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Do Parties Matter for Fiscal Policy Choices? A Regression-Discontinuity Approach*

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Abstract

This paper presents a method for measuring the causal effect of party control on fiscal policy outcomes. The source of identifying information comes from an institutional feature of the election system, namely that party control changes discontinuously at 50 percent of the vote share, i.e., a party that receives more than 50 percent of the votes will be in office. The approach is applied to a very large panel data set from Swedish local governments, which offers a number of attractive features. The results show that there is large and significant party effect: on average, left-wing parties spend and tax 2.5 percent more than right-wing governments. The party effect constitutes 1 percent of average municipality income, clearly a sizeable effect.

JEL classification: C1, C2, C9, D7, E6, H0, H1, H3, H7, P16

Key words: political parties, party control, regression-discontinuity design

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1. Introduction

A long-standing issue in political economics is whether party control makes a difference in determining policy outcomes. While parties are often characterized as ideologically based organization with distinct agendas, little systematic evidence support that party control delivers measurable fiscal policy differences.[1] This finding is perhaps not surprising considering the prediction from the well-known median voter theorem that competition for votes will drive opposing parties to the ideal policies of the median voter and therefore equilibrium policies will be characterized only by the preferences of the median voter.[2] The mixed support of a measurable party difference should not, however, be interpreted that party control does not matter for policy outcomes since previous empirical work has not adequately addressed the problem of identifying a *causal or a ceteris paribus relationship* between party control and policy outcome.

The key problem of estimating the causal effect is that is that parties are not randomly selected to govern political entities and therefore any correlation between party control and fiscal policy outcomes might be spurious.[3] In other words, since voters select parties to govern there may be a self-selection problem due to unmeasured voter preferences.[4] However, if we could randomize parties in government over political entities we could in principle solve the selection problem since randomization assures that there is no *systematic* difference between political entities with governments of various stripes. In this case, the average difference in policy outcomes between the entities with different party control is an unbiased estimate of the causal party effect. However, such an experiment would not be feasible since it would clash with our notion of democracy. Thus, we are left with drawing inference from non-experimental data. Even though we cannot conduct a randomized experiment we can still try to approximate

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[1] See Besley and Case (2002) for evidence from US states, Blais et al. (1993) for evidence from cross-country and U.S. states, and Imbeau et al (2001) for a meta-analysis on OECD data.

[2] The prediction of convergence applies both two a two-party and a multi-party system, although it may be somewhat weaker in the latter case. See Osborne (1995) for overview and discussion of the literature.

[3] Faust and Irons (1999) criticize the empirical literature about partisan cycles in macroeconomic outcomes precisely on this point. They claim that there is little evidence that party control matter when econometric identification issues have been properly addressed.

the evidence generated by a randomized experiment, namely to use a quasi-experiment or a natural experiment. In this paper, the source of identifying information of the party effect comes from an institutional feature of the election system, namely that party control changes discontinuously at 50 percent of the vote share. In other words, a political party that receives more than 50 percent of the votes will be in office. The distinctive feature of this particular type of quasi-experimental design, the *sharp regression-discontinuity design*, [5] is that the variables that determine the assignment to "treatment" groups, i.e., different political parties holding office, are known and quantified. Here the vote share is the *only* systematic determinant of party control and therefore an unbiased estimate of the party effect is obtainable. The general attractiveness of this particular quasi-experiment in search for unbiased treatment effect rests on its close similarity to an ideal randomized experiment, that is, treatment (i.e., party control) is assigned randomly, *conditional* on the assignment variable (i.e., vote share), which is also known as conditional mean independence, or "selection on observables" (Goldberger 1972, Heckman and Robb 1985).

In this paper, I employ the sharp regression-discontinuity design on a data set from Swedish local governments. The use of this data set offers some attractive features in the search for a causal party effect. It is a large panel data set of 288 municipalities over a 21 year period of time (1974-1994) making it possible to use actual, i.e., rule-triggered, changes of party control as the source of identifying information of the party effect and thereby avoiding any bias associated with a cross-section regression-discontinuity method, as discussed by Hoxby (2000). Swedish local governments are also very homogeneous. In particular, they operate within a common political framework and face the same institutional setting. Thus, fiscal policies and political parties are quite comparable across political entities, which otherwise is a major obstacle in cross-country studies. The Swedish election system is also characterized by strong parties, making it is

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[4] The general selection problem is subject of an extensive literature. For example, see Heckman and Robb (1985) and Manski (1989).

[5] There are two types of regression-discontinuity designs: the sharp and the fuzzy design. In the sharp design, treatment is known to depend in a deterministic way on some observed variables, whereas in the fuzzy design there are also some unmeasured factors affecting selection into treatment. This paper deals only with the sharp design.

possible to treat parties as unitary actors without having the additional complication of dealing with the impact from individual legislators on policy outcomes.[6] Swedish local governments also have the constitutional right of self-government, no restrictions on borrowing, and no balanced budget rules.[7] Moreover, only 20 percent of their income comes from grants, whereas the rest mostly comes from a proportional income tax, which each municipality can set freely. Thus, they have a large degree of freedom, which has resulted in quite large differences in fiscal policy outcomes across the local governments.[8]

The result of this paper show that left-wing parties spend and tax, on average, 2.5 percent more than right-wing governments. The party effect constitutes 1 percent of the average municipality income, clearly a sizeable effect.[9] In addition, since Swedish local governments raise the bulk of their revenues through a proportional income tax, the excess burden associated with such a distortionary tax is probably non-negligible.[10]

The paper is organized as follows. Section 2 describes the problem of identifying a causal relationship between party control and policy outcomes and discusses the sharp regression-discontinuity design as a possible solution. Section 3 describes the data, while section 4 presents the results. Section 5 discusses the interpretations of the findings and section 6 concludes.

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[6] There is a literature in American politics addressing a related question of party effects, but this literature looks at measures of legislative voting outcomes, i.e., roll call votes, instead of fiscal policy outcomes. This literature also deals with the complexity of separating individual legislators characteristics from party effects. The literature addressing this question is voluminous: Levitt (1996), Snyder and Groseclose (2000) and McCarty et al. (2001) are good examples of recent work. For the older literature, see Levitt (1996) and the references cited therein. There is still a debate whether there is a causal relationship between party control and roll-call votes.

