to total employment due to the definitions explained above.⁶

Table 2. Summary Statistics

Variables	Number of observations ⁷	Mean	Standard deviation	Minimum	Maximum
Outcome variables					
Total employment (%)	6,372	77.7	4.36	51.4	89.1
Public employment (%)	6,948	18.7	4.13	5.41	35.8
Private employment (%)	6,948	45.8	12.5	11.8	124 ⁸
Income tax rate (%)	6,948	21.3	1.43	14.5	33.6 ⁹
Gini coefficient (×100)	3,480	32.2	3.16	25.6	52.8
Income statement*	6,084	716	1,511	-13,704	26,639
Intergovernmental grants*	6,084	9,410	5,899	-16,124	34,227
Covariates					
Population	6,948	32,070	62,922	2,421	962,154
20-65 years (%)	6,948	55.6	2.74	45.7	66.5
Average earned income*	6,948	236,970	55,393	131,765	634,585
Forcing variable					
Left mandates (%)	6,948	49	11.8	11.1	82.9

*expressed in current prices (SEK)

Income tax rate refers to the municipal income tax rate which is decided by the municipal council. The Gini coefficient measures the distribution of disposable income in each municipality, i.e. it is a proxy for income inequality. It ranges from 0 to 1 where 0 indicates complete equality (disposable income is the same for everyone) and a value of 1 indicates complete inequality (disposable income is concentrated to one individual). The income statement corresponds to municipalities' revenues minus expenses for the specific year. Revenues and expenses defined as "extraordinary" by Statistics Sweden are excluded. Sweden has an economic equalization system between municipalities that aims to equalise structural and taxing power differences between municipalities. Some municipalities receive grants and others pay

⁶ Furthermore, total employment measures the share of individuals between 16 and 64 years old whereas public and private employment measure the share of individuals older than 20 years.

⁷ Data is available from 1995 except employment (1997), Gini coefficient (2007), intergovernmental grants (1998) and income statement (1998) which explains the differences in number of observations.

⁸ Since private employment refers to the day-time population, it is possible that the share is higher than 100 percent. In these cases, there are more people between 20 and 65 years who work in the municipality than live there.

⁹ Excluding the municipality of Gotland, the maximum would be 23.95. The reason is that the municipality of Gotland uniquely also levies the regional taxes, being the only municipality in the region of Gotland.

a fee based on the equalization system. I refer to it as intergovernmental grants, where grants are shown as positive values and fees as negative values. From a democratic perspective, intergovernmental grants should neither be causally affected by left-wing mandates nor by the ruling composition.¹⁰ Income statement and intergovernmental grants are expressed in per capita. Average earned income is, more specifically, the average of the total earned income in a given year for individuals between 20 and 65 years old.

Table 3. Differences in means between left-wing and nonleft-wing majority and between left-wing and right-wing rule

Variables	Left-wing majority means (1)	Nonleft-wing majority means (2)	Difference in means (1)-(2) (3)	Left-wing rule means (4)	Right-wing rule means (5)	Difference in means (4)-(5) (6)
Outcome variables						
Total employment (%)	75.8	79	-3.2***	75.9	79.2	-3.3***
Public employment (%)	19.7	17.8	1.93***	19.7	17.4	2.23***
Private employment (%)	44.5	46.8	-2.38***	45.3	46.4	-1.05**
Income tax rate (%)	21.7	21	.699***	21.7	20.9	.798***
Gini coefficient (×100)	31.2	32.7	-1.5***	31.4	33	-1.6***
Intergovernmental grants	10,054	8,943	1,111***	10,095	7,993	2,102***
Income statement	553	834	-281***	586	825	-240***
Covariates						
Population	35,766	29,022	6,745***	38,020	30,471	7,549***
20-65 years (%)	55.9	55.4	.491***	55.8	55.5	.314***
Average earned income	220,130	250,856	-	222,709	252,550	-29,841***
Forcing variable						
Left mandates (%)	59.3	40.5	18.8***	58.2	39.6	18.6***

* p<0.05, ** p<0.01, *** p<0.001

Table 3 presents the means and differences in means for all left- and nonleft-wing majorities as well as all left- and right-wing governments. All variables differ significantly which is expected. However, it is important that the covariates are pre-determined and not an effect of the treatment status when using an RDD. In other words, there should be no significant difference between the covariates for observations just below and observations just above the threshold since I assume that treatment is as good as randomly assigned to these observations. To examine that, table 4 is presented which is the same as Table 3 except for being restricted to observations close to the 50 percent threshold ($\pm 2\%$). There are significant differences in the employment variables for both majority and rule differences. The differences between the covariates of the majority types are not significant which indicates that they are not affected by

¹⁰ Previous studies of Swedish municipalities have shown that intergovernmental grants are to some extent tactically distributed (Johansson, 2003) and that the grants are shifted to more public spending but has no negative effect on the tax rate (Dahlberg et al., 2008).

treatment status. However, the average earned income is significantly larger for municipalities with left-wing rule which could indicate that the treatment is not randomly assigned around the threshold. This is partly the reason I will do specification tests. Note that showing significant differences in means on each side of the threshold, as shown in table 4, is not sufficient to confirm the existence of causal effect of left-wing majority or left-wing rule.

