

# Opportunistic Political Cycles: Test in a Young Democracy Setting<sup>∇</sup>

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## Abstract

Despite the fact that theoretical research on opportunistic political cycles is very intuitive and well developed, empirical literature has found fairly weak evidence of opportunistic political cycles. This paper tests the theory in a decade-old democracy – Russia. We find strong evidence of very short opportunistic political cycles and provide evidence and explanation why many previous attempts to find evidence failed. Using the comprehensive list of Russia's regional elections and regional monthly panel data between 1996 and 2001, we find that: (1) opportunistic political cycles in regional fiscal policies are sizable and short-lived on average; (2) the magnitude of opportunistic cycles decreases with voters' rationality and awareness (measured by urbanization, computerization, education, and freedom of media); (3) there is a learning curve for voters: cycles become smaller with time; (4) cycles in fiscal policies increase political popularity and chances for re-election of incumbent governors. Our results confirm that maturity of democracy as well as rationality and awareness of electorate are very important factors in determination of the scope for opportunistic cycles.

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## **Abstract**

Despite the fact that theoretical research on opportunistic political cycles is very intuitive and well developed, empirical literature has found fairly weak evidence of opportunistic political cycles. This paper tests the theory in a decade-old democracy – Russia. We find strong evidence of very short opportunistic political cycles and provide evidence and explanation why many previous attempts to find evidence failed. Using the comprehensive list of Russia's regional elections and regional monthly panel data between 1996 and 2001, we find that: (1) opportunistic political cycles in regional fiscal policies are sizable and short-lived on average; (2) the magnitude of opportunistic cycles decreases with voters' rationality and awareness (measured by urbanization, computerization, education, and freedom of media); (3) there is a learning curve for voters: cycles become smaller with time; (4) cycles in fiscal policies increase political popularity and chances for re-election of incumbent governors. Our results confirm that maturity of democracy as well as rationality and awareness of electorate are very important factors in determination of the scope for opportunistic cycles.

*“You can fool some of the people all of the time, and all of the people some of the time, but you cannot fool all of the people all of the time.”*

(Attributed to Abraham Lincoln (1809-1865))

## 1. Introduction

Despite the fact that theoretical research on opportunistic political cycles is very intuitive and well developed, empirical literature produced mixed results in attempts to find convincing evidence of opportunistic cycles for almost a quarter of a century after pioneering work of Nordhaus (1975).<sup>1</sup> This apparent contradiction between the theory and evidence created an intellectual puzzle. Why did many tests fail? Should the theory or the empirics be held responsible? Motivated by this gap, several recent works argued that opportunistic cycles should be most sizable in developing countries with immature democratic regimes.<sup>2</sup> Although, the evidence has been strongly supportive of this view, the tests using data from the developing countries have not been very high-powered due to data limitations.<sup>3</sup> This paper attempts to shed further light on this puzzle by putting the opportunistic cycle theory to one more test using regional monthly panel data from a decade-old democracy.

According to the theory, asymmetric information and irrationality magnify the size of opportunistic cycles.<sup>4</sup> Thus, to ensure the maximum possible size of the cycles, the tests should be done in environment with these properties. This paper uses data from Russia’s regional elections of executives. First, Russian democracy is very young and many regions are notorious for governor’s control over mass media and large fraction of uninformed, naïve and myopic electorate. Second, detailed monthly

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<sup>1</sup> See Drazen (2000) and Franzese (2002) for detailed surveys of theoretical and empirical literatures.

<sup>2</sup> See, for instance, Gonzalez (2000).

<sup>3</sup> See Gonzalez (2000), Shi and Svensson (2001), Block (2001), Block, et. al. (2001), Krueger and Turan (1993).

<sup>4</sup> Rogoff and Sibert (1988), Rogoff (1990), Gonzalez (2000).

regional-level data allows more powerful tests of the theory than the ones that have been done so far. And third, high regional variation in possible determinants of opportunistic cycles, on the one hand, and high uniformity in many (would-be-important and missing in most papers) controls like electoral institutions, scope for policymaking, culture, trust, etc., on the other, help to analyze how cycles vary with voters' rationality, awareness, and learning. Thus, Russia provides an ideal case for an empirical test of opportunistic political cycles. Indeed, we find very strong evidence of cycles in fiscal policies of Russian governors.

Two strands of literature, opportunistic political business cycles and partisan theory, explain inefficient economic fluctuations around elections. The literature on opportunistic cycles argues that electoral pressure forces politicians to manipulate public policy in order to increase chances of reelection with the help of pre-electoral improvements at expense of deterioration after elections. Low asymmetry of information between politicians and the public, rationality of voters, and availability of institutional checks and balances reduce opportunistic cycles. The alternative approach – partisan theory – argues that policies are predetermined by ideology. Economic fluctuations arise as a result of policy changes when different parties alternate in office: each party in office focuses on the short run improvements for its own constituency. Binding commitments to co-operative common policy rule and reputation reduce partisan cycles.

Kalecki (1943) was the first to develop the idea that politicians might alter policies in the face of elections. Subsequently, theory of opportunistic cycles and partisan theory developed in parallel. The theoretical literature came in two waves. The first “non-rational” wave came in 1970s. Nordhaus (1975) built the first opportunistic model based on adaptive expectations of voters. Hibbs (1977) developed the first partisan model. Frey (1978) and Frey and Schneider (1978)

combined the features of opportunistic theory and partisan theory to develop weak partisan theory. The second wave reconciled rational expectations with political cycles. Alesina (1987) built the rational partisan theory that attributed cycles to wage rigidities and uncertainty of election's outcome. Rogoff and Sibert (1988), Rogoff (1990), Persson and Tabellini (1990) modeled rational opportunistic cycles that are based on asymmetry of information between the incumbent politician and the voters. Adaptive expectations models exploited the Phillips curve framework and, thus, predicted cycles in growth, unemployment, and inflation. Opportunistic models based on rational expectations predict cyclical pattern in fiscal policies rather than real outcomes. Recently, Gonzalez (2000) extended the rational models to accommodate importance of the institutional context of policymaking: transparency and maturity of democratic traditions.