[7] However, as from 1998 there is a balanced budget rule in place.

[8] In a series of papers of mine, Pettersson-Lidbom (2001, 2002, 2003) and Pettersson-Lidbom and Dahlberg (2003), I also find strong support for Swedish local policy discretion.

[9] It is difficult to compare the size of the estimates of party effects across studies since neither of the previous studies has been able to convincingly identify a causal effect. Nevertheless, the size of the party effect in this paper is still much larger. For example, Besley and Case (1995) find that Democratic governors increase spending and taxes with less than 0.1 percent of average state income as compared to Republican governors.

[10] According to Barro's (1979) "tax-smoothing" hypothesis, the tax rates should be held constant to minimize the excess burden.

2. The regression-discontinuity method and party control

How can we test whether party control has a causal effect on fiscal policy outcomes? An experiment would be the gold standard-standard to establish causality. Parties in government would be randomly assigned to a large number of political entities and the average difference in policy outcomes between the entities with left-wing and right-wing governments would be interpreted as the causal effect of parties. We would be able to make this casual statement because randomization would make the party control variable *independent* of other variables which also might be related to policy outcomes such as voter preferences. However, it would not be possible to conduct such an experiment since it would clash with our notion of democracy, i.e., voters elect parties to govern. If we cannot make a randomized trial we can at least try to approximate one. This is the idea behind the quasi-experimental research design employed here: *the sharp regression-discontinuity design*.[11] The general idea of sharp the regression-discontinuity approach is that a known rule influences how subjects are assigned to treatment groups. In our context the vote share is the deterministic rule that assigns parties to political entities. If one party receives more than 50 percent of the votes it will be in office. Thus, party controls are the different "treatments", which the political entities will be assigned to. Since the vote share is the only systematic determinant of treatment status an unbiased estimate of the party effect may be obtained. The regression-discontinuity design can be formalized as follows.

Consider a causal model that links some policy outcome P_i in a political unit i to a treatment indicator T_i, equal to one if the there is left-wing party in office in unit i and zero if there is a right-wing party in office instead.[12] Let ε_i be any other variable that may be related to both the treatment and the policy outcome variable. We now have the following policy outcome equation:

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[11] Thistlethwaite and Cambell (1960) is the first paper that introduces the sharp regression-discontinuity design. Its statistical properties, however, was first discussed formally by Goldberger (1972) using a linear set up. Later, Rubin (1977) and Heckman and Robb (1985) both have formal discussions of more general specifications. More recently, Hahn et al. (2001) discuss an alternative minimal parametric estimation methods in the regression-discontinuity design

[12] For expositional clarity, there are only two treatments groups, but the regression-discontinuity design can deal with many treatment groups as well.

$$P_i = \alpha + \delta T_i + \varepsilon_i \quad (1)$$

where the parameter δ measures the causal party effect,[13] i.e., the average difference in policy outcomes between left- and right-wing parties holding all other factors fixed. The key identifying assumption is that without any treatment, the party effect δ would be zero, which formally is expressed as zero conditional mean: $E[\varepsilon_i \mid T_i] = 0$. However, the zero conditional mean assumption will typically not hold, in particular because T_i is almost certainly correlated with voter preferences since party control will depend on voters' choices. However, we can use information about the selection into treatment to get an unbiased measure of the party effect. We know that the vote share is the *sole* deterministic variable that assigns a party to a political unit. If one party receives more than 50 percent of the vote share it will be in office. In other words, there is going to be two distinct treatment groups (Left-wing party: T_i =1) and (Right-wing party T_i =0) solely on the basis of whether assignment variable v_i is below or above the 50 percent cutoff. The assignment or selection rule can formally be expressed as $T_i=T(v_i) =1[v_i \geq 50]$, where 1[.] is an indicator function. Since T_i is a *nonrandom* function of v_i, then the error term ε_i in equation (1) will be mean independent of T_i, conditional on v_i. In other words, the sharp regression discontinuity method builds on the *conditional mean independence* assumption, i.e., $E[\varepsilon_i \mid T_i, v_i] = E[\varepsilon_i \mid v_i]$.[14] Under the conditional mean independence assumption, the observed or unobserved characteristics in the error term v_i, may be correlated with v_i, but given v_i the conditional mean of the error term ε_i does *not* depend on the treatment T_i. In this case, the parameter δ will be the causal effect of party control, that is, the difference in conditional expectations: $E(P_i \mid T_i =1, v_i)$- $E(P_i \mid T_i =0, v_i)$. This difference is also the causal effect defined by the experiment where the political units with a given vote share v_i are *randomly assigned* to left-wing and right-wing majorities. Since the causal party effect does not depend on v_i,[15] it is also the causal effect of party control for a randomly selected political unit of the population. In other words, the

[13] I have here invoked the assumption of a constant-coefficients regression model, namely that the party effect is the same across municipalities. Below, I discuss how regression-discontinuity set up must be changed when this assumption does not hold.

[14] Conditional mean independence is also known as "selection on observables" or "ignorability of treatment"

regression discontinuity method mimics an ideal randomized experiment and therefore we can get an unbiased estimate of the true causal party effect.

One approach to estimate the party effect is to specify and include the conditional mean function $f(v_i)=E[\varepsilon_i \mid v_i]$ as a "control function" in equation (1) (Goldberger 1972, Heckman and Robb 1985). For example, when the population conditional mean function is linear, the equation to be fitted is:

$$P_i = \alpha + \delta T_i + \theta v_i + \varepsilon_i \tag{2}$$

The inclusion of vote share v_i as a regressor will now free T_i from the contamination which leads to selection bias since it will capture any correlation between T_i and ε_i, and therefore δ will be an unbiased measure of the party effect. However, we do not know if the population conditional mean function is linear. A common approach is therefore to specify a flexible parametric control function as to avoid functional form misspecification. However, if we include a too flexible functional form, the control function will have sharp jumps or "spikes", which will create a problem for the regression-discontinuity method because the identifying variation for estimating the treatment effect comes from the discontinuities that the assignment rule induces at certain known values. Therefore, we must assume that there is a smooth relationship between the assignment variable (i.e., vote share) and the outcome of interest (i.e., policy outcome), otherwise the treatment effect (i.e., party effect) would not be identifiable.[16]

A second method is to restrict the data around the point of discontinuity (i.e., around 50 percent of the vote shares) to circumvent the problem of having to rely on functional form assumptions about the control function in identifying the party effect. This approach could, however, produce very imprecise measures of the party effect since the regression-discontinuity method is subject to a large degree of sampling variability.[17]

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[15] This is true if the party effect is constant.