Table 4. Differences in means between left-wing and nonleft-wing majority and between left-wing and right-wing rule within $\pm 2\%$ from the 50% threshold

Variables	Left-wing majority means (1)	Nonleft-wing majority means (2)	Difference in means (1)-(2) (3)	Left-wing rule means (4)	Right-wing rule means (5)	Difference in means (4)-(5) (6)
Outcome variables						
Total employment (%)	76.5	77.6	-1.1***	76.5	77.6	-1.1***
Public employment (%)	18.7	18	.639*	18.6	17.8	.747*
Private employment (%)	46.5	44.4	2.1**	47.1	42.3	4.75***
Income tax rate (%)	21.5	21.3	.238*	21.5	21.4	.0104
Gini coefficient ($\times 100$)	32.2	32	.223	32.4	31.7	.689
Intergovernmental grants	9,952	9,607	344	9,935	8,370	1,565**
Income statement	809	815	-5.77	822	670	152
Covariates						
Population	47,069	36,624	10,446	56,547	43,793	12,754
20-65 years (%)	56.2	55.9	.262	56.3	56.5	-.207
Average earned income	234,355	236,160	-1,806	237,548	226,117	11,431**
Forcing variable						
Left mandates (%)	51.2	48.8	2.43***	50.7	49.2	1.52***

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

3. Method

Optimally, the best way to find the causal effect of left-wing majority or left-wing rule would be to randomize mandates and ruling parties, respectively, in all municipalities. With a sufficient number of observations, the average difference in economic outcomes between left-wing rule and nonleft-wing rule could then be interpreted as the causal effect of having left-wing rule. Applying this randomization in reality would conflict with our concept of democracy and, thus, I am left with choosing non-experimental methods that can reflect the randomization process.

I apply a regression discontinuity design (RDD) which is a quasi-experimental method and useful in situations where observations get a treatment, or an intervention, depending on if their value for a continuous variable (called forcing variable) falls below or above a certain threshold.¹¹ The basic idea behind RDD is that observations that fall just below the threshold should, in average, be identical to observations just above the threshold except for the exposure of treatment. To put it differently, there is a randomization of the treatment status for observations around the threshold. This enables one to estimate the causal effect of the treatment.¹²

Literature typically distinguishes two types of RDD – “sharp” RDD and “fuzzy” RDD. The latter is used when reaching the threshold does not necessarily imply treatment whereas the former is of deterministic character and used when reaching the threshold does always imply treatment. In other words, fuzzy RDD means that when the forcing variable exceeds the threshold, the observation has a probability of getting treated and if this probability is equal to one, it is called a sharp RDD. In the context of this study, the observations are municipalities, the forcing variable refers to left-wing mandate share and the threshold is where the left-wing parties have 50 percent of the mandate share, i.e. have a majority.

Specification tests is done to reassure that the treatment is as good as randomly assigned and not determined by pre-treatment covariates. Furthermore, I will run placebo tests where I set the threshold at different values. The purpose of placebo tests is to show that the treatment is causing the effect by giving examples of that other, arbitrary, thresholds will not show any significant effects.

3.1 Majority effect

When estimating the majority effect, I apply a sharp RDD since reaching the threshold implies, by definition, to get treated by a left-wing majority. The treatment status is then a deterministic function of

¹¹ The application of RDD is widely used in economic research and in particular when evaluating policies, for instance, the impact of financial aid offers on students’ college decisions (van der Klaauw, 2002), if class size has effect on test scores (Angrist & Lavy, 1999), the effect of air quality on the housing market (Chay & Greenstone, 2005) and unionization’s effect on private employees (DiNardo & Lee, 2004).

¹² For a more technical view of RDD, see Imbens & Lemieux (2008).

