Opportunistic politicians and fiscal outcomes: the curious case of Vorarlberg

Köppl Turyna, Monika

Agenda Austria

1 June 2015

Opportunistic politicians and fiscal outcomes: The curious case of Vorarlberg

Monika Köppl–Turyna

Agenda Austria, Schottengasse 1/3 1010 Vienna, Austria

Abstract

Using a unique set of electoral rules present in the Austrian state of Vorarlberg, we explore the question whether direct election of the mayor affects the size of local governments. Using difference-in-differences estimation and propensity score matching, we find evidence that direct elections of the mayor are associated with lower expenditure on public administration and public personnel, and higher expenditure in the visible categories of spending i.e. transportation infrastructure and subsidies to the economy.
JEL classifications: H72, H75, H77, D72
Keywords: local expenditure, opportunistic politicians, electoral rules

1. Introduction

The relationship between fiscal federalism and the size of the public sector remains an area in which no clear empirical picture prevails. Recent analyses of fiscal federalism highlight that office–oriented politicians might abuse their power over the local budgets according to their own objective functions, e.g. involvement in rent extraction or corruption. Most theories of competitive federalism support the existence of smaller public sectors in decentralized countries, on the basis of the argument that local decision makers are more accountable to local voters and therefore have few opportunities to misbehave. Moreover, if the taxes are raised at the local level, the local population will keep a close watch on the efficiency of provision of public services financed from their own pockets. Therefore, political accountability at the local level should provide a strong incentive to the politicians to reduce inefficient spending as well as involvement in rent seeking.

Wide literature addresses the question of whether fiscal performance at the local level affects the reelection probabilities. Following the seminal articles of Nordhaus (1975) and Rogoff et al. (1990), Akhmedov and Zhuravskaya (2004), find in the new–democracy context that pre-electoral manipulation of fiscal instruments increases the incumbent's chances of getting reelected, while Veiga and Veiga (2007) use data from Portuguese municipalities and speculate that higher expenditures over the whole term (and specifically in election years) increase the chances of political success. This literature suggests that incumbents

---

have the incentive to expand the budgets of the municipalities in order to affect their reelection probabilities.

In this work, we want to explore the question of whether direct election of the mayor, which affects the incentives of politicians, matters for the size of local government spending using a unique set of electoral rules present in the Austrian federal state of Vorarlberg. Arguably, the three parallel voting systems in place in Vorarlberg represent an original source of variation in electoral incentives of the incumbents. We want to analyze whether direct election of the mayor has an effect on the overall levels of local expenditure as well as the composition of the expenditure.

Previous literature exploiting the differences in local electoral systems focused mostly on Swiss cantonal and municipal elections as well as German electoral law reforms. Pommerehne (1978) exploits the fact that in the 1970s some Swiss municipalities were direct democracies whereas others used a representative democratic system, finding that the median voter model better reflects the pattern of expenditures if decisions are made directly. Similarly, Feld and Kirchgässner (1999) find that direct democracy has an impact on debt levels of municipalities. More recent literature analyzes some fiscal aspects of electoral rules for the German municipalities that have undergone a change in the electoral regime. Köthenbürger et al. (2013) find that municipalities with appointed mayors react less strongly to changes in fiscal incentives. The change in municipal tax rates is three times smaller compared with a system of direct mayoral elections. Similarly, Hessami (2014), using the case of Hesse, reveals that municipalities with a directly elected mayor attract 5% more investment transfers from the state. This effect only materializes in the election year, which suggests that mayors under the new electoral rule put more effort into grant applications for highly visible infrastructure projects in order to increase their re-election probability. Blume et al. (2008) find that change to directly elected mayors has led to lower expenditures and revenues in the German state Schleswig-Holstein compared with Baden-Wuerttemberg, where direct elections were used all along. However, the analysis suffers from the fact that expenditures are aggregated at the state-level. Egger et al. (2007) focus on the introduction of direct mayor elections in Lower Saxony. The authors find that local spending increases with direct mayor elections, in particular redistributive spending. Unfortunately, this setting does not allow the authors to distinguish whether the effect on spending levels can be attributed to electoral incentives or to the increased power of the mayor which give the latter more room for decision making. Finally, Ade (2014) analyzes a switch of mayor appointment by the local council to direct mayor elections in three German states and provides evidence that tax rates are lower and public spending is higher with directly elected mayors.

We contribute to the existing literature in several ways. First of all, we analyze whether the electoral incentives matter not only for the size but also the composition of local expenditure. Arguably, expenditure categories visible to the voters should be affected in a different way than the categories that are not directly observed, and therefore less relevant for reelection prospects. Secondly, we are able to identify the effect of the direct election stemming only from the electoral incentives and independent of any changes to the competences of the mayor, or the way he or she is recalled from office. Finally, since we concetrate

on one region only, we are able to eliminate most of the unobservable characteristics that could differentiate the municipalities in a cross–country or a cross–regional sample.

We believe that the set of presented rules offers a unique opportunity to explore the research questions. Unlike for the case of cross–country studies and country–level studies, there are comparatively few factors that would affect the fiscal outcomes and differentiate the local entities. Municipalities in the analyzed region differ in terms of some demographic and economic variables, for which we control, but do not differ in terms of budgeting rules or access to central government transfers, as these are either centrally or regionally (for the whole of Bundesland) predetermined. Some differences in the access to financing stem from the fiscal equalization scheme, which aims at reducing the discrepancies between the municipal "financial strength" (*Finanzkraft*), i.e. providing the means necessary for the municipalities to perform a basic provision of public services. These differences are controlled for as explained in Section 3.

Our main results show that when the mayor is elected directly, we observe less expenditure on personnel, public administration and privately provided services, and at the same time higher expenditure on transportation infrastructure and promotion of the economy, the latter category comprising different kinds of direct subsidies to economic agents. The overall level of expenditure remains unaffected.

In the next section, we present the institutions present in Vorarlberg and formulate hypotheses about the impact of these institutions on the levels of public expenditure. Section 3 presents the dataset, variables used in the regression, and the methodology of analysis. Section 4 presents the main results and Section 5 concludes the work.

2. Institutions and hypotheses

2.1. Institutional background

The state (German: *Bundesland*) of Vorarlberg is the westernmost federation state of Austria. It is further divided into 96 municipalities (*Gemeinden*) of diverse size and area. Mostly populated is Dornbirn with 47,420 inhabitants whereas the least populated Dünserberg inhabits only 144 members of the community. Municipalities in Austria and in particular in Vorarlberg are divided into three administrative categories typically associated with size: normal municipalities, market municipalities, and cities. There are currently 80 normal municipalities, 11 market municipalities, and 5 cities in Vorarlberg.

Typically, in most European democracies electoral rules at the local level are centrally governed. In federal states, electoral rules may differ at the state level, for instance in Germany and Austria. In Austria the latter is true for most federal states: state law governs electoral laws for municipal elections. Austria has undergone a series of reforms, each state switching from the indirect to the direct election of the mayor: in 1991 the state of Carinthia, in 1992 Burgenland and Tirol, in 1994 Salzburg, in 1997 Upper Austria and in 2000 the state of Vorarlberg[1] The reform has taken a unique form in the federal state

---

[1]Styria, Lower Austria and Vienna still choose their mayors indirectly.

of Vorarlberg. The electoral rules are set at the local level and since the year 2000 three parallel systems have been in place: semi–open list for the municipal council together with a direct election of the mayor, semi–open list system often connected with preselection of the list members by the electorate, and finally an open election in which each eligible voter freely decides on whom to elect as a the member of the municipal council. Before the year 2000, all municipalities have used a semi–open party system without the direct election of the mayor.

In the semi–open list system, the local parties populate the party lists as well as suggest candidates for the office of the mayor. Eligible electors can place one vote for a list to the municipal council and one vote for a mayor's office candidate. Additionally, each voter may place up to five votes for individual candidates on the chosen list. If in a direct election of the mayor only one candidate stands for the election, voters can still utilize a Yes/No poll. In 2015, 60 out of 96 municipalities conducted an election according to these rules.

The second variant is a different version of a semi–open list party system. It is often preceded by a consultation with the electorate. Parties either send empty polls to the voters, who then place the names of desired candidates on the lists, or send a preselected candidate lists and voters may decide on the order of placement. The mayor is, however, not directly elected but chosen by the council of the municipality among their freshly elected members. In 2015, the system was used in 20 municipalities.

The last system is entirely open. Each voter receives an empty voting sheet on which he or she is eligible to place names of desired members of the municipal council freely chosen from all members of the community with a passive suffrage. The voter can choose a number of names up to a double of the arranged seats in the local council. The newly elected members of the council subsequently choose the mayor among themselves. In 2015, this rule was used in 16 municipalities.

The role of the mayor in local decision making is very strong, both in legal and in practical terms. According to the Municipality Act of the Federal State of Vorarlberg (short. GG) provisions, mayors chair the municipal council and the municipal board. They are chiefs of the communal administration and implement the decisions of the local bodies. They have power over the municipal budget and represent the community to the outside. The mayor can be, however, recalled from office by a majority of 2/3 of the municipal council, irrespective of whether the election has been a conducted directly or not (Art. 31 of the GG). Therefore, unlike in Ade (2014) and previous works on the German reforms, the differences in the electoral procedures are associated only with differences in the electoral motives of the mayor, and do not constitute a change from a "parliamentary" to a "presidential" system. It is important to notice that the electoral reform of 2000 has not changed any competences of the mayor.[2]

It is important to determine which categories of expenditure can be affected by the municipalities, and thus by the mayors and which are determined by the higher levels of

---

[2]The last time that the competences of the mayors have changed was during the reforms of 1988 and 1994, when Austria has adapted its local self–governance laws to the European Union standards.

government. Muncipalities have little power to decide on the expenditure on health and security. Discretionary expenditure in the category education includes mostly management of kindergartens and nurserys. The categories in which the communes have the most discretionary powers are subsidies, transportation and management of streets and roads and services.

2.2. Hypotheses

The literature offers a set of suggestions as for how electoral rules should affect the fiscal outcomes. Firstly, the electoral rules systematically and independently of individual characteristics of the mayor affect the probabilities of reelection. The probability of reelection in turn affects the incentives faced by the politicians in making expenditure decisions. An important factor for determining the incentives of the local governments to manipulate the expenditure levels is the rule-dependent prospect of reelection. The general idea behind electoral incentives is that elections may motivate politicians to act in the interest of voters via the threat of not being re-elected. In reality, this may lead for example to higher spending prior to an election, which may only be in the interest of voters in the short run. A reasonable hypothesis is that when the mayor is elected directly, incentives to exert effort in a way that is visible to voters are larger than in the opposite case system because the incumbent needs the support of the electorate at large rather than only the (typically guaranteed) support of his or her party to be re-elected.

Therefore, our main hypothesis is that whenever the mayor is elected directly, he or she has an incentive to increase expenditure in the visible categories. With balance budget requirements in place, this should be simultaneously associated with the need to decrease expenditure levels in other categories of spending. Moreover, we do not expect a strong decrease in the overall expenditure, as the municipalities do not have strong taxation competences. Unlike in other federations such as Switzerland, own tax revenues constitute a small fraction of the local revenue (in our sample the fraction of own taxation in the overall revenue varies between 0.6% and 40% with an average of 15%). Therefore, the mayors face a strong incentive to exhaust the allocated budgets. Additionally, since 1999 the Maastricht criteria have been applied also at the communal level, which means that the yearly budget deficit cannot exceed 0.3% of GDP (this quota is further divided between the communes and the states: for the more detailed description of the application please refer to the Austrian Stability Pact.), that is the budget needs to be balanced. We expect, therefore, that there will mostly be change in the composition rather than the level of municipal spending: we expect a shift in the expenditure towards visible categories of expenditure.

More formalized predictions as for impact of the electoral system on the incentive to change the composition of spending can be derived from Drazen and Eslava (2010). The two politicians' types of Drazen and Eslava (2010): the "desks-type" and the "people-type" correspond to the incentives schemes under the two voting systems. A desks-type politician corresponds to the indirect system, as he or she needs to target his or her party to assure reelection. In practical terms, he or she might have an incentive to invest in the actual desks, that is bureaucracy. On the other hand, in the direct system, the politician targets

the voters at large. Drazen and Eslava (2010) predict that the people-type spends overall more resources on targeted policies, over the whole electoral period, wheres the desks-type is expected to increase targeted spending only in the pre-electoral period. Given this interpretation of the Drazen and Eslava (2010) model we can form the following hypotheses:

Hypothesis 1. *Direct election of the mayor changes the composition of the expenditure, towards targeted (visible) categories over the whole electoral period.*

Hypothesis 2. *Indirect election of the mayor is associated with a stronger political budget cycle in the targeted expenditure.*

3. Methodology and data

3.1. Data

The data comprise information for 96 municipalities in Vorarlberg between 1982 and 2013, a total of 2,634 observations. Fiscal and demographic data have been obtained from the Austrian Statistical Office database, whereas electoral data have been collected from the electoral database of Land Vorarlberg (Vorarlberg, 2015). Descriptive statistics of all control variables can be found in Table 7 in the Appendix.

Figure 1 shows the mean expenditure levels in the two groups of municipalities: those that have switched to the direct election of the mayor in 2000 and those which have not[3].

