Do Parties Matter for Fiscal Policy Choices?

A Regression-Discontinuity Approach[*]

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Abstract

This paper tests whether parties matter for fiscal policy choices. It is part of conventional wisdom that left-wing parties spend and tax more than right-wing parties However, very few empirical studies find a significant party effect and those who do find an effect could equally well be interpreted as an effect induced by voter preferences. This paper tries to solve the Gordian knot of separating the party effect from voter preferences on fiscal policy by using a regression-discontinuity approach, which is employed on a panel data set from Swedish local governments. The results show that there is strong evidence for a party effect independent of voter preferences: left-wing parties spend and tax more than right-wing parties.

1. Introduction

There is a wildly held assumption that left-wing parties prefer a larger size of government than right-wing parties. However, the assumption is questionable both from a theoretical and an empirical point of view. In the theoretical literature there is the well-known median voter theorem, i.e., in a two party system the competition for votes will drive opposing parties to the ideal policies of the median voter. Thus, equilibrium policies will be characterized only by the preferences of the median voter. The results from empirical studies are also debatable. A few studies (e.g., see Blais, Blake and Dion 1993 and the studies cited therein) claim to have found support for a significant party effect on fiscal policy outcomes. The way the party effect has been empirically identified, however, the result could equally well be interpreted as an effect induced by voters' preferences.

The issue whether parties matter for fiscal policy choices is a very important one. The answer to the question could have an impact on how we should think about the role of parties in our democracy. Do we think of parties as agents of the voters, as expressed by philosophers such as John Stuart Mill and Jeremy Bentham, or do we think of parties as choosing their own agenda, as expressed in the writings of Plato and Edmund Burke. The answer to the question would also have an implication for how economists and political scientists should theoretically model party competition. Should we use models where the parties offer the same equilibrium policies or models, which predict divergent outcomes?[1]

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[1] Some models do predict divergent outcomes. For example, Alesina (1988) criticize the political science literature for the assumption of commitment to partyplatforms. Once one drop this assumption there is

To be perfectly certain that parties have an independent effect from voter preferences on policy outcomes one would have to make a randomized experiment, i.e., to randomize parties in government over political entities. However, such an experiment would clash with our notion of democracy. An alternative to a randomized experiment is to employ a "natural" or a quasi-experimental design.[2] There are a number of different quasi-experimental designs but one seems particularly apt to handle the problem of the party effect, namely the regression-discontinuity design.[3] The design resembles an experiment in that a known rule determines how persons are assigned to treatment. However, the assignment to treatment is not random and persons who receive treatment may differ systematically from those who do not. By controlling for the rule in the regression analysis it might be possible to get an unbiased estimate of the treatment effect. In fact, this design is considered among the strongest of the quasi-experimental approaches in making causal statements about treatment effects.[4]

In this paper, I employ the regression-discontinuity design on a data set from Swedish local governments. The use of this data set offers some particular advantages in the quest for an independent party effect on fiscal policy.[5] First, heterogeneity is much less of a problem than in cross-country studies for example. Specifically, Swedish local governments operate within a

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policy divergence. Some other examples of divergence are: Palfrey (1984) where the existence of a third-party candidate causes the two major-party candidates to adopt different election campaing platforms, and Ingberman and Villani (1993) who show non-convergence if the issue space is multidimensional and parties are risk averse.

[2] See Meyer (1995) for an overview of different types of quasi-experiments in economics, although he does not cover the quasi-experimental design used in this paper.

[3] Thistlethwaite and Campbell (1960) was the first paper to introduce the regression-discontinuity design. More recently, Van der Klaauw (2000) and Angrist and Lavi (1999) have used this design. Trochim (1984) gives a good textbook treatment of the approach.