left-wing mandate share where reaching 50 percent implies getting treated. More formally, it can be written as

$$T_{it} = 1(\text{Left mandate share}_{it} \geq 50\%) \quad (1)$$

where T_{it} is a dummy variable equal to one if the left-wing vote share in year t for municipality i is larger or equal to 50 percent. Hence, T_{it} shows whether there is a left-wing majority or not. The models used when estimating the majority effect have the form:

$$Y_{it} = \alpha + \beta T_{it} + f(Z_{it}) + X_{it} + \lambda_t + \gamma_i + \varepsilon_{it} \quad (2)$$

where Y_{it} represents the outcome for municipality i in year t . T_{it} denotes treatment status, as explained in equation (1). β is the coefficient of interest showing the effect of the treatment and can be referred to as the treatment effect on the treated. Z_{it} is the forcing variable (left-wing mandate share) and $f(Z_{it})$ is the functional form which, ideally, reflects the relation between the forcing variable and the outcome. In the simplest model, the functional form only consists of left-wing mandate share as a linear function but for flexibility, it will also include higher-order polynomials in left-wing mandate share.¹³ X_{it} is a set of covariates that should not significantly affect the estimates if the functional form is correctly specified and, thus, including this is a specification test. λ_t and γ_i are the year fixed effects and municipality fixed effects which controls for differences between municipalities and between years. For instance, decisions taken at national government level in a specific year, which may affect economic outcomes, will now be controlled for.

3.2 Rule effect

When estimating the rule effect a fuzzy RDD is applied since reaching the threshold does not automatically imply treatment i.e. having a left-wing rule. Nevertheless, the threshold still induces a change in the probability of treatment, which is presented in figure 1, although not from zero to one. In the case of a fuzzy RDD, β in equation (2) reflects the intention-to-treat effect which is the effect of eligibility rather than the treatment itself. To get the effect for those who actually get treated, called the local average treatment effect (LATE), I use an instrumental variable (IV) design where the threshold rule works as an instrument for the treatment. Unlike the intention-to-treat effect, the LATE represents the rule effect. In an IV design it is important to verify a *first stage*, which in this case is to verify a discontinuity in the probability of treatment when reaching the threshold. Also, one must estimate the relation between getting treated and the outcomes – this relation is called the *reduced form*.

¹³ In similar studies, high-order polynomials are almost always used. However, Gelman & Imbens (2019) highlights some disadvantages with using high-order polynomials.

I do not suspect different relations between the forcing variable and the outcomes on either side of the threshold which is the reason that I will not use interactions variables between the forcing variable and treatment status.

$$D_{it} = \delta_0 X_{it} + \delta_1 T_{it} + v_{it} \quad (\text{First stage})$$

$$Y_{it} = \pi_0 X_{it} + \pi_1 T_{it} + \varphi_{it} \quad (\text{Reduced form})$$

The first stage estimates the effect of reaching the threshold on the treatment status D_{it} . If δ_1 is large, the change in probability is large meaning there is a *strong* first stage. Figure 1 illustrates the first stage where one can see a clear jump in the share of left-wing governments at the 50 percent threshold. The reduced form estimates the effect of reaching the threshold on outcome variable Y_{it} . By using these two equations, one can estimate the LATE by using the two-stage least-squares estimator.

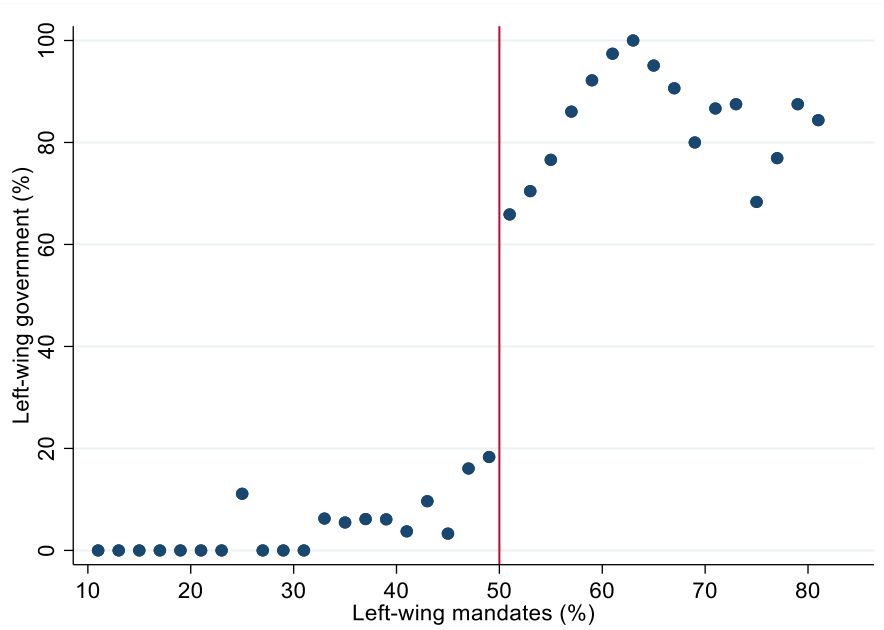


Figure 1. First stage¹⁴

One assumption needed to implement a proper instrument is the *exclusion restriction* which says that the reason for the instrument to have an impact on the outcome variable is through the first stage. For this study, it means that the effect of having majority on outcome variables exists due to the effect of having majority on having left-wing rule. If this would hold, it means that left-wing parties only can affect the outcome variables when they are ruling, i.e. that the opposition has no political power. This may be a naïve assumption but there are no proper ways to test whether the exclusion restriction holds.

