

Do Parties Matter for Economic Outcomes ? A Regression-Discontinuity Approach*

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Abstract

A long-standing issue in political economics is to what extent party control makes a difference in determining fiscal and economics policies. This question is very difficult to answer empirically since parties are not randomly selected to govern political entities. This paper uses a regression-discontinuity design, i.e. party control changes discontinuously at 50 percent of the vote share, which can produce “near” experimental causal estimates of the effect of party control on economic outcomes. The method is applied to a large panel data set from Swedish local governments with a number of attractive features. The results show that there is an economically significant party effect: left-wing governments spend and tax 2-3 percent more than right-wing governments. Left-wing governments also have 7 percent lower unemployment rates, which is partly due to left-wing governments employing 4 percent more workers than right-wing governments.

Key words: political parties, party control, partisan politics, regression-discontinuity design, natural experiments, unemployment, government employees, fiscal policy

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1. Introduction

This paper estimates the *causal* effect of party control on fiscal and economic policies.¹ Estimating the party effect without bias is a very challenging identification problem since parties are not randomly selected to govern political entities. For example, since voters select parties to govern, there may be an omitted variable problem due to unmeasured voter preferences.² Thus, a correlation between party control and some policy outcome does not necessarily imply causation. The large empirical literature dealing with partisan cycles in macroeconomic outcomes (e.g., growth, unemployment and inflation) is also plagued by similar problems of endogeneity of party control.³ Voters may, for example, elect conservative governments when recession is anticipated which will lead to a spurious relationship between party control and economic outcomes. While many studies claim to find strong empirical support for partisan differences in some macroeconomic outcomes, Faust and Irons (1999) argue that there is only weak evidence that party control is of importance when issues of simultaneous causality bias and omitted variable bias are properly accounted for in a vector autoregression framework.

The causal party effect could be convincingly estimated if parties in government could be randomized over political entities since randomization ensures that there is no systematic difference between political entities with governments of various stripes. In that case, the average difference in economic outcomes between entities with different party control is an unbiased estimate of the true 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. Nevertheless, we can still try to approximate the evidence generated by a randomized controlled trial, namely using a quasi-experimental design.

¹ For evidence on the party effect for fiscal policies, see Besley and Case (2003) for a survey of work on U.S. states, and Blais et al. (1993) for a survey of cross-country studies and U.S. states. See also Imbeau et al (2001) for a meta-analysis of studies using OECD data. For evidence on macroeconomic outcomes, see Alesina et al. (1997) and the references cited therein.

² For work that stresses the endogeneity of other political institutions see, for example, Aghion et al. (2004, 2005).

³ I use the word endogeneity as a catchall for problems with selection, omitted variables and simultaneous causality since all these problems will make the explanatory variable (party control) correlated with the error term.

In this paper, the source of identifying information of the party effect comes from an institutional feature of the election system, that is, party control changes discontinuously at 50 percent of the vote share which makes it possible to implement a *regression discontinuity design*. The general idea of the regression-discontinuity design is to compare the outcomes for units (e.g., political jurisdictions) whose value of an underlying targeting variable (e.g., vote share) is “just below” and “just above” a fixed threshold (e.g., 50 percent of the votes) since they will, on average, have similar characteristics except for the treatment (e.g., party control). In other words, those units slightly below the threshold will provide the counterfactual outcome for those units slightly above, since the treatment status will be “as good as randomly assigned” in a neighborhood of the treatment threshold. The inference from a regression discontinuity analysis can therefore be as credible as that from a randomized experiment (e.g., Lee 2003). In particular, the regression discontinuity approach shares the same attractive feature as a randomized controlled trial, namely that it can actually be tested whether treatment status is likely to be “as if” randomized.

I employ the 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 on economic outcomes. First, it is a large panel data set (288 municipalities over a 21-year period) making it possible to use a regression discontinuity design since there must be enough data “close” to the treatment threshold for the method to be useful. Second, Swedish local governments are very homogeneous. In particular, they operate within a common political framework and face the same institutional setting. Thus, economic outcomes and political parties are quite comparable across political entities, which is otherwise a major obstacle in cross-country studies. One potential weakness with the data set, however, is the multi-party feature of the Swedish political system. Nevertheless, 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.⁴ Hence, to a first approximation we can treat the Swedish electoral system as bipartisan.⁵

The results of this paper show that party control has a causal effect on spending, taxes and unemployment. The party effect is also quite substantial. For example, left-wing governments spend, as a share of income, about 2-3 percent more and have about 7 percent lower unemployment rates than right-wing governments. Left-wing governments also employ about 4 percent more workers than right-wing governments. I also present evidence in support for party control being as “good as randomly assigned” among those municipalities that are close to the treatment threshold of 50 percent of the vote share, which provides strong support for a causal interpretation of my results.