[4] See Trochim (1984, p.65).

common political framework and classifying parties as left or right in a commensurable is less of an issue. The comparability of fiscal policy variables, such as expenditures and taxes, is also less of a problem since the local governments have similar spending programs and raise the bulk of their revenues through a proportional income tax. Now it might be argued, that is misconceived to look at local governments' fiscal policies as determined by local political factors because Swedish local governments have limited autonomy. However, the fact Swedish local governments have the constitutional right of self-government and the results derived from my empirical analysis strongly suggest there exist a non-trivial local fiscal policy choice. An additional advantage of using this data set is that is a panel, consisting of 288 local governments from 1974 to 1994 (a 21 year-period), which allows me to control for unobservable and unchanging characteristics, and give attention to dynamic issues. There has been a neglectfulness of dynamic issues in previous work on party effects, and since there are good reasons for believing in some inertia in fiscal policy outcomes this could possible have lead to biased inference.

There is a related literature in American politics, which also try to address the question of voters vs. party effects, but this literature looks at legislative voting behavior instead of economic policy outcomes.[6] This literature also deal with the additional complexity of separating

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[5] Hamermesh (1999) argues that we too often use data sets that are not really apt for the answering the research question at hand.
[6] Peltzman (1984) and Kalt and Zupan (1984) are two early studies, while Levitt (1996), Snyder and Groseclose (2000) and McCarty et al. (2001) are examples of more recent work.

individual legislators preferences from party effects,[7] which have lead to the development of new kinds of statistical methods to deal with this particular problem. This is a contentious literature and there is yet no consensus whether there is a party effect on roll-call voting patterns.

The paper is organized as follows. Section 2 describes the identification problem of party effects and presents a possible solution: the regression discontinuity design. Section 3 presents the econometric specification and the data to which the regression-discontinuity approach is applied. Section 4 presents the results. Section 5 concludes.

2. The identification problem of party effects and the regression-discontinuity design

What constitutes clear evidence of a significant party effect on policy outcomes? Previous studies have regarded correlation between measures of partisanship and policy choices as evidence of the significance of parties. In these studies, partisanship has been measured both as a dummy variable,[8] and as the proportion of votes or seats received for left-wing parties among all parties participating in government or in parliament.[9]

It may be problematic to draw causal inference of a party effect using either of these two measures. To begin with, the dummy variable is almost certainly correlated with voter preferences, which makes it very difficult to separate the effect of parties from voter preferences

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[7] An additional advantage with using data from Sweden is that the Swedish election system is entirely party based which allows me to focus on voters vs. party effects without the additional problem with individual legislator effects.

[8] Studies that have used a dummy variable and claim to have found a significant party effects: Alt and Lowry (1994) and Besley and Case (1995).

[9] Studies that have used as the proportion of votes or seats and claim to have found support of a party effect: Cameron (1978), Swank (1988), Blais et al. (1993), and Perotti and Kontopoulos (1998)

on policy outcomes. Causal interpretation from votes or seats shares is even more problematic: these may measure the electoral support of left-wing parties participating in government as well as their bargaining power within the cabinet. In other words, we have a now what has been termed a selection problem, meaning that the assignment of observations (i.e., voters' choice of political parties in elections) to treatment groups (i.e., different political parties holding office) could lead to a correlation between assignment and outcomes in absence of treatment.[10]

How can we test whether parties have a causal effect on fiscal policy outcomes? A randomized experiment would be the best method to answer this question. 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 measure 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 a randomized experiment since it would clash with our notion of democracy, i.e., voters elect parties to govern. If we cannot make a randomized experiment we can at least try to approximate one. This is the idea behind the quasi-experimental research design employed here: the regression-discontinuity method.