[16] That continuity is a requirement for identification in the regression-discontinuity approach is discussed by Hahn et al (2001).

[17] The regression discontinuity method is a correlated design, which implies that the standard errors will be larger than compared to an uncorrelated design, i.e., a randomized experiment. The larger is the correlation between the control function and the treatment indicator the larger is the variance of any estimates of the treatment effect. In other words, much more observations are needed in the regression-discontinuity design to give the same precision as in an experiment. A detailed discussion of efficiency of the regression-discontinuity method is provided in Goldberger (1972)

Hence, when using the regression discontinuity method in practice, there is a trade-off between bias and efficiency. However, by employing both methods we can get a sense whether the control function approach produces biased estimates of the treatment effect by comparing the estimates across the two methods, and if the estimates are similar we can base our inference on the control function approach since it is more efficient.

Another issue when implementing the regression-discontinuity method is raised by Hoxby (2000). She argues that the regression-discontinuity method based on cross-section data may lead to biased estimates unless the data used in the estimation is based *solely* on the discontinuity, i.e., those observations precisely at 50 % of the vote share. Her recommendation is instead to use the observations where there has been a *rule-triggered change* in the variable of interest, namely when there is an *observed* change in party control. Since my data is a panel, I can implement her suggestion by using only the within-municipality variation to identify the party effect. This is equivalent of including an individual intercept for each municipality, that is, a fixed-municipality effect specification. In addition, I will also include a full set of time dummies since I do not want to attribute behavioral significance to any across-municipality correlations that are really due to common national influences such as the effect of the national business cycle.

In principle, as discussed above, there is no need to include additional covariates in the regression-discontinuity approach other than the control function. In practice, however, there may still be reasons for including other regressors. First, there is an efficiency reason for including additional covariates since it reduces the variance of the error term, which could be quite important since the regression-discontinuity method has large sampling variability. For example, if there are some unobserved determinants of policy outcome that are persistent over time for a given municipality, including fixed-municipality effects, would enhance efficiency.[18] Second, even if we could conduct a randomized experiment of party control there still may be a need to include additional controls since the randomization could be less than perfect in the sample at hand. Thus, bias is always a potential issue even in a randomized experiment. However, here it is

important not to include experimental outcomes as additional covariates since these will bias the estimate of the treatment effect. (Rosenbaum 1984) For example, including lagged values of policy outcomes among the control variables is not advisable since these variables are affected by the treatment (party control) themselves. Thus, one should only include pretreatment characteristics, which are not influenced by the experimental treatment. Third, we can assess whether the estimate of the treatment or party effect is sensitive to inclusion of any observable pretreatment variable. Since party control should be as good as randomly assigned (conditional on the control function), the inclusion of additional covariates should not have a significant influence on the estimate of the party effect. In other words, this is an empirical test for random receipt of treatment.

Another important issue in this sharp regression-discontinuity approach is there must be perfect assignment of treatments relative to the cutoff point. Otherwise, we must use a modified version of the regression-discontinuity approach.[19]

Finally, to avoid any misunderstanding in the interpretation of the regression-discontinuity method, it is very important to point out that it is only the party effect δ that has any causal interpretation. For example, in equation (2) the estimate of θ typically has *no* causal interpretation since under the conditional mean assumption; the vote share is allowed to be correlated with the error term. In other words, the vote share only plays the role of an assignment variable for treatment in the regression-discontinuity method and it is therefore *incorrect* to interpret the regression discontinuity method as solving the selection problem caused by unmeasured voter preferences by including the vote share as a proxy for these preferences. In fact, if one addressed the selection bias problem by using a proxy variable of voter preferences, such as a survey-based measure of public opinion,[20] this approach would *not* lead to an unbiased measure of the party effect.

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[18] The R^2 from OLS regressions on the policy outcomes used in the empirical analysis and the fixed-municipality effects are in the range of 0.46 to 0.60. In other words, the municipality fixed effects explains a large amount of the variation in the policies.

[19] This is the fuzzy regression-discontinuity design explored by Angrist and Lavy (1999), Van der Klaauw (2002), and Pettersson-Lidbom (2003) among others.

[20] For example, Erikson et al. (1993) use a proxy variable method.

3. The data

To test whether party control matters for fiscal policy outcomes I will use a quite a large panel data set from Swedish local governments, but before turning to the description of the data it is perhaps helpful to digress briefly on the workings of Swedish local governments. Sweden is currently divided into 290 local governments (or municipalities), which cover the entire country. Local governments play an important role in the Swedish economy, both in terms of the allocation of functions among different levels of government and economic significance. They are, for example, responsible for the provision of day care, education, care of the elderly, and social welfare services. To quantify their economic importance, note that in the 1980s and 1990s their share of spending out of GDP was in the range 20 to 25 percent and they employed roughly 20 percent of the total Swedish workforce. Swedish local governments also have a large degree of autonomy. They have the constitutional right of self-government, they have no restrictions on borrowing, and they have no balanced budget rules.[21] Moreover, during the period of investigation 1974-1994, the bulk of revenues were raised trough a proportional income tax, which each municipality was allowed to set freely,[22] and only 20 percent of the total revenues came from intergovernmental grants.