Empirical research so far revealed much stronger evidence of partisan than opportunistic cycles; particularly, it is apparent in studies of developed countries. For instance, using quarterly post-war US data, Alesina and Sachs (1988) found strong support for rational partisan theory and Klein (1996) little support for opportunistic cycles. Berger and Woitek (1997) rejected both partisan and opportunistic cycles in Germany on monthly data. Reid (1997) relaxed the assumption of exogeneity of election time and found weak evidence of opportunistic cycles in Canadian provinces. Alesina and Roubini (1992) tested the competing theories in a unified framework, using panel of 18 OECD countries with quarterly data, and found strong robust support for rational partisan cycles and, in selected countries, some evidence of rational opportunistic cycles.

Non-convincing evidence for opportunistic cycles in developed countries motivated the new wave of empirical research using data from developing countries with an emphasis on the determinants of cycles: for instance, development of

democratic institutions and availability of checks and balances. Schuknecht (1998, 2000) found evidence of cycles in budget expenditures and investment based on quarterly panel data for 25 developing countries and showed that cycles are much larger in countries with low inflation costs. Block (2001) used annual data for 69 developing countries to show that budget cycles took place only in countries with sufficient electoral competition. Block, et. al. (2001) confirmed this result on data for 44 African countries. In addition, they found that cycles got weaker as voters learned. Shi and Svensson (2000) analyzed data from 123 countries and also found some evidence of cycles, which is significantly stronger for developing countries. Gonzalez (2000) found some evidence of fiscal cycles in Mexico and showed that they are related to the levels of democracy and transparency. Thus, recent research with the focus on fiscal instruments in developing countries produced more convincing evidence of opportunistic cycles. This evidence, however, is not always as strong as one could expect and as it sounds. Important reasons for this are insufficient frequency of data and lack of appropriate controls; we address these problems in this paper.

Keller and May (1984) used the case of President Nixon's election campaign to argue that one needs to look at the political actions rather than the real economic outcomes to find evidence of opportunistic cycles. Overall, pure (non-rational) theories à la Nordhaus (1975) and Hibbs (1977) are proven to be inconsistent with results of virtually all empirical tests: Drazen (2000) surveys empirical literature to show that, when found, cycles affect fiscal and monetary policies rather than outcomes.

Based on the case of the four federal Russian elections, Treisman and Gimpelson (2001) argued that traditional empirical approach underestimates opportunistic cycles because it considers policy instruments separately, whereas,

politicians change them from one election to another depending on political environment. Large samples, however, should allow observing opportunistic cycles in each policy instrument separately despite this underestimation.

This paper tests for the presence of opportunistic political cycles in Russian regions in a whole range of fiscal policy instruments as well as real economic outcomes. Then, it addresses the question of how rationality of voters, their access to unbiased information, and maturity of democracy affect the magnitude of the cycles. Finally, we study whether opportunistic cycles help governors to get reelected.

We find opportunistic political cycles in many regional fiscal policies controlling for region-specific characteristics, federal trend, seasonality, and fluctuations driven by differences in ideology (partisan cycles). Just as predicted by the rational theory of opportunistic cycles, total budgetary expenditures, spending on education, healthcare, social disbursements, industrial subsidies, and mass media as well as repayments of wage arrears to public workers exhibit clear cyclical pattern. They start to grow approximately nine months before elections, continue rising gradually, and jump high up one month before elections. Right after elections spending drops and wage arrears start to accumulate. Above average inflationary pressure, leads to price rises right after elections. Industrial growth is not significantly affected by elections in contrast to predictions of the first wave of non-rational opportunistic models à la Nordhaus (1975).

Use of monthly panel data allowed us very careful measurement of even very short cycles. This turned out to be very important because most sizable positive changes happen a month before and negative changes a month after elections, thus, lower frequency data would have substantially underestimated cycles. To the best of our knowledge all empirical studies on developing countries so far used quarterly or

annual data.<sup>5</sup> This seems to be the reason for why results have not been very satisfactory: very short cycles, like the ones in Russia, cannot be clearly seen in quarterly data since elections often occur in the middle of the quarter.

What determines the magnitude of opportunistic cycles? We find that our proxies for rationality and informational symmetry (education level, urbanization, computerization, and freedom of media) significantly decrease the amplitude of the cycles. In addition, cycles get smaller with time, which may be an indication of voters' learning as Russia's democracy matures. The results suggest that maturity of democracy is a very important factor determining scope for the effective use of political cycles: it pays in young democracies and it does not in the environments with high voter's rationality and awareness as well as mature democratic institutions. We also find that cycles in fiscal policy instruments significantly increase popularity of incumbents and help them win.

The remainder of the paper is organized as follows. The next section describes the data. Section 3 formulates hypotheses and empirical methodology. Section 4 presents the results. Section 5 concludes.

## **2. The data**

### **2.1. Sample and data sources**

The comprehensive list of regional governor elections that took place in Russia between August 1995 and June 2002 consists of 169 electoral events.<sup>6</sup> We exclude elections in Chechnya and Ingushetia from our sample because the fluctuations in fiscal policies of these regions have been driven by war rather than

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<sup>5</sup> The only paper that uses monthly data is Berger and Voitek (1997). Even though, the data were suited well for the test, they reject hypothesis of opportunistic cycles for the German developed democracy.

<sup>6</sup> This list covers elections in all regions but Dagestan, the only region where the governor is appointed.



elections. Data on most policy instruments and outcomes are available for 149 elections that took place in the period between 1996 and 2001.

The source of data on elections is *Tsentrizbirkom*, the Central Elections Committee of the Russian Federation. Data on regional fiscal policies and outcomes come from two sources: *Goskomstat*, the State Committee of Statistics, that provided monthly data on wages and income, wage arrears including arrears from the regional budgets, price level, and industrial output between 1995 and 2001; and the Ministry of Finance of the Russian Federation that provided detailed monthly regional budgetary data for the period between 1996 and 2001. Data on computerization, urbanization and education also come from *Goskomstat*. The data on freedom of media in the regions were provided by the Institute of Free Media ([www.freepress.ru](http://www.freepress.ru)). Appendix A presents descriptive statistics of the data.