The general idea of the regression-discontinuity design is that a known rule influences how units are assigned to treatment groups.[11] In our context the vote share is the deterministic

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

[11] There are two types of regression-discontinuity designs: the sharp and the fuzzy design. With the sharp design, treatment is known to depend in a deterministic way on some observerable variable, whereas in the

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 majorities are the different treatments, which the political entities will be assigned to. Since the vote share is the only systematic determinant of treatment status it might be possible to get an unbiased party effect by conditioning on vote share in the policy outcome equation of interest. The regression-discontinuity design can be formalized as follows.[12]

Consider a causal model that links some policy outcome P_i in a political unit i to a left-wing party majority indicator variable D_i, where $D_i=1$ if the left-wing vote share $v_i > 50$ percent and zero otherwise. We now have the following policy outcome equation:

$$P_{it} = \boldsymbol{a} + \boldsymbol{d}D_i + \boldsymbol{e}_i \qquad (1)$$

where the parameter $\boldsymbol{d}$ is the causal party effect, interpreted as the difference in policy outcomes between left and right wing majorities. The key identifying assumption is that without any treatment, the party effect $\boldsymbol{d}$ would be zero, i.e., $E[\boldsymbol{e}_i \mid D_i] = 0$. However, this condition will typically not hold, in particular because D_i is almost certainly correlated with voter preferences. In this case, an unbiased estimate of the party effect can be obtained by including the correct functional form of the conditional mean $E[\boldsymbol{e}_i \mid D_i, v_i]$ in equation (1), which now becomes

fuzzy design there are other factors, some of which are unobserved, which affect treatment. This paper deals with the sharp design.

$$E[P_i \mid D_i, v_i] = \boldsymbol{a} + \boldsymbol{d}D_i + E[\boldsymbol{e}_i \mid D_i, v_i] \tag{2}$$

Because assignment to treatment groups $D_i = D(v_i) = 1\{v_i > 50\}$ occurs through a known and deterministic decision rule the following equality applies to equation (2): $E[\boldsymbol{e}_i \mid D_i, v_i] = E[\boldsymbol{e}_i \mid v_i]$. Hence, entering the correct specification of $f(v_i) = E[\boldsymbol{e}_i \mid D_i, v_i]$ will free any correlation between D_i and the error term $\boldsymbol{e}_i$ and thereby give an unbiased estimate of the party effect. There are, however, some critical assumptions of the regression-discontinuity design that need to be fulfilled in order to make a causal interpretation of the party effect. The most important assumption (which is untestable) is that $f(v_i)$ is a smooth function of the policy outcome P_i, i.e., when the vote share changes with a small amount, the policy outcome also changes with a small amount. In other words, the party effect is identified only because there is a discontinuity of the treatment status (left or right wing majorities) at the threshold value of the vote share $v_i = 50$ percent. The parametric specification of the assignment rule is the key to get an unbiased party effect. Therefore, I will try two different approaches. The first approach is to control for a flexible polynomial function of $f(v_i)$ by using linear, quadratic and cubic terms.[13] The second approach is to restrict the analysis to units with a vote share close to the point of discontinuity 50 percent. The idea is that this subsample would be more representative of a true randomized experiment and therefore any bias due to misspecification of the assignment rule could possible be avoided. Another issue of the validity of the regression-discontinuity design, which is also

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[12] Goldberger (1972) presents an early formal discussion of the statistical properties of the regression discontinuity design in the case of a linear model. Heckman and Robb (1985) have a more general discussion of this design. More recently, Hahn et al. (2000) discuss a minimal parametric approach.

related to the previous assumption, is whether there are any other factors other than D_i that would produce a discontinuity at the threshold value. In an experimental situation, this would not be a problem but since this is an observational study there is a need to control for other variables that might cause such effects.[14] Moreover, adding additional covariates will improve the precision of the party effect estimate.[15] So far we have made the assumption that the causal party effect to be the same for every unit, but there could be reasons to expect that the party effect could be heterogeneous.[16] In the case of heterogeneous party effects, the parameter d in equation (1) would instead be interpreted as the average effect of treatment on the treated.[17] Hence, we can still make a causal interpretation of the party effect.[18] A final issue in this particular regression-discontinuity design is there must be perfect assignment of treatments relative to the cutoff value, otherwise we would have to use some other approach, such as an instrumental variable approach.[19]

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[13] I have also tried higher order polynomial functions but the results presented in the paper are robust to this consideration.