To implement the sharp regression-discontinuity method the mechanics of Swedish election system need to be discussed in some detail. The election schedule is fixed and elections were held every third year on the third Sunday of September during the sample period.[23] During the same period, voter turnout has been very high, close to 90 percent, in the local elections. The decision-making body in each of the municipalities is an elected municipal council and the Swedish Elections Act prescribes that in elections to municipal council seats should be distributed proportionally between parties on the basis of election results in each constituency, where the distribution is based on the adjusted odd-number method. As a result, the election system is entirely party based, i.e., a closed-

[21] As from year 2000 there is a balanced budget rule.
[22] From 1991 to 1993, however, the central government imposed a temporary tax cap.
[23] As from 1994, elections are held every fourth year.

list system, and with the existence of several political parties.[24] The multi-party issue raises the question of how to define treatment or party control. However, the Swedish political map has been characterized by a very clear dividing line between socialist and non-socialist parties leading to a quite stable two-bloc system.[25] Hence, to a first approximation we can treat the Swedish electoral system as bipartisan,[26] and define the treatment indicator T_i as 1 for left wing majorities and zero otherwise.[27] The party effect should thus more accurately be addressed as a majority coalition effect, but for simplicity I retain the former name. The multi-party feature of the political system also raises the issue of heterogeneous party or treatment effects. Implicitly, I have assumed a constant coefficient model in equations 1 and 2, namely the party effect is the same across municipalities. In other words, the assumption is that the party effect is the same for, say, a left-wing majority with a 10-41 votes share split for (the smaller) Leftist and (the larger) Social Democratic Party, as with a 25-26 split between the parties.[28] Thus, inter coalition bargain does not depend on the included parties relative vote shares. However, the constant party effect assumption is an empirical issue that can be tested by allowing for interactions between the party control variable and the control function in the regressions. However, in the case of varying party effects there is no single party effect, since the impact is conditional on the control function. One option is to report the local average treatment effect, i.e., the party effect for those at the margin or 50 percent of the vote

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[24] Whether proportional election system is a cause of the multitude of parties or whether the number of parties is caused by a heterogeneous distribution of voter preferences is still in dispute.

[25] To the best of my knowledge, there is no evidence that any of the socialist parties did form a coalition with any of the non-socialist parties or vice versa during the sample period 1974 to 1994. That this was not the case was checked extensively against the official newspaper (www.kommunaktuellt.com) of Swedish Association of Local Authorities, an association of all Sweden's municipalities. This newspaper gives a quite detailed coverage of local politics.

[26] For example, Alesina et al. (1997) also classify Sweden as a bipartisan system (along with U.S. and other political system with a clear left-right division) in their empirical analysis.

[27] The classification is taken from the official newspaper of the Swedish Association of Local Authorities. The socialist bloc includes the Leftist Party and the Social Democratic Party. The non-socialist bloc includes three parties: the Conservative Party, the Centrist Party and the Liberal Party. However, since 1988 it includes a fourth party: the Christian Democratic Party. In the 1991 election a fifth party was included in the non-socialist bloc: the New Democratic Party.

[28] Out of the total of 826 cases which are defined as having a left wing government, the Social Democratic Party had own party control 515 times, while the Leftist Party never had a majority of the seats. For government controlled by the right wing parties, there are only in 6 out of the total of 833 cases where a single party controlled the government.

shares,[29] while another option is to report the average treatment effect, namely the party effect at the mean of the assignment variable.[30] If there is a constant party effect, the same results follows from either procedure. As it turns out, the results presented in this paper do not reject a constant party effect.

There are also two caveats with my data that need to be mentioned. A first caveat is that in a few cases where the vote share is less than 50 percent for a bloc, but it is still in power. This oddity arises as the distribution of seats is not based on strict proportionality but on the previously mentioned adjusted odd-number method. To avoid the problem of misclassifying party control or treatments, I will use the seat share instead of the vote share. In practice, however, using seats shares instead of vote shares will probably make little difference since this problem has happened only 4 times during the sample period and the correlation between vote and seat shares is larger than 0.99.

A second caveat possibly more important is the existence of several small parties - often one-issue parties- at the local level, which are not part of the two blocs. These parties sometimes hold the balance of power, which creates a problem of defining party control since these are not easily classified along the left-right ideological spectrum. I call these kinds of constellations undefined majorities.[31] The problem with undefined majorities, however, can be solved by the including a separate dummy variable for the undefined majority together with an additional control function, i.e., $f(r_i) = \mathrm{E}[\varepsilon_i \mid r_i]$ where r_i is vote share of the right-wing majority. The party effect will now be correctly identified as the average difference in policy outcomes between left and right wing majorities.[32]

Table 1 summarizes the number of left, right wing and undefined governments in every election period during the sample period 1974-1994. There was a left-wing majority

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[29] Hahn et al (2001) shows that when the treatment effects are heterogenous, the average treatment effect at the margin is non-parametrically identified under a functional form restriction and weak form of conditional independence. To estimate this effect we modify (2) as $P_i = \alpha + \delta_m T_i + \theta_1 v_i + \theta_2(v_i - 50) + \varepsilon_i$, where δ_m measures the average treatment effect at 50 percent of the vote shares.

[30] To estimate the average treatment effect in case of varying treatment effects we modify (2) as $P_i = \alpha + \delta_a T_i + \theta_1 v_i + \theta_2(v_i - \mathrm{E}(v)) + \varepsilon_i$, where δ_a measures the effect at the mean of the assignment variable $\mathrm{E}(v)$.

[31] This classification is compiled from the distribution of seats in local councils. If either of the blocs receives more than 50 percent of the seats it is defined accordingly, otherwise it is classified as undefined.

in 826 cases, and a right-wing majority in 833 cases. Thus, the two blocs have been in power almost the same number of times.[33] Table 1 also shows that there has been an undefined majority in 312 cases, which corresponds to 15 % of all observations. Table 2 shows the frequency of government changes for the municipalities. The number of government changes is very unequally dispersed among the different municipalities. For example, 122 municipalities (42 percent of the sample) had no change of power (69 had left wing and 45 right wing governments). It is important to stress that the 122 municipalities with zero turnovers will not be part of identifying the party effect since only the within-municipality variation will be used, as was discussed in section 2. Table 2 also shows the vote share for the incumbent in each group of municipalities.[34] Incumbents in those municipalities with no change of power on average obtained more than 62 percent of the votes while those who had 3 or more changes got less than 54 percent.