3.3 Non-parametric approach

So far, I have taken a parametric approach, that is, using all data when running the regressions. The precision is high when using all data but the disadvantage is the risk of introducing high levels of bias since it is difficult to specify the correct functional form, in this case $f(Z_{it})$ in equation (2). Therefore,

¹⁴ One bin represents municipalities within a bandwidth of two percentage points. So, for instance, the first bin to the left of the threshold contains the municipalities with left-wing mandate share between 48 and 50 percent.

as Hahn et al. (2001) and Lee & Lemieux (2010) suggest, a non-parametric method is also applied to estimate the treatment effect and as a robustness check for the parametric estimates. In a non-parametric method, only observations close to the threshold are used – reducing bias which would otherwise result from using observations far away from the threshold.¹⁵ As discussed above, the characteristics of the observations can, on average, be assumed to be similar except the treatment status. This means that the non-parametric method is neither dependent on specifying a correct functional form nor covariates, unlike in the parametric case.

There is a trade-off between parametric and non-parametric RDD. On the one hand, when applying the parametric approach the precision becomes greater. On the other hand, there is a risk for higher bias since it is difficult to specify the correct model – which is the reason different equations for the parametric model will be used. Conversely, when applying the non-parametric approach the bias will be low but the statistical power will drop since less data is used. This trade-off is not easy to deal with but a convenient solution is to present the estimated effects in both methods as well as having different control functions in the parametric models and different bandwidths in the non-parametric models. If the estimated effects in all models are similar, it is an indication of robustness.

¹⁵ In my data set, there are 1,048 observations within two percentage points from the 50 percent threshold.

4. Results

Tables 5 and 6 present the results of the regressions on left-wing majority and left-wing rule, respectively, where all estimates represent one separate regression. All outcome variables are in logarithmic form enabling a percentage change interpretation of the coefficients.¹⁶ When estimating the majority effect (table 5) equation (2) is used and when estimating the rule effect (table 6) IV design is used giving the LATE. All data is used when estimating the majority effect whereas municipalities defined with a mixed government are excluded when estimating the rule effect. This means that there are slightly more observations used when estimating the majority effect. See Appendix A for an overview of the number of observations in each regression.

It is difficult to specify the correct functional form and therefore I use seven different models that are similar for both the majority and rule effect. Models 1-4 go from first- to fourth-order polynomial in left-wing mandate share. Model 5 includes covariates (total earned income, the population of the municipality and share of the population between 20 and 65 years), as well as the fourth-order polynomial of left-wing mandate share. Adding the covariates should not affect the estimates but only reduce standard errors since they should be pre-determined and not affected by treatment and, thus, model 5 can be viewed as a specification test.

Models 6-7 are non-parametric so only data from municipalities closer than two and three percentage points, respectively, from the 50 percent threshold is used. Neither left-wing mandate share nor covariates are included in models 6-7. The reason behind this is that the treatment is as good as randomly assigned for these municipalities making the addition redundant. All models include year fixed effects and municipality fixed effects. If the coefficients are similar in all models, it is an indication of unbiased estimates. I choose not to use standard errors clustered to municipalities since it is not a multi-level model (it would be if I for instance also used regional councils). However, in Appendix B, the same output tables are presented but where the standard errors are clustered to municipalities. Similar studies are not consistent with having clustered standard errors or not – e.g. Pettersson-Lidbom (2008) has it whereas Hyytinen et al. (2018) have not.¹⁷

¹⁶ Income statement and intergovernmental grants can take negatively values that cannot be log-transformed. Therefore, prior the log-transformation, I have added the smallest value (plus 1) to all observations for both these variables making the minimum value equal to one.

¹⁷ See Bartalotti & Brummet (2017) for an insight about whether to use clustered standard errors in RDD.

Table 5. Majority effect

Logged outcome variables	1	2	3	4	5	6	7
Total employment	0.0012 (0.0011)	0.0013 (0.0011)	0.0020 (0.0013)	0.0020 (0.0013)	0.0028* (0.0012)	0.0071*** (0.0014)	0.0068*** (0.0011)
Public employment	0.022*** (0.0037)	0.022*** (0.0037)	0.016*** (0.0042)	0.017*** (0.0042)	0.0083* (0.0040)	0.018** (0.0061)	0.013* (0.0055)
Private employment	0.0083* (0.0039)	0.011** (0.0039)	0.0033 (0.0045)	0.0031 (0.0045)	0.0061 (0.0043)	0.015** (0.0057)	0.013** (0.0048)
Income tax rate	0.0064*** (0.00086)	0.0065*** (0.00086)	0.0037*** (0.00098)	0.0039*** (0.00098)	0.0029** (0.00098)	0.0010 (0.0012)	0.0025* (0.0011)
Gini coefficient	-0.0033* (0.0014)	-0.0033* (0.0014)	-0.0039* (0.0015)	-0.0036* (0.0015)	-0.0024 (0.0013)	-0.0062*** (0.0016)	-0.0061*** (0.0014)
Income statement	-0.0019 (0.0091)	-0.0020 (0.0091)	-0.0037 (0.010)	-0.0030 (0.010)	-0.0019 (0.010)	-0.0014 (0.0079)	0.0018 (0.0069)
Intergovernmental grants	-0.0087 (0.0074)	-0.0095 (0.0074)	-0.015 (0.0084)	-0.012 (0.0084)	-0.018* (0.0077)	-0.017** (0.0052)	-0.020*** (0.0048)
Sample	Full	Full	Full	Full	Full	±2%	±3%
Polynomial of left-wing mandate share	First	Second	Third	Fourth	Fourth	None	None
Covariates	No	No	No	No	Yes	No	No