## **2.2. The first glance at the data**

Figures 1 and 2 show dynamics of the four month moving average of the Russia's aggregate of seasonally adjusted de-trended regional policy instruments and outcomes from a year before to a year after elections. Zero-month is the month of elections. The policy instruments and outcomes are in logs and normalized so that zeros on the vertical axes represent the average levels of the variables over the incumbent's term in power. Two horizontal lines on each graph represent average values of the instruments in a year before and in a year after elections.

Total budgetary expenditures, budget deficit, expenditures on culture and media, and share of social expenditures show gradual increase during the year before elections and a sharp drop at the time of elections. Expenditures on healthcare and education peak half a year before elections, remain high till elections, and decline sharply thereafter. Budgetary revenues are relatively smooth and steady before

elections and drop after elections. The average values of all these instruments in the year before elections are higher than in the year after elections (this can be seen from the comparison of horizontal lines). Cyclical changes in social expenditures, industrial and agricultural subsidies are less profound, but in each case elections fall on local maxima. Graphs clearly show opportunistic political cycles in total and regional wage arrears that decrease throughout the year before elections and rebound right after the elections. Industrial output, wage level, and money income peak right before elections as well. Prices steadily decline the first nine months in the pre-election year. Inflationary pressure drives prices up closer to elections. Series of tax revenues and industrial growth do not show a visible cyclical pattern.

Overall, the first glance at the data tentatively suggests that fiscal policies are subject to opportunistic political cycles in the Russian regions. One, however, needs to see whether the visual results survive rigorous econometric analysis.

### **3. Hypotheses and empirical methodology**

#### **3.1. Test of opportunistic cycles**

Theory of opportunistic cycles predicts that governors pursue expansionary fiscal and/or monetary policies before elections irrespective of their ideological platform. Rogoff and Sibert's (1988) model predicts that politicians manipulate fiscal policies in pre-election times with subsequent increase in inflation and no effect on real economic outcomes. Nordhaus's (1975) model predicts that cycles affect real economic activity: unemployment and GDP growth. We test whether these predictions are consistent with the data treating election dates as exogenous. Reid (1997) and Heckelman and Berument (1998) pointed out that opportunistic cycles can also occur as a result of setting election date at a time of a boom. We abstract from

this possibility because less than 15% of Russia’s regional elections happened more than a month of their expected date (the number insufficient for quantitative analysis); moreover, several times elections were shifted exogenously, because governors were promoted to the federal government before their terms ended.<sup>7</sup>

To test the existence of cycles and analyze their duration, we utilize the event study methodology. The following specification of panel regressions with regional fixed-effects was used:

$$\ln(y_{it}) = \alpha + \sum_{j \in \{-12;12\}} \alpha_j m_{jit} + \beta_1 \sum_{j \in \{1;4\}} \ln(y_{it-j}) / 4 + \beta_2 Term_{it} + \beta_3 Left_{it} + f_i + \varepsilon_{it} \quad (1)$$

where  $i$  – region;  $t$  – ordinal number of a month;  $y$  stands for a de-trended monthly instrument or outcome of regional policy.<sup>8</sup>  $m_{jit}$  is a dummy that equals to 1, if  $t$  is  $j$  months away from elections in region  $i$  (negative  $j$  means that  $t$  is prior to elections, positive – that  $t$  is after elections,  $j=0$  in the month of elections);  $f_i$  – regional fixed effects.<sup>9</sup>  $Term$  equals to 0, 1, 2, or 3 depending on which term is the governor serves in office; 0 indicates that the governor is appointed and has not been elected before; 1 indicates that the governor was elected for the first time, etc.  $\sum_{j \in \{1;4\}} \ln(y_{it-j}) / 4$  is the average of the values of the policy instrument in the previous four months; this variable accounts for autocorrelation processes in equation (1).<sup>10</sup> Electoral campaign platforms of Russia’s governors are polarized into the “communist left” and the “liberal-democratic ideology”.<sup>11</sup>  $Left$  is a dummy that equals 1 if the incumbent

<sup>7</sup> It is illegal to shift the date of regional elections.

<sup>8</sup> The detailed description of how the policy instruments were de-trended is presented below in this section.

<sup>9</sup> The regression has a constant term because  $f_i$ ’s joint mean is normalized to zero.

<sup>10</sup> We use smoothed lagged value of the policy instruments because of better fit due to large month-to-month volatility in the data.

<sup>11</sup> Partisan theory implicates *a priori* differences in ideology: “communist left” ideology opts for a larger size of government and larger redistribution. This implies that, *ceteris paribus*, social expenditures, government deficit, and inflation should be greater if communist governors are in power.

governor is supported by the Communist Left wing coalition<sup>12</sup>. This dummy controls for the partisan cycles. The variation in the data is insufficient, however, to have a proper test of this theory because there are only seven cases when new governor of the opposite ideological platform replaced an incumbent governor after elections; and, therefore, ideology of governors is almost perfectly collinear with regional fixed effects. Thus, we do not put emphasis on the interpretation of the sign of coefficient of the “left-wing” dummy.

We consider three groups of policy instruments and outcomes: budgetary expenditures (total budgetary expenditures, expenditures on social programs, education, culture, healthcare, mass media, and subsidies to industry and agriculture), budgetary revenues and deficit (total budgetary revenues, tax revenues, and deficit) and economic performance indicators (growth, inflation, the level of industrial output, total wage arrears and regional wage arrears, wage level, price level and money income).

An important methodological question is how to control for the macroeconomic shocks and the federal policy that affects the regions. In particular, this is essential, because in 1996 at one instance, several regional elections and the federal elections took place in Russia. In order to eliminate the effects of the federal policy (which can also be cyclical in the face of the federal elections) we tried each of the three following options that produced the same results.<sup>13</sup> First, we defined each of the policy instruments as a ratio of the actual value of the instrument to the federal level of this variable (which is calculated as population-weighted average of the regional values). Second, we added the federal trend as regressor in the panel

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<sup>12</sup> The name of the coalition is Peoples Patriots Union of Russia (“*Narodno-Patriotichesky Souz Rossii*”); most governors in this coalition are the members of the Communistic Party of the Russian Federation.