Turning to the policy outcome variables, four different variables will be used in the empirical analysis: total expenditures, current expenditures, total revenues and the proportional income tax rate. The difference between total and current expenditures is mainly that investments are included in the former. Roughly 85 percent of total spending is classified as current spending. Total revenues include tax receipt from a proportional income tax rate, fees, and governmental grants. Since total revenues might reflect non-discretionary local government decisions, a more discretionary measure is to use the proportional income tax itself. On average, about 55 % of the total revenues come from the income tax. Expenditures, current expenditures, and the total revenues are expressed in per capita terms and in 1991 prices and the tax rate is expressed in percent.[35] Table 1 presents summary statistics for the four dependent variables. This table shows a considerable variation in total spending and revenues. For example, real expenditure per capita was on average SEK 28,527 ($ 4755), the standard deviation 5,804 ($ 967), the

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[32] Another approach would be to exclude these observations from analysis altogether. It turns out that it does not matter which of these two approaches I use for the results about the party effect presented below.
[33] This is perhaps surprising given the social democratic party hegemony at the national level.
[34] The vote share is compiled from the distribution of seats in local councils. However, because the Swedish electoral system is based on proportional representation, vote shares are almost equivalent to seat shares. For example, in the 1994 election the simple correlation between vote and seat shares was larger than 0.99.
[35] I have used the implicit GDP deflator. The deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices.

minimum value 14,392 ($ 2,400), and maximum value 70,032 ($ 11,672).[36] Table 1 also presents summary statistics for covariates considered a standard set of controls in the local public finance literature: proportion of people of age 0 to 15, proportion of people older than 65, population size, population density, income, and grants-in-aid.[37]

All the data used are publicly available and were obtained from Statistics Sweden (SCB) or its publications.[38]

4. Results

In this section I present empirical evidence regarding the party effect. As discussed in section 2, two different approaches will be used. First, I present results from the control function approach including various polynomials of the vote share as covariates together with the party control variable. This approach will give an unbiased estimate of the party effect unless the control function is misspecified. Second, I show the results from the approach of restricting the sample around the point of discontinuity of party control, i.e., around 50 percent of the votes. In this way we can avoid bias due to the misspecification of the control function. Nevertheless, this approach has a drawback, namely the party effect will be less precisely measured due to the large sampling variability associated with the regression-discontinuity method. However, if the estimates are similar across the two methods we can rather safely base the inference on the control function approach since this suggests that the estimate of the party effect is likely to be unbiased in this case.

A. Control function

I present empirical evidence of the party effect using the control function approach. As discussed in section 2, all regressions include fixed municipality and time effects. The reason for including fixed effects is to avoid the potential bias associated with cross-

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[36] The expenditures are expressed in 1991 prices using the implicit GDP deflator. The deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices. The equivalent amount in 1991 dollars (i.e., SEK 6=$1) is shown within parentheses.

[37] One can argue whether grants in general are exogenous with respect to fiscal decisions: matching grants are typically not, whereas grant in aid are more likely to be this. In the Swedish case (until 1993), about 80 percent of the total grants were matching grants while 20 percent were grant-in-aid. Even the grant-in-aid program was to some extent determined by the fiscal behavior of the municipalities.

[38] The publications used are: How much do local public services cost in Sweden, Local government finance, and Statistical yearbook of administrative districts of Sweden.

section regression-discontinuity method since the party effect will now only be identified when a municipality has had at least one change of power, and time effects will pick any across-municipality correlations in policy that are really due to common national influences, such as national business cycle and changes in the definition of the dependent variables.[39] Tables 4, 5, 6 and 7 shows the results from party control on four measures of fiscal policy outcomes: total spending, current spending, total revenues and the proportional income tax rate. Column I in each of the tables show the results from specifications without a control function, whereas columns II to V present the results from a linear, quadratic, cubic, and quartic in vote shares respectively. Table 4 shows that total spending is significantly higher for left-wing governments than right wing for all specifications.[40] The size of the party effect is quite similar across the specifications expect for the regression without a control function. The estimated party effect is in the range 500-700 per capita for spending for the specifications including control functions as shown in columns III to VI. These effects are in the order of 2-2.5 percent of mean spending (i.e., SEK 28,257 per capita), which constitute about 1 percent of average municipality income (i.e., SEK 72624 per capita). From Table 4 we can also make two additional observations. First, the estimate of the party effect without a control function in column II is roughly twice as large compared to the others estimates. Thus, half of this estimate of the party effect is a selection effect. This implies that including fixed effects is not sufficient to control for selection bias. Second, the size of party effect seems to be quite stable across the different specifications of the control function. Therefore, a linear specification seems to be a good approximation of the population conditional mean function.

Turning to the other policy outcomes: current spending, total revenues, and the proportional income tax rate, we get a quite similar picture. In all specifications there are positive and statistically significantly party effects. The estimate for the party effect is in

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[39] Including time effects are important since Statistics Sweden has changed the definition of expenditures and revenues over time.

[40] I follow the usual approach of reporting Huber-White robust standard errors. However, because there could be serial dependence in the errors withinin municipalities, I also report (in brackets) the more conservative Huber-White standard errors clustered at the municipality level following the suggestions of Bertrand, Duflo and Mullainahtan (2002) and Kézdi (2002).

range SEK500-750 per capita for current spending (i.e., columns III to IV in Table 5), in the range SEK 400-650 per capita for total revenues (i.e., columns III to IV in Table 6), and in the range 10-13 % for the income tax rate (i.e., columns III to IV in Table 7), which is almost 1 percentage points, given an average tax rate of 16.46 percent. The various estimates of the party effect without including a control function are at least twice as large then the one with control functions and reinforce the previous finding of substantial selection bias. Once again, a linear specification seems to be a good approximation of the population conditional mean function.

As was discussed in section 2, there might be practical reasons to include other pretreatment covariates than the assignment variable in the regression-discontinuity design since this can enhance efficiency and allow for a test of randomization of party control since the underlying assumption is that the party control should not be systematically related to any observed or unobserved variables once the assignment variable is controlled for. Thus, party control should be as good as randomly assigned conditional on the assignment variable. Table 8 presents the results from the same regressions as in Tables 4-7 except for the inclusion of the additional covariates: proportion of people of age 0 to 15, proportion of people older than 65, population size, population density, income, income lagged twice,[41] and grants-in-aid. Table 8 reveals that by adding additional covariates, this does not significantly affect any of estimates of party control as compared to the corresponding estimates in Tables 4-7, giving further support of a causal interpretation of the measured party effect. As expected, the party effect is also more precisely measured in Table 8 than in the previous tables. Moreover, almost all the estimates of the party effect is even significant at the 5 percent level when the more conservative estimates of the standard errors (in brackets) are used for testing the null hypothesis of no effect.