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5 and 6 show a strikingly similar pattern – all corresponding coefficients goes in the same direction except for income statement.¹⁸ This fact makes my inference about what direction left-wing party control has on these outcomes more reliable. Employment and income tax rate are positively affected by both left-wing majority and left-wing rule. For instance, public employment increases with between 0.8 and 2.2 percent when having a left-wing majority suggesting that the increase of total employment could be partly explained by an increase in public employment, i.e. public sector expansion. Interestingly, total employment seems to be positively affected by left-wing majority according to these models although the differences of means around the threshold presented in table 4 suggested the opposite. This highlights the fact that one should not draw any strong inferences from only comparing means since the difference could be “part” of the relation between mandate share and total employment.

¹⁸ I did run the same regressions only on the last year in power based on the hypothesis that the ruling parties will not have any impact during the first years in power. Moreover, I did also run regressions excluding the three most populated municipalities motivated by their different labour market characteristics. None of these showed any notable differences from the ones presented.

Table 6. Rule effect

Logged outcome variables	1	2	3	4	5	6	7
Total employment	0.0045* (0.0021)	0.0048* (0.0021)	0.0084** (0.0027)	0.0083** (0.0027)	0.0091*** (0.0026)	0.0085* (0.0038)	0.0093** (0.0034)
Public employment	0.036*** (0.0070)	0.033*** (0.0071)	0.023** (0.0089)	0.024** (0.0089)	0.0061 (0.0085)	0.053* (0.024)	0.023 (0.021)
Private employment	0.021** (0.0070)	0.030*** (0.0070)	0.022* (0.0089)	0.021* (0.0089)	0.024** (0.0085)	0.037* (0.015)	0.017 (0.013)
Income tax rate	0.011*** (0.0016)	0.011*** (0.0016)	0.0068*** (0.0020)	0.0069*** (0.0020)	0.0050* (0.0020)	0.0066 (0.0040)	0.014*** (0.0035)
Gini coefficient	-0.0019 (0.0030)	-0.0019 (0.0030)	-0.0061 (0.0037)	-0.0062 (0.0037)	-0.0040 (0.0032)	-0.011* (0.0056)	-0.014** (0.0054)
Income statement	0.0036 (0.018)	0.0046 (0.018)	0.0024 (0.023)	0.0029 (0.023)	0.0057 (0.023)	-0.0051 (0.031)	0.0076 (0.028)
Intergovernmental grants	-0.0083 (0.016)	-0.011 (0.016)	-0.032 (0.021)	-0.027 (0.021)	-0.034 (0.019)	-0.057** (0.018)	-0.054** (0.018)
Sample ¹⁹	Full	Full	Full	Full	Full	±2%	±3%
Polynomial of left-wing mandate share	First	Second	Third	Fourth	Fourth	None	None
Covariates	No	No	No	No	Yes	No	No

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

The Gini coefficient is negatively affected by left-wing influence, indicating that income inequality decreases when having a left-wing majority or left-wing rule. None of the coefficients for income statement are significantly different from zero. Intergovernmental grants are negatively affected meaning that having a left-wing majority or left-wing rule makes municipalities receive less from the economic equalization system.

The estimates of the non-parametric models 6-7, differ slightly from the rest indicating that there is misspecification of the functional form in the parametric models 1-5 but, however, the sign is the same regardless of model. Furthermore, there are consistently higher standard errors for the non-parametric approach. This suggests that the parametric approach is preferred, just as Pettersson-Lidbom (2008) argues.