<sup>13</sup> The same approaches were used by Alesina and Roubini (1992).

regression. Third, we added month dummies as regressors. The results of these three approaches are very similar, thus, throughout the paper we use the specifications of the first approach: all considered policy instruments and outcomes are already de-trended.

Another methodological question is how to control for seasonal fluctuations. It is important because a large portion of regional elections had taken place in the same months (elections frequently happen in the end of the year, particularly often in December). We tried the following alternative strategies. First, to control for the common to all regions seasonal fluctuations, it is sufficient to include eleven month dummies in the regression or de-trend just as described in the previous paragraph. Thus, controls for the federal trend take care of common to all regions seasonality. Second, to control for region-specific seasonality, we included fixed effects for each of the region-month combinations (86 regions times 11 months) in equation (1). Each strategy produced very similar results in magnitude with a slight loss of significance in the latter case due to a significant loss of degrees of freedom. Thus, we report regression results for the control for common to all regions seasonality, since most seasonal fluctuations are common to all regions and, therefore, they are subtracted from the policy instruments together with the federal trend.

Significant coefficients at dummies indicating the time distance from elections ( $\alpha_j$ ) point toward the shifts in the autocorrelation process of the policy instrument. Thus, positive significant values of the estimates of  $\alpha_j$  before elections and negative significant values of the estimates of  $\alpha_j$  after elections would serve as evidence of the opportunistic political cycles.

Lags in panel regressions may bias the estimators (Hansen, 1982 and White, 1982). The bias converges to zero when time dimension of a panel goes to infinity.

Our monthly panel covers almost seventy months (six years). Therefore, the asymptotic properties can be applied. Nonetheless, we estimated equation (1) using the Arellano-Bond procedure (Arellano and Bond, 1991). Our results were not changed by this technique.

### 3.2. Determinants of opportunistic cycles

The second step of our analysis is the study of the determinants of the magnitude of opportunistic cycles. We test the predictions of the theory that the size of opportunistic cycles is negatively related to rationality of voters, access to unbiased sources of information, and the maturity of democracy (see, for instance, Rogoff and Sibert (1988) and Gonzalez (2000)).

As a measure of the amplitude of the cycle in a particular policy instrument for a particular election, we take the pre-electoral month deviation of the value of the policy instrument from the regional trend net of seasonal fluctuations. Thus, we define the amplitudes as pre-electoral month residuals from estimation of the following equation separately for each region in the sample:

$$\ln(y_{it}) = \sum_{j \in \{1,12\}} \alpha_j dm_{jt} + \beta Time_t + \varepsilon_{it} \quad (2)$$

As  $y_{it}$ , we used only fiscal policy instruments, since they turned out to have the significant opportunistic cycles after estimation of equation (1).  $dm_{jt}$  in equation (2) stands for month dummies corresponding to 12 calendar months.  $Time_t$  is the real time measured in months. In addition to considering the amplitudes in each budgetary item separately, we took an aggregate measure the magnitude constructed as the first principal component of the amplitudes in individual fiscal policies with the most profound electoral cycle, *viz.*, total budgetary expenditures, social expenditures,

expenditures on culture, education, mass media, and regional industrial subsidies.<sup>14</sup> Summary statistics for each individual proxy of the amplitude are presented in table 1. For all policy instruments, the mean amplitudes are positive and, for ten out of thirteen policy instruments, they are significant.

To examine which factors influence the magnitude of the cycles, we run the following equation on the pooled cross section of elections:

$$A_i = \beta_0 + \beta_1 R_i + \beta_2 Time_i + \beta_3 Left_i + \varepsilon_i \quad (3)$$

where  $i$  is the ordinal number of elections.  $A_i$  is the amplitude of the cycle (as described above).  $R_i$  is a proxy for rationality of voters and their awareness. We use the following measures of  $R_i$ : the share of population with higher education, the number of computers per capita, the share of urban population, and the index of freedom of media production. Negative significant coefficient at  $R_i$  is interpreted as evidence that irrationality or unawareness is associated with higher amplitude of the cycle. Real time ( $Time_i$ ) was included in the regression to find out how cycles change with time, in other words, whether learning by voters and maturity of democracy decrease incentives to use cycles. Learning is not the only possible interpretation of the negative coefficient at  $Time_i$ : an alternative story is the disciplining role of Putin's more centralized administration compared to Yeltsin's time. There has not been any formal institutional change in the regional elections mechanism and authority of regional governments over spending in their budgets from Yeltsin's to Putin's time, however. As before,  $Left_i$  controls for the ideological platform of incumbents. In estimation of equation (3), from the sample we excluded elections that took place in 9 *Autonomous Okrugs*, the regions that are subdivisions of other larger regions.

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<sup>14</sup> The first principal component explains 43% of the total variation in these measures. The factor loadings are as follows: 0.70 (Total Budget Expenditures) + 0.57 (Social Expenditures) + 0.73 (Expenditures on Culture) + 0.77 (Education Expenditures) + 0.29 (Media Expenditures) + 0.04 (Expenditures on Industry).

### 3.3. Do cycles help wining?

Finally, we investigate whether it pays to pursue cyclical opportunistic policies. Russia's incumbent governors have been active in trying to retain power (95% of them ran for re-election) and they have been more successful than the opposition in attaining votes (66.5% of those who ran for another term won).<sup>15</sup> This is an impressive score considering deep recession that Russia experienced for most of the transition period. To test whether cycles helped incumbent governors to get re-elected, we estimate how the probability to win and the share of incumbents' votes depend on the amplitude of the cycles and the time trend, controlling for the ideology of incumbent governor and governor's performance in the last term. We also test if the effect of cycles on the probability to win depends on proxies for rationality and learning. We estimate the following equations on the pooled cross section of elections:

$$\ln(P_i) = \gamma_0 + \gamma_1 A_i + \gamma_2 (R_i - \bar{R})(A_i - \bar{A}) + \gamma_3 (R_i - \bar{R})(Time_i - \overline{Time}) + \gamma_4 Left_i + \gamma_5 Time_i + \gamma_6 Perform_i + \gamma_7 R_i + \varepsilon_i \quad (4)$$