[14] The regression-discontinuty approach is based on selection of observables or ignorable treatment assignment assumption. The assumption is discussed by Barnow et al. (1981) and Rubin (1977).

[15] Goldberger (1972) suggests that one needs at least 2 ½ times as many program participant for the regression discontinuity design as for a randomized experiment in order to attain the same degree of precision in estimating the program effect.

[16] I do not think that heterogeneous party effects is of such great importance in my data set, because Swedish local governments operate under the same institutional setting, the electoral systems is exclusively party based, and the internal party organization is heavily centralized. Thus, these three factors would suggest that the local branches within each party are quit homogenous with respect to fiscal policy choices.

[17] Given that we have no omitted variables bias.

[18] On this point, see Angrist and Kreuger (1999).

[19] This is the fuzzy discontinuity design explored by Angrist and Lavy (1999), Van der Klaauw (2001) and Pitt and Khandker (1998).

3. The empirical identification strategy and the data

The objective of this paper is to test whether parties matter for policy choices by using a regression-discontinuity design together with a panel data set consisting of 288 Swedish local governments during the period 1974 to 1994. We will use the following estimating equation:

$$P_{it} = \boldsymbol{m}_i + \boldsymbol{p}_t + \boldsymbol{d} \, D_{it} + \boldsymbol{q}f(v_{it}) + x_{it}\boldsymbol{b} + \boldsymbol{e}_{it} \qquad (2)$$

with i denoting local governments (or municipalities) and t denoting time. P_{it} is the policy outcome variable, $f(v_{it})$ is the assignment rule for partisanship, $\boldsymbol{m}_i$ denotes the fixed unit effect, $\boldsymbol{p}_t$ the fixed time effect, $\boldsymbol{e}_{it}$ is an i.i.d. random disturbance term for the ith unit at time t, D_{it} is the indicator variable for left-wing government majority, $\boldsymbol{d}$ is the parameter of interest: a positive and significant value of $\boldsymbol{d}$ implies that left-wing parties spend and tax more than right-wing parties independent of voter preferences.

A number of comments of the empirical specification (2) and the data to which is applied is warranted. First, the indicator variable D_i is used to distinguish the two treatment groups, left or right-wing majority. In the Swedish context, this distinction is implemented by interpreting the proportional electoral system as bipartisan,[20] which is quite reasonable since there have traditionally been two main opposing party blocs, the socialist and the non-socialist

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[20] Treating Sweden as a bipartisan system is the predominant view among political scientists and economists. For example, see Laver and Schofield (1990) and Alesina et al. (1997).

bloc.[21] However, there is a caveat. At the local level several small parties exist, which are not part of the two major blocs, and sometimes these parties hold the balance of power. I call these kinds of constellations undefined blocs.[22] These create a problem since there is no general information about the constellation of parties of this bloc. But the regression discontinuity approach can solve this dilemma. Including a separate indicator variable for this group of municipalities and controlling for an additional assignment rule makes the interpretation of the party effect correct,[23] namely as the average difference in policy outcomes between left and right-wing majorities. The prediction of the effect on policy outcome for the undefined bloc is rather unclear, but if we entertain the assumption that the policy outcome is some linear combination of left and right-wing policy outcomes,[24] we would expect the effect to be positive but less than the party effect *d*. If this additional prediction is true it would, perhaps, further strengthen the causal interpretation of the party effect. Table 1 summarizes the number of left, right and undefined governments in every election period during the sample period 1974-1994. There was a left-wing majority in 826 cases, a right-wing majority in 833 cases, and an undefined majority in 312 cases. Table 2 shows the frequency of government changes for the municipalities. The number of government changes is very unequally dispersed among the

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[21] 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, from 1974 until 1988. Since 1988 it includes a fourth party: the Christian Democratic Party. In the 1991 election there was a fifth part was included in the non-socialist bloc: the New Democratic Party.