4.1 Specification tests

When adding the pre-treatment covariates (model 5), the estimates differ for some outcome variables. This could be problematic since it indicates that the treatment may not be as good as randomly assigned

¹⁹ The samples do not include mixed governments, even if it says “Full”.

which is a fundamental assumption when applying an RDD. Ideally, the estimate of model 5 should be the same as in models 1-4 except having lower standard errors. Partly due to this, I have done two additional specification tests which are presented in table 7. All estimates represent one separate regression. I use the fourth-order polynomial in left-wing mandate share as well as municipality and year fixed effects. In columns 1-2, the covariates are the outcome variable and the presented estimates show the effect of left-wing majority and left-wing rule, respectively. In columns 3 and 4, the coefficients represent the impact of the covariate on left-wing majority and left-wing rule, respectively. If the coefficients are not significantly different from zero, it is a strong indication that the pre-treatment covariates are independent of treatment status. The coefficients shown in table 7 are fairly reassuring since there is only one significant coefficient, namely population's effect on left-wing majority.

Table 7. Specification tests

Covariates	Left-wing majority as independent variable (1)	Left-wing rule as independent variable (2)	Left-wing majority as dependent variable (3)	Left-wing rule as dependent variable (4)
20-65 years (%)	-0.0979 (0.129)	-0.0218 (0.0839)	-0.00462 (0.00610)	-0.00219 (0.00847)
Average earned income	-59.68 (1491.8)	-967.7 (716.4)	-2.65e-08 (6.64e-07)	-9.17e-07 (7.00e-07)
Population	-2071.0 (1586.2)	-91.16 (410.3)	-2.28e-06** (7.22e-07)	-2.14e-07 (1.01e-06)
Number of observations	6948	6948	6948	6948

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

4.2 Placebo tests

To further test if the treatment is the driving force behind the effects I check whether there are discontinuities at other values of left-wing mandate share. This is called a placebo test, or falsification test, and is a common robustness check when using RDD.²⁰ In practice, it means that I use the same method as before and examine other, arbitrary, thresholds to see if any significant effect is found. In Appendix C, I use a placebo threshold at 45 percent of left-wing mandate share for both majority and rule effect. Thus, models 1-7 are the same as the models in table 5 and 6 except for using a different threshold. The majority effect is significant for only income statement which was the only outcome that did not show significance when using the “real” threshold. The rule effect is significant for several variables. I did run additional placebo tests with other placebo thresholds and the majority effect rarely showed any significant coefficients whereas the rule effect often did. This implies that the majority effect estimates in this study are reassuring but the rule effect estimates are not.

²⁰ For example, see Lee (2008) or Croke et al. (2016).

5. Discussion

In this paper I estimated the causal effect of left-wing majority and left-wing rule by applying an RDD on Swedish municipalities between the years 1995 and 2018. By exploiting the discontinuity around 50 percent of the vote share, I found a way to make interpretations similar to when having a randomization process. The findings suggest that left-wing party control has a positive effect on employment and income tax rate but a negative effect on income inequality and intergovernmental grants. The increased employment is partly explained by left-wing governments employing more within the public sector and the decreased income inequality could be an effect of the higher tax rate.²¹ Income statement seems to be unaffected by party control. Various specification tests and placebo tests have to some extent made these findings more assured.

My estimated effects on income tax rate and employment are in line with Pettersson-Lidbom's (2008) except being slightly smaller and less significant. The fact that we use a completely different period of time and have different definitions of employment does most likely explain the differences. My estimates of left-wing majority on income tax rate are particularly smaller than Pettersson-Lidbom's and more similar to Folke's (2014) who uses a more alike period of time.

The majority effect does not take in to account the existence of left-wing local parties and that left-wing parties could still form a government even without having majority since a local party or a centre right-wing party could support them. These two issues with the majority effect are solved by estimating the rule effect using the actual ruling composition of the municipalities. Having majority will then work as an instrument for having an actual left-wing rule. Together, the majority and rule effect estimates are a convenient approximation of the causal effect of left-wing party control. The results have the same pattern both when estimating the majority effect and the rule effect which indicates of reliable results.

However, my specification and placebo tests signalled that the rule effect could have flaws. As Caughey & Sekhon (2011) argue often is the case, there could be covariate imbalance around the threshold, as indicated when adding the pre-treatment covariates in model 5. Furthermore, the placebo test gave hints that there is not only the treatment which is the driving force of the estimated rule effects. I believe that this arises since the exclusion restriction does not perfectly hold which was mentioned in section 3 meaning that one should be careful when drawing too strong inferences of the rule effects estimates. It is difficult to test the exclusion restriction but one way could be to create a power index for parties as Freier & Odendahl (2015) did and see if individual opposing parties have any significant influence. Alternatively, the flaws of my estimated rule effects could indicate that economic outcomes are not affected by the ruling composition but, only, by the left-wing mandate share or left-wing majority.

²¹ Lambert (1993) elaboratively discusses the positive relation between income tax rate and income inequality.

