

Testing Political Agency Models*

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Abstract

This paper tests for political agency effects, that is, whether governments that were *ex-post* re-elected performed better than those who were replaced, and whether re-elected governments performed better than newly elected ones. As a testing ground, I use Swedish local governments which offer a number of attractive features. I find strong evidence for political agency effects.

Key words: Political agency, accountability, elections

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1. Introduction

Political agency models study the choices of politicians facing the threat of re-election where the politician is the agent and voters are the principals.¹ In other words, political agency models deal with arguably the most important political institutions in a democracy – appointment of politicians through democratic elections – and to what extent elections can resolve the conflict of interest between the citizens and their elected representatives.

There are basically two strands of work in the political agency tradition.² In the first generation models (e.g., Barro 1973 and Ferejohn 1986), the assumption is that voters view all politicians as identical and they employ a cut-off voting rule, that is, they retain the politician if her performance exceeds a threshold and replace her otherwise. In these models, elections are therefore seen as a *sanction* device that induces elected officials to do what the voters want. In the second generation models (e.g., Banks and Sundaram 1998, Coate and Morris 1995, Fearon 1999, and Rogoff 1990), politicians are explicitly modeled to be of different types (e.g. politicians have different motivation or competence). This work shows that there exists equilibria where voters use a cut-off voting rule, that is, they vote for the incumbent politician if and only if they believe she is a better candidate than the challenger, and politicians separate according to type, with better politicians taking superior actions. In this case, the role of elections is not only to create incentives but also to *select* or to sort in politicians with desirable characteristics.

The contribution of this paper is to test predictions from political agency models regarding politicians' behavior both before and after elections which have, as far as I know, not been tested before. As noted above, both the first and second generation models predict that only those politicians that perform better than some threshold value will be re-elected. This suggests that one can test, *ex-post*, for whether those politicians who were re-elected performed better (in the eyes of the voters) than those who were replaced. Thus, there should be a *pre-election agency effect*. If politicians also differ according to types then an additional implication from the political agency literature is

¹ See Besley (2006) for an excellent overview of the political agency literature.

² There are also political agency models based on the career concern model of Holmstrom (1999) such as Person and Tabellini (2000) develop). These models differ from the above agency models since politicians and voters are symmetrically informed.

that re-elected politicians should perform better than newly elected ones since they on average will be of better types. Thus, a second empirical test of political agency models is to test for a *post-election agency effect* among incumbent politicians by comparing the behavior of re-elected politicians with the behavior of newly elected ones. In addition, if politicians separate according to type, agency models also prescribe that the results from the first and second tests must be consistent with each other. Consequently, a re-elected incumbent's performance must be superior to both a replaced incumbent (i.e., the pre-election agency effect) as well as newly elected one (i.e., the post-election agency effect). Moreover, the pre-election agency effect should be larger than the post-election agency effects since those politicians who perform badly will not be re-elected while only some of the newly elected politicians will perform badly.

I will use Swedish local governments as a testing ground for these predictions since they offer some attractive features for testing political agency models. First, there are relatively many government turnovers during a rather short period of time, i.e., 277 turnovers over a 16-year period (1979-1994), which makes it possible to implement the political agency tests discussed in the next section since they will be based on only the variation "within" political jurisdictions, i.e., only those jurisdictions with at least one government turnover will identify the political agency effect. Second, the election date is fixed which avoids any endogeneity issues associated with flexible election schedules.³ Third, a large numbers of different fiscal performance measures are available (e.g., income taxes, specific spending programs, and administrative political costs). In addition, we can measure what voters want in terms of public service due to surveys of the Swedish electorate that takes place before every election, i.e., Swedish Election Studies.⁴ Thus, it is possible to implement much more powerful tests of the political agency models than by only looking at broad measures of performance, such as aggregate spending and taxes, since it is now possibly to operationalize what is meant by saying that an incumbent politician performed well *in the eyes of the voters*.⁵ According to the

³ Shi and Svensson (2006) discuss the problem caused by flexible elections.

⁴ The Swedish election studies are based on surveys of the Swedish electorate, and they have been conducted around each election since 1956. All studies are based on large samples and they have comparatively low levels of non-response. The Swedish election studies are, together with the slightly older American election studies, the most comprehensive material for systematic studies of voter behavior

⁵ The political agency models assume that voters do have a common interest in achieving some outcome.

surveys, a large majority of voters wanted much more local government-provided childcare services which make childcare an ideal spending program to evaluate from a political agency point of view. Swedish childcare policy is also interesting in its own right since it is an example of a childcare system where the government heavily subsidizes day care activities for young children with working mothers (e.g., local governments spend about 5-6 percent of GDP on day care). The Swedish child care system has therefore received some attention among economists (for example, see Becker (2005), Bergstrom and Blomquist (1996), Gustafsson and Stafford (1992), and Rosen (1996, 1997)). Another attractive feature of using Swedish local governments as a testing ground for political agency is that there is a relatively clear connection between responsibilities and outcomes (e.g., fiscal performance) since Swedish local governments have constitutional right of self-government, no restriction on borrowing, and no balanced budget rules.⁶ Moreover, on average only 20 percent of their income comes from grants, whereas the rest mostly come from a proportional income tax, which each municipality can set freely. This substantial degree of fiscal freedom together with the fact that there are relatively little disagreement among Swedish voters about local public service makes it more likely that voters are able to hold politicians to account which, in turn, will make politicians more responsive to voters' wishes. In addition, many of the fiscal policies are clearly visible to the voters, such as the extent of childcare services and the proportional income tax rate. Thus, there is a strong reason to expect that there is *real* (de facto) electoral accountability -as opposed to only formal (de jure) - in Swedish local governments.⁷ A potential drawback for using Swedish local governments is that voters cast their vote for political parties and not for individual candidates, i.e., Sweden has a party-list proportional representation system. This will tend to weaken the mapping from outcomes to re-election decisions on individual candidates. Nevertheless, Swedish voters are basically faced with only two choices: vote for a left or for a right-wing government, since the Swedish political map is characterized by clear dividing between left and right

⁶ As from 2000, however, there is a balanced budget rule in place.

⁷ See, Besley 2006 for a discussion of real vs. formal accountability. Meyerson (2006) also provides a theoretical argument why sub-national policymakers should be accountable to voters based on political agency.

parties.⁸ This two bloc feature makes it fairly easy for voters to untangle responsibilities for local government outcomes.