[22] 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 undefined.

[23] The additional assignment rule is to include the vote share (with linear, quadratic, and qubic terms) of right-wing incumbents.

[24] Usually, there is some party in the center of the left-right scale, such as the Green Party, which holds the balance of power and it often choose to alternate between left-wing and right-wing parties as partners in the coalition government.

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), while 90 (32 percent of the sample) had 3 changes or more. Table 2 also shows the average vote share for the incumbent in each group of municipalities.[25] 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.

Another comment on the specification (2) concerns the fixed effect m_i. One of the main advantages of a panel data set is that we can control for unobserved and unchanging characteristics that could be related to both the majority indicator D_{it} and the policy outcome P_{it}, by including the fixed effect. Specifically, this implies that 122 local governments would not be part of the identification of the party effect since there has not been any change in political power in these units during the sample period, which can be seen from Table 1. Equally important is the inclusion of a fixed time effect p_t since there might be common shocks that could create a discontinuity at the threshold value $\gamma=50$, (as we mentioned in the previous section, this could jeopardize the causal interpretation of the party effect) such as the national business cycle or general changes in the voters' preferences. As a case in point, Table 2 shows that there has been a large swing in the number of government's changes (i.e., voters preferences) such as in the 1991 and 1994 elections. There might also be other covariates that could create discontinuities at the cut off value, or otherwise be regarded as a standard set of

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[25] The vote share is compiled from the distribution of seats in local councils, due to missing data. 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 share was larger than 0.99.

determinants of local fiscal policy.[26] I will therefore control for average income, proportion of

the population aged 0 to 15, proportion of the population above 65, population density,

municipality population size, and intergovernmental grants.

Finally, I will also control for past outcomes of the dependent variable, since there are at

least two good reasons for doing that. First, the regulation that the central government imposes

on sub-national governments or the incremental routines of budget making (e.g. Wildavsky

1974) might cause some inertia in fiscal policy outcomes. Second, if governments use economic

policy either as a mean to affect is likelihood of reelection, as suggest by the political business

cycle literature, or because of strategic reasons, as suggested by the strategic debt literature

(e.g. see Persson and Tabellini 2000),[27] these types of manipulations might lead to an biased

party effect unless one control for past policy outcomes. This is reminiscent of the "Ashenfelter

dip" in the analysis of training program, where participants often experience a pre-program dip

in earnings, and ignoring this fact would generate a spurious positive training effect.[28] The

inclusion of a lagged dependent in a panel data context can create potential estimation problems.

However, the number of time periods here is quite large (T=21), which makes the prospective

bias smaller.[29] As the main concern of this study is the party effect and not the lagged dependent

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[26] Although there is yet no consensus of the right model of government fiscal policy outcomes, many empirical studies have used a similar set of covariates.

[27] In an empirical study, using part of this data set, Lidbom-Pettersson (2001) finds that the level of debt is used strategically by an incumbent government, which is not likely to be reelected, in order to affect the policies of its successor.

[28] Ashenfelter (1978) was the first one to discover this decline in pre-program earnings.

[29] See Nickell (1981) on this point.

variable per se, [30] I will ignore these effects and use the fixed effect estimator in the empirical analysis.

As dependent variables I will use four different measures: 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, perhaps a more discretionary measure is to use the proportional income tax rate itself.[31] 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.[32] Table 1 presents summary statistics for the four dependent variables.