Neither majority nor rule effect takes into account that coalitions in different political questions can be formed. As Bäck (2008) argues, parties should not be characterized as unitary actors across municipalities – two parties in one municipality may cooperate in a political issue whereas the same two parties in another municipality may refuse to cooperate in a similar issue. This illustrates one weakness of the current study and the need for a more thorough investigation of the workings of coalitions and how coalitional bargaining power changes as a party's mandate share changes. One way is to estimate the individual mandate effect as Folke (2014) and Fiva et al. (2018) did, instead of exploiting the majority threshold. This choice of focus is a matter if one believes that parties only matter when they are decisive for creating majorities or, as Lijphart (1999) argues and what the findings of Fiva et al. suggests, that minority parties have political influence proportional to their mandates. However, Folke (2014) argued that the income tax rate is a matter of who has majority whereas some variables are affected by the proportion of mandates, holding majority constant. This fact could indicate that policy actions, such as level of income tax rate and to some extent the number of public employees, are affected by the political positioning of the ruling government whereas economic outcomes which are only indirectly affected by the government, such as private sector employment and income inequality, are affected by individual party representation.

Finding significant majority effects indicates a high level of partisanship in municipalities. This implies a rejection of the median voter theorem and Tiebout sorting. The former is not rejected when the parties position themselves around the centre making the majority effects close to zero and the latter is not rejected when all citizens live in their preferred municipality regarding the level of tax rates and services implying that there is no need for partisanship within municipalities. Still, the hypotheses may to some extent hold and I believe it is a topic for future work to determine how much partisanship is constraint by the median voter theorem and Tiebout sorting.²²

In the introduction I mentioned the 1994 local election of the Swedish municipality of Falköping where the right-wing parties barely got majority. This study predicts what would happen in a counterfactual scenario where the left-wing parties got an additional mandate and, thus, the possibility to form a majority coalition. In that case, the people of Falköping would, according to my estimates, receive a smaller amount of intergovernmental grants and face higher employment, income tax rate and income equality.

²² Besley & Case (2003) and Ferreira & Gyourko (2009) discuss the relevance of median voter theorem and Tiebout sorting, respectively.

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Appendix A

Number of observations in each regression

	Majority effect	Rule effect
Total employment	6372	5065
Public employment	6948	5535
Private employment	6948	5535
Income tax rate	6940	5527
Gini coefficient	3480	2758
Income statement	6082	4830
Intergovernmental grants	6084	4830
±2%	1,048	704
±3%	1,316	916

Appendix B

Majority effect with clustered standard errors

Logged outcome variables	1	2	3	4	5	6	7
Total employment	0.0012 (0.0023)	0.0013 (0.0023)	0.0020 (0.0026)	0.0020 (0.0026)	0.0028 (0.0024)	0.0071* (0.0027)	0.0068** (0.0024)
Public employment	0.022** (0.0081)	0.022** (0.0082)	0.016 (0.011)	0.017 (0.0100)	0.0083 (0.0084)	0.018 (0.012)	0.013 (0.011)
Private employment	0.0083 (0.0091)	0.011 (0.0088)	0.0033 (0.0090)	0.0031 (0.0091)	0.0061 (0.0085)	0.015 (0.014)	0.013 (0.012)
Income tax rate	0.0064*** (0.0019)	0.0065*** (0.0019)	0.0037 (0.0020)	0.0039 (0.0020)	0.0029 (0.0019)	0.0010 (0.0026)	0.0025 (0.0021)
Gini coefficient	-0.0033 (0.0028)	-0.0033 (0.0028)	-0.0039 (0.0031)	-0.0036 (0.0031)	-0.0024 (0.0026)	-0.0062 (0.0039)	-0.0061 (0.0033)
Income statement	-0.0019 (0.010)	-0.0020 (0.010)	-0.0037 (0.010)	-0.0030 (0.010)	-0.0019 (0.010)	-0.0014 (0.0096)	0.0018 (0.0083)
Intergovernmental grants	-0.0087 (0.0061)	-0.0095 (0.0060)	-0.015 (0.0096)	-0.012 (0.0087)	-0.018 (0.0095)	-0.017 (0.010)	-0.020* (0.0088)
Sample	Full	Full	Full	Full	Full	±2%	±3%
Polynomial of left-wing mandate share	First	Second	Third	Fourth	Fourth	None	None
Covariates	No	No	No	No	Yes	No	No

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Rule effect with clustered standard errors