I find strong evidence for political agency effects. Specifically: (a) there are large pre-election agency effects, (b) the pre-election agency effect is larger closer to elections, (c) the sign of the pre-election agency effect across the various performance measures are consistent with models of political agency (e.g., the pre-election agency effect is negative for taxes and political rents, while it is positive for valuable public spending), (d) there are significant post-election agency effects, (e) the post election-agency effect has the same sign as the pre-election agency effect, and (f) the pre-election agency effect is usually larger than the post-election agency effect.

This paper is related to a number of empirical literatures that also uses a political agency framework. They focus, however, on different predictions regarding politicians' behavior than in this paper. There are some studies that uses term limits to test political agency model (e.g., Besley and Case 1995a, and List and Sturm 2006), i.e., whether politicians act differently when they can stand for re-election versus when they cannot (i.e., term-limited). This paper is also related to the political business cycle literature, which tests whether politicians behave differently during election years versus non-election years (e.g., Akhmedov and Zhuravskaya 2005, Brender and Drazen 2005, and Shi and Svensson 2006).⁹ There is also related literature that looks directly on voting behavior (e.g., Peltzman 1992, Besley and Case 1995b, and Wolfers 2002) in order to test models of political agency. There are, however, some good reasons for testing political agency models by looking at the behavior of politicians conditional on voters responses, as in this paper, rather than the other way around, i.e., regressing voting outcomes on policy. First, one avoids the endogeneity of policy, i.e., policymakers can influence their own probability of re-election through their own behavior. Second, if voters use a cut-off strategy then the probability of reelection will be a discontinuous function of performance (i.e., a step-function) which gives rise to a functional form misspecification problem when one tries to regress voting outcomes on policy. Third, one avoids measurement

⁸ For an overview of the Swedish political system see Petersson (1994).

⁹ See Alesina et al. (1997) for a textbook treatment of the political business cycle literature.

error bias if one cannot correctly measure the politicians' performance as perceived by the voters (i.e., in the eyes of the voters).

The outline of the paper is as follows. The next section describes the empirical tests of the political agency models. Section 3 discusses the data while section 4 presents the empirical results. Section 5 summarizes and gives some concluding remarks.

2. Empirical tests of political agency effects

In this section, we discuss two empirical tests of political agency models, namely a test for *pre-election agency effects* and a test for *post-election agency effects*.

Pre-election agency effect

Common to most political agency models is that voters employ a cut-off voting rule. The idea is that voters observe a measure of the incumbent policymaker's performance before an election and base their voting decisions on whether the incumbent's performance is above or below a certain cut-off point. This voting rule gives the incumbent politician an incentive to work hard or to signal its type before elections. This suggests that one can test, *ex-post*, whether those politicians that were re-elected performed better (in the eyes of the voters) than those who were replaced.

To formalize such a test let Y_t denote some measure of the incumbent's performance in some period t before an election. Then if voters are using a cut-off voting rule then they should re-elect the incumbent if and only if Y_t is larger than some threshold c . Thus, if we could observe the threshold c then we could test for whether politicians that had a higher performance than c was actually re-elected. Unfortunately, the threshold is not observable (at least to the econometrician) but it is still possible to carry out this test if we have *variation across time* in the election outcome from a single jurisdiction. Then we can run the following regression

$$(1) \quad Y_t = \pi_0 + \pi_1 D_t + u_t$$

where D is an indicator function taking the value 1 if the incumbent was re-elected in the upcoming election, zero if the incumbent was replaced, and u includes all other determinates of Y . The parameter of interest is the *pre-election agency effect* π_1 which

measure the average difference between re-elected and replaced incumbents, i.e., $\pi_l = E[Y|D=1] - E[Y|D=0]$. The prediction from the political agency models is that $\pi_l > 0$ (if performance is positively related to performance measure Y). It is clear from equation (1) that one needs turnover among incumbents in order to identify π_l . The identification, however, also depends crucially on that the cut-off threshold c that voters are using is constant across time otherwise $\pi_l > 0$ is not necessarily true. Fortunately, political agency models assume that the voting rule is stationary, i.e., time-independent.¹⁰ The stationary assumption, however, makes it difficult to use a single time series to test this hypothesis since there are only a few elections (i.e., few government turnovers) that take place during any given period of time in a single jurisdiction for which the assumption is likely to be satisfied. A more powerful test, where the stationary assumption is less likely to be violated, is to use panel data, that is, large number of jurisdiction over a rather short period of time. We can then run the following generalized panel data regression

$$(2) \quad Y_{it} = \mu_i + \gamma_t + \beta D_{it} + v_{it}$$

where Y_{it} is a measure of performance of an incumbent policymakers in jurisdiction i in period t , D_{it} is a again an indicator variable taking the value 1 if the incumbent was re-elected in the following election, μ_i is a fixed political unit effect, v_{it} includes all other determinates of Y , and $\beta = E[Y | c_i, \gamma_t, D=1] - E[Y | c_i, \gamma_t, D=0]$. In equation (2) it is quite important to include fixed unit effects since this means that the cut-off value is allowed to differ from one jurisdiction to the next. Put differently, if we did not include these fixed effects that would mean that voters in all jurisdictions are using the same cut-off threshold, $c_j = c$, which seems to be an unrealistically restrictive assumption. In addition, specification (2) allows for that the threshold may change over time for all political units since fixed-time effects γ_t are included. In other words, fixed time effects allow there to be a common but *non-parametrically* specified time trend in the cut off value.¹¹

As was discussed in the introduction, there are a number of reasons why one should test political agency models by looking at the incumbent politician's behavior, i.e.,

¹⁰ See, for example, Banks and Sundarm (1998).

¹¹ An alternative specification is to include a linear trend for each jurisdiction. However, this would require a rather long time series in order to be implemented in practice.

equation (2), rather than regressing voting outcomes on policy (i.e., $Prob[D=1|Y]$), namely one avoids policy endogeneity, functional form misspecifications, and bias due to miss-measurement of performance.

Post-election agency effect

If incumbents differ according to types, the political agency models predicts that better types are selected, which suggest that one should compare the performance of re-elected and newly elected incumbents after an election. We can run the following panel data regression

$$(3) \quad Y_{it} = \alpha_i + \theta_t + \gamma T_{it} + u_{it}$$

Where is Y_{it} a measure of performance for an incumbent in jurisdiction i in the period after an election, T_{it} is equal to one if the incumbent was re-elected in last election and zero if the incumbent is newly elected. The parameter of interest is the *post-election agency effect* γ which measures the mean difference between reelected and newly elected incumbents. We would expect that re-elected incumbents perform better than newly elected ones, i.e., $\gamma > 0$.