Inspection of Figure 1 reveals the need for an empirical approach which, would take into account the initial differences between the municipalities that have switched to the direct election system in 2000 and those that have not. In most categories of expenditure we observe systematic differences between the two groups. As explained in the next subsection we shall address this issue by combining propensity score matching with the difference-in-differences approach.

Dependent variables in the regressions are the expenditure levels per capita for the total expenditure as well as subcategories of expenditure:

1. Public Administration
2. Security and Public Order
3. Education and Sport
4. Culture and Religion
5. Social Support and Housing
6. Health Protection
7. Transportation
8. Promotion of the Economy (i.e. Subsidies and grants to indursties and agriculture)
9. Services

---

[3]The length of the panel raises concern as whether the dependent variables are stationary. The results of the panel unit root test, reported in Table 8 in the Appendix suggest that unit roots are not a concern in our case.

Figure 1: Trends in the dependent variables
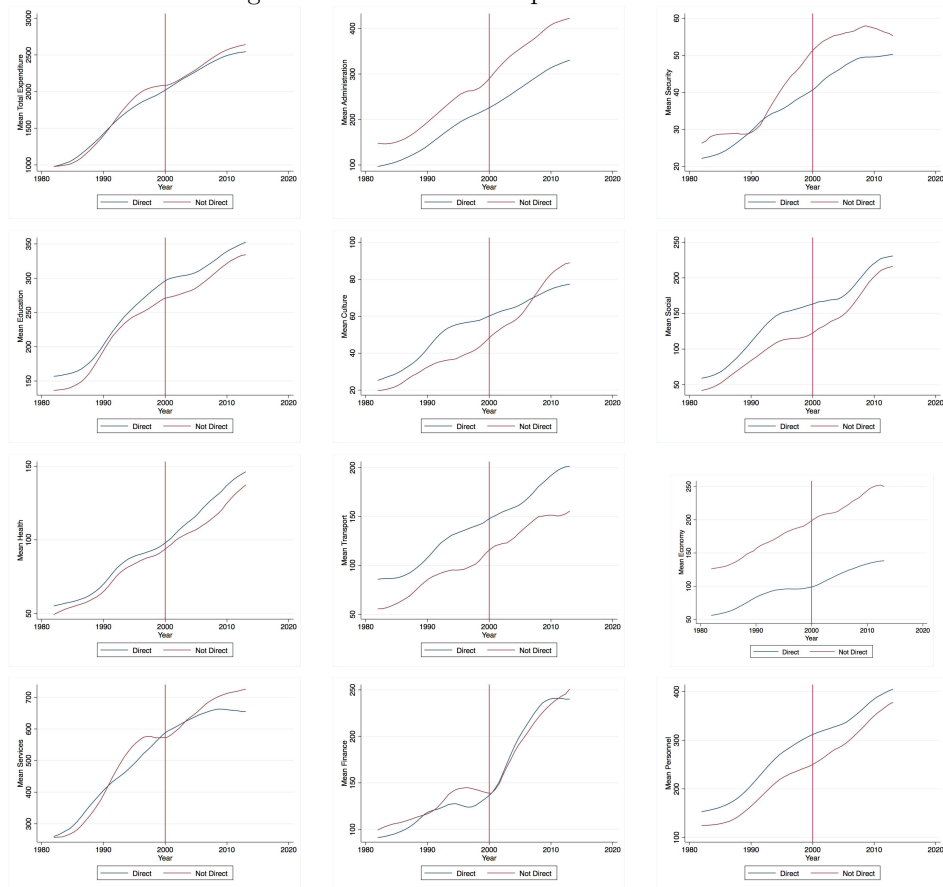
10. Finance (i.e. Debt payments)

11. Public Personnel

The main economic determinants of expenditure levels obviously include the overall level of revenue (*Revenue* – per capita), as well as access to sources of taxation. We include two variables to describe the financing patterns: *Local Taxes* and *Profit Shares*. Local taxes include taxes on local economic activities such as tourist taxes, administrative fees, trade taxes, and property taxes as well as communal taxation, and reflect the economic development of the region. Profit shares are the shares of the municipality in the general taxation stemming from the fiscal equalization scheme. Fiscal equalization schemes are negotiated within the parliament every four years and determine the shares of the municipalities in the common taxation; these shares depend mostly on the population sizes but also on the *Financial Strength* measured on the basis of tax base in property and the 3% municipal wage taxation (*Kommunalsteuer*). The overall revenue of a municipality comprises additional grants and transfers, which are mostly earmarked.

Political variables have also been found in the literature to affect the levels of local expenditure. Municipal elections in the region take place every five years, and in our sample the relevant years are 1985, 1990, 1995, 2000, 2005, and 2010. We also control for party effects with dummy variables: OVP for the Christian–democratic party, SPO for the social–democratic party, and FPO for the populist Freedom Party of Austria, as opposed to independent local commitees (base level). The dummy for OVP also measures the vertical political alignment (in all analyzed periods the OVP party has won the state elections), which has also been found (e.g. Bracco et al., 2013) to affect expenditure levels. We also control for the fragmentation of the council (compare e.g. Houlberg and Pedersen, 2015): the variable HHI is the Herhfindal Index of the of the council seat share of the parties. Additionally, we add a dummy $SingleParty$ for the cases when the HHI equals 1, therefore all council members come from the same party list. We control for the turnout at the election and the level of political competition measured by the number of electoral lists standing for the election, both before and after the electoral reform. Additionally we control for the incumbency advantage, that is dummy *Incumbent* equals 1 if the current mayor has been reelected. Dummy *Divided* equals 1 when the mayor in the direct election comes from a different party than the majority in the municipal council.

Public expenditure at the local level is typically also determined by demographic and geographic variables. These variables typically include the size of the population (Werck et al., 2008; Costa-Font and Moscone, 2009) population density (Sanz et al., 2002), fraction of the elderly and young inhabitants (Hayo and Neumeier, 2012; Veiga and Veiga, 2007), unemployment rates (Foucault et al., 2008), and some country specific controls. We control for all of these, however our measure of unemployment captures all members of the community who are aged between 15 and 64 years and who are not active in the employment market (excluding students and pupils), and not only the individuals registered as unemployed or actively seeking a job. Therefore, these numbers are in fact slightly higher than the official unemployment rates. Since we dispose of information on the actual number of retired persons, we use this variable instead of population over 65 in the regressions. In

fact, in Austria Austria a large portion of the population, for various reasons, retires before the usual legal age, and the actual number of retired inhabitants is, in this case, a much better measure of demand for social services than the raw age structure.

3.2. Empirical methodology

The unique feature of the municipal elections in Vorarlberg is that the choice of the electoral rule is chosen by each municipality and for each election separately. Therefore, there is variation in the electoral rule both between and within the municipalities. This variation is jointly determined by observable characteristics of the municipalities, as well as potentially unobservable variables. Therefore, we combine propensity score matching with difference-in-differences to address the issue of potential endogeneity of the rules. This methodology can be summarized as follows:

1. In the first stage we calculate propensity scores for the municipalities on the basics of exogenous characteristics between 1982 and 1990, using a probit model.
2. The propensity scores are used for a year–by–year kernel propensity score matching using the Stata routine proposed by Villa (2014).
3. The weights derived from the kernel estimation are then used in the final difference–in–differences regression with additional covariates.

The choice of the exogenous characteristics that enter the matching model is restricted to the initial observed period, i.e. the first eight years. By this we want to avoid any changes in the characteristics of the municipalities in the last two electoral periods before the reform, which might happen in expectation of the changes (anticipation effect). Inclusion of the initial outcome variable in the propensity score model should reflect the *a priori* preferences for different types of expenditure intrinsic to each community.

The final model, results of which are presented, is therefore:

$$outcome_{it} = \beta_0 + \beta_1 \cdot reform_t + \beta_2 \cdot treatment_{it} + \beta_3 \cdot reform_t \cdot Direct_{it} + \quad (1)$$
$$\mathbf{\Gamma} \cdot \mathbf{X_{it}} + u_i + v_t + \epsilon_{it} \quad \text{weighted by} \quad weights_{it},$$

where $reform_t$ defines the time span starting in the year 2000, $treatment_{it}$ defines the municipalities with directly elected mayor, β_3 is the difference–in–differences estimator, $\mathbf{X_{it}}$ is the vector of controls, and u_i and v_t are the municipality and time fixed effects, respectively. $weights_{it}$ denotes the analytical weights derived from the propensity score matching procedure. In all regressions, all observations are included (unless explicitly stated otherwise, in the case of robustness checks) with the weights equivalent to the propensity scores.

The main regression formulation serves to test Hypothesis 1. We expect significant differences in the composition of expenditure, with direct election associated with higher targeted (visible) expenditure. To test Hypothesis 2, we look at the strength of the political budget cycles and additionally analyze the interaction term between the budget cycle and the direct election of the mayor. We expect the budget cycle to be present for both types of electoral procedure, yet the cycle should prove stronger in the indirect system.

In the first stage probit calculation of the propensity scores, we match the municipalities on the basics of initial characteristics, that is values for years 1982 to 1990: the levels of revenue per capita, local taxation per capita, profit shares per capita, and the initial values of the outcome variables. Results of the probit estimations are presented in Table 1.

Table 1: First stage probit propensity score prediction model: Marginal Effects

	(1) treatment	(2) treatment	(3) treatment	(4) treatment	(5) treatment	(6) treatment	(7) treatment	(8) treatment	(9) treatment	(10) treatment	(11) treatment	(12) treatment
Profit Shares	0.38** (2.36)	0.40** (2.40)	0.43*** (2.59)	0.46*** (2.71)	0.46*** (2.83)	0.41** (2.24)	0.26* (1.68)	0.51*** (3.06)	0.22 (1.43)	0.42*** (2.58)	0.45*** (2.74)	0.30* (1.67)
Taxes	0.13 (1.55)	0.16* (1.90)	0.12 (1.44)	0.15* (1.69)	0.12 (1.46)	0.13 (1.48)	0.13 (1.61)	0.12 (1.38)	0.24** (2.57)	0.13 (1.48)	0.13 (1.49)	0.10 (1.13)
Revenue	0.07 (0.37)	-0.09 (-0.63)	-0.14 (-1.07)	-0.24* (-1.69)	-0.20 (-1.48)	-0.20 (-1.53)	-0.25* (-1.90)	-0.26* (-1.89)	0.01 (0.07)	-0.13 (-0.94)	-0.19 (-1.46)	-0.26** (-1.98)
Total Expenditure	-0.00 (-1.45)											
Administration		-0.00** (-2.50)										
Security			-0.00* (-1.83)									
Education				0.00 (1.35)								
Culture					-0.00 (-0.15)							
Social						0.00 (0.46)						
Health							0.00*** (2.70)					
Transport								0.00 (1.20)				
Economy									-0.00** (-2.54)			
Services										-0.00 (-0.69)		
Finance											0.00 (0.23)	
Personnel												0.00 (1.33)
Observations	632	632	632	625	632	632	622	632	632	632	632	632
Correctly Classified	73.9%	75.8%	74.7%	74.0%	74.2%	73.7%	74.0%	74.3%	75.5%	74.0%	74.9%	74.2%

Municipalities that receive higher shares of the tax revenue from the fiscal equalization scheme are more likely to change to the direct election system afterwards. As for individual categories of spending, higher probability of treatment occurs for municipalities with initially higher levels of expenditure on healthcare and lower levels of expenditure on public administration, security, and promotion of the economy. The probabilities of the treatment are now used in the second stage to match the municipalities using kernel density matching, on a year–by–year basis and for each expenditure category separately. The choice of the kernel matching algorithm is dictated by practical purposes: there are relatively few untreated observations in the sample, and the use of a matching algorithm without replacement would result in a substantial loss of the available data.

Supports of the propensity scores are depicted in Figure 2 in the Appendix. Results of the balancing of the outcome variables are presented in Figures 3 to 14 in the Appendix. We can observe that the matching is reliable, and the common support is very broad. Therefore, the estimated propensity scores can be reliably used in the next stage.[4]

Obviously, budgeting decisions in are not taken in separation from each other and the expenditure shares in each subcategory are not independent: if a mayor wants to increase expenditure in one direction, there needs to be a cutback in another. To account for these additional restriction we re-stimate all equations, assuming a common error structure across the equations (seemingly unrelated regression) and test whether the changes in expenditure levels in each subcategory sum up to the overall change in expenditure[5].

4. Results

Table 2 presents the main results of the estimations. Notice first that the propensity score matching procedure has significantly reduced the initial bias: in the main results, there are no statistically significant differences in the outcome variables dependent on belonging to the treatment group (dummy Treatment). Low significance of the *Treatment* dummy suggests, therefore, that the matching procedure was effective.

---

[4]For space–saving purposes we do not report the test results of balancing covariates; these can be obtained from the author upon request.

[5]The personnel costs are excluded from the coefficient testing, since they do not constitute a separate category, but overlap with other expenditure types.