So far we have assumed a constant party effect, but it may not be correct since both treatments groups, i.e., left wing and right wing governments, consists of several parties and the bargain within a coalition government may depend on the relative strength

of the included parties. But as was discussed in section 3, we can relax this assumption by allowing for interactions between the control function and party control variable. However, in the case of varying party effects there is no single party effect, since the treatment effects will be conditional on the control function. One option is to report the local average effect, i.e., the party effect for those at the margin or at 50 percent of the votes, while another option is to report the average treatment effect, namely the party effect at the mean of the assignment variable. If there is a constant party effect, the same results follows from either procedure. Table 9 presents the results from including interactions. For ease of comparison, the first row restates the results from column II in Table 8, while the second and third rows present the results at the margin of 50 percent and at the mean of vote share respectively. The estimates of party effect in the first row provide a benchmark for assessing whether the assumption of a constant party effect is reasonable and therefore these estimates should be compared to the corresponding estimates from the next two rows. If the party effect is constant, these estimates should be similar (except for sampling variability). For example, for total spending the party effect estimates across the different specifications with additional covariates are 690, 665 and 854. For current spending the corresponding estimates are 701, 705, and 703; for total revenues the estimates are 654, 619, and 872; and for taxes the estimates are 0.135, 0.130, and 0.172. These estimates are quite similar across the different specifications within each policy category, although they tend to be somewhat larger for the interaction specification evaluated at the mean of vote shares. In any case, these estimates are statistically indistinguishable from each other. Thus, the assumption of a constant party effect cannot be rejected.

To summarize, using the control function approach we find statistically significant party effects: left wing governments spend and tax more than right wing governments. The size of the party effect is also quite large: on average left-wing governments increase both spending and revenues with roughly 2.5 percent, which constitute about 1 percent of average municipality income. The party effect is also quite robust to the parameterization

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[41] Due to centralization of tax collection, the tax receipts to the local governments in year *t* are based on the taxable personal income in year *t-2*. I have tried to deal with this feature by including both the average

of the control function, which suggests that a linear control function is a good approximation to the true conditional mean function. However, to probe this issue further we turn to the other approach of restricting the sample around the discontinuity.

B. Discontinuity samples

In the regression-discontinuity method, the source of identifying information of the party effect comes from the discontinuity that the electoral rule induces at 50 percent of the vote share. The idea is that observations close to the cutoff are more representative of a random experiment and therefore any misspecification of the control function might be avoided. However, restricting the sample comes at a cost, namely that the party effect will be less precisely measured, as discussed in section 2. I will present results from a number of subsamples. I start by restricting 5 percentage points around the discontinuity, i.e., in the interval [45, 55]. Then I decrease the interval down to 1 percentage point, i.e., [49, 51]. Here it is important to point out that municipality and time specific effects are included in all discontinuity-sample regressions. Therefore, and analogous to the previous control function approach, the party effect will only be identified from the within-municipality variation, that is, when there has been electoral rule-triggered change in party control.

Table 10 presents the results from this approach, but it also gives information about the number of municipalities in each discontinuity sample together with the average number of observations per municipality. For example, Table 10 reveals that in the +5/-5 interval there are 132 municipalities with an average of 10.2 observations per municipality, while there are only 26 municipalities included in the +1.0/-1.0 interval with an average of 4.2 observations per municipality. For total spending Table 10 shows the party effect varies in the range SEK 350-1,150 per capita, but the standard errors are also quite large. For example, the standard errors in regressions on total spending are 3 times larger or more than in the control function approach. This finding illustrates the large sampling variability associated with the regression discontinuity method, as was discussed in section 2. The large standard errors imply that we cannot reject that these

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municipality income in year *t* and *t-2* as covariates.

estimates of party effect is significantly different from the ones in the control function approach, which are in the range SEK 500-700 per capita. We can make the same conclusion for the other policy outcomes as well: the party effect is in the range of SEK 450-1,350 per capita for current spending, in the range of SEK 200-1,200 per capita for total revenues, and 9-56 percentage points for the tax rate. However, the party effects are not statistically significantly different from the counterparts in the control function approach. Thus, this finding suggests the functional form of the conditional mean function is reasonably specified in the control function approach since the estimates across the two different approaches do not differ significantly. As a result, we can base our inference on the more efficient control-function method.

5. Discussion

In this section I discuss the interpretation of a significant party effect in Swedish local governments.

The results of this paper strongly reject the notion of strict convergence, namely that all parties have similar preferred policy outcomes. However, one could argue that Sweden is not a two-party system and therefore the prediction about convergence from a model where two candidates competing for office does not apply. However, there are two arguments against this kind of reasoning. First, the prediction about convergence is a more general feature of political competition and not just exclusive to a two-candidate model.[42] Second, and more importantly, there are good reasons to treat Sweden "as if" it is a two-candidate system. As mentioned in section 3, Sweden has had a very stable two-bloc system: socialist and non-socialist parties. The result from the empirical analysis is also quite consistent with the two-party view since the estimate of the party effect seems to be roughly constant across different specifications of the control function. In other words, when allowing for interactions between the party control variable and the assignment variable the party effects seem to be roughly constant. Moreover, when we restrict the sample around the discontinuity to increasingly smaller intervals, we are at the same time reducing the number of municipalities that helps identify the party effect. For

[42] See Osborne (1995) on this point.

example, Table 9 reveals that in the +5/-5 interval there are 132 municipalities with an average of 10.2 observations per municipality, while there are only 26 municipalities included in the +1.0/-1.0 interval with an average of 4.2 observations per municipality. These municipalities are governed by different coalitions of parties, left or right, where the relative party strength (as measured by vote or seat shares) can possibly be quite different within each government. Despite these differences in relative strength, the party effect seems roughly constant across the different sample (given sampling variability). I interpret this as quite strong evidence that one can treat the Swedish political system as a two-party system. In other words, it seems that inter coalition bargain in Swedish local governments does not depend on the included parties relative vote shares.