This paper is related to, but distinct from, the literature that investigates whether representatives from different political parties vote differently.⁶ Specifically, Lee et al. (2004) make use of a similar regression-discontinuity design in their study of the voting records of Democratic and Republican congressmen in the U.S. House of Representatives from 1946 to 1995.⁷ Although their analysis is interesting, it does not say whether, or to what extent, parties are of importance for policy outcomes since the mapping between votes and policy outcomes is not analyzed. For example, many votes in Congress are supported by large supermajorities. If the difference in voting between Democrats and Republicans mainly arises in these types of votes, then the effect on policy is nil.

The paper is organized as follows. Section 2 describes the regression discontinuity design and how it is implemented in this paper. Section 3 describes the data, while section 4 presents the results. Section 5 discusses the findings and concludes.

⁴ For an overview of the Swedish political system, see Petersson (1994). For a detailed description of local governments in Sweden, see Gustafsson (1988).

⁵ For example, Alesina et al. (1997) also classify Sweden as a bipartisan system (along with the U.S. and other political systems with a clear left-right division) in their empirical analysis.

⁶ See, for example, Levitt (1995), Snyder and Groseclose (2000), and McCarty et al. (2001).

⁷ The first version of this paper was written in May 2001 (Pettersson-Lidbom 2001) while the first version of the Lee et al. paper is from 2002. My paper is cited in their working paper, Lee et al. (2002), but not in the published version Lee et al. (2004).

2. Empirical framework

In this section, I will discuss the regression-discontinuity method and how it is implemented in this paper.

In the “sharp” regression-discontinuity design, treatment status is a deterministic function of some underlying continuous variable, that is,

$$(1) \quad T_i = T(x_i) = 1[x_i \geq \bar{x}],$$

where $1[.]$ is an indicator function and x is a continuous variable or an assignment variable, and \bar{x} is a treatment threshold separating the units into two mutually exclusive groups: those units receiving treatment ($T=1$) and those which do not ($T=0$). The idea is to compare the outcomes for units whose value of the underlying targeting variable is “just below” and “just above” the treatment threshold \bar{x} , since they will on average have similar characteristics except for the treatment. In other words, those units slightly below the threshold will provide the counterfactual outcome for those units slightly above, since the treatment status will be randomized in a neighborhood of treatment threshold. In our context, the vote share is the assignment variable that assigns parties to political entities and where the treatment threshold is at 50 percent of the proportion of votes.

In practice, the regression-discontinuity design can be implemented in a number of ways.⁸ The simplest possible approach is to just compare average outcomes in a small neighborhood on either side of the treatment threshold. This approach could, however, produce very imprecise measures of the treatment effect, since the regression-discontinuity method is subject to a large degree of sampling variability and this procedure would therefore require very large sample sizes. An *equivalent*, but much more efficient, method is to use all available data and a control function approach, that is, to regress the outcome of interest, say Y_i , on a low-order polynomial in the treatment-determining covariate x_i , i.e., the control function, and the binary treatment indicator T_i . This procedure will yield an unbiased estimate of the treatment effect, unless the control function is misspecified, since x_i is the *only* systematic determinant of T_i and therefore the

⁸ See Hahn et al (2002) for a non-parametric approach.

control function will capture *any* correlation between T_i and the population error term. The control function approach is my preferred method since there is only a limited number of observations close to the threshold in my data set (i.e., there are only 89 municipalities within ± 2 percentage points from the 50 percent threshold). Nevertheless, I will also present results where I only use data close to the threshold, i.e., in the range [48, 52], as a specification check since the estimate from the control function approach and the discontinuity sample should be the same (except for sampling variability) if the control function is correctly specified.

In this paper, a panel data set from Swedish local governments will be used to estimate regression models of the form

$$(2) \quad Y_{it} = \mu_i + \lambda_t + \pi T_{it} + f(\text{Left vote share})\varphi + v_{it}$$

where Y_{it} is an economic outcome (e.g., spending per capita, taxes, unemployment, and government employees per capita) for local government i in time period t , μ_i is a locality-fixed effect, λ_t is a time-specific effect, T_{it} is a treatment indicator taking the value of 1 for left-wing governments and zero for right-wing governments, and $f(\text{Left vote share})$ is a control function, i.e., some low-order polynomial in *Left vote share*. The parameter of interest is π – the party effect – which measures the average difference in economic outcomes between left- and right-wing governments.⁹ The main reason for including fixed municipality and time effects is to enhance efficiency since there is no need to include additional covariates except for $f(\cdot)$ in (2) to get an unbiased estimate of π . However, Hoxby (2000) argues that a “within-unit” regression-discontinuity method is “more powerful and less subject to bias” than a cross-section discontinuity analysis when there is only a limited number of observations close to the threshold. Thus, specification (2) takes into account her concern since it only uses the within-municipality variation to identify the party effect. A number of other controls (e.g., income, population size, proportion of people below 15, and proportion of people above 65) will also be added to