Table 2: Difference in differences – Main Results

	(1) Total Expenditure	(2) Administration	(3) Security	(4) Education	(5) Culture	(6) Social	(7) Health	(8) Transport	(9) Economy	(10) Services	(11) Finance	(12) Personnel
Reform	-5.35	17.94**	9.99	-76.29***	-0.26	-37.38***	-21.10*	-74.76***	11.10	127.71***	25.74	0.35
	(-0.27)	(2.21)	(1.08)	(-3.21)	(-0.04)	(-3.58)	(-1.88)	(-4.07)	(0.93)	(3.65)	(0.94)	(0.03)
Treatment	-57.26	36.95**	28.73	-56.77	-23.81	-42.03*	45.48*	-75.65*	43.40	61.24	-63.82	58.07**
	(-1.25)	(1.99)	(1.36)	(-1.04)	(-1.55)	(-1.76)	(1.76)	(-1.80)	(1.59)	(0.76)	(-1.01)	(2.56)
Diff-in-diff	-18.14	-35.72***	-12.71	1.02	-15.47**	9.16	-13.59	79.43***	24.24**	-91.10***	30.37	-32.30***
	(-0.95)	(-4.58)	(-1.44)	(0.04)	(-2.41)	(0.91)	(-1.26)	(4.51)	(2.12)	(-2.71)	(1.15)	(-3.40)
HHI	80.06**	7.84	23.67	88.77**	5.26	-33.20*	3.23	-59.76*	31.45	137.73**	-134.90***	17.15
	(2.31)	(0.56)	(1.48)	(2.15)	(0.45)	(-1.83)	(0.17)	(-1.88)	(1.52)	(2.27)	(-2.84)	(1.00)
Single Party	-8.22	-7.35	-0.74	-80.80***	-2.56	20.07*	28.78**	3.47	-12.23	-49.37	96.57***	28.76**
	(-0.36)	(-0.78)	(-0.07)	(-2.94)	(-0.33)	(1.66)	(2.21)	(0.16)	(-0.89)	(-1.22)	(3.05)	(2.51)
Turnout	63.85	-53.37**	-111.91***	-37.21	5.68	81.90**	170.15***	-99.87*	89.57**	185.09*	-136.61	64.66**
	(1.05)	(-2.15)	(-3.98)	(-0.51)	(0.28)	(2.57)	(4.95)	(-1.78)	(2.46)	(1.73)	(-1.63)	(2.14)
Lists	10.80	-2.91	7.64**	17.60*	0.87	3.96	14.91***	-8.68	-10.20**	-6.62	-7.95	16.10***
	(1.39)	(-0.92)	(2.12)	(1.89)	(0.33)	(0.97)	(3.39)	(-1.21)	(-2.19)	(-0.48)	(-0.74)	(4.16)
SPO	-25.98	-6.98	20.14	-48.31	-12.06	57.39**	90.16***	-57.86	37.44	-273.92***	225.60***	127.16***
	(-0.49)	(-0.32)	(0.83)	(-0.77)	(-0.68)	(2.07)	(3.03)	(-1.19)	(1.19)	(-2.95)	(3.10)	(4.85)
FPO	-79.23	12.80	29.63	11.08	15.23	162.78***	-97.60***	-46.83	155.71***	-216.78**	-103.76	176.44***
	(-1.39)	(0.55)	(1.13)	(0.16)	(0.80)	(5.46)	(-3.04)	(-1.19)	(4.58)	(-2.17)	(-1.32)	(6.24)
OVP	2.13	13.82	28.52	-20.23	17.76	68.42***	-46.09**	-108.40***	22.92	-125.33*	154.72***	51.67***
	(0.06)	(0.90)	(1.64)	(-0.45)	(1.41)	(3.48)	(-2.18)	(-3.14)	(1.02)	(-1.90)	(2.99)	(2.77)
Divided	9.64	-44.89***	2.24	49.48	-4.09	-32.07	6.63	1.64	-70.48***	101.51	1.20	-46.17**
	(0.24)	(-2.74)	(0.12)	(1.03)	(-0.30)	(-1.52)	(0.29)	(0.04)	(-2.93)	(1.43)	(0.02)	(-2.31)
Incumbent	8.72	-11.87*	-16.95**	-40.89**	-8.96*	17.95**	-15.36*	39.55***	8.54	-53.78*	85.22***	-17.44**
	(0.54)	(-1.82)	(-2.29)	(-2.14)	(-1.67)	(2.14)	(-1.70)	(2.69)	(0.89)	(-1.91)	(3.87)	(-2.20)
1 Year Before	12.99	2.77	6.49	-30.06**	5.79*	36.16***	-3.24	-7.11	8.10	-3.47	-1.93	2.88
	(1.24)	(0.65)	(1.34)	(-2.40)	(1.65)	(6.59)	(-0.55)	(-0.74)	(1.29)	(-0.19)	(-0.13)	(0.55)
Election Year	17.29*	5.67	1.78	-9.90	5.57*	29.73***	10.89*	-0.21	8.11	-15.91	-16.33	14.86***
	(1.73)	(1.39)	(0.38)	(-0.83)	(1.65)	(5.67)	(1.93)	(-0.02)	(1.36)	(-0.91)	(-1.19)	(2.99)
1 Year After	0.46	-4.63	3.76	12.62	-0.94	22.27***	1.50	8.70	1.14	-28.88*	-13.24	2.48
	(0.05)	(-1.20)	(0.86)	(1.11)	(-0.29)	(4.47)	(0.28)	(0.99)	(0.20)	(-1.73)	(-1.01)	(0.52)
Unemployment	-202.55***	-39.08**	0.55	-83.88*	-8.55	7.58	-35.72*	-182.19***	30.65	64.43	41.42	25.14
	(-5.42)	(-2.57)	(0.03)	(-1.88)	(-0.68)	(0.39)	(-1.70)	(-5.22)	(1.37)	(0.98)	(0.81)	(1.36)
Profit Shares	-0.05	0.14***	-0.00	-0.29***	0.05*	-0.01	0.10***	-0.23***	0.18***	-0.22*	0.23***	0.13***
	(-0.67)	(5.00)	(-0.08)	(-3.39)	(2.06)	(-0.19)	(2.63)	(-3.52)	(4.18)	(-1.74)	(2.36)	(3.75)
Taxes	0.07**	0.04***	-0.04***	-0.19***	-0.01	0.07***	-0.04**	-0.09***	0.48***	-0.27***	0.11***	0.13***
	(2.30)	(3.21)	(-2.66)	(-5.30)	(-1.32)	(4.39)	(-2.16)	(-3.29)	(26.05)	(-5.06)	(2.74)	(8.85)
Revenue	0.96***	0.03***	0.02***	0.07***	0.00**	0.01***	0.02***	0.07***	0.05***	0.51***	0.18***	0.04***
	(187.81)	(15.81)	(7.69)	(10.99)	(2.53)	(3.04)	(6.35)	(15.55)	(15.71)	(56.80)	(25.67)	(13.88)
Young	-172.12**	39.50	64.00**	7.38	29.19	50.86	-71.72*	-263.35***	-149.31***	175.92	-68.54	64.71*
	(-2.50)	(1.41)	(2.01)	(0.09)	(1.26)	(1.41)	(-1.85)	(-4.13)	(-3.63)	(1.45)	(-0.72)	(1.89)
Retired	18.69	-11.69	104.18**	58.66	-17.49	-138.44***	-31.06	12.79	-54.58	548.90***	-441.04***	-128.33***
	(0.19)	(-0.29)	(2.30)	(0.50)	(-0.53)	(-2.69)	(-0.56)	(0.14)	(-0.93)	(3.18)	(-3.26)	(-2.63)
Pop Density	-0.15	0.20***	0.07	-0.24	-0.07	0.28***	0.78***	0.00	0.01	-1.42***	0.33*	-0.29***
	(-1.02)	(3.46)	(1.11)	(-1.41)	(-1.43)	(3.68)	(9.71)	(0.03)	(0.08)	(-5.65)	(1.68)	(-4.09)
Inhabitants	-0.01	-0.03***	-0.01	0.02	0.01	-0.01	-0.09***	-0.04***	-0.01	0.16***	-0.03	0.05***
	(-0.35)	(-4.21)	(-1.11)	(1.31)	(1.59)	(-1.08)	(-10.86)	(-2.79)	(-1.44)	(6.12)	(-1.24)	(6.16)
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2634	2634	2634	2634	2625	2634	2634	2626	2634	2634	2634	2634

t-Statistics with robust clustered S.E. in parentheses, * 0.1 ** 0.05 *** 0.01

13

Table 3: Difference in differences – Interaction of direct election with the strength of the cycle

	(1) Total Expenditure	(2) Administration	(3) Security	(4) Education	(5) Culture	(6) Social	(7) Health	(8) Transport	(9) Economy	(10) Services	(11) Finance	(12) Personnel
Reform	-2.61	39.49***	20.33**	-66.23***	-9.25	-64.22***	-14.00	-47.77***	-36.67***	189.98***	-21.07	-5.36
	(-0.15)	(4.87)	(2.50)	(-3.09)	(-1.39)	(-6.11)	(-1.08)	(-2.71)	(-2.80)	(5.87)	(-0.85)	(-0.46)
Treatment	-13.02	-3.71	-3.28	23.84*	9.10**	-36.63***	10.83	1.36	-17.65**	-33.80	44.32***	-21.26***
	(-1.13)	(-0.70)	(-0.61)	(1.69)	(2.07)	(-5.31)	(1.27)	(0.12)	(-2.06)	(-1.59)	(2.71)	(-2.79)
Diff-in-Diff	-20.95	-51.48***	-9.58	14.28	-4.14	39.92***	-15.07	70.42***	33.26**	-133.63***	22.84	-13.30
	(-1.17)	(-6.24)	(-1.16)	(0.65)	(-0.61)	(3.73)	(-1.14)	(3.93)	(2.50)	(-4.06)	(0.90)	(-1.12)
HHI	24.86	-40.21***	17.03	8.33	23.84**	80.32***	-42.25**	-94.37***	69.47***	81.37	-79.08**	18.53
	(0.90)	(-3.18)	(1.34)	(0.25)	(2.29)	(4.89)	(-2.08)	(-3.43)	(3.40)	(1.61)	(-2.03)	(1.02)
Single Party	21.95	18.39**	0.09	-7.51	-15.75**	-36.29***	75.45***	28.10	-18.12	-80.32**	60.16**	12.15
	(1.18)	(2.14)	(0.01)	(-0.33)	(-2.23)	(-3.26)	(5.49)	(1.51)	(-1.31)	(-2.35)	(2.28)	(0.99)
Turnout	43.53	12.53	-35.65	-23.12	-22.48	40.54	27.31	-94.87	130.27***	106.92	-69.26	-19.37
	(0.75)	(0.47)	(-1.33)	(-0.33)	(-1.03)	(1.17)	(0.64)	(-1.64)	(3.03)	(1.00)	(-0.85)	(-0.51)
Lists	21.57***	-9.52***	4.62*	-3.17	1.58	2.82	36.16***	-13.21**	-5.46	10.57	-1.04	21.35***
	(3.60)	(-3.46)	(1.67)	(-0.44)	(0.70)	(0.79)	(8.19)	(-2.21)	(-1.23)	(0.96)	(-0.12)	(5.40)
SPO	-10.30	16.88*	-6.96	18.37	-26.90***	51.40***	105.18***	34.78	-70.75***	-57.54	-62.93**	106.41***
	(-0.48)	(1.71)	(-0.70)	(0.70)	(-3.32)	(4.01)	(6.64)	(1.62)	(-4.44)	(-1.46)	(-2.07)	(7.51)
FPO	-2.09	23.75***	-16.06*	32.53	-1.40	56.90***	-44.39***	60.40***	33.50**	-55.01	-90.41***	61.19***
	(-0.11)	(2.68)	(-1.81)	(1.39)	(-0.19)	(4.96)	(-3.13)	(3.14)	(2.35)	(-1.56)	(-3.32)	(4.81)
OVP	-14.27	13.20***	-10.24**	49.54***	2.67	5.38	13.07*	11.40	-3.07	-40.22**	-59.62***	36.67***
	(-1.51)	(3.04)	(-2.35)	(4.30)	(0.75)	(0.95)	(1.87)	(1.21)	(-0.44)	(-2.32)	(-4.46)	(5.87)
Divided	9.90	0.04	3.56	15.43	-27.11**	-34.16**	8.50	-14.42	-54.27**	31.97	84.01**	-0.92
	(0.35)	(0.00)	(0.27)	(0.44)	(-2.51)	(-2.00)	(0.40)	(-0.51)	(-2.56)	(0.61)	(2.08)	(-0.05)
Incumbent	2.82	3.87	-1.52	-25.08	-19.94***	4.15	-34.20***	-3.22	17.78	-8.07	60.65***	-17.60*
	(0.19)	(0.55)	(-0.27)	(-1.35)	(-3.47)	(0.46)	(-3.04)	(-0.21)	(1.57)	(-0.29)	(2.82)	(-1.75)
1 Year Before	10.80	-2.84	6.94	-12.21	8.52*	49.09***	-1.05	-12.73	7.62	-24.15	-5.89	2.53
	(0.85)	(-0.48)	(1.18)	(-0.79)	(1.77)	(6.46)	(-0.11)	(-1.00)	(0.81)	(-1.03)	(-0.33)	(0.30)
Election Year	14.57	2.98	6.48	12.06	9.98**	37.20***	2.62	-6.20	12.52	-51.49**	-8.79	9.62
	(1.28)	(0.57)	(1.23)	(0.87)	(2.30)	(5.47)	(0.31)	(-0.54)	(1.48)	(-2.46)	(-0.55)	(1.28)
1 Year After	-0.03	-6.69	3.44	28.74**	3.34	27.12***	-1.36	-0.81	3.00	-51.12**	-2.89	-1.88
	(-0.00)	(-1.31)	(0.67)	(2.13)	(0.80)	(4.10)	(-0.17)	(-0.07)	(0.36)	(-2.51)	(-0.18)	(-0.26)
1 Year Before * Diff-in-diff	12.88	12.70	-6.35	-52.31*	-3.90	-48.03***	-26.41	-5.59	39.23**	64.52	38.64	-15.20
	(0.55)	(1.18)	(-0.59)	(-1.84)	(-0.44)	(-3.44)	(-1.53)	(-0.24)	(2.26)	(1.50)	(1.17)	(-0.98)
Election * Diff-in-diff	16.79	4.49	-15.38	-83.28***	-7.66	-21.96	-10.54	-5.24	35.95**	96.10**	29.35	-14.27
	(0.74)	(0.43)	(-1.47)	(-3.01)	(-0.89)	(-1.62)	(-0.63)	(-0.23)	(2.13)	(2.30)	(0.91)	(-0.95)
1 Year After * Diff-in-diff	-1.89	5.92	7.41	-68.16**	-6.73	-12.23	-15.77	26.66	10.79	59.03	-6.71	-7.30
	(-0.08)	(0.57)	(0.71)	(-2.47)	(-0.91)	(-0.91)	(-0.94)	(1.18)	(0.64)	(1.42)	(-0.21)	(-0.49)
Unemployment	-157.39***	28.71***	1.09	-78.33***	-21.56**	-27.82**	-38.18**	34.82	-37.80**	-6.36	-6.39	-15.66
	(-6.92)	(2.74)	(0.10)	(-2.83)	(-2.52)	(-2.05)	(-2.28)	(1.51)	(-2.24)	(-0.15)	(-0.20)	(-1.04)
Profit Shares	-0.01	0.09***	-0.03	-0.29***	0.06***	-0.00	-0.07	-0.36***	0.51***	-0.26***	0.38***	0.01
	(-0.22)	(3.27)	(-0.91)	(-3.99)	(2.79)	(-0.11)	(-1.49)	(-5.89)	(11.37)	(-2.35)	(4.51)	(0.35)
Taxes	-0.00	0.06***	-0.01*	-0.11***	0.01***	0.06***	-0.03***	0.04***	0.37***	-0.27***	-0.13***	0.17***
	(-0.30)	(10.84)	(-1.65)	(-7.17)	(2.61)	(7.91)	(-3.12)	(3.51)	(39.81)	(-12.04)	(-7.20)	(20.42)
Revenue	0.98***	0.04***	0.02***	0.08***	-0.00	0.00	0.02***	0.05***	0.06***	0.53***	0.18***	0.04***
	(224.05)	(18.96)	(9.43)	(15.23)	(-0.63)	(1.08)	(7.69)	(11.75)	(18.35)	(65.06)	(29.07)	(13.70)
Young	-163.86***	-107.63***	-26.21	46.72	45.31***	67.22***	-77.56**	-114.80***	-72.68**	-201.37**	267.97***	-163.24***
	(-3.83)	(-5.47)	(-1.33)	(0.90)	(2.79)	(2.63)	(-2.46)	(-2.66)	(-2.29)	(-2.56)	(4.43)	(-5.78)
Retired	-125.66**	-175.93***	-100.56***	-102.15	4.33	216.83***	120.60***	195.53***	32.50	-40.63	-288.42***	232.28***
	(-2.30)	(-7.00)	(-3.99)	(-1.53)	(0.20)	(6.65)	(2.99)	(3.57)	(0.80)	(-0.40)	(-3.73)	(6.44)
Pop Density	-0.04***	0.07***	-0.01	-0.01	-0.04***	0.01	-0.05***	-0.02	-0.10***	0.07***	0.05**	-0.10***
	(-2.63)	(10.33)	(-1.60)	(-0.74)	(-6.18)	(0.70)	(-4.07)	(-1.17)	(-8.25)	(2.57)	(2.29)	(-10.08)
Inhabitants	-0.00*	-0.01***	-0.00***	-0.00	-0.00	0.00***	0.00	0.00	-0.00***	0.01***	-0.01***	0.01***
	(-1.80)	(-8.18)	(-2.78)	(-0.45)	(-0.97)	(5.10)	(1.34)	(0.14)	(-3.80)	(3.31)	(-2.98)	(11.72)
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2634	2634	2634	2634	2625	2634	2634	2626	2634	2634	2634	2634