Logged outcome variables	1	2	3	4	5	6	7
Total employment	0.0045 (0.0043)	0.0048 (0.0041)	0.0084 (0.0050)	0.0083 (0.0051)	0.0091* (0.0045)	0.0085 (0.0087)	0.0093 (0.0071)
Public employment	0.036* (0.016)	0.033 (0.017)	0.023 (0.024)	0.024 (0.023)	0.0061 (0.020)	0.053 (0.056)	0.023 (0.049)
Private employment	0.021 (0.017)	0.030 (0.016)	0.022 (0.018)	0.021 (0.018)	0.024 (0.017)	0.037 (0.042)	0.017 (0.035)
Income tax rate	0.011** (0.0035)	0.011** (0.0035)	0.0068 (0.0042)	0.0069 (0.0042)	0.0050 (0.0039)	0.0066 (0.0070)	0.014* (0.0064)
Gini coefficient	-0.0019 (0.0060)	-0.0019 (0.0060)	-0.0061 (0.0071)	-0.0062 (0.0071)	-0.0040 (0.0061)	-0.011 (0.012)	-0.014 (0.012)
Income statement	0.0036 (0.014)	0.0046 (0.014)	0.0024 (0.017)	0.0029 (0.017)	0.0057 (0.016)	-0.0051 (0.039)	0.0076 (0.038)
Intergovernmental grants	-0.0083 (0.011)	-0.011 (0.011)	-0.032 (0.020)	-0.027 (0.018)	-0.034 (0.021)	-0.057 (0.036)	-0.054 (0.033)
Sample	Full	Full	Full	Full	Full	±2%	±3%
Polynomial of left-wing mandate share	First	Second	Third	Fourth	Fourth	None	None
Covariates	No	No	No	No	Yes	No	No

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix C

Majority effect – Placebo threshold at 45 %

Logged outcome variables	1	2	3	4	5	6	7
Total employment	0.0013 (0.0027)	0.0020 (0.0023)	0.0025 (0.0025)	0.0030 (0.0023)	0.0042 (0.0021)	-0.0024 (0.0035)	-0.0016 (0.0028)
Public employment	0.0035 (0.0091)	-0.00046 (0.0100)	-0.0080 (0.0096)	-0.011 (0.014)	-0.0090 (0.012)	-0.00062 (0.0100)	0.0022 (0.0085)
Private employment	-0.0050 (0.0096)	0.0085 (0.0086)	0.0027 (0.0089)	0.0046 (0.0085)	0.0098 (0.0088)	-0.011 (0.012)	-0.0041 (0.011)
Income tax rate	0.0030 (0.0017)	0.0040* (0.0019)	0.0014 (0.0019)	0.00050 (0.0021)	0.00090 (0.0021)	0.0024 (0.0029)	0.0041 (0.0027)
Gini coefficient	0.00091 (0.0031)	0.00032 (0.0031)	0.00038 (0.0033)	-0.00039 (0.0032)	0.0023 (0.0027)	0.00096 (0.0054)	-0.00037 (0.0038)
Income statement	0.015* (0.0069)	0.017* (0.0068)	0.018* (0.0073)	0.016* (0.0077)	0.015 (0.0077)	-0.0037 (0.0093)	-0.0052 (0.0069)
Intergovernmental grants	0.014 (0.0076)	0.0084 (0.0078)	0.0076 (0.0085)	-0.0046 (0.013)	-0.012 (0.018)	-0.014 (0.0086)	0.00047 (0.0088)
Sample	Full	Full	Full	Full	Full	±2%	±3%
Polynomial of left-wing mandate share	First	Second	Third	Fourth	Fourth	None	None
Covariates	No	No	No	No	Yes	No	No

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Rule effect – Placebo threshold at 45 %

Logged outcome variables	1	2	3	4	5	6	7
Total employment	0.00037 (0.0056)	0.0029 (0.0059)	0.016 (0.017)	0.021 (0.017)	0.025 (0.016)	-0.017* (0.0083)	-0.016* (0.0080)
Public employment	0.045* (0.019)	0.016 (0.020)	-0.059 (0.058)	-0.11 (0.059)	-0.055 (0.054)	0.032 (0.034)	0.051 (0.033)
Private employment	-0.031 (0.019)	0.033 (0.020)	0.00081 (0.055)	0.035 (0.054)	0.093 (0.054)	-0.028 (0.029)	0.017 (0.028)
Income tax rate	0.020*** (0.0043)	0.023*** (0.0047)	0.027* (0.014)	0.013 (0.013)	0.018 (0.013)	0.036*** (0.0088)	0.045*** (0.0089)
Gini coefficient	0.0091 (0.0062)	0.0090 (0.0061)	0.0083 (0.0094)	0.010 (0.0096)	0.028** (0.0095)	0.019*** (0.0047)	0.010 (0.0055)
Income statement	0.042 (0.050)	0.057 (0.051)	0.17 (0.16)	0.14 (0.15)	0.14 (0.14)	0.042 (0.043)	-0.041 (0.038)
Intergovernmental grants	0.075 (0.044)	0.048 (0.045)	0.079 (0.14)	-0.062 (0.13)	-0.059 (0.12)	-0.031 (0.021)	0.039 (0.026)
Sample	Full	Full	Full	Full	Full	±2%	±3%
Polynomial of left-wing mandate share	First	Second	Third	Fourth	Fourth	None	None
Covariates	No	No	No	No	Yes	No	No

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$