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[30] Judson and Owen (1999) compare the biases for different dynamic panel data estimators when both N and T are fairly large in a Monte Carlo study. They conclude that they are unable to judge which of the estimator is less biased concerning the exogenous regressor. Moreover, the fixed estimator is less sensitive to violation of the strict exogeneity assumption, especially when T is large, than a difference estimator. When the processes are weakly dependent over time and T is large, the bias in the fixed effect estimator can be small (see, for example, Wooldridge 2001, Chapter 11). Moreover, Nickell (1981) shows that the bias of the exogenous variable dependent on its correlation with $[y_{it}-y_{i,-1}]$. The correlation between the party dummy variable and $[y_{it}-y_{i,-1}]$ is presumebly negative due to politically induced electoral budget cycles (e.g. Pettersson-Lidbom (2000). Hence, this will lead to a downward bias in the estimate of the party effect.

[31] Local governments are free to set their own tax rate.

[32] 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.

4. Results

4.1 Basic results from the full-sample

I present empirical evidence of the party effect on the full sample for four types of fiscal policy outcomes. The first two columns in Tables 4 and 5 consider the effect of parties on two measures of spending whereas the last two columns show the party effect on total revenues and the proportional income tax rate. The only difference between Tables 4 and 5 is that a lagged dependent variable is included among the covariates in the latter table. The main results are that I find a positive and significant party effect in all specifications: on average, left-wing governments increase both spending and tax compared to right-wing governments. The party effect on total and current spending is almost identical. For the static specifications the effect is about SEK 800 per capita, whereas the party effect is little more than SEK 400 per capita for the dynamic specifications. These effects are roughly in the order of 1.5 to 3 percent of mean spending. The party effect on total revenues is SEK 600 per capita for the static specification and SEK 327 for the dynamic. The party effect on the income tax rate is 0.14 and 0.09 percentage points for the static and dynamic specification respectively. This is almost 1 percent of the average tax rate. One general pattern to note is that the party effect is almost twice as big in the static than in the dynamic specification. Thus, it seems that dynamic issues have a rather large impact on the party effect. However, the results of the party effect in Table 5 are robust to the inclusion of more than one lag of the dependent variable.[33]

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[33] These results are avaible upon request.

It might be interesting to look at the effect of the undefined majority on the fiscal policy outcomes. As mentioned earlier, we do not have a strong prior on this effect but if we entertain the assumption that the undefined majority pursue some combination of left and right-wing policy outcomes, the effect should be positive but less than the party effect. From Tables 4 and 5, we can see that the effect from the undefined majority is less than the party effect in all cases, although this effect is not very precisely measured.

4.2 The discontinuity sample

Results for a subsample, where vote share for left and right wing local governments is in the range [47,53], are reported in Table 6.[34] The idea about restricting the subsample in this range is that the assignment rule induces a discontinuity at 50 percent of the votes and this discontinuity is the source of identifying information of the party effect. Although, that limiting a sample in such away in a panel data context may yield imprecise estimates of the party effect or be subject to other types of biases (e.g. bias due to small T in a dynamic fixed effect specification or attrition), a causal interpretation of the party effect would be strengthen if the results are consistent with the full sample. Table 6 presents the results from same econometric specification as in the previous full sample dynamic case. The results of the party effect are highly consistent with the full sample, although the effect is less precisely measured. The results for the undefined majority are also quite consistent with the findings in the full sample.

4.3 The unchanged majority sample

So far all the econometric specifications have included fixed unit effects, which imply that 122 local governments are not part of the identification of the party effect for the simple reason that they have not had any change in government. However, these local governments might provide some valuable insights of the cumulative party effect since applied studies using panel data tend to find that estimators based on the time-series component of the data, such as the previously used fixed effect estimator, tend to give short-run estimates whereas estimator based on the cross-sectional component of the data tend to give long-run estimates.[35] To exploit the cross-sectional component of the data I will use the between estimator,[36] where each variable is expressed as a time average for each unit. The data is restricted to those local governments which have had either left or right-wing majority during the whole sample period. There have been 69 municipalities with left wing and 45 with right-wing majority. Table 7 show the results from this sample using the between estimator. Even though that the causal interpretation of the party effect is much more questionable in this particular sample the results show a large and sometimes significant estimate of the party effect. For example, the party effect for total spending is SEK 3162 per capita, which are about 11 percent of average total spending.