The political agency models also predict that the pre-election and post-election agency effects should have the same sign. Thus, a third implication from the political agency framework is that $\text{sign}[\beta] = \text{sign}[\gamma]$. Moreover, one would also expect that the magnitude of the pre-election agency effect to be at larger than the post-election agency effect, i.e., $\beta \geq \gamma$. This has to do with that only those politicians who perform badly will not be re-elected, and only some of the newly elected politicians will perform badly. It is interesting to note that the comparison of the magnitude of the pre and post-election agency effects could not be made if one instead regressed the voting outcome on the performance measure.

3. Swedish Local Governments as a testing ground

I will use Swedish local governments as a testing ground for political agency models since they offer a number of attractive features as discussed in the introduction but before turning to the description of the data it is perhaps helpful to digress briefly on the workings of Swedish local governments

Sweden is currently divided into 290 local governments (or municipalities), which cover the entire country.¹² 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 some of the most important welfare services such as provision of day care, education, care of the elderly and social welfare services. To quantify their economic importance, it is sufficient to 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.

Below I will discuss the data in three sections, that is, (i) the various fiscal performance measures, (ii) data on government turnovers, and (iii) control variables

Fiscal performance measures

A number of different measures of performance relating to local public finance decisions will be used. I will use the level of taxation and more disaggregated measures of fiscal performance in order to get clear-cut predictions regarding incumbent's performance.¹³ While low taxes is a signal of good performance in most agency models low, total spending, in contrast, may or may not be associated with good performance since total spending can potentially consists of two kinds of public spending, that is, public spending that voters value and public spending devoted to private ends, i.e., political rents. In other words, high public spending may be associated with high performance if it comes from valuable public goods production rather than rent-seeking activities (see, for example, Besley 2006 and Rogoff 1990 for such models). Thus, one could use data from specific spending program since agency models predict that an incumbent who provides more

¹² There are also 21 counties which covers the entire country.

¹³ The precise predictions from agency models depend on the assumption of the policy process, the motivation of incumbents, and the information set of voters as discussed by Besley (2006) and Persson and Tabellini (2002).

valuable public spending stands a better chance of being re-elected. The problem is, however, to find what type of public spending voters like. Fortunately, we have such information from repeated surveys, i.e., Swedish Election Studies.¹⁴ These studies are based on surveys of the Swedish electorate, and they have been conducted around each election to the Swedish parliament since 1956. All studies are based on large samples and they have comparatively low levels of non-response. Due to the Swedish Election Studies, we can now actually measure voter preferences over specific fiscal outcomes.

One of the more important concerns of Swedish voters during the 1970s and 1980s was public childcare services. Figure 1 displays voter preferences in favor of establishing more publicly operated daycare centers for all the elections from 1976 to 1991. Figure 1 shows that a majority of voters are in favor of more public childcare services, which obviously raises the question why a majority of voters would want more childcare services. A theoretical argument is provided by Bergstrom and Blomquist (1996). They show that in an economy where there are high taxes on wage income, selfish taxpayers with no children in the day care system may favor substantial public subsidies to day care because such subsidies induce mothers to join the labor force and hence pay income tax.

Today, the overwhelming majority of children in Sweden receive childcare in one form or another, i.e., in 2003, about 75 % of all children aged 1-5 attended daycare centers, or 352,000 children in all, while 7% of all children aged 1-5 were attending family daycare facilities.¹⁵ Gross costs for childcare in Sweden amounted in 2003 to SEK

¹⁴ The Swedish election studies are carried out by Statistics Sweden in cooperation with the Department of Political Science at Göteborg.

¹⁵ The daycare centers cares for children while their parents are away working or studying. Centers are open all year round and daily opening times are varied to fit in with parents' working hours. Children are generally divided into groups of between 15 and 20. As a rule, three employees - preschool teachers and daycare attendants - are allocated to each group. The average daycare center comprises three such groups. Virtually all (98%) of staff in Swedish day care centers are trained to work with children. Each center must have a director with a university teaching or pedagogue qualification. Educational pedagogues (pre-school teachers) make up 60% of the personnel in the pre-schools. Like leisure-time pedagogues, they require a three-year tertiary degree from a higher level college or university. The pedagogues are assisted by child minders (38% of personnel) who are given a senior secondary, three-year vocational education. The family daycare home involves local government childminders providing care in their own homes while the parents are working or studying. The children are registered and opening hours are varied to fit in with the parents' schedules. Family daycare complements center care by providing in particular for children who for one reason or another need to be in smaller groups or who live far from the nearest day care center facility. This alternative is more common in rural areas and in small towns than in metropolitan areas. The number of children in family daycare has steadily declined since the late 1980s. Family day-care providers are not

46 billion, which corresponds to 13 % of the local governments' total costs, or almost 2 % of the Swedish GDP. The public childcare system in Sweden has developed rapidly in size. Figure 2 displays the development of center care from 1974 and 1994 and it shows that the number of children in day care centers has increased more than fivefold from 64,000 to 340,000, while the center care staff has tripled from 33,000 to about 100,000.¹⁶ Despite this rapid progress local authorities were unable to expand facilities to such an extent that the waiting list for childcare places disappeared (about 70,000 children was still on a waiting list in 1993). As a result, tougher legislation was introduced in 1995 (the law, SFS 1994:11, was decided in December 1993 in the Swedish Parliament) under which local authorities became duty bound to provide childcare without undue delay. Since the law implies that the local governments can no longer decide on the amount of supply of childcare service we only look at data up to 1993 for the child care program.

The upper panel of Table 1 presents summary statistics of the various performance measures. Below I will describe each of the performance measure in more detail.

The level of taxation will be measured as the level of the proportional income tax rate. This is an ideal measure of taxes since Swedish local governments only are allowed to raise their revenues through one single tax instrument.¹⁷ In comparison to the total tax receipt per capita used by other studies, the tax rate has the advantage of more closely reflecting of elected governments' intentions. The tax rate has increased from 16.0 to 19.2 percent during the sample period 1979 to 1994. The range of the income tax is also substantial. For example, in 1994, the range was 8.7 percentage points (i.e., a minimum of 13.2 and a maximum of 21.9 percent).¹⁸

required to have a qualification, but 72% have either a child-minder certificate or have received 50-100 hours of mandatory training from their local government employers.