$Prob\{\text{incumbent - below the 2}^{\text{nd}} \text{ place; incumbent - the 1}^{\text{st}} \text{ runner up; incumbent wins}\}_i =$

$$= \phi_0 \gamma_0 + \phi_1 A_i + \phi_2 (R_i - \bar{R})(A_i - \bar{A}) + \phi_3 (R_i - \bar{R})(Time_i - \overline{Time}) + \phi_4 Left_i + \phi_5 Time_i + \phi_6 Perform_i + \phi_7 R_i + \varepsilon_i \quad (5)$$

where  $P_i$  is the popularity of incumbent measured by the ratio of votes pro- to votes against incumbent on elections  $i$ .  $A_i$  is the proxy for the aggregate amplitude of the cycles prior to elections  $i$ , equal to the first principal component of the main cyclical fiscal policies as in equation (3).  $R_i$  are our proxies for rationality and awareness. The cross-terms  $(R_i - \bar{R})(A_i - \bar{A})$  and  $(R_i - \bar{R})(Time_i - \overline{Time})$  are included to test whether

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<sup>15</sup> This proportion is significantly larger than a half at 1% significance level. Table 1 presents summary statistics of Russia's regional elections.



rationality, awareness, and learning reduce the effectiveness of cyclical policies.<sup>16</sup> Positive significant coefficient at  $A_i$  is an indication that cycles help to get reelected. Negative coefficients at cross-terms mean that rationality, awareness and time reduce the influence of the amplitude on chances of reelection. Control variables are as follows:  $Left_i$  is as described above.  $Perform_i$  is a column vector of proxies for governor's performance in the last term comprised of the differences between the overall regional means and the regional means over the last term for the following variables: log share of social expenditures, log labor productivity and log price level.<sup>17</sup>

Equation (5) is the multivariate ordered probit model with the same set of regressors as in (4) and with three outcomes for the incumbent: wins, is the first runner up, is below the second place. We also estimate simple probit model with two outcomes: wins/loses. Both approaches produce almost identical results. In estimation of equations (4) and (5), from the sample we exclude elections with the single candidate, elections, in which incumbent did not run for reelection and did not name his successor, and elections in *Autonomous Okrug*s. In addition, we used Cook's distance approach (Cook, 1977) to exclude four outliers that had excessively strong effect on our estimates.

There is an endogeneity problem in estimation of equations (4) and (5), however. If governors are sure of winning because their rating is too high, they would have weak incentives to use cyclical policy since it is costly in the long run. In this case, the tighter the electoral competition, the higher the incentives for pre-electoral manipulations. Thus, this link from popularity and probability to win to the incentives for cyclical behavior implies negative correlation between popularity and cycles.

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<sup>16</sup> Upper bars denote the mean values. Subtraction of means before taking the cross-term does not change the interpretation of the coefficient at the cross-term, but makes the interpretation of the coefficients of  $A_i$  easier: it becomes equal to the full effect of the amplitude, evaluated at  $\bar{R}$  and  $\bar{Time}$ .

<sup>17</sup>  $\gamma$  and  $\phi$  are row vectors.

Thus, if there are governors that are sure of re-election, we are likely to underestimate a positive causal relationship between the magnitude of the cycles and the probability to win with regressions (4) and (5).<sup>18</sup> One could argue that too low probability to win may also reduce incentives for cyclical policy because budgetary resources instead of being spent in favor of voters could be diverted by incumbents who are sure of losing. This would have implied that we overestimate the causal effect of the magnitude of the cycles on popularity. There have not, however, been regional elections so far in Russia where incumbent governors did not have a good chance of winning. Therefore, overall we are likely to underestimate the effect.<sup>19</sup>

## 4. Results

The results of estimation of equation (1) are presented in tables 2 and 3. Table 2 presents regression results for fiscal policy instruments. Total budgetary expenditures experience the first significant jump up of 5.5% nine months before elections; then, up until month 3 prior to elections, there are no significant rises in expenditures (coefficients are mostly positive, but insignificant); at month 3 prior to elections, the second significant jump up in total expenditures of 6% occurs, the next and the biggest rise in expenditures of 11% happens one month before elections. The election month and the month right after the elections are characterized by the significant falls in total budget expenditures of 6% and 5%, respectively. We do not find significant changes in total expenditures after the month 2. Budgetary expenditures on education, culture, and healthcare follow the same but slightly more profound pattern. Education expenditures rise significantly by 6% twelve months

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<sup>18</sup> Cook's distance approach eliminated observations in which incumbents have the highest political rating. Therefore, it partly eliminated this problem.

<sup>19</sup> Instrumenting the amplitude is the way to solve this problem, but we do not have a good instrument.

prior to elections. All three expenditure items jump up by 14, 12, and 23%, respectively, in the three consecutive months, eight months before elections. Six months before elections, education and healthcare expenditures rise by 5% each. Cultural expenditures fall by 5% four months before elections. Healthcare spending jumps up by 6.5% two months before elections. Education, culture and healthcare expenditures jump up again by 13, 11, and 17%, respectively, one month before elections. These expenditure items fall by 7, 19, and 10% respectively during the three months right after the elections.<sup>20</sup>

Cycles in social spending item are more gradual: it rises significantly for two months, 9 months before elections (by total of 23%) and, then, jumps up each month prior to elections starting four months before elections. During the month right before elections social expenditures rise by 32%. Accumulated growth in social expenditures over the year before elections amounts to 135%, which is much higher than in any other expenditure item. Unlike other fiscal instruments, social expenditures do not drop sharply after elections. Dummies at the first and the second month after elections have small insignificant negative coefficients. Expenditures on industry (e.g., industrial subsidies) rise significantly by 29% a month before elections and fall insignificantly for two months after elections. Cycles in agricultural subsidies are insignificant. The pattern is the same, however: two months before elections, coefficients of the months dummies are positive and two months after – negative.