t-Statistics with robust clustered S.E. in parentheses, * 0.1 ** 0.05 *** 0.01

14

Table 4: Coefficients' equality test.

χ^2	1.479
p-val	0.224

In Table 5 we report the seemingly unrelated estimation results and test the assumption that the subcategories need to equate to the overall change in the expenditure, in order to verify the results[6]. We therefore test whether the coefficients on the difference-in-differences across the equations (2) to (11) sum up to the coefficient in (1). The results are shown in Table 4 and exhibit that the basic assumption about the interrelation between the budgetary components is satisfied.

According to the results presented in Table 2, unlike in Ade (2014) the change to the direct election of the mayor does not have an effect on the total level of expenditure. This observation is, as mentioned, not surprising in the Austrian context. Most revenues are obtained through fiscal equalization and transfers. Whereas higher financial autonomy of municipalities would give a mayor an incentive to reduce inefficient expenditure, if accompanied by a reduction in tax burden on the voters, this incentive is nonexistent for the case of Austrian municipalities due to the common pool problem in taxation. As expected, we observe a change in the composition of spending. After the reform, municipalities that elect the mayor directly spend on average 35 Euro per capita less on public administration (of 218 Euro on average, that is 16%), compared with those that have not changed the electoral system. Similarly, we observe a drop in the expenditure on private services (91 Euro of the average 649 in this group of municipalities, that is 14%) and on personal costs (32 Euro, 9% of the average). On the other hand, municipalities with directly elected mayors spend more on transportation (79 Euro, 40% of the average) and promotion of the economy (24 Euro, 20% of the average). The latter category comprises mostly subsidies and grants to industries, agriculture, and forestry. Other categories of expenditure are not affected by the reform. This finding is understandable in the context of the Austrian federal structure, which leaves little decision–making power to the local authorities. Most expenditure in the health and education sectors is regulated at the central or state level: these are the so-called compulsory expenditures (*Pflichtausgaben*) that constitute a high majority of health and education expenditures, and are legally predetermined (both in the composition and size) at the higher level of government and cannot be changed by the mayor in a discretionary way.

As for Hypothesis 2, Tables 2 and 3 reveal additional information. We read from Table 2, that the budget cycle exists in both types of electoral regime in the social expenditure, and, but only weakly, in cultural expenditure. Table 3 additionally reveals that social expenditure increases more ih the communes that elect the mayors directly over the whole electoral period. The discrepancy between the significance of the Diff-in-Diff dummy be-

---

[6]Some differences in the coefficients between Tables 2 and 5 stem mostly from numerical differences in the two different STATA routines used in the estimations: *diff* and *suest*.

tween Tables 2 and 3 stems from the inclusion of the interaction term between the cycle and the direct election. No difference between the two systems in the overall social expenditure can be, thus explained by the fact that this type of expenditure is generally higher throught the electoral period in the direct system, whereas it increases stronly in the pre–election year in the indirect system. These observations are precisely in line with the predictions of Drazen and Eslava (2010) and Hypothesis 2. Other categories of the expenditure do not show strong political cycles.

Although a direct assesment of the efficiency gains and economic effects cannot be conducted with the available data, a switch from personnel and administrative expenditure towards transportation infrastructure and subsidies to industries can be hypothesized to have a positive impact on the long–term development of the local economy. Therefore, although driven by electoral motives, budgetary conduct of directly elected mayors is associated with arguably more productive expenditure patterns, although we cannot exclude the possibility that the subsidies involve corrupt behavior.

As for other political variables, concentration of power in the municipal council is generally associated with higher total expenditure levels as well as expenditure on services and personnel. We observe strong party effects in expenditure social support, and personnel costs: left–wing mayors spend more on these categories and less on culture and economic promotion. Moreover, in municipal cases where the mayor comes from one of the two main national parties, OVP and SPO, the the expenditure in finance (repayment of debt) is higher. In most categories of expenditure, whenever the directly elected mayor belongs to a different party than the majority in the council, expenditure tends to be lower.

Table 5: Difference in differences – Seemingly Unrelated Estimations

	(1) Total Expenditure	(2) Administration	(3) Security	(4) Education	(5) Culture	(6) Social	(7) Health	(8) Transport	(9) Economy	(10) Services	(11) Finance	(12) Personnel
Reform	-4.43	18.76	10.07	-67.25***	0.33	-39.62**	-24.32***	-74.87***	12.47	133.23***	14.94	-2.87
	(-0.23)	(1.12)	(1.06)	(-2.64)	(0.03)	(-2.38)	(-2.90)	(-3.99)	(1.25)	(3.64)	(0.52)	(-0.22)
Treatment	-57.34*	36.89**	28.72	-57.53	-23.44	-41.84*	45.75*	-75.43*	43.26	60.77	-62.91	58.34**
	(-1.75)	(1.99)	(1.46)	(-1.14)	(-1.55)	(-1.76)	(1.84)	(-1.80)	(1.59)	(0.76)	(-1.01)	(2.06)
Diff-in-diff	-19.25	-36.71***	-12.81	-9.88	-16.12*	11.87	-9.71	79.75***	22.59**	-97.76***	43.41*	-28.42***
	(-0.93)	(-2.95)	(-1.30)	(-0.45)	(-1.74)	(0.96)	(-1.14)	(5.24)	(2.15)	(-2.93)	(1.68)	(-2.64)
HHI	80.06**	7.84	23.67	88.77**	5.33	-33.20*	3.23	-59.97	31.48	137.73**	-134.90***	17.15
	(2.13)	(0.55)	(1.30)	(2.21)	(0.39)	(-1.85)	(0.23)	(-1.37)	(1.43)	(2.26)	(-2.75)	(1.10)
Single Party	-8.22	-7.35	-0.74	-80.80***	-2.63	20.07**	28.78***	3.62	-12.16	-49.37	96.57***	28.76***
	(-0.37)	(-0.75)	(-0.07)	(-3.11)	(-0.33)	(2.23)	(3.34)	(0.12)	(-0.92)	(-1.18)	(3.11)	(3.12)
Turnout	63.70	-53.50	-111.92***	-38.62	5.85	82.25	170.65***	-99.87**	89.40**	184.23	-134.92	65.16
	(0.99)	(-0.81)	(-3.35)	(-0.59)	(0.33)	(1.63)	(5.42)	(-2.00)	(2.33)	(1.37)	(-1.59)	(1.34)
Lists	10.09	-3.55	7.57***	10.53	0.63	5.72	17.43**	-8.36	-11.25***	-10.94	0.49	18.62***
	(1.64)	(-1.42)	(2.78)	(1.52)	(0.40)	(1.58)	(2.23)	(-1.20)	(-3.18)	(-1.03)	(0.06)	(3.29)
SPO	-26.40	-7.36	20.10	-52.39	-12.30	58.40**	91.61*	-58.24	36.81**	-276.41***	230.48***	128.61***
	(-0.60)	(-0.28)	(0.73)	(-1.29)	(-0.94)	(2.48)	(1.68)	(-1.45)	(2.03)	(-3.73)	(3.77)	(3.48)
FPO	-79.50	12.56	29.60	8.45	14.93	163.44***	-96.66***	-47.22	155.30***	-218.39***	-100.61	177.37***
	(-1.54)	(0.41)	(1.53)	(0.19)	(1.58)	(6.37)	(-5.70)	(-0.94)	(5.60)	(-2.75)	(-1.42)	(7.82)
OVP	1.18	12.97	28.43*	-29.62	17.38**	70.76***	-42.74***	-108.55***	21.49	-131.07**	165.94***	55.01***
	(0.04)	(0.55)	(1.66)	(-0.93)	(2.17)	(3.27)	(-3.29)	(-3.09)	(1.63)	(-2.10)	(3.19)	(4.26)
Divided	8.92	-45.54***	2.18	42.33**	-4.17	-30.29***	9.18	1.98	-71.55***	97.14**	9.74	-43.62***
	(0.20)	(-3.76)	(0.17)	(1.65)	(-0.60)	(-2.81)	(0.75)	(0.08)	(-5.48)	(2.08)	(0.17)	(-3.49)
Incumbent	8.59	-11.98	-16.96**	-42.18**	-9.01**	18.27**	-14.90	39.65	8.35	-54.57	86.76***	-16.99*
	(0.62)	(-1.24)	(-2.15)	(-2.21)	(-2.15)	(2.06)	(-1.44)	(1.33)	(1.09)	(-1.63)	(3.76)	(-1.79)
1 Year Before	12.99	2.78	6.49	-30.01**	5.81	36.15	-3.26	-7.09	8.10	-3.44	-2.00	2.86
	(1.19)	(0.46)	(1.54)	(-2.43)	(1.54)	(1.41)	(-0.66)	(-0.90)	(1.18)	(-0.13)	(-0.11)	(0.42)
Election Year	17.36*	5.73	1.78	-9.23	5.62	29.56*	10.66**	-0.26	8.22	-15.51	-17.13	14.62*
	(1.66)	(1.20)	(0.34)	(-0.69)	(1.61)	(1.72)	(2.34)	(-0.03)	(1.25)	(-0.74)	(-1.01)	(1.89)
1 Year After	0.49	-4.61	3.77	12.85	-0.91	22.21	1.42	8.71	1.16	-28.74	-13.52	2.40
	(0.05)	(-1.14)	(0.92)	(0.59)	(-0.29)	(1.39)	(0.33)	(0.91)	(0.22)	(-1.58)	(-0.92)	(0.36)
Unemployment	-201.43***	-38.07**	0.66	-72.86	-8.76	4.85	-39.65***	-182.37*	32.27	71.17	28.25	21.22
	(-2.69)	(-2.05)	(0.04)	(-1.41)	(-0.92)	(0.25)	(-3.82)	(-1.84)	(1.36)	(0.67)	(0.38)	(1.40)
Profit Shares	-0.05	0.15***	-0.00	-0.27**	0.05**	-0.01	0.10***	-0.23***	0.18*	-0.21	0.21	0.13***
	(-0.29)	(3.07)	(-0.06)	(-2.48)	(2.21)	(-0.21)	(3.27)	(-2.76)	(1.69)	(-0.77)	(1.02)	(2.60)
Taxes	0.07	0.04	-0.04*	-0.19***	-0.01	0.07***	-0.04***	-0.09	0.48***	-0.27**	0.12	0.13***
	(1.28)	(1.28)	(-1.66)	(-2.68)	(-1.21)	(3.44)	(-2.89)	(-1.05)	(9.03)	(-2.08)	(1.25)	(7.13)
Revenue	0.96***	0.03***	0.02***	0.07***	0.00**	0.01**	0.02***	0.07***	0.05***	0.51***	0.18***	0.04***
	(81.20)	(4.24)	(3.73)	(4.83)	(2.27)	(2.28)	(5.05)	(2.67)	(4.85)	(16.14)	(8.20)	(7.97)
Young	-172.56*	39.11	63.96**	3.09	29.67	51.93	-70.19***	-263.03**	-149.87***	173.30	-63.41	66.23**
	(-1.69)	(1.17)	(2.00)	(0.03)	(1.37)	(1.34)	(-3.19)	(-2.30)	(-3.49)	(1.10)	(-0.50)	(2.27)
Retired	25.89	-5.26	104.83	129.35	-15.85	-156.00*	-56.25	9.17	-43.97	592.10**	-525.53***	-153.50***
	(0.19)	(-0.12)	(1.49)	(0.91)	(-0.38)	(-1.88)	(-1.56)	(0.08)	(-0.60)	(2.57)	(-2.75)	(-2.87)
Pop Density	-0.15	0.20**	0.07*	-0.24**	-0.07*	0.27***	0.78***	0.00	0.01	-1.42***	0.32**	-0.29**
	(-1.25)	(2.25)	(1.81)	(-2.52)	(-1.79)	(3.33)	(8.71)	(0.06)	(0.15)	(-6.96)	(2.20)	(-2.16)
Inhabitants	-0.01	-0.03***	-0.01**	0.02**	0.01**	-0.01	-0.09***	-0.04***	-0.01***	0.16***	-0.03**	0.04***
	(-0.53)	(-3.11)	(-2.11)	(2.57)	(2.50)	(-1.63)	(-8.10)	(-3.91)	(-2.80)	(7.70)	(-2.16)	(2.99)
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2634	2634	2634	2634	2625	2634	2634	2626	2634	2634	2634	2634