(2) as a way of checking whether party control is as good as randomly assigned. The inclusion of these additional covariates should not significantly affect the estimate of the party effect since party control should be as good as randomly assigned conditional on $f(\cdot)$. Here, it is important to *not* include variables that are themselves affected by the treatment, such as intermediate outcomes, since these will bias the estimate of the treatment effect.¹⁰ For example, including the lagged economic outcome Y_{it-1} among the control variables is not advisable in our context of measuring the causal effect of party control since it is an intermediate outcome,¹¹ and therefore affected by the treatment itself, i.e., party control. Nevertheless, it is possible to include the economic outcome from a *previous* treatment since that guarantees that it is a pretreatment variable, i.e., it was measured before the *current* treatment was chosen. Thus, one should only control for pretreatment characteristics to avoid bias. In practice, however, the covariates are often recorded at the same time as the outcome, subsequent to treatment. In this case, it must be assessed on a case-by case basis whether a particular covariate should be used as a control variable.

A final comment about specification (2) is that it is only the party effect π that has a causal interpretation since $f(\cdot)$ is allowed to be correlated with the error term v_{it} . Thus, it is not valid to interpret the coefficient on vote share φ as measuring the causal impact of voter preferences on economic outcomes. In other words, in the regression discontinuity approach, it is totally irrelevant whether the vote share can be considered as a good measure for voter preferences.

⁹ The estimated treatment effect from a regression-discontinuity design will typically not be the average treatment effect but a marginal treatment effect (see, e.g. Hahn et al 2001). This issue will be discussed below.

¹⁰ See Rosenbaum (1984) and Imbens (2004) for a discussion of the choice of covariates.

¹¹ This is related to the term-in-office being longer than one year. The term-in-office in Sweden is three years.

3. Data

To test whether party control is of importance for economic outcomes, I will use a panel data set from Swedish local governments, but before turning to the description of the data, it might be helpful to briefly digress on the workings of Swedish local governments.

As of 2005, there are 291 local governments (or municipalities) in Sweden 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 the constitutional right of self-government, no restriction on borrowing and no balanced budget rules.¹² Moreover, only 20 percent of their income come from grants, whereas the rest mostly comes from a proportional income tax, which each municipality can set freely. In other words, they have a relatively large degree of fiscal freedom.

To implement the regression-discontinuity method, the mechanics of the 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.¹³ 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 the municipal council, seats should be proportionally distributed among parties on the basis of the 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-list system, and has several political parties.¹⁴ The multi-party issue raises the question of

¹² As from 2000, however, there is a balanced budget rule in place.

¹³ As from 1994, elections are held every fourth year.

¹⁴ Whether the 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.

how to define treatment or party control. However, as previously discussed, 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.¹⁵ Hence, to a first approximation we can treat the Swedish electoral system as bipartisan,¹⁶ and define the treatment indicator T_i as 1 for left-wing majorities and zero otherwise. The party effect should thus more accurately be addressed as a majority coalition effect but, for simplicity, I retain the former name.¹⁷

There is also one caveat with my data that needs to be mentioned: 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 in defining party control since these are not easily classified along the left-right ideological spectrum. I call these kinds of constellations undefined majorities.¹⁸ The problem with undefined majorities is solved by including a separate dummy variable for the undefined majority, however. The party effect will now be correctly identified as the average difference in policy outcomes between left-wing and right-wing majorities.¹⁹

Table 1 summarizes the number of left-wing, right-wing and undefined governments in every election period during the sample period 1974-1994. There was a left-wing majority 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.²⁰ Table 1 also shows that there has been an undefined majority in 312 cases, which corresponds to 15 % of all

¹⁵ For a general overview of the Swedish political system, see Petersson (1994). For a detailed description of local governments in Sweden during the period of investigation, see Gustafsson (1988).

¹⁶ For example, Alesina et al. (1997) also classify Sweden as a bipartisan system (along with the U.S. and other political systems with a clear left-right division) in their empirical analysis.

¹⁷ To define the left-wing majorities and the right-wing majorities, I have relied on the standard classifications of parties along the left-right spectrum as discussed by Petersson (1994). According to this classification, the left-wing bloc includes the Social Democratic Party and the Leftist Party while the right-wing bloc includes five parties: the Conservative Party, the Centrist Party, the Liberal Party, the Christian Democratic Party and the New Democratic Party. The Christian Democratic Party is only included in the right-wing majority from the year 1988, however, and the New Democratic Party only from the year 1991.

¹⁸ 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.

¹⁹ Another approach would be to altogether exclude these observations from the analysis. It turns out that it is of no importance for the results about the party effect presented below which of these two approaches I use.

²⁰ This might be surprising given the Social Democratic party hegemony at the national level.

observations. Table 2 shows the frequency of government changes for the localities. The number of government changes is very unequally dispersed among the different municipalities. For example, 122 municipalities (42 percent of the total 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 discussed in section 2.