Cyclical dynamics in these expenditure items is supported by the intensive use of mass media: expenditures on mass media exhibit 34% growth in two pre-electoral months and 22% fall in two post-electoral months. Changes in social and media expenditures are disproportionately large compared to other expenditure items. Share of social expenditures in total expenditures rises significantly by 9, 15, 19, and 15% in

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<sup>20</sup> Changes in healthcare expenditures are consistently negative but insignificant in this period.

months -4, -2, -1, and 0, respectively. Share of media expenditures jumps up by 18% two months before elections.

Budgetary revenues grow by 5% one month before elections and decrease by 14% during two months right after elections. Taxes significantly jump up only three months before elections by 5% and fall right after elections by 6%. Budget deficit, e.g. ratio of expenditures to revenues, exhibits significant increases six, four, and one month before elections (by 8, 4, and 6%, respectively). Thus, incumbent governors pursue expansionary policy while trying not to overburden enterprises with taxation.

Table 3 presents the effect of elections on dynamics of real economic outcomes and wage arrears. Regional growth does not exhibit a significant cyclical pattern. The level of industrial output falls down significantly both before and after elections. This finding can be explained by tougher tax collection before elections (e.g., tax increase 3 months prior to elections) and lower industrial subsidies after elections. Tax reduction after elections can be attributed to post-electoral recession since magnitudes of their changes are similar.

Inflation does not shift significantly around elections, but the price level does. After elections price level increases significantly in months 2, 3, 4, 11, and 12. (Each of these increases, however, is very small – below half a percent). Although price increases after elections are econometrically significant, they are much smaller in magnitude than fluctuations in fiscal policy instruments. Thus, our fiscal policy cycles results hold irrespective of whether we take real or nominal values of the policy instruments.<sup>21</sup> Money income falls slightly in the middle of the year before elections, but grows in three pre-electoral months by 3, 8 and 11%. Wage level grows 2% in the pre-electoral month and falls by approximately 3% in months 3 and 4 after elections.

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<sup>21</sup> We tested for presence of cycles in nominal policy instruments directly (disregarding the price differences between regions). The results were the same.

Arrears in wages to public workers are repaid before elections. Wage arrears decline each month during the whole year prior to elections with the rate of approximately 4% per month and stabilize after elections. Wage arrears from the regional budgets start to decline nine months before elections, continue decreasing with a growing pace up until elections and also stabilize after elections. The total drop in regional wage arrears over the nine months before elections amounts to 77% of the initial level.

Overall, we find very strong evidence of opportunistic cycles in fiscal policies and no evidence of pre-electoral expansion in economic growth. Our results are fully consistent with opportunistic cycles à la Rogoff and Sibert (1988) and inconsistent with opportunistic cycle theory à la Nordhaus (1975): There is no output expansion before elections. Such policies as wage arrears repayments, social expenditures, and spending on mass media are the main instruments of fiscal pre-electoral manipulations. Cycles are very short: the largest shifts in policy instruments occur within a month or two from the election date.

Let us turn to the discussion of determinants of the size of cycles. Table 4 presents the results of estimation of equation (3). We report all regression result for the aggregate measure of the amplitude of the cycles and only those regressions that have significant results for rationality and awareness proxies in the regressions of the amplitudes in individual policy instruments. The data confirm the theoretical prediction that rationality and awareness smoothens cycles. Different measures of rationality and awareness are significant for different measures of the amplitude, but there are many significant results, all of which are of the right sign.

Urbanization and higher education significantly negatively affect the aggregate measure of cycles' magnitude. The other two proxies for rationality and awareness, computerization and the freedom of media, have negative insignificant

coefficients. Presence of educated population significantly reduces cycles in education and healthcare expenditures: A 10% increase in the share of population with higher education decreases the magnitude of the cycles approximately by 2%. Urbanization has negative significant effect on the cycles in total, social, and cultural expenditures: A 10% increase in the share of urban population decreases cycles in total spending and spending on culture by 2% and social spending by 5%. Computerization significantly affects cycles in total budget expenditures: A 10% increase in the number of computers per capita decreases cycles by 0.7%. Freedom of mass media reduces cycles in social and cultural expenditures: A one standard deviation increase in the index of freedom of media leads to a 10.6% decrease in cycles in social expenditures and a 4% decrease in expenditures on culture.

Time, our proxy for learning and maturity of democracy, also negatively significantly affects the size of the cycle. Cycles fade away relatively fast: Each additional year decreases the magnitude of cycles by about 3.5%. Thus, maturity of democracy is an important factor reducing opportunistic cycles. We also estimated the effect of an additional round of regional elections in an average region just as was done by Block, et. al. (2001): An additional election in a region reduces the magnitude of the cycles by approximately 34%. Thus, the second wave of regional elections had substantially smaller cycles. Time in our opinion is a more appropriate measure of learning and maturity of democracy because voters can learn from other electoral events like federal elections, elections in regional legislature, etc.

Table 5 presents the results of the test whether cycles help to get reelected (estimation of equations (4) and (5)). We find strong evidence of political benefits of cyclical policies: *ceteris paribus*, an increase in magnitude of the fiscal policy cycle significantly affects the political rating of the incumbent governor and the probability of re-election. A 10% increase in the magnitude of the cycle leads to a 4% growth in

incumbents' popularity. We should emphasize that we most probably underestimate the effect of the cycles on chances to get re-elected because of possible endogeneity (discussed in the methodology section) and because we assumed the same mix of fiscal policy instruments for the measure of magnitude across regions. Cross-terms are insignificant in all regressions, except that the cross-term of the amplitude and computerization has negative significant coefficient in regression of incumbent's popularity: the effect of cycles on incumbent's popularity decreases with computerization. Thus, there is no robust relationship between the effect of cycles on popularity, on the one hand, and learning and rationality, on the other.

## **5. Conclusion**

In this paper we tested for existence of opportunistic political cycles and studied whether rationality, awareness and maturity of democracy has effect on cycles. We also examined if cycles increase governors' chances to get reelected and if this effect depends on rationality and learning. The monthly regional panel data allowed us to define timing of the cycles more precisely than it has been done in the literature and avoid problems with usually missing institutional controls.