¹⁶ Despite rapid progress in the 1970s and 1980s local authorities were unable to expand facilities to such an extent that the waiting list for places disappeared. As a result, tougher legislation was introduced in 1995 under which local authorities became duty bound to provide childcare without undue delay

¹⁷ Local governments have the constitutional right to set their own proportional income tax. On average, more than 60 percent of their revenues come from the income tax while less than 20 % comes from grants. Various fees make up the rest

¹⁸ There are 3 municipalities (Gotland, Göteborg, and Stockholm) that handle tasks that normally is handled by the counties. These municipalities have therefore much larger tax rates than the others. For example, in 1994 there tax rate was between 29-32 percent.

Turning to the Swedish child care program, three different performance measures will be used to capture the extent of public provision, namely spending on childcare in real per capita terms, percentage of children aged 0 to 6 in center care, and center care employees per capita. As was discussed above, there has been a dramatic increase public child care service. From 1979 to 1993, real spending on childcare increased from SEK 1,700 to 3,200 per capita, the coverage in day care centers increased from 12 to 34 percent, and center care employees per capita increased from 0.5 to 1 percent. The cross-sectional variation is also large. For example, in 1993 the percentage of children aged 0 to 6 in center care ranged from 6 to 84 percent.

The last performance measure will try to capture the amount of rent-extraction, that is, the level of public spending devoted to private ends. I will use real administrative political costs per capita as a rent-seeking measure since this measure the costs of running the local government. This seems to be a rather good measure for capturing rent seeking behavior since it includes wages paid to local government politicians and local public financial support to political parties. Local governments were given the legal right to distribute resources to the parties represented in the local council in 1970. Local party support is an important source of revenues for the Swedish political parties since it constitutes roughly half of the parties' total revenues.

Election data

Swedish local governments have a *fixed* and *synchronized* election schedule, that is, elections are always held on the third Sunday of September every third year in all local governments.¹⁹ The benefit of an exogenously fixed election schedule is that it avoids any endogeneity problems associated with that incumbents may strategically choose when to call an election which otherwise may be a major obstacle in establishing a causal relationship from elections to fiscal performance.²⁰ The synchronized electoral cycle facilitates the test of political agency effects since incumbent politicians face the same electoral incentives. Another attractive feature with Swedish governments is that the

¹⁹ As from 1994, elections are held every fourth year.

²⁰ See Shi and Svensson (2006) for a discussion of the problem with endogenous election timing in cross-country data.

fiscal year is the same as the calendar year, which avoids bias due to that fiscal policy variables are not synchronized with the election cycle.²¹

Election data for six consecutive local government elections will be used, namely the elections years 1979, 1982, 1985, 1988, 1991 and 1994. Thus, the sample period is 1979 to 1994 which is a fairly short-period making it more plausible that the cut-off threshold that voters are using is roughly constant during this time. Clearly, we face a trade-off when deciding on the number of elections to include in the empirical analysis; a very short sample period (e.g., a minimum of 2 consecutive elections) will provide little variation in election outcomes while a longer period provides more variation in the election outcomes but will also make it more likely that the cut-off threshold to change. To take into account that the cut-off value might change somewhat during the sample period, I also allow for there to be a common time trend (non-parametrically specified) in the cut off value since time effects are included in specifications 2 and 3 as discussed in section 2. Below, I will provide detailed information on government turnovers since variation in political turnovers is a key condition for testing the agency models as discussed previously.

There are total of 1,716 local governments elections periods (286 local governments \times 6 elections) altogether but I restrict the analysis to those period where there has been a left or a right-wing incumbent government in order to unambiguously define when an incumbent government has been re-elected or not.²² The classification of government turnovers is compiled from the distribution of seats in local councils, which, due to the PR system, is basically equivalent to vote shares. Incumbent governments are classified as left-wing, right-wing or undefined. A government change is defined as a change of power between left-wing, right-wing or undefined governments. This classification of regime changes is quite reasonable in the Swedish context since two

²¹ The work on cross-country data, for example, faces this problem when having to define election year depending on whether the election is held early or late during the election year.

²² To define the left-wing majority 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 and the Liberal Party, the Christian Democratic Party, and the New Democratic Party. The Christian Democratic Part is however only included in the right-wing majority from year 1988 and the New Democratic Party only from year 1991. The classification of the blocs 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.

main opposing blocs characterize the political map: the left- and right-wing blocs. Moreover, voter approval of incumbent governments is the focus in testing the political agency models, so it seems natural to define a change of power when the incumbent party bloc has lost its majority of votes. Consequently, we drop 277 election periods (i.e., 16 % of all election periods) since these are classified as having an undefined incumbent government.

Table 2 shows the number of re-elected and replaced governments during each election, that is, there was 26 turnovers in 1979, 35 turnovers in 1982, 24 turnovers in 1985, 30 turnovers in 1988, 52 turnovers in 1991, and 110 turnovers in 1994. Table 3 displays the number of government turnovers across local governments. This table reveals that there are 79 local governments had one turnover, 60 local governments had 2 turnovers, 14 local governments had 3 turnovers, and 9 local governments had 9 turnovers. Table 2 also shows that 124 local governments (i.e., 43 % of all local governments) had zero government turnovers during the sample period. Since we include fixed municipality effects in the regressions (see, equations 2 and 3), these local governments will not identify the parameters of interest. Consequently, I could basically drop 124 local governments (i.e., 744 election periods) from the empirical analysis without affecting the results but I choose to include them for efficiency reasons. In the results section, I will in fact show that the results are unaltered when I exclude local governments with zero political turnovers from the analysis.

Control variables

There might also be other determinants of local governments' fiscal outcomes. The lower panel of Table 1 presents summary characteristics of the control variables: proportion of people of age 0 to 15, proportion of people older than 65, population size, income per capita, unemployment rate, party control, and left-wing vote shares. These set of controls are often used in the local public finance literature (e.g. Besley and Case, 1995a, 1995b, and 2003). For example, including controls for proportions of young and elderly are often rationalized by their link to the cost and benefits of local government spending, while population size and population is included because they capture the possibility of congestion effects or scale economies in the provision of local government services.