The average party effect is also sizeable, about 2.5 percent of the budget or 1 percent of average municipality income. The excess burden from changing the tax rate must clearly be non-trivial since all municipalities raise the bulk of their revenues through a proportional income tax rate and the estimated change in the tax rate is almost 1 percentage point.

The finding of a large and significant party effect also raises the issue about the mechanism that makes the two opposing blocs pursue quite distinct fiscal policies. Perhaps the most compelling answer is that parties cannot make binding commitments to their electoral platforms. This is the explanation set out by Alesina (1988) in an article where he criticizes the political science literature with outcome-motivated candidates. He shows that once one drop the commitment assumption the equilibrium outcome will be one of full divergence. The only credible announcements are the bliss points of the parties. Thus if left and right wing parties have different preferences over policy outcomes we would expect to see a causal relationship between party control and fiscal policy outcomes, and therefore a rejection of policy convergence.

Many scholars of local public finance hold the view that since voters can “vote with their feet”, the well known result from the Tiebout model where people sort themselves into jurisdictions depending on their preference nullifies the importance of politics at the local level. The results of this paper constitute evidence against this view

6. Conclusions

This paper proposes a regression-discontinuity method to resolve the question whether party control matters for fiscal policy outcomes. The source of identifying the party effect comes from the discontinuity that the electoral rule induces at 50 percent of the vote share: if a party receives more than 50 percent of the votes it will be in office. Using panel data from Swedish local governments with several attractive features, I find strong evidence of a sizeable party effect: on average left-wing parties spend and tax 2.5 percent more than right-wing governments, a difference of about 1 percent of average municipality income.

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Table 1. Summary of party control

Election period[a]	# left-wing governments	# right-wing governments	# undefined governments
1974-1976	117	125	35
1977-1979	112	131	34
1980-1982	123	118	38
1983-1985	148	88	48
1986-1988	127	105	52
1989-1991	125	94	65
1992-1994	74	172	40
Sum 1974-1994	826	833	312

a. In Sweden there was an election every third year until 1994, when four-year-terms where introduced.

Table 2. Frequency of government turnovers and vote shares

Frequency of government turnovers	Number of governments	Average vote shares
0	122	62.64
1	30	57.69
2	43	55.80
3	41	53.84
4	29	53.02
5	13	52.90
6	8	51.95
7	0	-

Note. - A government turnover is defined as a change of power between left-wing, right-wing or undefined governments. The calculation of average vote shares only includes left- or right-wing incumbent governments

Table 3. Summary statistics for the fiscal policy outcomes and other covariates

Variables	Mean	Standard d.	Min	Max
Total expenditures	28,257	5,804	14,391	70,031
Current spending	26,790	6,748	11,889	70,924
Total revenues	28,207	5,699	15,515	71,699
Income tax rate	16.46	2.12	9.7	31.75
Left vote share	47.66	11.93	13.33	77.78
Right vote share	48.26	11.38	14.28	84.44
Proportion of young, 0-15	21.14	2.83	12.65	36.69
Proportion of old, 65+	17.63	4.29	3.27	27.89
Income, *t*	72,624	12,357	15,945	162,962
Income, *t-2*	59,915	12,483	17,950	151,977
Population size	29,774	52,551	2,865	692,954
Population density	107	360	0.28	3700
Tax equalization grants	2,114	2,192	-3,963	19,599

Average income is expressed in per capita terms and in 1991 prices.

Table 4. The party effect: Total spending

	I	II	III	IV	V
Left-wing government	1,205	558	658	654	590
	(195)	(207)	(206)	(220)	(224)
	[329]	[354]	[348]	[368]	[376]
Undefined government	209	-38	-89	66	67
	(147)	(155)	(154)	(160)	(160)
	[250]	[251]	[247]	[248]	[245]
Left		98	437	-233	614
		(18)	(57)	(197)	(571)
		[44]	[125]	[394]	[1139]
$Left^2$			3.58	11.0	-19
			(.58)	(4.2)	(19)
			[1.33]	[8.4]	[39]
$Left^3$				-.099	.35
				(.028)	(.28)
				[.057]	[.56]
$Left^4$					-.0024
					(.0015)
					[.0030]
Right		-54	-69	-782	-1208
		(14)	(59)	(193)	(533)
		[33]	[141]	[332]	[732]
$Right^2$			.14	16.3	31.5
			(.56)	(4.0)	(17.8)
			[1.28]	[6.9]	[25.7]
$Right^3$				-.11	-.34
				(.03)	(.26)
				[.05]	[.38]
$Right^4$					.0012
					(.0013)
					[.0020]
Municipality effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R^2	0.8343	0.8368	0.8386	0.8394	0.8395
R^2 (within)	0.5898	0.5958	0.6005	0.6024	0.6026
Number of observations	5,913	5,913	5,913	5,913	5,913

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 5. The party effect: Current spending

	I	II	III	IV	V
Left-wing government	1,347	511	600	628	599
	(208)	(216)	(214)	(224)	(227)
	[356]	[375]	[369]	[385]	[386]
Undefined government	216	-68	-132	-47	-38
	(150)	(155)	(154)	(158)	(158)
	[271]	[269]	[268]	[272]	[272]
Left		139	428	156	451
		(15)	(46)	(170)	(504)
		[33]	[99]	[370]	[1074]
$Left^2$			3.1	2.8	-8.1
			(0.5)	(3.7)	(17)
			[1.1]	[8.1]	[36]
$Left^3$				-.039	.13
				(.025)	(.25)
				[.056]	[.52]
$Left^4$					-.00091
					(.00013)
					[.00027]
Right		-61	-11	-355	-294
		(12)	(41)	(124)	(360)
		[28]	[95]	[227]	[584]
$Right^2$			-.51	7.3	5.1
			(.39)	(2.8)	(12.6)
			[1.1]	[5.1]	[21.2]
$Right^3$				-.055	-.023
				(.019)	(.18)
				[.036]	[.32]
$Right^4$					-.00016
					(.00097)
					[.00016]
Municipality effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R^2	0.8936	0.8967	0.8980	0.8981	0.8981
R^2 (within)	0.8020	0.8078	0.8101	0.8103	0.8104
Number of observations	5,913	5,913	5,913	5,913	5,913

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 6. The party effect: Total revenues