t-Statistics with robust clustered S.E. in parentheses, * 0.1 ** 0.05 *** 0.01

4.1. Robustness analysis

Table 9 in the Appendix presents the results of the estimations that correct for the potential outliers, thus taking into account the asymmetric distribution of the outcome variables, including only the observations below the 90th percentile of the outcome variable. By this, we eliminate some extreme values of the dependent variable, which might contribute to the significance of the coefficients. Additionally, Table 10 in the Appendix presents the results of the estimation without the municipalities with the city or market status. These corrections do not change the main conclusions: a significant decrease in the spending on public expenditure and personnel costs is observed. A known problem whenever using propensity score matching is a potential for too low standard errors, as estimation steps add variation beyond the normal sampling variation. One way to deal with this problem is to use bootstrapping as suggested e.g. by Lechner (2002), amongst others. Table 11 in the Appendix presents the results of the estimation with bootstrapped standard errors.

In all robustness checks we can observe that the main pattern of the results remains visible. A decrease in the administrative expenditure remains in all cases significant at 1% level. The same is true for the drop in the personnel expenditure. Exclusion of cities and markets as well as bootstrapping the errors does not change any of the main conclusions about the significance and signs of the positive coefficients on transportation and promotion of the economy variables. The effect on the promotion of the economy does however turn insignificant if we remove the 10% of the observations with the highest expenditure levels by each category.

5. Conclusions

It is often hypothesized that a direct election of a president or a mayor can lead to a reduction in the size of the public sector. In this work, we show that the local expenditure levels are not necessarily lower if the mayor is elected directly. In our sample, this result relies on the fact that local governments self-finance their activities to a limited extent. Unlike in the case of Swiss municipalities, which rely mostly on their own taxation, Austrian local governors face incentives to keep expenditure levels high. The common pool problem in this case does not allow for a full expression of fiscal responsibility.

We do find, however, that the composition of spending changes in the municipalities with directly elected mayors: the expenditures on public adminstration, public personnel, and public services are lower, whereas expenditure of infrastructure and subsidies to the economy increases. We link the latter finding to the opportunistic behavior of mayors, who wish to guarantee reelection by shifting expenditure towards visible categories. Opportunistic behavior in this case, however, can be associated with positive effects for the economy: a shift from administrative expenditure to investments in the infrastructure and subsidies for the economy can contribute to stimulating the long–term growth of the region.

References

Ade, F., 2014. Do constitutions matter? Evidence from a natural experiment at the municipality level. Public Choice 160 (3-4), 367–389.

Akhmedov, A., Zhuravskaya, E., 2004. Opportunistic political cycles: Test in a young democracy setting. The Quarterly Journal of Economics, 1301–1338.

Blume, L., Döring, T., Voigt, S., 2008. Fiskalische Effekte der Kommunalverfassungsreformen der 1990er Jahre in Deutschland/Reforming local constitutions in Germany during the 1990stheir fiscal effects. Jahrbücher für Nationalökonomie und Statistik, 317–344.

Bracco, E., Porcelli, F., Redoano, M., 2013. Incumbent effects and partisan alignment in local elections: A regression discontinuity analysis using Italian data.

Costa-Font, J., Moscone, F., 2009. The impact of decentralization and inter-territorial interactions on Spanish health expenditure. In: Spatial Econometrics. Springer, pp. 167–184.

Drazen, A., Eslava, M., 2010. Electoral manipulation via voter-friendly spending: Theory and evidence. Journal of Development Economics 92 (1), 39–52.

Egger, P., Koethenbuerger, M., Smart, M., 2007. Disproportionate influence? special-interest politics under proportional and majoritarian electoral systems. Tech. rep., Working Paper, University of Munich.

Feld, L. P., Kirchgässner, G., 1999. Public debt and budgetary procedures: Top down or bottom up? Some evidence from Swiss municipalities. In: Fiscal institutions and fiscal performance. University of Chicago Press, pp. 151–180.

Foucault, M., Madies, T., Paty, S., 2008. Public spending interactions and local politics. empirical evidence from French municipalities. Public Choice 137 (1-2), 57–80.

Hayo, B., Neumeier, F., 2012. Leaders' impact on public spending priorities: The case of the German Länder. Kyklos 65 (4), 480–511.

Hessami, Z., 2014. Electoral rules for mayors and incentives to pork-barrel: Quasi-experimental evidence from German municipalities.

Houlberg, K., Pedersen, L. H., 2015. Political consensus and fiscal outcomes. Local Government Studies 41 (1), 78–99.

Köthenbürger, M., Egger, P., Smart, M., 2013. Do electoral rules make legislators differently responsive to fiscal transfers? Evidence from German municipalities.

Lechner, M., 2002. Some practical issues in the evaluation of heterogeneous labour market programmes by matching methods. Journal of the Royal Statistical Society: Series A (Statistics in Society) 165 (1), 59–82.

Nordhaus, W. D., 1975. The political business cycle. The Review of Economic Studies, 169–190.

Pommerehne, W. W., 1978. Institutional approaches to public expenditure: Empirical evidence from Swiss municipalities. Journal of Public Economics 9 (2), 255–280.

Rogoff, K., et al., 1990. Equilibrium political budget cycles. American Economic Review 80 (1), 21–36.

Sanz, I., Velazquez, F. J., de Economía Europea, G., 2002. Determinants of the composition of government expenditure by functions. Universidad Complutense de Madrid. Grupo de Economía Europea.

Veiga, L. G., Veiga, F. J., 2007. Political business cycles at the municipal level. Public Choice 131 (1-2), 45–64.

Villa, J. M., 2014. DIFF: Stata module to perform differences in differences estimation. Statistical Software Components.

Vorarlberg, 2015.
    URL http://www.vorarlberg.at/wahlen

Werck, K., Heyndels, B., Geys, B., 2008. The impact of central places on spatial spending patterns: Evidence from Flemish local government cultural expenditures. Journal of Cultural Economics 32 (1), 35–58.

Appendix

Table 6: Summary statistics of the outcome variables

	No Change		Change to Direct		
	Mean	Std. Dev.	Mean	Std. Dev.	Diff. (t-test)
Total Expenditure	2688.111	2365.352	2106.484	1621.859	-6.441
Administration	291.119	210.122	213.56	135.966	-9.735
Security	95.52	190.921	52.528	77.523	-6.131
Education	298.928	239.441	313.101	273.327	1.385
Culture	52.136	70.192	60.212	82.667	2.620
Social	146.925	128.269	175.2	127.759	5.260
Health	106.241	101.186	142.163	189.849	6.644
Transport	175.793	229.784	176.551	222.966	0.006
Economy	217.752	419.033	120.377	330.404	-6.001
Services	994.706	1321.105	635.425	724.01	-7.309
Finance	308.815	511.054	224.92	387.57	-4.215
Personnel	306.102	289.319	335.64	237.402	2.425

Table 7: Summary statistics of the control variables

	Mean	Std. Dev.	Min	Max
HHI	0.623	0.301	0	1
Turnout	0.854	0.115	0.485	1
Lists	2.465	1.353	1	8
Unemployment	0.243	0.137	0.022	0.097
Profit Shares	539.527	225.619	175.300	1882.694
Taxes	427.751	570.209	37.856	6050.783
Revenue	2299.323	1895.870	492.705	22601.465
Young	0.186	0.040	0.031	0.363
Retired	0.135	0.028	0.015	0.236
Pop Density	236.408	276.060	7.931	1299.034
Inhabitants	3721.462	6318.428	128	45922

Table 8: Unit root tests of the dependent variables (including deterministic trends)

	Im-Pesaran-Shin Z-tilde	Fisher ADF Mod. Inv χ^2
Total expendiutre	-19.68	32.75
Administration	-19.25	35.80
Security	-24.38	59.77
Education	-18.78	27.30
Culture	-20.29	41.15
Social	-18.12	26.85
Health	-16.43	24.81
Transport	-20.25	40.95
Economy	-21.28	42.43
Services	-20.53	37.49
Finance	-23.44	55.81
Personnel	-6.41	3.81

Figure 2: Propensity scores

22
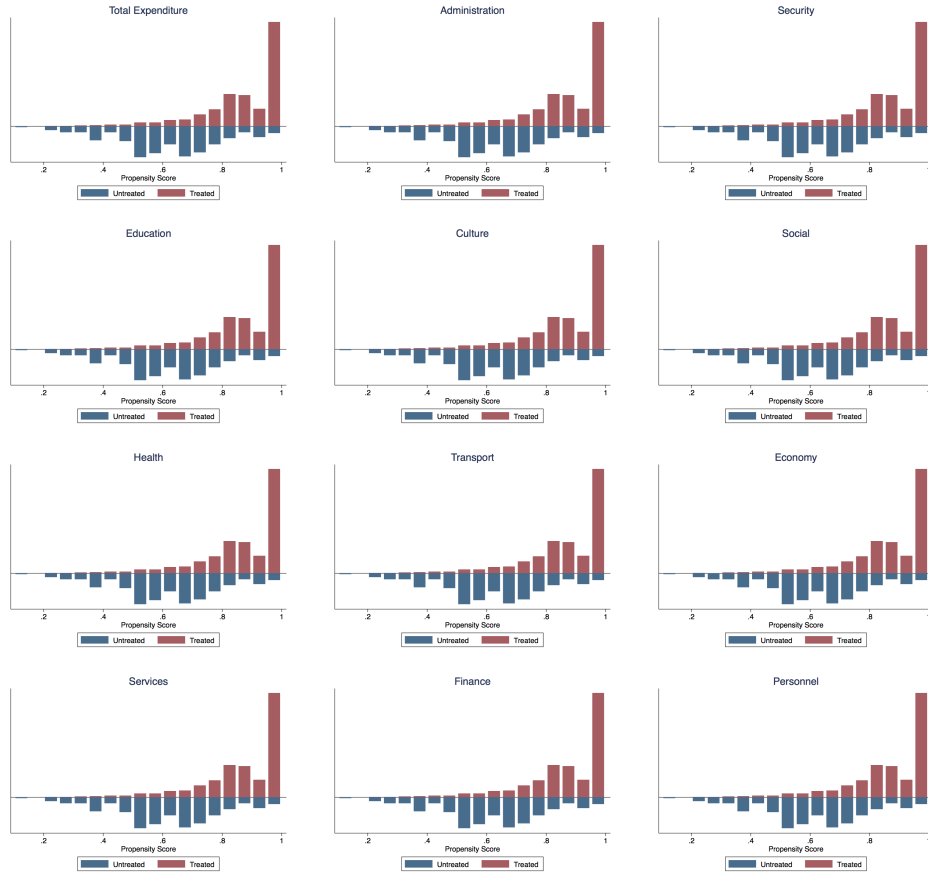
Figure 3: Matching on the outcome variable: Total Expenditure