Income is related to the fiscal capacities of municipalities, which may have independent effects on fiscal choices. The unemployment rate might also have an independent effect on fiscal outcomes since it will capture time-varying economic shocks. Finally, I will also include measures of partisanship along the lines of Pettersson-Lidbom (2003, 2006). He uses a regression-discontinuity approach (i.e., an indicator variable for left-wing majority together with polynomials in left-wing vote share) to answer whether party control matters for fiscal policy outcomes in Swedish local governments. He finds strong evidence that party control matters for policy outcomes.

All the data used are publicly available and were obtained from Statistics Sweden (SCB) or its publications.²³

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.

4. Results

In this section, I report results on the test of political agency models discussed in section 2. I begin with presenting the results on pre-election agency effects which is then followed by results on post-election agency effects. As discussed in the previous section, the following five measures of fiscal performance will be used: income tax rate, spending on childcare per capita, percentage of children aged 0 to 6 in center care, center care employees per capita, and administrative political costs per capita. I report Huber-White standard errors clustered at the local government level following the suggestions of Bertrand et al. (2004) and Kézdi (2002).

Testing for a pre-election agency effect

Tables 4-6 show the result on pre-election agency effects (e.g., equation 2), namely whether re-elected incumbents had different performance than replaced ones *ex-post*. In Tables 4 and 5, the data is restricted to only election years while Table 6 includes all pre-election years. The reason for looking at the data in this way is that we expect the pre-election agency effect to be larger the closer one is to the election date since the incentives for a politician to appear competent and to perform well are likely to be stronger just ahead of elections.

The first column in each of the Tables 3 to 5 shows the results for the income tax rate, the second displays the results for childcare spending per capita, the third column show the results for logarithm of the percentage of children in day care centers, the fourth shows the results for the logarithm of center care employees per capita, and fifth displays the results for administrative political costs per capita.

We start by looking at a fixed effects (local government effects + time effects) specification without any control variables in Table 4.²⁴ The empirical results are striking: those governments that were re-elected had lower income tax rates (-0.12 percentage points), higher spending on childcare (SEK 230 per capita), more child care coverage (7.4 percent), more child care workers (8.3 percent) and less political costs

²⁴ In section 2, I discussed that the identification of the parameters is based on the local governments with at least one turnover. However, there are 42 % (124 out of 286) of the local governments have no turnovers. I include this for efficiency reason. In the appendix, Table 1A shows that the estimates of the pre-election agency effects do not change when I exclude them.

(SEK -100 per capita) as compared to those governments that were replaced. All of these pre-election agency effects are also statistically different from zero except for political costs. When we include controls in Table 5, the estimates of the political agency effects hardly change at all. Thus, the fact that the estimates change very little when we add controls bolster our faith that these effects are real (i.e., causal) political agency effects. The pre-election agency effects also seems to economically large. For example, voters get 8 percent more of child care services (both center care coverage and number of employees) during election years in localities where the government has been re-elected. The other effects are also non-trivial: the effect on taxes is about 1 percent of the average tax rate (i.e., $0.17/16.9$), and the effect on political cost is about 5 percent of average administrative cost (i.e., $100/2038$). The pattern of the pre-election agency effects across the various performance measures are also what one would expect. The effect on the tax rate is negative (column 1), the effect on child care service (columns 2-4) is positive, the effect on political rents is negative (column 5).

Table 6 shows the results on the pre-election agency effects using all pre-election years instead of only the election years. Fixed effects and control variables are also included in all specifications. The results are qualitatively similar to ones in Table 5. However, all the pre-electoral agency effects are smaller than their counterparts in Table 5. These smaller effects are in line with one would expect since the incentives for a politician and to perform well are likely to be stronger just ahead of elections.

Testing for a post-election agency effect

Tables 7-9 display the result on post-election agency effects (e.g., equation 3), namely whether re-elected incumbents had different performance than newly elected ones. The analysis is similar to one in the previous subsection, i.e., the same performance measures are being used, the data is divided into only first post-election years (Tables 7 and 8) and all post-election years (Tables 9). Again, fixed time and municipality effects are included in all the tables while controls are included in Tables 8 and 9.

Starting with data only from first post-election years, Table 7 reveals that there is statistically significant a post-election agency effect in many of the performance measures such as childcare services (column 2 and 4). When we add controls in Table 8

we find significant effects for taxes (column 1) and childcare services (column 2). We also get similar results when we include data from all post-election years as can be seen from Table 9.

It is now also interesting to compare the sign and magnitude of the pre- and post-election agency effects as discussed in section 2. We can see that the sign of the pre- and post election effects are the same for all agency effects that are statistically different from zero, i.e., tax rate (column 1) and child care services (columns 2-4). Moreover, the pre-election agency effects are typically larger than the post-election effects, which is consistent with agency models. For example, the pre-election agency effect for child care spending per capita is about twice as large as the post-election agency effect, i.e., 231 (column 2 in Table 5) vs. 112 (column 2 in Table 8)

To sum up, the evidence show the following: (a) there are large pre-election agency effects (i.e., Tables 4-6), (b) the pre-election agency effects are larger closer to elections (i.e., Table 5 vs. Table 6) (c) the signs of the pre-election agency effects across the various performance measures are consistent with models of political agency (e.g., the pre-election agency effect is negative for taxes and political rents, while it is positive for valuable public spending; see Table 4-6), (d) there are significant post-election agency effects (i.e., Tables 7-9), (e) the post election-agency effect has the same sign as the pre-election agency effect (e.g., Table 5 vs. 9), and (f) the pre-election agency effects are typically larger than the post-election agency effects (i.e., Table 5 vs. Table 9).

5. Conclusion

Understanding how elections function is a key in understanding agency problems in politics. This paper presents empirical evidence that elections actually can induce politicians to give voters what they want which is consistent with predictions from political agency models.

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Table 1. Descriptive statistics

Variables	Mean	Standard deviation	Minimum	Maximum
<i>Fiscal performance measures</i>				
Proportional income tax rate	16.90	2.02	9.7	31.75
Spending on childcare per capita	2,616	1,040	182	7,500
Percentage of children aged 0 to 6 in center care	22.15	11.81	0	83.71
Center care employees per capita	0.00761	0.00373	0.00012	0.02402
Administrative political costs per capita	2,038	1,178	18	13,467
<i>Control variables</i>				
Proportion young (percentage aged 0 to 15)	20.78	2.55	12.65	36.3
Proportion old (percentage aged 65 and above)	18.00	4.23	4.32	27.89
Income per capita	74,470	12,384	35,147	162,962
Population size	27,490	42,952	2,865	692,954
Left majority (=1)	0.505	0.50	0	1
Unemployment rate	3.22	2.17	0.19	12.24
Proportion left votes	47.50	12.64	13.81	76.69

Income per capita, spending on childcare per capita and administrative political costs per capita are expressed in 1991 prices.