	I	II	III	IV	V
Left-wing government	1163	540	647	432	377
	(192)	(202)	(202)	(217)	(220)
	[315]	[341]	[337]	[359]	[366]
Undefined government	188	-66	-116	-59	-60
	(145)	(154)	(153)	(160)	(160)
	[230]	[233]	[230]	[240]	[234]
Left		89	456	-447	307
		(16)	(49)	(167)	(486)
		[40]	[108]	[353]	[1040]
$Left^2$			-3.86	16.2	-10.6
			(.50)	(3.6)	(16.9)
			[1.18]	[7.7]	[36.3]
$Left^3$				.-137	.26
				(.025)	(.25)
				[.053]	[.54]
$Left^4$					-.0021
					(.0013)
					[.0029]
Right		-57	-87	-661	-1082
		(13)	(55)	(186)	(539)
		[31]	[126]	[310]	[760]
$Right^2$			.31	13.3	28
			(.51)	(3.9)	(18)
			[1.14]	[6.6]	[26]
$Right^3$				-.092	-.31
				(.026)	(.26)
				[.044]	[.38]
$Right^4$					.0011
					(.0013)
					[.0020]
Municipality effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R^2	0.8401	0.8424	0.8446	0.8456	0.8457
R^2 (within)	0.6044	0.6101	0.6155	0.6180	0.6182
Number of observations	5,912	5,912	5,912	5,912	5,912

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 7. The party effect: Income tax rate

	I	II	III	IV	V
Left-wing government	.295 (.043) [.072]	.101 (.046) [.077]	.122 (.045) [.073]	.114 (.049) [.081]	.107 (.049) [.080]
Undefined government	.136 (.033) [.064]	.055 (.033) [.059]	.039 (.033) [.060]	.031 (.034) [0.61]	.028 (.034) [.061]
Left		.027 (.003) [.007]	.094 (.012) [.027]	.106 (.042) [.092]	.233 (.116) [.220]
$Left^2$			-.00072 (.00011) [.00024]	-.00096 (.00088) [.0019]	-.0054 (.0039) [.0071]
$Left^3$				1.59e-06 (5.91e-06) [.000012]	.000066 (.000055) [0.0001]
$Left^4$					-3.34e-07 (2.87e-07) [5.12e-07]
Right		-.018 (.003) [.008]	-.0045 (.011) [.022]	.022 (.037) [.079]	-.105 (.096) [.191]
$Right^2$			-.00014 (.00011) [.00023]	-.00075 (.00081) [.0017]	.0038 (.0033) [.0064]
$Right^3$				4.29e-06 (5.65e-06) [.000012]	-.000063 (.000047) [.000091]
$Right^4$					3.55e-07 (2.48e-07) [4.71e-07]
Municipality effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R^2	0.9388	0.9404	0.9412	0.9412	0.9412
R^2 (within)	0.8614	0.8650	0.8666	0.8667	0.8667
Number of observations	5,913	5,913	5,913	5,913	5,913

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 8. Party effect: Including other covariates

Dependent variable	I	II	III	IV	V
			Total spending		
Left-wing government	1000	690	726	792	725
	(183)	(197)	(198)	(215)	(219)
	[309]	[331]	[331]	[349]	[357]
			Current spending		
Left-wing government	1207	701	718	796	751
	(189)	(200)	(200)	(214)	(216)
	[335]	[358]	[358]	[379]	[379]
			Total revenues		
Left-wing government	967	653	697	602	540
	(178)	(191)	(192)	(210)	(213)
	[290]	[316]	[315]	[336]	[343]
			Income tax rate		
Left-wing government	.272	.135	.143	.139	.129
	(.041)	(.044)	(.043)	(.048)	(.048)
	[.070]	[.073]	[.072]	[.080]	[.080]
Control variables	Yes	Yes	Yes	Yes	Yes
Municipality effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 9. Party effect: Linear control function with and without interactions

	Total spending	Current Spending	Total revenues	Income tax rate
	Party effect with a linear control function			
Left-wing government	690	701	653	.135
	(197)	(200)	(191)	(.044)
	[331]	[358]	[316]	[.073]
	Party effect evaluated at 50 % of vote share			
Left-wing government	665	705	619	.130
	(197)	(199)	(190)	(.044)
	[336]	[359]	[319]	[.073]
	Party effect evaluated at mean of vote share			
Left-wing government	854	703	872	.172
	(213)	(215)	(205)	(.046)
	[340]	[375]	[324]	[.076]
Control variables	Yes	Yes	Yes	Yes
Municipality effects	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.

Table 10. The discontinuity samples

Samples	Total spending	Current spending	Total revenues	Income tax rate	Number of municipalities (Aver. obs. per municipality)
+5/-5	532	456	559	.156	132
	(262)	(234)	(255)	(.063)	(10.2)
	[408]	[371]	[402]	[.085]	
+4/-4	592	680	650	.130	119
	(277)	(256)	(265)	(.070)	(9.2)
	[433]	[427]	[426]	[.086]	
+3/-3	399	691	217	.204	94
	(402)	(340)	(379)	(.111)	(6.4)
	[592]	[495]	[551]	[.127]	
+2/-2	1178	1148	964	.221	77
	(425)	(401)	(430)	(.165)	(5.4)
	[359]	[449]	[471]	[.213]	
+1.5/-1.5	922	1102	457	.152	73
	(353)	(302)	(351)	(.104)	(5.0)
	[354]	[447]	[440]	[.169]	
+1.3/-1.3	982	1047	568	.092	69
	(372)	(283)	(387)	(.108)	(4.5)
	[445]	[439]	[643]	[.184]	
+1.1/-1.1	829	629	176	.092	44
	(522)	(335)	(515)	(.126)	(4.6)
	[549]	[555]	[737]	[.175]	
+1.0/-1.0	363	1343	1165	.561	26
	(1380)	(871)	(1428)	(.288)	(4.2)
	[1165]	[606]	[855]	[.248]	
Municipality fixed effects	Yes	Yes	Yes	Yes	
Year effects	Yes	Yes	Yes	Yes	

Note- Huber-White robust standard errors are in parentheses. More conservative Huber-White standard errors allowing for clustering at the municipality level to account for possible serial correlation in the errors within municipalities are presented in brackets.