Figure 4: Matching on the outcome variable: Administration

Figure 5: Matching on the outcome variable: Security

Figure 6: Matching on the outcome variable: Education

Figure 7: Matching on the outcome variable: Culture

Figure 8: Matching on the outcome variable: Social

Figure 9: Matching on the outcome variable: Health

Figure 10: Matching on the outcome variable: Transport

Figure 11: Matching on the outcome variable: Economy
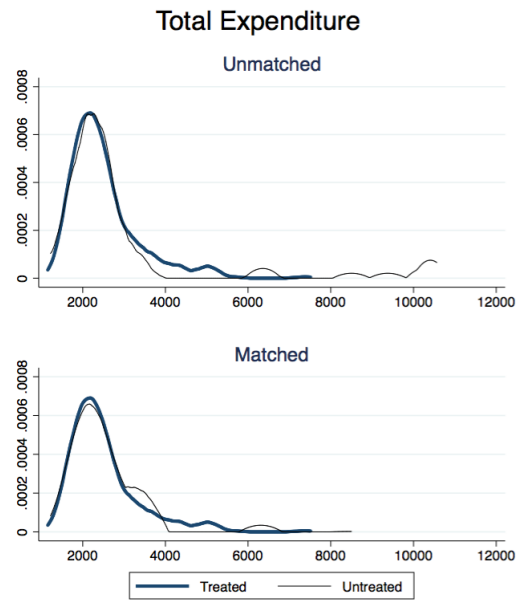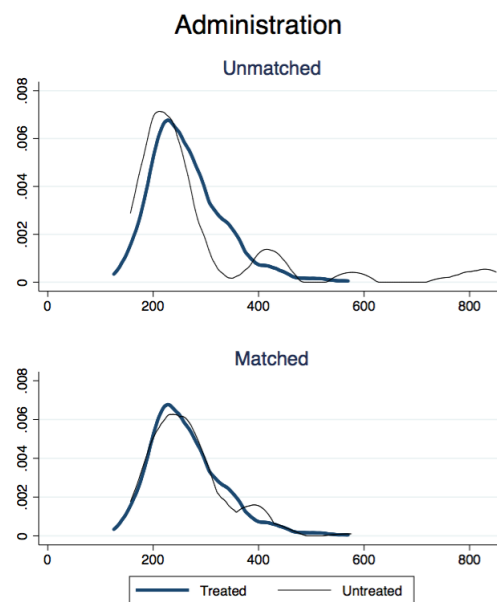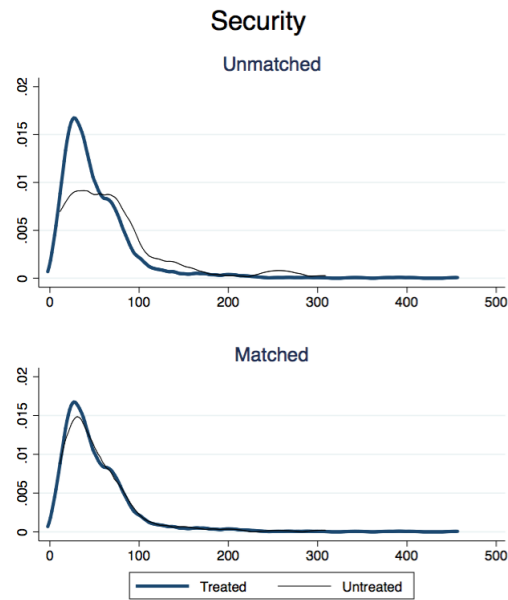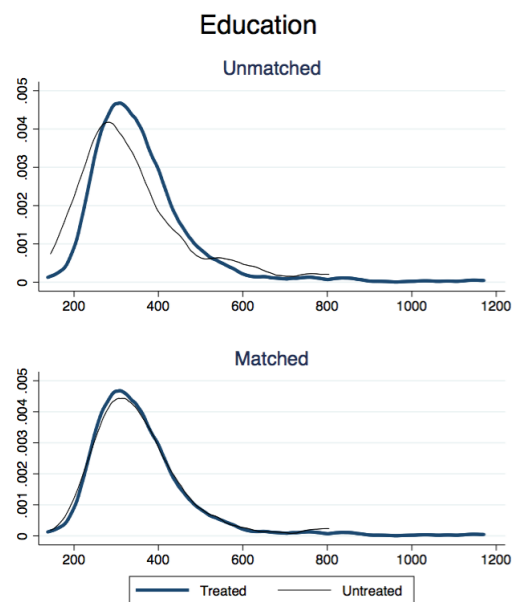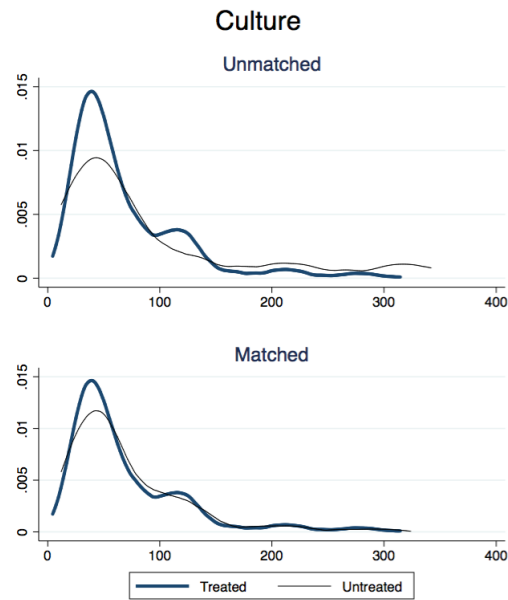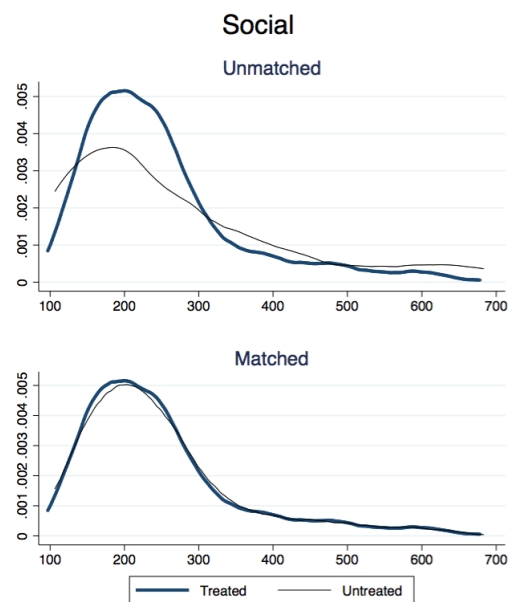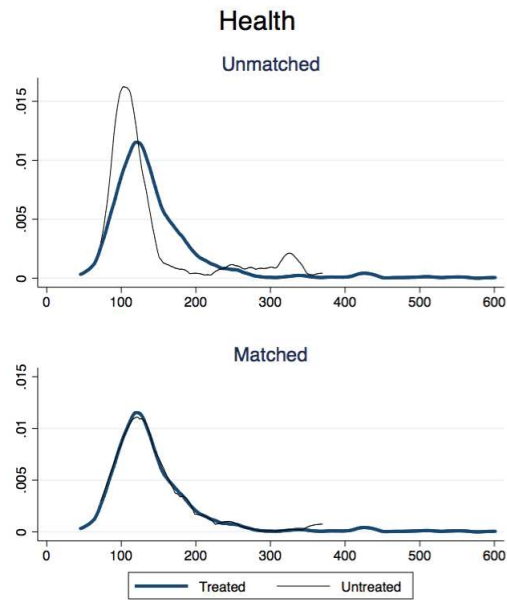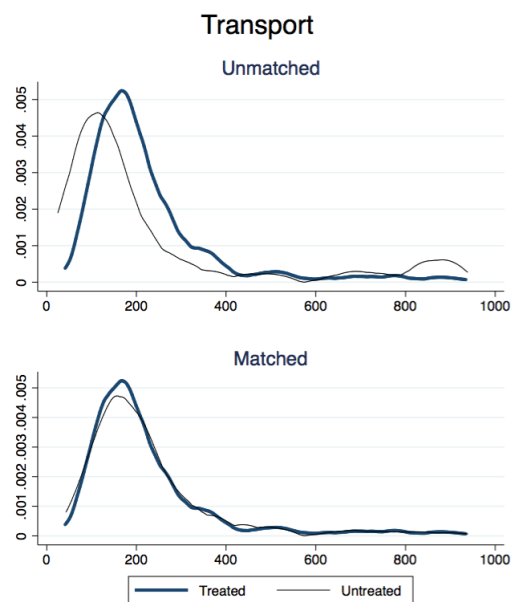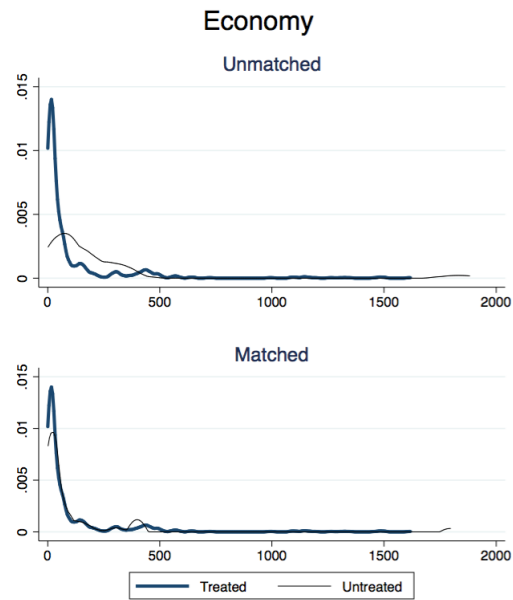

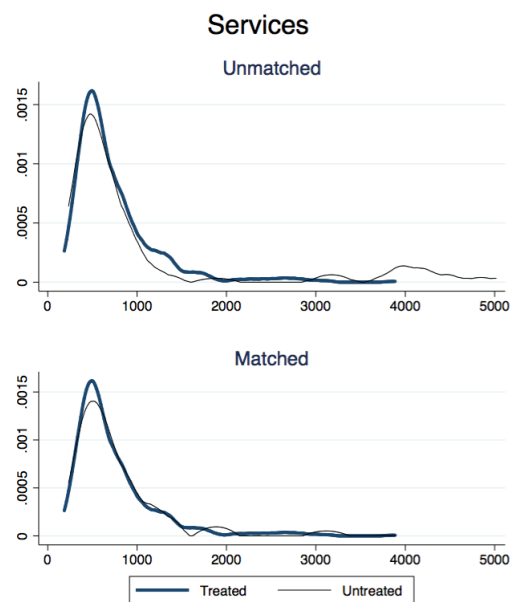Figure 12: Matching on the outcome variable: Services

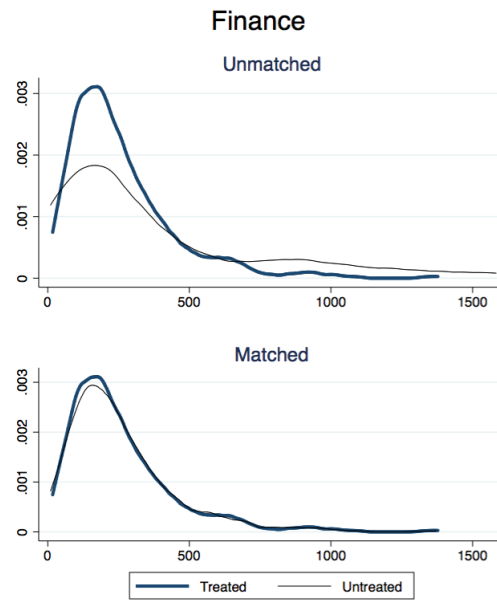Figure 13: Matching on the outcome variable: Finance