Turning to the economic outcomes, nine different variables will be used in the empirical analysis: total expenditures per capita, total expenditures as a share of income, current expenditures per capita, current expenditures as a share of income, total revenues per capita, total revenues as a share of income, proportional income tax rate, the unemployment rate, and the number of local government employees per capita. The difference between total and current expenditures per capita is mainly that investments are included in the former. Roughly 85 percent of total spending is classified as current spending. Total revenues per capita include tax receipts from a proportional income tax rate, fees and governmental grants. Since total revenues might reflect non-discretionary local government decisions, using the income tax rate itself is a more discretionary measure.²¹ The unemployment rate is only available from 1979 and therefore I will lose 5 years of data, as compared to the other outcomes, when I use this variable as the economic outcome of interest. Total expenditures, current expenditures, total revenues and income are expressed in 1991 prices. Total expenditures as a share of income, current spending as a share of income, total revenues as share of income, the proportional tax rate, the unemployment rate and government employment per capita are expressed as percentages.²² Table 3 presents summary statistics for the nine outcome variables. Table 3 also presents summary statistics for a standard set of controls in the local public finance literature (see e.g., Besley and Case 2003): average income, proportion of people of age 0 to 15, proportion of people older than 65 and population size. I consider these variables as not affected by the treatment, which is the key requirement for using them as controls as

²¹ On average, about 55 % of the total revenues come from the income tax.

discussed in section 2. All data used is publicly available and was obtained from Statistics Sweden (SCB) or its publications.²³

As a further description of the data, Table 4 presents means for left-wing governments (column 1), right-wing governments (column 2) and the difference in means between left-wing and right-wing governments (column 3). Column 3 reveals that left-wing governments are strongly statistically significantly associated with much higher total spending per capita (2,775 or 10.0 %), higher total expenditures as a share of income, (3.52 or 9.3 %), higher current total spending per capita (3,086 or 12.3 %), higher current expenditures as a share of income (4.00 or 11.5%), higher total revenues per capita (2,686 or 10.0 %), higher total revenues as a share of income (3.39 or 9.0 %), higher income tax rates (0.57 or 3.5 %), and more government employment per capita (0.24 or 4.2 %). Surprisingly, left-wing governments are associated with strikingly higher unemployment rates (0.59 or 20.0 %) than right-wing governments. The difference is also highly statistically significant from zero ($t=3.73$). In other words, there is a very large selection bias that causes the estimate of the party effect to even switch signs. This shows the problem of even interpreting the sign of the estimate as telling anything about the direction of the causal party effect and this may thus be one reason for why previous studies have reached different conclusions about whether party control is of importance for economic outcomes

It is also interesting to look at the differences in means for left- and right-wing governments that are close to the treatment threshold, i.e., within 4 percentage points from 50 percent of the votes. Table 5 shows these results and as we can see from column (3), all differences in the policy outcomes between left-wing and right-wing governments now have the expected signs. Specifically, the unemployment rate is now lower for left-wing governments than for right-wing governments, which illustrates that the estimated party effect is less biased when comparing governments closer to the treatment threshold. Nonetheless, none of these differences in outcomes are statistically significant from zero.

²² I have used the implicit GDP deflator. This deflator is constructed by taking the ratio of GDP at current market prices to GDP at fixed market prices.

²³ The publications used are: *How Much do Local Public Services Cost in Sweden, Local Government Finance* and *Statistical Yearbook of Administrative Districts of Sweden*.

Moreover, if we were to go closer to the treatment threshold, say only those governments within 2 percentage points from the 50% vote share, we would lose a substantial number of observations, as can be seen from Table 6. This illustrates the extraordinary requirements of the cross-sectional regression discontinuity method as discussed by Hoxby (2000). Therefore, we move on to the results from the “within-unit” regression-discontinuity method which should be “more powerful and less subject to bias” than a cross-section discontinuity analysis when there is only a limited number of observations close to the threshold as previously discussed.

4. Results

In this section, I present empirical evidence of the party effect. Tables 7 and 8 show the results from regressions of economic outcomes on party control (i.e., equation 2). All economic outcomes are in logarithmic form so the estimated party effect will have a percentage change interpretation. As described in section 2, I include fixed-municipality and time effects in all regressions as way of increasing efficiency but also to avoid a potential weakness with the cross-section regression-discontinuity method as discussed by Hoxby (2000). Since a major concern in a regression discontinuity design is whether the control function is correctly specified, I will use a number of different specification checks. To begin with, I will go from a first-order up to a fourth-order polynomial in vote share (see columns 1-4) as a way of testing whether the estimate of the council-size effect is sensitive to the different specifications of the control function. As a further specification check, I will also estimate the council-size effect only using observations that are +/- 2 percentage points from the 50 percent threshold without any parametric controls for vote shares (see column 5). The idea is that this discontinuity sample will be a close approximation to a randomized trial and therefore, it is unnecessary to include the control function. Consequently, the estimate from the discontinuity sample should now be equal (apart from sampling variability) to the estimate from the control function approach, unless the control function is misspecified. Another specification check is to include other covariates (pretreatment characteristics) in a quartic specification in vote shares (see column 6). This is related to the underlying assumption in the control function