We found very strong evidence that:

- 1) Opportunistic political cycles in budgetary spending have taken place in Russian regions in the period between 1996 and 2001. Most sizable cyclical changes happen within a month away from elections. Short length of the cycles explains why many previous tests of the theory did not find convincing evidence of cycles.
- 2) The magnitude of the cycles decreases with education, urbanization, computerization, and the freedom of media as well as with time. Thus, informational

symmetry, rationality and maturity of democracy are very important factors influencing cycles.

3) The scale of pre-electoral improvements increases popularity of incumbent governors and the probability to get re-elected. This effect is not robustly related to maturity of democracy or proxies for rationality and awareness: Of all our proxies, only computerization decreases the effect of cycles on chances to get reelected.

The main contribution of our paper to the existing literature on opportunistic cycles is in showing why previous studies underestimated the cycles. First, quarterly frequency of data is insufficient: cycles are short-lived. And second, in maturing democracy cycles disappear very fast. Ten years of Russia's democratic tradition very significantly reduced cycles. Each round of regional elections reduces cycles by over 30%.

Russian democracy is very young. Thus, the presence of cycles itself may not be such a bad piece of news because it confirms that there is some electoral pressure on Russia's governors. Thus, so-called "administrative resource", e.g., the ability to influence the vote count, does not completely annihilate political incentives. Evidence suggests that voters learn quite fast, and therefore, Russian governors are bound to respond to the political pressures in increasingly more efficient way.



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**Table 1. Summary statistics for the constructed measures of the amplitude of the cycles**

	<b>Obs.</b>	<b>Mean</b>	<b>Median</b>	<b>S.E.</b>	<b>Min</b>	<b>Max</b>
<b>Amplitude of the Cycle in:</b>						
<b>Total Budget Expenditures</b>	123	0.086*	0.079	0.017	-0.449	0.591
<b>Social Expenditures</b>	125	0.180*	0.144	0.036	-1.164	1.759
<b>Education Expenditures</b>	125	0.087*	0.081	0.016	-0.384	0.494
<b>Expenditures on Culture</b>	125	0.077*	0.117	0.023	-0.723	0.693
<b>Healthcare Expenditures</b>	125	0.080*	0.072	0.021	-0.691	0.823
<b>Media Expenditures</b>	124	0.140*	0.123	0.043	-1.250	1.454
<b>Expenditures on Industry</b>	120	0.237*	0.273	0.064	-1.542	2.154
<b>Ratio of Expenditures to Revenues</b>	123	0.046*	0.054	0.014	-0.456	0.485
<b>Negative of Total Wage Arrears</b>	72	0.059*	0.038	0.015	-0.288	0.389
<b>Negative of Regional Wage Arrears</b>	59	0.148*	0.091	0.041	-0.401	1.115
<b>Expenditures on Agriculture</b>	121	0.058	-0.019	0.051	-1.516	1.613
<b>Total Budget Revenues</b>	125	0.036	0.023	0.019	-0.703	0.575
<b>Negative of Tax Revenues</b>	125	0.006	0.004	0.018	-0.666	1.090