Finance

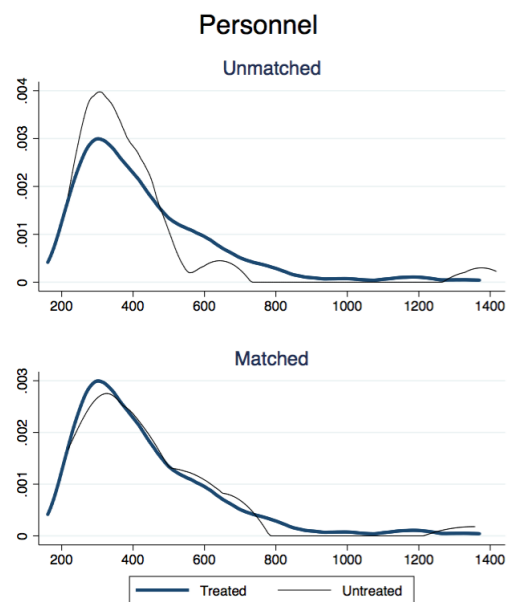Figure 14: Matching on the outcome variable: Personnel

Personnel

Table 9: Difference in differences – Outlier correction: 90th percentile cutoff

	(1) Total Expenditure	(2) Administration	(3) Security	(4) Education	(5) Culture	(6) Social	(7) Health	(8) Transport	(9) Economy	(10) Services	(11) Finance	(12) Personnel
Reform	-10.14	24.77***	2.14	-42.23***	-7.71**	-27.33***	-20.01***	-56.31***	12.24	126.32***	20.39	-0.81
	(-0.58)	(3.63)	(0.34)	(-2.82)	(-2.18)	(-3.48)	(-2.67)	(-5.80)	(1.47)	(4.11)	(1.04)	(-0.09)
Treatment	-56.48	36.41**	15.19	-72.89**	0.98	-31.46*	15.83	-63.25***	54.83***	6.75	-23.49	53.47**
	(-1.42)	(2.33)	(1.05)	(-2.13)	(0.12)	(-1.76)	(0.92)	(-2.85)	(2.89)	(0.10)	(-0.52)	(2.54)
Diff-in-Diff	-9.72	-42.98***	-0.52	-7.85	-2.03	1.10	-0.23	60.83***	2.39	-74.66**	8.52	-29.71***
	(-0.58)	(-6.53)	(-0.09)	(-0.55)	(-0.59)	(0.15)	(-0.45)	(6.49)	(0.30)	(-2.52)	(0.45)	(-3.34)
HHI	32.77	0.40	0.45	87.73***	-4.94	-24.90*	10.64	-58.32***	27.81*	144.74***	-111.63***	17.64
	(1.07)	(0.03)	(0.04)	(3.38)	(-0.80)	(-1.84)	(0.82)	(-3.42)	(1.91)	(2.71)	(-3.28)	(1.09)
Single Party	16.03	-1.36	5.89	-48.77***	0.26	17.86**	-0.30	10.16	-6.16	-56.18	39.51*	27.15**
	(0.79)	(-0.17)	(0.80)	(-2.82)	(0.06)	(1.98)	(-0.03)	(0.90)	(-0.64)	(-1.58)	(1.75)	(2.53)
Turnout	34.93	-50.18**	-101.94***	2.93	-17.74	48.00**	126.21***	-21.34	81.56***	277.94***	-119.41**	91.90***
	(0.66)	(-2.41)	(-5.27)	(0.06)	(-1.64)	(2.00)	(5.52)	(-0.72)	(3.22)	(2.96)	(-2.00)	(3.25)
Lists	5.71	-3.81	3.78	9.33	1.10	1.07	-3.74	-7.63**	-6.39**	-9.98	0.21	15.73***
	(0.84)	(-1.43)	(1.53)	(1.60)	(0.80)	(0.35)	(-1.25)	(-2.01)	(-1.97)	(-0.83)	(0.03)	(4.35)
SPO	-16.91	21.97	35.33**	1.31	-8.35	28.30	170.33***	-26.03	47.94**	-210.43***	178.29***	168.64***
	(-0.37)	(1.21)	(2.08)	(0.03)	(-0.88)	(1.35)	(8.27)	(-1.01)	(2.19)	(-2.60)	(3.44)	(6.85)
FPO	-61.22	47.39**	13.37	39.47	9.21	134.34***	-70.47***	4.44	204.86***	-154.05*	-67.15	167.96***
	(-1.23)	(2.42)	(0.74)	(0.93)	(0.91)	(5.92)	(-3.30)	(0.16)	(8.67)	(-1.76)	(-1.20)	(6.37)
OVP	10.24	39.08***	13.35	13.87	9.16	44.16***	-31.36**	-68.37***	35.30**	-101.09*	115.65***	47.18***
	(0.31)	(3.01)	(1.11)	(0.49)	(1.38)	(2.91)	(-2.23)	(-3.72)	(2.27)	(-1.76)	(3.13)	(2.72)
Divided	10.44	-43.70***	3.49	46.34	-9.95	-17.47	-6.74	3.57	-51.81***	100.12	11.62	-63.69***
	(0.30)	(-3.18)	(0.27)	(1.54)	(-1.37)	(-1.11)	(-0.45)	(0.18)	(-3.11)	(1.62)	(0.29)	(-3.42)
Incumbent	7.48	-7.33	-15.16***	-32.72***	-13.62***	-1.27	5.29	-2.42	2.86	-37.21	47.23***	-7.88
	(0.53)	(-1.34)	(-2.98)	(-2.72)	(-4.79)	(-0.20)	(0.88)	(-0.31)	(0.43)	(-1.51)	(3.00)	(-1.06)
1 Year Before	6.84	0.80	3.24	-20.02**	2.75	9.36**	1.87	1.65	9.80**	11.59	-3.04	5.27
	(0.74)	(0.22)	(0.97)	(-2.54)	(1.48)	(2.22)	(0.48)	(0.32)	(2.24)	(0.72)	(-0.29)	(1.08)
Election Year	9.37	-0.86	-1.75	-1.59	3.88**	29.01***	13.63***	10.57**	7.88*	-0.69	-19.30**	16.98***
	(1.07)	(-0.25)	(-0.55)	(-0.21)	(2.17)	(7.40)	(3.62)	(2.17)	(1.88)	(-0.04)	(-1.97)	(3.65)
1 Year After	-1.30	-3.29	-0.94	15.12**	-2.16	23.76***	4.16	8.45*	2.37	-28.73*	2.93	5.40
	(-0.16)	(-1.31)	(-0.31)	(2.12)	(-1.27)	(6.38)	(1.16)	(1.82)	(0.60)	(-1.96)	(0.31)	(1.23)
Unemployment	-187.58***	-43.97***	-2.73	-64.18**	-7.81	12.19	-30.21**	-47.91**	-32.91**	65.52	-21.81	29.37*
	(-5.67)	(-3.40)	(-0.23)	(-2.29)	(-1.18)	(0.83)	(-2.16)	(-2.54)	(-2.10)	(1.14)	(-0.59)	(1.68)
Profit Shares	-0.17**	0.07**	-0.03	-0.05	0.04***	-0.02	0.07***	-0.05	0.10***	0.16	0.07	0.17***
	(-2.33)	(2.43)	(-1.34)	(-0.91)	(2.76)	(-0.83)	(2.81)	(-1.49)	(2.95)	(1.35)	(0.90)	(4.61)
Taxes	0.07**	0.05***	0.02**	-0.01	0.01	0.08***	0.00	0.04**	0.31***	-0.11**	-0.03	0.13***
	(2.18)	(4.21)	(2.04)	(-0.53)	(0.95)	(6.84)	(0.09)	(2.40)	(20.33)	(-2.19)	(-0.90)	(8.55)
Revenue	0.96***	0.02***	0.01***	0.03***	0.00	0.00	0.01***	0.02***	0.03***	0.40***	0.11***	0.03***
	(174.78)	(9.60)	(5.08)	(6.24)	(0.51)	(1.09)	(2.94)	(7.41)	(11.30)	(41.55)	(18.31)	(11.72)
Young	-173.68***	69.82***	58.23***	0.20	3.19	16.27	-73.55***	-32.36	6.66	187.63*	109.39	52.60*
	(-2.84)	(2.93)	(2.66)	(0.00)	(0.26)	(0.60)	(-2.85)	(-0.95)	(0.23)	(1.75)	(1.59)	(1.65)
Retired	76.78	26.77	2.92	201.37***	-24.86	-62.64	-18.57	28.47	-73.91*	588.93***	-504.04***	-99.96**
	(0.87)	(0.78)	(0.09)	(2.73)	(-1.62)	(-0.50)	(-1.41)	(0.58)	(-1.76)	(3.84)	(-5.08)	(-2.17)
Pop Density	-0.18	0.16***	0.02	-0.22**	-0.06**	0.23***	0.65***	-0.00	-0.09	-1.48***	0.12	0.04
	(-1.46)	(3.23)	(0.51)	(-2.06)	(-2.55)	(4.14)	(12.03)	(-0.03)	(-1.43)	(-6.72)	(0.88)	(0.51)
Inhabitants	-0.00	-0.02***	-0.00	0.02*	0.01**	-0.00	-0.08***	-0.02***	-0.01	0.16***	-0.00	0.01
	(-0.11)	(-4.48)	(-0.88)	(1.78)	(2.35)	(-0.62)	(-14.08)	(-3.38)	(-0.86)	(6.90)	(-0.27)	(0.63)
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2337	2347	2346	2346	2339	2340	2352	2333	2345	2343	2336	2339