approach, namely that party control should not be systematically related to any observed or unobserved variables once the assignment variable is controlled for. In other words, adding control variables should not affect the estimate from the control function approach but only reduce the standard errors. A final specification check is to allow the control function to be time varying by interacting the quartic in vote share with a full set of time-specific effects (see column 7). The standard errors are clustered at the party's term-in-office since the treatment, party control, is the same during these years. This way of calculating the standard errors should therefore take into account the issues about biased standard errors due to serial correlation as raised by Bertrand et al. (2004).

Starting with total expenditures per capita, the first row in Table 7 shows that left-wing governments have more than 2 percent larger spending per capita than right-wing governments. The estimates are strikingly similar (i.e., range between 2.0-2.7 percent) which suggests that the estimated party effect is an unbiased estimate of the causal effect. In fact, when the sample is restricted really close to the 50 percent threshold so as to avoid any misspecifications of the control function, the estimated party effect is 2.4 percent which is very similar to the other estimates in the first row. All estimates except the discontinuity sample in column 5 are significantly different from zero at the 5 percent level. Not surprisingly, the standard error from the discontinuity estimate in column 5 is about 30 percent larger than the others. This nicely illustrates why the control function approach is the preferred method since it is much more efficient than just comparing the average outcomes in a small neighborhood on either side of the treatment threshold as discussed in section 2. It is also reassuring to note that the estimate hardly changes when we add a number of covariates (e.g., income, proportion of young, proportion of old and population size) in column 6.²⁴ In other words, this suggests that party control is in fact "as good as" randomly assigned since adding control variables should not affect the original estimate but only reduce the standard errors.

Turning to the results from the other fiscal policy outcomes in rows 2-7 in Table 7, they reveal a remarkably similar picture. In all specifications, there is a positive party

²⁴ These covariates have significant explanatory power since they are strongly jointly significantly different from zero, i.e., $F(4, 1970)=70$ with a P -value of 0.0000.

effect which is typically statistically different from zero at least at the 5-percent level. The party effect estimates are also quite similar across the different specifications. For example, in the last row, the estimated effect for the income tax rate ranges from 1.1-1.4 percent. Moreover, the estimates from the discontinuity sample in column 5 are also quite similar to the other specifications in each of the rows and the estimates are hardly affected by the inclusion of additional controls (column 6). To sum up, Table 7 reveals a strikingly consistent picture, namely that left-wing governments spend and tax significantly more than right-wing governments.

Turning to economic policies, i.e., unemployment and government employment as displayed in Table 8, we also get a quite consistent pattern of the party effect, namely that the unemployment rate is lower while government employment is higher for left-wing governments than for right-wing governments. For unemployment, the estimates are negative across all specifications although they are less precisely measured than the estimates from the fiscal policy in Table 7. However, this is not surprising since the data on the unemployment rate is only available for a shorter time period (e.g., 1979-94 instead of 1974-94). Nevertheless, in the most flexible specification in column 7, the estimate -7.0 percent is significantly different from zero at the 5 percent level. For government employees' per capita, the estimates are in the range 3.0-3.9 percent and all but one of them are statistically significantly different from zero at the 1-percent level. Once again, the regression-discontinuity results from Table 8 show that party control is of importance for economic policies.

Specification tests

In this subsection, I make further tests of whether party control is as good as randomly assigned. As previously discussed, I have already done one such specification test in Tables 7 and 8, i.e., including pretreatment covariates and this did not significantly affect any estimates of party control. Another way of testing whether party control is as good as randomly assigned is to test run a regression of party control on the pretreatment characteristics covariates and the control function and test whether the coefficients are significantly different from zero. Table 9 displays the results from this regression. None of the coefficients is individually statistically significant different from zero. Moreover,

the coefficients on the covariates are also not jointly significantly different from zero as can be seen from the F -statistic of 1.23 with a corresponding p -value of 0.30. Yet another specification test of whether party control is as good as randomly assigned is shown in Table 10. Here, the idea is that the pretreatment characteristics should not have any effect at the discontinuity since the pretreatment characteristics should, on average, be similar for left- and right-wing governments that are close to the threshold. As can be seen from the table, none of the estimates for income, population size, proportion young and proportion old is significantly different from zero. To sum up, I cannot reject the hypothesis that party control is randomized, which provides strong support for a causal interpretation of the measured party effect.

5. Discussion and conclusions

This paper shows strong evidence that party control has a causal effect on economic outcomes since it uses a regression-discontinuity approach where party control should be as good as randomly assigned in a neighborhood of 50 percent of the proportion of votes. The plausibility of randomization of party control is also supported by various specification tests.