Table 2. Number of re-elected and replaced governments across election years

	1979	1982	1985	1988	1991	1994	Sum
Number of re-elected governments	217	206	212	202	167	136	1140
Number of replaced governments	26	35	24	30	52	110	277
Sum	243	241	236	232	219	246	1417

The classification of government turnovers is compiled from the distribution of seats in local councils, which, due to the PR system, is basically equivalent to vote shares. Incumbent governments are classified as left-wing, right-wing or undefined. A government turnover is defined as were a left-wing or a right-wing government has lost its majority of seats.

Table 3. Number of government turnovers 1979-1994

	0	1	2	3	4	5	6
Number of government turnovers	0	1	2	3	4	5	6
Number of local governments	124	79	60	14	9	0	0

The classification of government turnovers is compiled from the distribution of seats in local councils, which, due to the PR system, is basically equivalent to vote shares. Incumbent governments are classified as left-wing, right-wing or undefined. A government turnover is defined as were a left-wing or a right-wing government has lost its majority of seats.

Table 4. Pre-election agency effects: only election years

	Income tax rate	Childcare spending per capita	Children aged 0-6 in center care	Center care employees per capita	Political costs per capita
	(1)	(2)	(3)	(4)	(5)
Pre-election agency effect	-0.120* (0.067)	230*** (75)	0.074*** (0.028)	0.083*** (0.024)	-100 (123)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R ²	0.9511	0.8850	0.8928	0.9135	0.6127
Number of observations	1,415	1,113	1,166	1,165	1,417

Notes- The pre-election agency effect is the difference in outcomes between re-elected and replaced governments. Columns 1 and 5 include data from the following election years: 1979, 1982, 1985, 1988, 1991 and 1994, while columns 2-4 include data from the same election years except for 1994. Standard errors clustered at local government level are within parentheses.

Table 5. Pre-election agency effects: only election years + additional control variables

	Income tax rate	Childcare spending per capita	Children aged 0-6 in center care	Center care employees per capita	Political costs per capita
	(1)	(2)	(3)	(4)	(5)
Pre-election agency effect	-0.172*** (0.064)	231*** (73)	0.078*** (0.027)	0.078*** (0.022)	-97 (123)
Income per capita	-1.0e-06 (9.5e-06)	-0.0004 (0.01)	-0.00001 (4.2e-06)	-4.8e-06 (2.9e-06)	-0.01 (0.01)
Proportion young	0.076 (0.025)	-6 (29)	-0.006 (0.01)	0.03 (0.01)	-15 (43)
Proportion elderly	0.021 (0.040)	1 (37)	-0.002 (0.02)	-0.005 (0.02)	111 (58)
Population size	-0.000033 (9.5e-06)	0.01 (0.02)	-5.3e-06 (9.4e-06)	-0.00002 (8.30e-06)	0.01 (0.04)
Unemployment rate	0.047 (0.026)	-91 (27)	-0.04 (0.02)	-0.04 (0.01)	-10 (53)
Left-wing government	0.26 (0.10)	34 (170)	-0.07 (0.07)	-0.01 (0.06)	-163 (246)
Proportion left votes	0.036 (0.010)	23 (13)	0.02 (0.005)	0.01 (0.004)	19 (20)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R ²	0.9558	0.8886	0.8991	0.9254	0.6164
Number of observations	1,415	1,111	1,164	1,163	1,415

Notes- The pre-election agency effect is the difference in outcomes between re-elected and replaced governments. Columns 1 and 5 include data from the following election years: 1979, 1982, 1985, 1988, 1991 and 1994, while columns 2-4 include data from the same election years except for 1994. Standard errors clustered at local government level are within parentheses.

Table 6. Pre-election agency effects: all pre-election years + additional control variables

	Income tax rate	Childcare spending per capita	Children aged 0-6 in center care	Center care employees per capita	Political costs per capita
	(1)	(2)	(3)	(4)	(5)
Pre-election agency effect	-0.166*** (0.059)	99** (47)	0.050** (0.022)	0.034** (0.017)	-67 (98)
Income per capita	-2.8e-06 (7.6e-06)	0.002 (0.01)	-0.00001 (3.1e-06)	-6.2e-06 (2.5e-06)	-0.01 (0.01)
Proportion young	0.079 (0.022)	6 (22)	-0.002 (0.01)	0.03 (0.01)	14 (36)
Proportion elderly	-0.007 (0.037)	-21 (35)	-0.02 (0.02)	-0.02 (0.02)	156 (51)
Population size	-0.000038 (0.000014)	-0.001 (0.02)	-0.00002 (5.3e-06)	-0.00002 (4.9e-06)	0.01 (0.03)
Unemployment rate	0.033 (0.022)	-64 (18)	-0.03 (0.01)	-0.02 (0.01)	-10 (42)
Left-wing governments	0.28 (0.09)	156 (98)	0.02 (0.03)	0.04 (0.03)	-124 (223)
Proportion left votes	0.032 (0.008)	17 (8)	0.02 (0.004)	0.01 (0.003)	11 (17)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R ²	0.9568	0.8631	0.8972	0.8931	0.6040
Number of observations	3,758	3,387	3,500	3,494	3,758

Notes- The pre-election agency effect is the difference in outcomes between re-elected and replaced governments. Columns 1 and 5 include data from all pre election years 1979-1994, while columns 2-4 include data from the same election years except for 1994. Standard errors clustered at local government level are within parentheses.

Table 7. Post-election agency effects: only the first post-election year

	Income tax rate	Childcare spending per capita	Children aged 0-6 in center care	Center care employees per capita	Political costs per capita
	(1)	(2)	(3)	(4)	(5)
Post-election agency effect	-0.077 (0.086)	155** (68)	0.046 (0.030)	0.065** (0.033)	23 (113)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R ²	0.9370	0.8801	0.	0.9012	0.6737
Number of observations	1,165	1,137	1,166	1,158	1,174

Notes- The post-election agency effect is the difference in outcomes between re-elected and newly elected governments. Post elections years included: 1980, 1983, 1986, 1989, and 1992. Standard errors clustered at local government level are within parentheses.