t-Statistics with robust clustered S.E. in parentheses, * 0.1 ** 0.05 *** 0.01

Table 10: Difference in differences – Outlier correction: No cities and markets

	(1) Total Expenditure	(2) Administration	(3) Security	(4) Education	(5) Culture	(6) Social	(7) Health	(8) Transport	(9) Economy	(10) Services	(11) Finance	(12) Personnel
Reform	-3.02	19.00**	11.43	-98.81***	3.56	-36.11***	-16.44*	-56.45***	13.12	125.18***	31.63	-6.99
	(-0.13)	(2.09)	(1.09)	(-3.71)	(0.49)	(-3.61)	(-1.90)	(-2.75)	(0.98)	(3.25)	(1.03)	(-0.78)
Treatment	-58.86	35.39*	30.44	-64.69	-24.28	-51.79**	21.99	-75.87*	39.32	103.58	-73.47	40.69**
	(-1.18)	(1.77)	(1.32)	(-1.10)	(-1.49)	(-2.34)	(1.15)	(-1.68)	(1.33)	(1.22)	(-1.08)	(2.06)
Diff-in-diff	-17.04	-31.82***	-12.83	2.69	-17.39**	11.20	4.54	64.54***	30.63**	-103.69***	35.69	-13.64
	(-0.78)	(-3.62)	(-1.27)	(0.10)	(-2.45)	(1.15)	(0.54)	(3.25)	(2.35)	(-2.78)	(1.20)	(-1.58)
HHI	77.34**	7.51	24.58	75.26*	4.93	-27.18	-5.65	-65.91*	39.13*	158.54**	-135.39***	7.88
	(2.00)	(0.49)	(1.38)	(1.66)	(0.39)	(-1.59)	(-0.38)	(-1.89)	(1.71)	(2.42)	(-2.60)	(0.52)
Single Party	-6.86	-10.10	1.13	-76.23**	-3.33	11.41	33.16***	4.14	-17.27	-49.82	100.96***	25.20**
	(-0.26)	(-0.97)	(0.09)	(-2.51)	(-0.39)	(0.99)	(3.35)	(0.18)	(-1.12)	(-1.13)	(2.88)	(2.47)
Turnout	86.35	-62.71**	-123.89***	-27.45	-0.64	61.17*	153.91***	-66.06	87.43**	211.86*	-148.02	114.51***
	(1.20)	(-2.17)	(-3.73)	(-0.32)	(-0.03)	(1.92)	(5.58)	(-1.01)	(2.05)	(1.73)	(-1.52)	(4.03)
Listen	11.13	-2.94	10.31**	20.28*	-0.19	3.80	16.90***	-9.85	-9.25*	-12.03	-6.06	17.10***
	(1.18)	(-0.78)	(2.37)	(1.83)	(-0.06)	(0.91)	(4.69)	(-1.16)	(-1.66)	(-0.75)	(-0.48)	(4.61)
SPO	-16.40	-1.28	15.47	-61.54	16.38	72.94**	-70.72***	-14.48	78.89*	-310.73***	259.91***	66.77**
	(-0.23)	(-0.05)	(0.48)	(-0.75)	(0.72)	(2.36)	(-2.64)	(-0.23)	(1.90)	(-2.61)	(2.74)	(2.42)
FPO	-102.22	-1.12	25.23	11.66	20.95	159.86***	-99.87***	-23.39	180.23***	-222.89**	-151.11*	173.98***
	(-1.57)	(-0.04)	(0.84)	(0.15)	(0.99)	(5.55)	(-4.00)	(-0.40)	(4.67)	(-2.02)	(-1.71)	(6.78)
OVP	-7.83	8.31	26.68	-31.64	21.27	64.09***	-55.89***	-102.26***	20.01	-113.08	155.65***	28.37*
	(-0.19)	(0.50)	(1.40)	(-0.65)	(1.59)	(3.52)	(-3.54)	(-2.75)	(0.82)	(-1.62)	(2.79)	(1.75)
Divided	4.14	-49.00***	3.47	54.49	-4.58	-41.32**	-6.27	-3.90	-83.59***	132.45*	1.38	-89.61***
	(0.09)	(-2.72)	(0.17)	(1.03)	(-0.31)	(-2.08)	(-0.36)	(-0.10)	(-3.14)	(1.74)	(0.02)	(-5.06)
Incumbent	10.48	-12.56	-18.76**	-54.32**	-4.65	27.75***	5.10	68.60***	7.69	-98.72***	90.35***	3.49
	(0.53)	(-1.59)	(-2.06)	(-2.35)	(-0.73)	(3.18)	(0.68)	(3.85)	(0.66)	(-2.95)	(3.38)	(0.45)
1 Year Before	11.21	0.56	8.15	-43.36***	6.37	20.61***	0.59	-12.07	11.42	9.73	9.08	2.89
	(0.90)	(0.11)	(1.42)	(-2.97)	(1.58)	(3.75)	(0.12)	(-1.08)	(1.55)	(0.46)	(0.54)	(0.59)
Election Year	18.98	3.49	2.55	-13.18	6.82*	10.57**	9.02*	-6.96	9.97	-1.77	-1.39	11.25**
	(1.58)	(0.72)	(0.46)	(-0.93)	(1.73)	(1.99)	(1.96)	(-0.64)	(1.40)	(-0.09)	(-0.09)	(2.37)
1 Year After	-0.46	-8.26*	5.66	4.55	-0.29	8.77*	0.31	7.02	1.23	-18.65	-0.61	1.29
	(-0.04)	(-1.77)	(1.06)	(0.33)	(-0.08)	(1.71)	(0.07)	(0.67)	(0.18)	(-0.94)	(-0.04)	(0.28)
Unemployment	-277.78***	-78.41***	-9.57	-89.05	-16.30	-6.94	-34.35*	-212.28***	52.51*	47.45	68.99	3.11
	(-5.78)	(-4.07)	(-0.43)	(-1.58)	(-1.05)	(-0.33)	(-1.87)	(-4.79)	(1.85)	(0.58)	(1.06)	(0.16)
Profit Shares	-0.08	0.14***	-0.00	-0.37***	0.03	0.00	0.13***	-0.25***	0.17***	-0.20	0.27***	0.14***
	(-1.00)	(4.20)	(-0.05)	(-3.86)	(1.19)	(0.00)	(4.09)	(-3.47)	(3.60)	(-1.42)	(2.46)	(4.32)
Taxes	0.09***	0.04***	-0.04**	-0.20***	-0.01	0.07***	-0.03**	-0.09***	0.49***	-0.23***	0.09**	0.16***
	(2.72)	(3.09)	(-2.53)	(-4.89)	(-1.25)	(4.31)	(-2.05)	(-2.87)	(23.98)	(-4.00)	(2.03)	(11.60)
Revenue	0.96***	0.03***	0.02***	0.07***	0.01***	0.01***	0.01***	0.07***	0.05***	0.51***	0.18***	0.03***
	(167.07)	(14.81)	(7.23)	(10.45)	(2.75)	(2.81)	(5.04)	(13.94)	(14.44)	(51.99)	(23.72)	(11.71)
Young	-161.75**	44.63	71.43**	-29.62	38.15	35.19	-42.63	-280.30***	-161.30***	212.99	-51.11	38.56
	(-2.11)	(1.45)	(2.02)	(-0.33)	(1.53)	(1.04)	(-1.45)	(-4.02)	(-3.55)	(1.64)	(-0.49)	(1.28)
Retired	-33.41	-59.82	105.71**	-144.12	-26.01	-231.67***	-18.77	-9.85	-20.60	741.33***	-372.44**	-193.57***
	(-0.30)	(-1.33)	(2.05)	(-1.10)	(-0.71)	(-4.68)	(-0.44)	(-0.10)	(-0.31)	(3.90)	(-2.46)	(-4.39)
Pop Density	0.39	0.51***	0.12	-0.35	-0.09	0.44***	1.11***	0.05	-0.10	-1.09**	-0.20	1.07***
	(1.34)	(4.44)	(0.89)	(-1.05)	(-0.97)	(3.47)	(10.08)	(0.17)	(-0.60)	(-2.23)	(-0.52)	(9.44)
Inhabitants	-0.06*	-0.06***	-0.01	0.05	0.01	-0.02	-0.15***	-0.03	-0.00	0.11*	0.04	-0.14***
	(-1.76)	(-4.09)	(-0.82)	(1.19)	(0.85)	(-1.25)	(-10.74)	(-0.82)	(-0.05)	(1.70)	(0.84)	(-9.42)
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2199	2199	2199	2199	2190	2199	2199	2191	2199	2199	2199	2199

t-Statistics with robust clustered S.E. in parentheses, * 0.1 ** 0.05 *** 0.01

Table 11: Difference in differences – Bootstrapped standard errors

	(1) Total Expenditure	(2) Administration	(3) Security	(4) Education	(5) Culture	(6) Social	(7) Health	(8) Transport	(9) Economy	(10) Services	(11) Finance	(12) Personnel
Reform	-5.35	17.94	9.99	-76.29***	-0.26	-37.38**	-21.10**	-74.76***	11.10	127.71***	25.74	0.35
	(-0.26)	(1.01)	(1.03)	(-2.94)	(-0.03)	(-2.48)	(-2.31)	(-3.97)	(1.26)	(3.15)	(0.76)	(0.02)
Treatment	-57.26*	36.95**	28.73*	-56.77*	-23.81	-42.03*	45.48**	-75.65***	43.40	61.24	-63.82	58.07***
	(-1.96)	(2.16)	(1.88)	(-1.71)	(-0.83)	(-1.78)	(2.30)	(-4.49)	(1.02)	(1.13)	(-1.21)	(3.24)
Diff-in-diff	-18.14	-35.72***	-12.71	1.02	-15.47*	9.16	-13.59	79.43***	24.24**	-91.10***	30.37	-32.30**
	(-0.87)	(-2.88)	(-1.40)	(0.04)	(-1.78)	(0.70)	(-1.39)	(5.00)	(2.41)	(-2.63)	(0.98)	(-2.48)
HHI	80.06**	7.84	23.67	88.77**	5.26	-33.20*	3.23	-59.76	31.45	137.73**	-134.90**	17.15
	(1.99)	(0.50)	(1.38)	(2.09)	(0.41)	(-1.92)	(0.20)	(-1.42)	(1.39)	(2.18)	(-2.46)	(1.29)
Single Party	-8.22	-7.35	-0.74	-80.80***	-2.56	20.07**	28.78***	3.47	-12.23	-49.37	96.57***	28.76**
	(-0.37)	(-0.61)	(-0.07)	(-3.39)	(-0.33)	(2.48)	(3.21)	(0.13)	(-0.86)	(-1.19)	(3.19)	(2.70)
Turnout	63.85	-53.37	-111.91***	-37.21	5.68	81.90	170.15***	-99.87**	89.57**	185.09	-136.61	64.66
	(0.97)	(-0.83)	(-3.11)	(-0.61)	(0.31)	(1.57)	(4.69)	(-2.27)	(2.32)	(1.49)	(-1.57)	(1.41)
Lists	10.80	-2.91	7.64***	17.60**	0.87	3.96	14.91*	-8.68	-10.20**	-6.62	-7.95	16.10**
	(1.42)	(-1.16)	(2.94)	(2.52)	(0.38)	(1.00)	(1.91)	(-1.57)	(-2.54)	(-0.60)	(-0.95)	(2.45)
SPO	-25.98	-6.98	20.14	-48.31	-12.06	57.39***	90.16	-57.86	37.44*	-273.92***	225.60***	127.16***
	(-0.57)	(-0.28)	(0.74)	(-1.06)	(-0.76)	(2.84)	(1.36)	(-1.36)	(1.96)	(-3.32)	(3.25)	(3.52)
FPO	-79.23*	12.80	29.63	11.08	15.23	162.78***	-97.60***	-46.83	155.71***	-216.78***	-103.76	176.44***
	(-1.76)	(0.42)	(1.55)	(0.24)	(1.33)	(6.78)	(-4.48)	(-1.32)	(5.38)	(-2.78)	(-1.32)	(8.91)
OVP	2.13	13.82	28.52*	-20.23	17.76*	68.42***	-46.09***	-108.40**	22.92	-125.33*	154.72***	51.67***
	(0.07)	(0.62)	(1.91)	(-0.53)	(1.83)	(3.80)	(-3.07)	(-2.45)	(1.56)	(-1.86)	(2.61)	(4.46)
Divided	9.64	-44.89***	2.24	49.48*	-4.09	-32.07***	6.63	1.64	-70.48***	101.51**	1.20	-46.17***
	(0.20)	(-3.39)	(0.16)	(1.69)	(-0.55)	(-3.02)	(0.44)	(0.07)	(-5.15)	(2.00)	(0.02)	(-5.38)
Incumbent	8.72	-11.87	-16.95**	-40.89**	-8.96**	17.95*	-15.36	39.55	8.54	-53.78*	85.22***	-17.44*
	(0.58)	(-1.18)	(-2.08)	(-2.45)	(-2.40)	(1.83)	(-1.51)	(1.29)	(1.24)	(-1.76)	(3.71)	(-1.70)
1 Year Before	12.99	2.77	6.49	-30.06***	5.79	36.16	-3.24	-7.11	8.10	-3.47	-1.93	2.88
	(1.08)	(0.51)	(1.49)	(-2.69)	(1.62)	(1.43)	(-0.63)	(-0.92)	(1.04)	(-0.13)	(-0.09)	(0.41)
Election	17.29*	5.67	1.78	-9.90	5.57	29.73*	10.89**	-0.21	8.11	-15.91	-16.33	14.86*
	(1.66)	(1.25)	(0.27)	(-0.86)	(1.54)	(1.77)	(2.33)	(-0.02)	(1.09)	(-0.73)	(-0.85)	(1.91)
1 Year After	0.46	-4.63	3.76	12.62	-0.94	22.27	1.50	8.70	1.14	-28.88	-13.24	2.48
	(0.06)	(-1.24)	(0.92)	(0.60)	(-0.34)	(1.56)	(0.34)	(0.80)	(0.21)	(-1.38)	(-1.00)	(0.42)
Unemp.	-202.55***	-39.08**	0.55	-83.88	-8.55	7.58	-35.72***	-182.19**	30.65	64.43	41.42	25.14
	(-2.69)	(-2.52)	(0.03)	(-1.57)	(-0.85)	(0.36)	(-3.17)	(-2.22)	(1.39)	(0.59)	(0.48)	(1.51)
Profit Shares	-0.05	0.14***	-0.00	-0.29***	0.05*	-0.01	0.10***	-0.23***	0.18	-0.22	0.23	0.13***
	(-0.32)	(3.01)	(-0.06)	(-2.91)	(1.93)	(-0.13)	(3.63)	(-2.79)	(1.40)	(-0.79)	(1.14)	(3.20)
Taxes	0.07	0.04	-0.04	-0.19***	-0.01	0.07***	-0.04**	-0.09	0.48***	-0.27**	0.11	0.13***
	(1.39)	(1.14)	(-1.54)	(-2.82)	(-1.14)	(3.52)	(-2.41)	(-0.97)	(9.58)	(-2.48)	(1.19)	(7.18)
Revenue	0.96***	0.03***	0.02***	0.07***	0.00**	0.01**	0.02***	0.07***	0.05***	0.51***	0.18***	0.04***
	(79.34)	(4.82)	(3.70)	(5.38)	(2.08)	(2.16)	(4.69)	(2.69)	(4.98)	(17.96)	(8.72)	(7.67)
Young	-172.12*	39.50	64.00*	7.38	29.19	50.86	-71.72***	-263.35**	-149.31***	175.92	-68.54	64.71**
	(-1.67)	(1.24)	(1.85)	(0.06)	(1.17)	(1.05)	(-3.37)	(-2.22)	(-3.90)	(0.89)	(-0.57)	(1.97)
Retired	18.69	-11.69	104.18	58.66	-17.49	-138.44*	-31.06	12.79	-54.58	548.90**	-441.04*	-128.33**
	(0.15)	(-0.26)	(1.34)	(0.46)	(-0.45)	(-1.67)	(-0.85)	(0.13)	(-0.75)	(2.56)	(-1.81)	(-2.34)
Pop Density	-0.15	0.20**	0.07*	-0.24**	-0.07*	0.28***	0.78***	0.00	0.01	-1.42***	0.33**	-0.29*
	(-1.23)	(2.27)	(1.94)	(-2.17)	(-1.75)	(3.13)	(7.14)	(0.04)	(0.13)	(-7.89)	(2.23)	(-1.75)
Inhabitants	-0.01	-0.03***	-0.01**	0.02***	0.01**	-0.01*	-0.09***	-0.04***	-0.01***	0.16***	-0.03*	0.05***
	(-0.49)	(-3.06)	(-2.23)	(2.87)	(2.18)	(-1.65)	(-6.49)	(-3.76)	(-2.70)	(9.26)	(-1.91)	(2.41)
FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Time	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Observations	2634	2634	2634	2634	2625	2634	2626	2634	2634	2634	2634	2634

t-Statistics with bootstrapped (50 repetitions) S.E. in parentheses, * 0.1 ** 0.05 *** 0.01

31