The size of the party effect is quite large.²⁵ For example, left-wing governments have about 2-3 percent higher expenditures as a share of income and about 7 percent lower unemployment rates than right-wing governments.²⁶ Left-wing governments also employ about 4 percent more workers. If we could, for example, generalize the effect of parties on the unemployment rate to the U.S. context, which have had an average unemployment rate of about 5.6 percent after World War II, then this would imply a difference between Republican and Democratic administrations of 0.4 percentage points. This is about half of the actual difference of 0.9 percentage points between Republican and Democratic presidential administrations.²⁷

Finding such a large party effect also has some important implications for our understanding of party competition more generally. First, the notion of strict convergence as implied by the median voter model,²⁸ namely that all parties prefer to have similar policy outcomes in equilibrium, is strongly rejected.²⁹ A natural question that follows the rejection of strict convergence is whether parties implement their most preferred policy as

²⁵ This size of the party effect should be assessed from the viewpoint that many of the spending programs are mandatory or heavily regulated. For example, Murray (1985) estimates that about 20 percent of total expenditures are altogether free from central government regulations and grant formulas. Thus, the party effects discovered in this paper could even be much larger if the local governments were totally unconstrained.

²⁶ Although no other work has convincingly identified a causal party effect on policies, the estimated party effects in this paper are still of an order of magnitude larger than those found previously. For example, Besley and Case (1995) find that states with Democratic governors increase spending with \$13 per capita or about 0.1 percent of the average state income (\$8589 per capita), as compared to Republican governors. This number should be compared with an estimated party effect in this paper of about 2-3 percent.

²⁷ See, for example, Bartels (2004) for recent evidence of partisan differences in the U.S.

²⁸ The median voter model has recently been criticized by Besley and Case (2003).

²⁹ It could be argued 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, as discussed by Osborne

in Alesina (1988) where he criticized the political science literature with outcome-motivated candidates. He shows that once the commitment assumption is dropped, the equilibrium outcome will be one of full divergence in a one-shot game. Although I cannot test whether there is only partial convergence or complete divergence as in Lee et al. (2004), there are reasons to believe that the average party effect, i.e., the effect of party control on a randomly selected unit from the population, can be even larger. This is related to the regression-discontinuity design itself since it only identifies a causal effect for those units close to 50 percent of the vote share (see, e.g., Hahn et al. 2001). It would therefore be expected that the governments that are close to 50 percent have an incentive to converge to the middle since the intensity of party competition is likely to be particularly high in this case. Thus, the average party effect would probably be larger than the effect for those governments close to the treatment threshold.

A second implication for finding a substantial party effect at the local government level is that it rejects a common perception among some scholars of local public finance, namely that Tiebout sorting nullifies the role of parties at the local level (Henderson 1985).³⁰ The intuition is that if sorting were perfect and everyone were located in a jurisdiction providing just the desired public goods, there would seem to be little need for partisanship. Naturally, stylized outcome does not reflect reality in most places, so the key issue is whether competition among a large number of jurisdictions restricts policymakers' ability to pursue policies which do not reflect the residents' desire? Epple and Zelenitz (1981) show theoretically that increasing the number of jurisdictions (i.e., the degree of competition among jurisdictions) limits but cannot completely eliminate governmental monopoly power. Although this paper presents evidence suggesting that "Tiebout needs politics", there is still an issue whether Tiebout-like forces provide a mechanism to constrain partisanship. To empirically test this argument, it would have to be determined how the party effect varies with the degree of competition (number of jurisdictions). In that case, an exogenous source of variation in the number of political

(1985), convergence is a more general feature of political competition and not just exclusive to a two-candidate model.

³⁰ Tiebout sorting remains an active current research topic. See Rhode and Strumpf (2003) and the references cited therein.

jurisdictions would be needed. In future work, I intend to test this hypothesis using variation from local government boundary reforms in Sweden. From 1862 to 1952, there existed about 2,500 local governments. In 1952, Sweden enacted the first of two municipal boundary reforms. Overall, the total number of local governments declined from 2,500 to 1,000. There was a second boundary reform during the period 1969-73. Upon completion, the number of local governments declined to about 300. The idea is to compare the party effect, i.e., the difference between left- and right-wing government policies, across these three periods. If the operation of Tiebout-like forces at the local level is important, then the party effect should be smallest when the number of jurisdictions is 2,500 and largest when the number of jurisdictions is 300.