Table 8. Post-election agency effects: only first post-election years + additional control variables

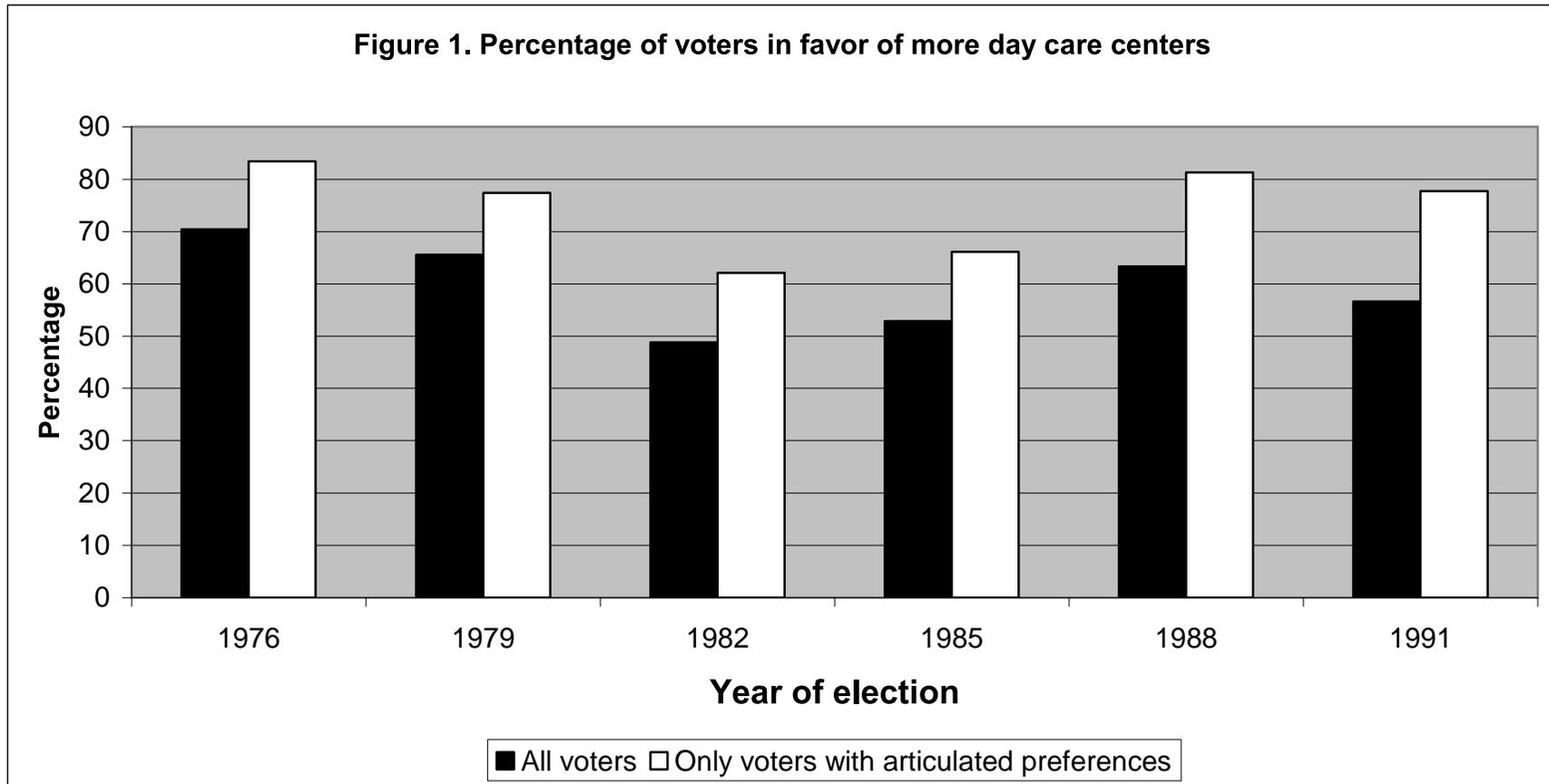
	Income tax rate	Childcare spending per capita	Children aged 0-6 in center care	Center care employees per capita	Political costs per capita
	(1)	(2)	(3)	(4)	(5)
Post-election agency effect	-0.184** (0.088)	112* (64)	0.019 (0.029)	0.039 (0.029)	20 (123)
Income per capita	0.000010 (0.000011)	-0.01 (0.01)	-9.5e-06 (4.5e-06)	-8.6e-06 (3.8e-06)	-0.01 (0.01)
Proportion young	0.094 (0.028)	-2 (28)	0.001 (0.01)	0.03 (0.01)	-8 (48)
Proportion elderly	-0.012 (0.047)	-10 (44)	-0.03 (0.02)	-0.03 (0.02)	146 (58)
Population size	-0.00005 (0.000016)	0.01 (0.04)	-0.00002 (6.7e-06)	-0.00002 (6.8e-06)	0.02 (0.05)
Unemployment rate	-0.023 (0.032)	-60 (27)	-0.02 (0.02)	-0.02 (0.01)	23 (39)
Left-wing governments	0.32 (0.12)	92 (111)	0.04 (0.04)	0.05 (0.04)	86 (267)
Proportion left votes	0.032 (0.010)	19 (10)	0.01 (0.005)	0.01 (0.004)	4 (21)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R ²	0.9578	0.8834	0.9059	0.9183	0.6743
Number of observations	1,165	1,136	1,161	1,157	1,165

Notes- The post-election agency effect is the difference in outcomes between re-elected and newly elected governments. Post elections years included: 1980, 1983, 1986, 1989, and 1992. Standard errors clustered at local government level are within parentheses.