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[34] I have experimented with other subsamples close to the treshold (for example, when both left and right-wing incumbents vote shares are less than 51 percent) and these results are also consistent with the results from full sample.

[35] See Baltagi and Griffin (1984).

[36] The between estimator is biased if there is a correlation between the fixed effect and the other covariates.

5. Conclusions

Previous studies of whether parties matter for fiscal policy choices have regarded correlation between measures of partisanship and policy choices as evidence of the significance of parties. However, this conclusion might be premature since the different ways the partisanship variable have been made empirically operational, these party effects could equally well be interpreted as effects induced by the preferences of voters. To be perfectly certain that parties matter for fiscal policy choices one would have to make an randomized trial, i.e., randomize political parties in government over jurisdictions, however this kind of experiment would be impossible to conduct in a democracy. This paper tries to overcome this difficulty by employing a quasi-experimental design, the regression-discontinuity design, to the question of the importance of political parties in shaping economic policy. Using a panel data from Swedish local governments, I find strong evidence that party's matters for fiscal policy choices independently from voter preferences. Left-wing parties spend and tax more than right-wing parties.

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Table 1. Partisanship summary

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. Descriptive statistics for the dependent and control variables

Variables	Mean	Standard d.	Min	Max
Total expenditures	28257	5804	14391	70031
Current spending	26790	6748	11889	70924
Total revenues	28207	5699	15515	71699
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	72624	12357	15945	162962
Income, t-2	59915	12483	17950	151977
Population size	29774	52551	2865	692954
Population density	107	360	0.28	3700
Tax equalization grants	2114	2192	-3963	19599

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

Table 4. The party effect on policy outcomes: a static specification

	Dep var: total spending	Dep var: current spending	Dep var: total revenues	Dep var: income tax rate
Left-wing	791	795	600	0.14
government	(3.68)	(3.72)	(2.86)	(2.89)
Undefined	94	-45	-7	0.03
government	(0.63)	(-0.30)	(-0.05)	(0.82)
Left vote share	-292	2.9	-436	0.07
(%)	(-1.67)	(0.02)	(-2.93)	(1.75)
Left squared vote	8.5	1.7	12	-0.001
share	(2.27)	(0.52)	(3.77)	(-1.31)
Left cubic vote	-0.07	-0.02	-0.1	4.35e-06
share	(-2.65)	(-0.68)	(-4.34)	(0.75)
Right vote share	-693	-276	-599	0.03
(%)	(-3.88)	(-2.43)	(-3.62)	(0.98)
Right squared	14	5.5	12	-0.001
vote share	(3.82)	(2.12)	(3.40)	(-1.32)
Right cubic vote	-0.1	-0.04	-0.08	6.20e-06
share	(-3.95)	(-2.35)	(-3.46)	(1.11)
Muncipality	0.07	0.009	0.11	-1.62e-06
income per capita	(2.93)	(0.61)	(4.41)	(-0.35)
(t)				
Muncipality	0.04	-0.003	0.04	-8.71e-06
income per capita	(2.20)	(-0.03)	(2.20)	(-2.76)
(t-2)				
Population size	-0.19	-0.19	-0.19	-0.00003
	(-6.80)	(-5.83)	(7.38)	(-3.74)
Proportion	130	86	104	0.02
population young	(3.43)	(2.73)	(2.73)	(2.42)
(0-15)				
Proportion	-173	54	-157	0.03
population elderly	(-3.45)	(1.19)	(-3.31)	(2.39)
(65+)				
Population	-15	-14	-14	-0.003
Density	(-6.85)	(-5.56)	(-5.66)	(-5.38)
Tax equalization	0.48	0.49	0.59	0.00006
grants	(7.75)	(8.93)	(9.86)	(5.39)
R^2	0.855	0.9096	0.8640	0.9444
Number of	5913	5913	5912	5913
observations				

All regressions include year and municipality effects. Robust standard errors where used in calculating t-statistics.