Finally, the results of this paper also shed some light on whether the number of parties in government is related to the size of government. There is a voluminous literature which argues that bargaining inefficiencies inside coalition governments lead to high spending and other distortions.³¹ If this is the case, a local government under a right-wing government would be expected to spend more than under a left-wing government since the right-wing government includes 3-5 parties while a left-wing government only includes 1-2 parties. In other words, if there are bargaining inefficiencies inside coalition governments, this will make it even harder to find a significant party effect in the Swedish context. However, it is in fact a small coalition government (left-wing government) that spends significantly more than a large coalition government (right-wing government). Moreover, if there are bargaining inefficiencies inside coalition governments, this will be expected to not only show up in spending but also in debt as suggested by this literature (see, e.g., Alesina and Perotti 1995). For this reason, I have tested whether party control is of importance for the level of debt, but I cannot find any such an effect.³²

To conclude, this paper provides strong evidence that party control has a causal effect on economic outcomes. Specifically, the evidence is consistent with the following interpretation: when a left-wing government replaces a right-wing government, it employs

³¹ See, for example, Roubini and Sachs (1989), Alesina and Perotti (1995), Kontopoulos and Perotti (2002), Persson, Roland and Tabellini (2003) and Bawn and Rosenbluth (2003).

³² The estimated party effect for log (debt per capita) is 0.0055 (s.e.=0.036), when controlling for a fourth-order polynomial in the assignment variable (vote share).

more government workers and therefore the unemployment rate decreases. This increase in employment affects the budget since both spending and taxes are increased to the same extent, i.e., there is no effect of the budget deficit or debt.³³

³³ About 70 percent of total spending go to wage bills.

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Table 1. Party control in Swedish local governments from 1974 to 1994

Election period	Number of left-wing governments	Number of right-wing governments	Number of 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

Note. In Sweden there was an election every third year until 1994, when four-year terms were 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

Variables	Mean	Standard deviation	Minimum	Maximum
<i>Economic outcomes</i>				
Total expenditures per capita	28,257	5,804	14,391	70,031
Total expenditures as a share of income (%)	39.40	7.91	16.39	92.86
Current spending per capita	26,790	6,748	11,889	70,924
Current spending as a share of income (%)	37.07	7.97	14.93	88.47
Total revenues per capita	28,207	5,699	15,515	71,699
Total revenues as a share of income (%)	39.39	8.07	15.90	96.29
Proportional income tax rate (%)	16.46	2.12	9.7	31.75
Unemployment rate (%)	3.18	2.15	0.19	12.23
Local government employees per capita (%)	5.81	1.74	2.16	14.00
<i>Assignment variable</i>				
Left vote share	47.23	11.66	13.81	76.69
<i>Control variables</i>				
Income per capita	72,624	12,357	15,945	162,962
Population size	29,774	52,551	2,865	692,954
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

Note. Total expenditures per capita, current expenditures per capita, total revenues per capita and income per capita are expressed in 1991 prices.

Table 4. Descriptive statistics for left- and right-wing governments

Variables	Left-wing governments Means (1)	Right-wing governments Means (2)	Difference in means(1)-(2) (3)
<i>Economic outcomes</i>			
Total expenditures per capita	29562	26787	2775 ^{***}
Total expenditures as a share of income (%)	41.11	37.58	3.52 ^{***}
Current spending per capita	28162	25083	3086 ^{***}
Current spending as a share of income (%)	38.92	34.89	4.00 ^{***}
Total revenues per capita	29493	26807	2686 ^{***}
Total revenues as a share of income (%)	41.06	37.67	3.39 ^{***}
Proportional income tax rate (%)	16.71	16.14	0.57 ^{***}
Unemployment rate (%)	3.52	2.93	0.59 ^{***}
Local government employees per capita (%)	5.89	5.65	0.24 ^{***}
<i>Assignment variable</i>			
Left vote share	58.11	37.06	21.05 ^{***}
<i>Control variables</i>			
Income	72,454	72,657	-203
Population size	29,925	26,327	3,598
Proportion of young, 0-15 (%)	20.55	21.95	-1.40 ^{***}
Proportion of old, 65+ (%)	17.69	17.38	0.31

Note. Total expenditures per capita, current expenditures per capita, total revenues per capita and income per capita are expressed in 1991 prices.* Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 5. Descriptive statistics for left- and right-wing governments within 4 percentage points from the 50% threshold

Variables	Left-wing governments	Right-wing governments	Difference in means(1)-(2)
	Means (1)	Means (2)	
Total expenditures per capita	29,237	29,059	178
Total expenditures as a share of income (%)	40.41	39.75	0.66
Current spending per capita	27908	27250	630
Current spending as a share of income (%)	38.27	36.99	1.28
Total revenues per capita	29,152	29,009	143
Total revenues as a share of income (%)	40.38	39.74	0.64
Proportional income tax rate (%)	16.68	16.56	0.12
Unemployment rate (%)	2.94	3.00	-0.06
Local government employee per capita (%)	5.93	5.91	0.02
Income	72,852	73,962	-1,110
Population size	41,120	41,671	-551
Proportion of young, 0-15 (%)	21.11	21.56	-0.45
Proportion of old, 65+ (%)	17.09	17.08	0.01

Note. Total expenditures per capita, current expenditures per capita, total revenues per capita and income per capita are expressed in 1991 prices.