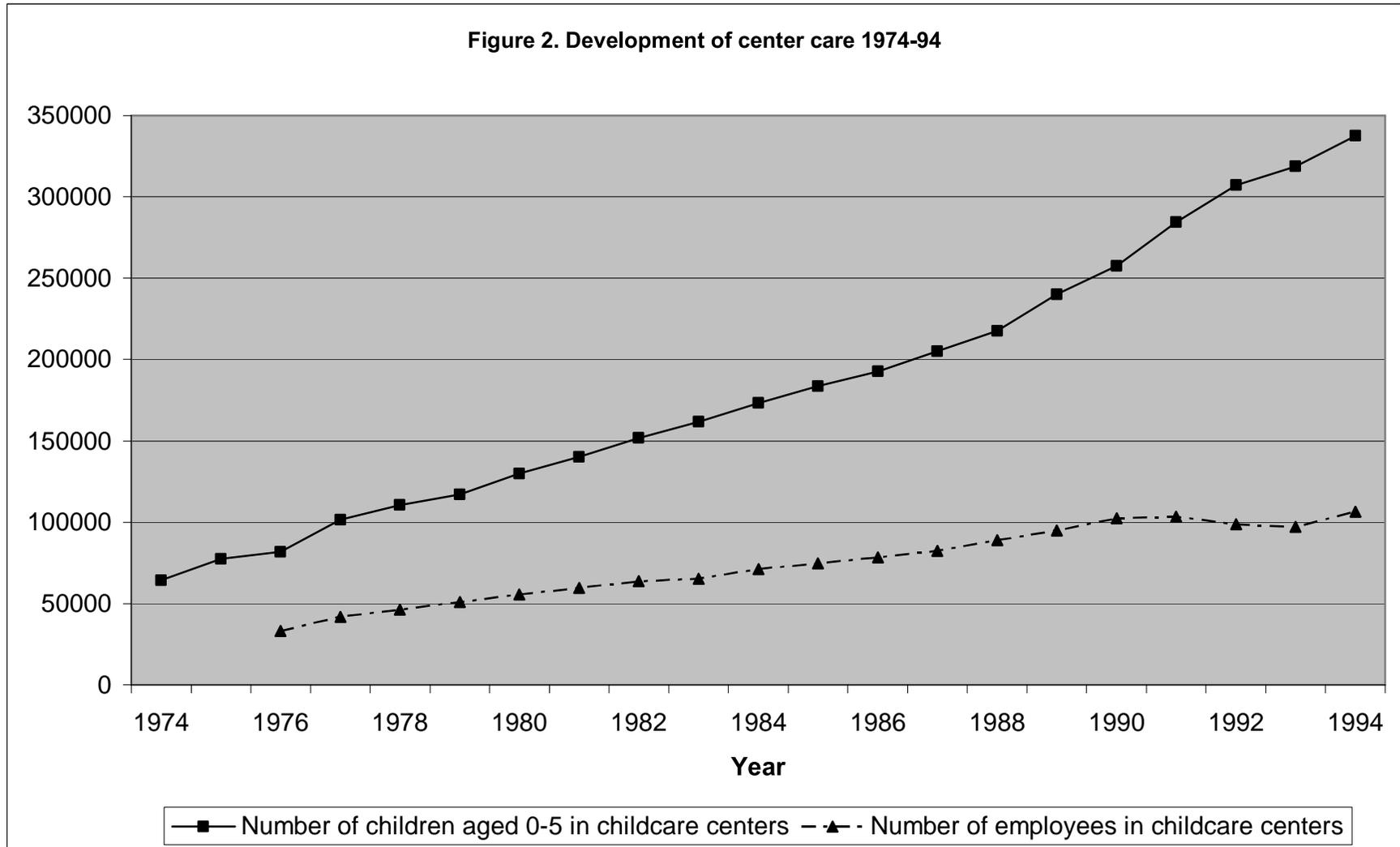
Table 9. Post-election agency effects: all post-election year + additional control variables

	Income tax rate	Childcare spending per capita	Children aged 0-6 in center care	Center care employees per capita	Political costs per capita
	(1)	(2)	(3)	(4)	(5)
Post-election agency effect	-0.192*** (0.071)	91* (47)	0.020 (0.014)	0.015 (0.024)	-35 (114)
Income per capita	2.9e-06 (7.6e-06)	0.01 (0.01)	-5.6e-06 (2.4e-06)	-7.0e-06 (2.7e-06)	-0.01 (0.01)
Proportion young	0.065 (0.024)	18 (25)	-0.004 (0.01)	0.04 (0.01)	15 (44)
Proportion elderly	-0.023 (0.041)	-29 (38)	-0.02 (0.01)	-0.03 (0.02)	172 (58)
Population size	-0.000039 (0.0000014)	-0.01 (0.02)	-8.6e-06 (4.2e-06)	-0.00002 (5.2e-06)	0.01 (0.03)
Unemployment rate	0.031 (0.023)	-66 (18)	-0.03 (0.01)	-0.02 (0.01)	-8 (45)
Left-wing governments	0.33 (0.10)	128 (97)	-0.01 (0.03)	0.05 (0.03)	-114 (237)
Proportion left votes	0.037 (0.009)	18 (8)	0.01 (0.003)	0.01 (0.004)	7 (19)
Fixed effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
R ²	0.9552	0.8570	0.8405	0.8934	0.6074
Number of observations	3,497	3,161	3,253	3,235	3,497

Notes- The post-election agency effect is the difference in outcomes between re-elected and newly elected governments. Columns 1 and 5 include data from 1980-1994, while columns 2-4 include data from 1980-1993. Standard errors clustered at local government level are within parentheses.



The data comes from the Swedish Election Studies. A representative sample of voters is asked the question “what is your opinion about the proposal to build more day care centers for children?” The answers are (1) Very good proposal: very important that is implemented. (2) Fairly good proposal: fairly important that is implemented. (3) Does not really matter. (4) Fairly bad proposal: fairly important that is not implemented. (5) Bad proposal: very important that is not implemented. The first result (black bars) is based on the number of respondents answering 1 or 2 in relation to all respondents, while the second result (white bars) is based on the number of respondents answering 1 or 2 in relation to only those who answers 1, 2, 4 or 5.



Appendix

Table A1. Pre-election test: only election years ++ additional control variables: excluding local governments with zero turnovers.

	Income tax rate	Childcare spending per capita	Children aged 0-6 in center care	Center care employees per capita	Political costs per capita
	(1)	(2)	(3)	(4)	(5)
Pre-election agency effect	-0.177*** (0.065)	174*** (65)	0.068** (0.029)	0.066*** (0.024)	-80 (117)
Controls	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes
Year effects	Yes	Yes	Yes	Yes	Yes
Number of observations	705	557	578	576	705

Notes- Standard errors clustered at local government level are within parentheses.