Table 5. The party effect on policy outcomes: a dynamic specification

	Dep var: total spending	Dep var: current spending	Dep var: total revenues	Dep var: income tax rate
Left-wing government	420 (2.37)	460 (2.72)	327 (1.93)	0.09 (2.98)
Undefined government	142 (1.17)	103 (1.00)	56 (0.46)	0.02 (1.20)
Left vote share (%)	-78 (-0.53)	54 (0.53)	-263 (-2.15)	-0.01 (-0.43)
Left squared vote share	2.5 (0.79)	-0.56 (-0.24)	7.2 (2.74)	0.001 (0.25)
Left cubic vote share	-0.02 (-0.99)	0.004 (0.26)	-0.05 (-3.12)	-1.67e-06 (-0.45)
Right vote share (%)	-354 (-2.40)	-157 (-1.76)	-359 (-2.63)	-0.003 (-0.10)
Right squared vote share	7.7 (2.44)	3.5 (1.81)	7.8 (2.65)	-0.001 (-1.56)
Right cubic vote share	-0.05 (-2.48)	-0.03 (-1.96)	-0.05 (-2.72)	7.96e-06 (1.56)
Lagged dependent variable	0.57 (31.16)	0.74 (23.68)	0.57 (28.73)	0.77 (58.44)
Muncipality income per capita (t)	0.05 (2.61)	0.03 (2.59)	0.07 (3.51)	-2.92e-07 (-0.11)
Muncipality income per capita (t-2)	0.03 (2.15)	0.001 (0.18)	0.03 (2.04)	-2.84e-06 (-1.54)
Population size	-0.09 (-3.54)	-0.06 (-1.81)	-0.08 (-4.07)	-7.22e-07 (-0.26)
Proportion population young (0-15)	108 (3.26)	77 (3.22)	91 (2.72)	0.02 (3.22)
Proportion population elderly (65+)	-108 (-2.58)	37 (1.21)	-87 (-2.35)	0.0003 (0.05)
Poulation Density (km^2)	-8.8 (-5.61)	-5.9 (-2.97)	-8.6 (-5.44)	-0.0009 (-4.51)
Tax equalization grants	0.27 (5.48)	0.23 (5.32)	0.31 (6.88)	0.00002 (4.03)
R^2	0.8988	0.9523	0.9053	0.9797
Number of observations	5627	5627	5625	5627

All regressions include year and municipality effects. Robust standard errors where used in calculating t-statistics.

Table 6. The party effect: The discontinuity sample [47,53]

	Dep var: total spending	Dep var: current spending	Dep var: total revenues	Dep var: income tax rate
Left-wing	596	940	214	0.07
government	(0.53)	(1.06)	(0.18)	(0.37)
Undefined	40	120	131	0.04
government	(0.09)	(1.34)	(0.31)	(0.43)
The same covariates as in Table 5	Yes	Yes	Yes	Yes
R^2	0.9622	0.9762	0.9597	0.9866
Number of observations	560	560	560	560

All regressions include year and municipality effects. Robust standard errors where used in calculating t-statistics. The sample is restricted to include only those local governments who has received a vote share are in the range of 47 and 53 percent.

Table 7. The party effect: The unchanged majority sample

	Dep var: total spending	Dep var: current spending	Dep var: total income	Dep var: income tax rate
Left-wing	3162	1634	3697	0.11
government	(1.55)	(0.84)	(1.82)	(0.13)
R^2	0.7993	0.8118	0.7975	0.6688
Number of observations	114	114	114	114

All the regressions are based on the between estimator, i.e, time averages for both dependent an independent variables and then run a cross-sectional regression.