Table 6. Number of governments that are close to the 50 percent threshold

Election period	Number of left-wing governments		Number of right-wing governments	
	Closer than 2 percentage points	Closer than 4 percentage points	Closer than 2 percentage points	Closer than 4 percentage points
1974-1976	21	39	19	36
1977-1979	21	40	26	42
1980-1982	14	36	29	43
1983-1985	16	37	10	20
1986-1988	19	42	9	29
1989-1991	28	43	23	34
1992-1994	12	26	21	44
Sum 1974-1994	131	263	137	248

Table 7. Party effect: Fiscal policies

	1	2	3	4	5	6	7
Log (Total spending per capita)	0.024** (0.009)	0.027*** (0.009)	0.023** (0.010)	0.021** (0.010)	0.024* (0.013)	0.020** (0.009)	0.022** (0.010)
Log (Total spending as a share of income)	0.021** (0.010)	0.025** (0.010)	0.024** (0.010)	0.025** (0.011)	0.034* (0.018)	0.021** (0.009)	0.024*** (0.009)
Log (Current spending per capita)	0.024** (0.010)	0.027*** (0.010)	0.027** (0.011)	0.026** (0.011)	0.019 (0.013)	0.025** (0.010)	0.027** (0.011)
Log (Current spending as a share of income)	0.022* (0.011)	0.025** (0.011)	0.028** (0.012)	0.030*** (0.012)	0.029 (0.018)	0.026*** (0.009)	0.029*** (0.010)
Log (Total revenues per capita)	0.024*** (0.009)	0.027*** (0.009)	0.019** (0.009)	0.017* (0.009)	0.015 (0.013)	0.017* (0.009)	0.014 (0.010)
Log (Total revenues as a share of income)	0.021** (0.010)	0.025** (0.010)	0.020** (0.010)	0.021** (0.010)	0.025 (0.018)	0.018** (0.009)	0.017* (0.009)
Log (Proportional income tax rate)	0.012*** (0.004)	0.013*** (0.004)	0.012*** (0.004)	0.013*** (0.004)	0.011 (0.008)	0.013*** (0.004)	0.014*** (0.004)
Sample	Full	Full	Full	Full	±2	Full	Full
Left vote share polynomial	First	Second	Third	Fourth	None	Fourth	Fourth×time
Controls	No	No	No	No	No	Yes	Yes

Note. Standard errors clustered at the local government's term in office level are within parentheses. Each entry is a separate regression. All regressions also include municipality-specific effects, time effects and an indicator for undefined majority governments. The full sample includes 5913 observations and the ±2 sample includes all observations that are in the range of [48, 52] of the left vote share and there are 828 such observations. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 8. Party effect: Economic policies

	1	2	3	4	5	6	7
Log (Unemployment rate)	-0.017 (0.033)	-0.032 (0.031)	-0.056* (0.032)	-0.056* (0.032)	-0.121 (0.089)	-0.048 (0.031)	-0.070** (0.033)
Log (Government employees per capita)	0.030** (0.012)	0.033*** (0.012)	0.035*** (0.012)	0.036*** (0.012)	0.039*** (0.016)	0.032*** (0.011)	0.036*** (0.012)
Sample	Full	Full	Full	Full	±2	Full	Full
Left vote share polynomial	First	Second	Third	Fourth	None	Fourth	Fourth×time
Controls	No	No	No	No	No	Yes	Yes

Note. Standard errors clustered at the local government's term in office level are within parentheses. Each entry is a separate regression. All regressions also include municipality specific effects, time effects, and an indicator for undefined majority governments. The full sample includes 5913 observations for government employment and 4520 for unemployment. The ±2 sample includes all observations that are in the range of [48, 52] of the left vote share and there are 828 such observations for government employment and 603 for unemployment. * Significant at the 10 percent level, ** Significant at the 5 percent level, *** Significant at the 1 percent level.

Table 9. Specification test of whether party control is as good as randomly assigned

Dependent variable: Party control	
Income	1.55e-06 (1.28e-06)
Population size	2.61e-06 (2.76e-06)
Proportion of young, 0-15	-0.0046 (0.0050)
Proportion of old, 65+	-0.0064 (0.0064)
<i>F</i> -test	1.22
<i>P</i> -value	(0.30)
Number of observations	5,913

Note. Standard errors clustered at the term in office are within parentheses. The regression also includes municipality specific effects, time effects, an indicator for undefined majority governments and a linear control function.

Table 10. Specification test of whether covariates have an effect at the discontinuity

	Income	Population size	Proportion of young, 0-15	Proportion of old, 65+
Party effect	506 (403)	402 (338)	-0.147 (0.158)	-0.112 (0.111)
Number of observations	5,913	5,913	5,913	5,913

Note. Standard errors clustered at the term in office are within parentheses. All regressions also include municipality specific effects, time effects, an indicator for undefined majority governments and a linear control function.