*Note: Stars denote mean amplitudes that are significantly different from zero.*

**Table 2. Cycles in regional budgetary expenditures and revenues.**

	Total Budget Expenditures	Social Expenditures	Education Expenditures	Expenditures on Culture	Healthcare Expenditures	Media Expenditures	Expenditures on Industry	Expenditures on Agriculture	Share of Social Expenditures	Share of Media Expenditures	Total Budget Revenues	Tax Revenues	Ratio of Expenditures to Revenues
<b>Lag</b>	0.458*** [23.04]	0.537*** [31.24]	0.223*** [10.27]	0.337*** [17.16]	0.272*** [12.98]	0.272*** [12.12]	0.501*** [27.76]	0.353*** [16.97]	0.483*** [26.25]	0.284*** [12.77]	0.478*** [24.96]	0.562*** [32.02]	-0.045* [1.81]
<b>Term in power</b>	-0.003 [0.30]	-0.001 [0.03]	0.01 [1.02]	0.02 [1.62]	0.023** [2.15]	-0.129*** [5.76]	-0.139*** [3.52]	-0.011 [0.41]	0.018 [1.20]	-0.110*** [5.38]	-0.008 [0.76]	0.002 [0.26]	0.028*** [3.94]
<b>Left wing party</b>	-0.004 [0.11]	-0.112** [2.00]	0.009 [0.27]	0.015 [0.33]	-0.054 [1.42]	0.006 [0.07]	-0.156 [1.10]	-0.075 [0.76]	-0.116** [2.14]	0.011 [0.15]	0.006 [0.16]	0.015 [0.46]	-0.070*** [2.61]
<b>month -12</b>	-0.005 [0.17]	0.037 [0.83]	0.059** [2.13]	0.055 [1.61]	0.047 [1.60]	0.057 [0.95]	-0.011 [0.10]	0.053 [0.68]	0.041 [0.99]	0.06 [1.09]	0.065** [2.30]	0.031 [1.23]	-0.078*** [3.74]
<b>month -11</b>	-0.012 [0.41]	0.059 [1.31]	-0.004 [0.16]	-0.003 [0.07]	0.015 [0.52]	0.087 [1.45]	-0.044 [0.40]	-0.1 [1.27]	0.072* [1.76]	0.09 [1.61]	0.017 [0.60]	-0.031 [1.23]	-0.024 [1.17]
<b>month -10</b>	0.015 [0.53]	0.094** [2.34]	0.067*** [2.69]	0.039 [1.25]	0.070*** [2.61]	0.117** [1.98]	-0.014 [0.14]	-0.04 [0.56]	0.043 [1.07]	0.098* [1.83]	0.022 [0.87]	-0.018 [0.80]	-0.008 [0.40]
<b>month -9</b>	0.055** [2.00]	0.110*** [2.72]	0.059** [2.37]	0.117*** [3.75]	0.090*** [3.34]	0.073 [1.23]	0.146 [1.44]	0.001 [0.02]	-0.015 [0.38]	-0.031 [0.57]	0.029 [1.14]	-0.019 [0.83]	0.012 [0.58]
<b>month -8</b>	-0.001 [0.05]	0.036 [0.85]	0.024 [0.91]	0.015 [0.47]	0.048* [1.73]	0.034 [0.59]	-0.002 [0.02]	-0.101 [1.34]	0.015 [0.38]	0.053 [1.00]	-0.002 [0.06]	0.004 [0.16]	-0.006 [0.29]
<b>month -7</b>	0.023 [0.92]	0.013 [0.33]	0.025 [1.03]	-0.015 [0.51]	0.036 [1.37]	0.019 [0.34]	0.003 [0.03]	-0.065 [0.93]	-0.052 [1.41]	-0.007 [0.13]	0.003 [0.12]	-0.014 [0.63]	0.007 [0.40]
<b>month -6</b>	0.029 [1.17]	-0.036 [0.90]	0.046* [1.87]	0.023 [0.73]	0.047* [1.79]	-0.102* [1.92]	0.003 [0.03]	-0.015 [0.22]	-0.046 [1.27]	-0.082* [1.68]	-0.029 [1.14]	-0.025 [1.12]	0.040** [2.20]
<b>month -5</b>	0.014 [0.56]	0.063 [1.62]	0.023 [0.95]	-0.045 [1.48]	0.016 [0.61]	-0.021 [0.41]	-0.076 [0.77]	0.023 [0.33]	0.036 [1.04]	-0.049 [1.04]	0.036 [1.48]	0.02 [0.92]	0.008 [0.43]
<b>month -4</b>	0.003 [0.14]	0.063* [1.65]	-0.018 [0.76]	-0.051* [1.72]	-0.015 [0.60]	0.006 [0.11]	-0.071 [0.75]	0.008 [0.12]	0.088** [2.53]	-0.008 [0.18]	-0.032 [1.35]	-0.007 [0.34]	0.030* [1.74]
<b>month -3</b>	0.059** [2.46]	0.071* [1.84]	-0.008 [0.34]	-0.005 [0.18]	0.012 [0.48]	0.012 [0.23]	-0.074 [0.77]	-0.002 [0.03]	0.04 [1.15]	-0.034 [0.72]	0.028 [1.17]	0.054** [2.52]	0.023 [1.31]
<b>month -2</b>	0.021 [0.86]	0.142*** [3.62]	0.018 [0.76]	0.003 [0.11]	0.065** [2.50]	0.203*** [3.90]	0.037 [0.38]	-0.012 [0.17]	0.144*** [4.08]	0.168*** [3.52]	0.022 [0.87]	0.021 [0.96]	0.023 [1.29]
<b>month -1</b>	0.103*** [4.20]	0.278*** [7.06]	0.121*** [4.99]	0.101*** [3.34]	0.115*** [4.40]	0.095* [1.82]	0.258*** [2.63]	0.088 [1.24]	0.179*** [5.03]	0.003 [0.06]	0.046* [1.87]	0.002 [0.08]	0.060*** [3.44]
<b>month 0 - elections</b>	-0.059** [2.40]	0.062 [1.56]	-0.047* [1.93]	-0.055* [1.81]	-0.036 [1.36]	-0.114** [2.16]	0.107 [1.10]	0.058 [0.82]	0.143*** [4.06]	-0.073 [1.51]	-0.080*** [3.24]	-0.005 [0.25]	0.027 [1.56]
<b>month 1</b>	-0.054** [2.23]	-0.041 [1.03]	-0.022 [0.89]	-0.117*** [3.92]	-0.038 [1.48]	-0.084 [1.63]	-0.072 [0.74]	-0.075 [1.09]	0.021 [0.58]	-0.053 [1.11]	-0.072*** [2.93]	-0.067*** [3.08]	0.003 [0.14]
<b>month 2</b>	-0.025 [1.04]	-0.018 [0.49]	-0.005 [0.23]	-0.045 [1.56]	-0.038 [1.55]	0.009 [0.18]	-0.033 [0.35]	-0.105 [1.61]	0.022 [0.62]	0.016 [0.34]	-0.015 [0.66]	-0.02 [0.96]	-0.015 [0.87]
<b>month 3</b>	0.034 [1.44]	0.057 [1.51]	-0.005 [0.24]	-0.041 [1.43]	0.005 [0.19]	-0.019 [0.36]	0.131 [1.42]	-0.047 [0.72]	0.018 [0.52]	-0.069 [1.45]	0.029 [1.25]	-0.003 [0.16]	-0.011 [0.65]
<b>month 4</b>	-0.013 [0.54]	0.034 [0.88]	0.008 [0.32]	-0.011 [0.38]	0.015 [0.58]	-0.024 [0.46]	0.073 [0.75]	-0.007 [0.10]	0.01 [0.28]	-0.014 [0.30]	-0.03 [1.20]	-0.013 [0.62]	0.031* [1.75]
<b>month 5</b>	0.032 [1.37]	0.091** [2.42]	0.046** [1.99]	-0.016 [0.54]	0.038 [1.54]	0.036 [0.71]	0.001 [0.01]	0.005 [0.07]	0.051 [1.50]	-0.007 [0.14]	-0.018 [0.76]	0.002 [0.11]	0.019 [1.11]
<b>month 6</b>	0.006 [0.26]	0.055 [1.45]	0.021 [0.89]	-0.065** [2.26]	-0.008 [0.31]	-0.036 [0.73]	0.058 [0.63]	0.076 [1.14]	0.043 [1.27]	-0.048 [1.05]	-0.016 [0.69]	0 [0.00]	-0.015 [0.89]
<b>month 7</b>	0.019 [0.81]	0.008 [0.21]	-0.003 [0.12]	-0.034 [1.18]	0.017 [0.70]	0.013 [0.27]	-0.097 [1.02]	-0.056 [0.85]	-0.005 [0.16]	-0.026 [0.58]	0.013 [0.55]	0.035* [1.67]	0.001 [0.04]
<b>month 8</b>	0.005 [0.24]	0.037 [0.98]	0.009 [0.37]	-0.045 [1.56]	0.008 [0.31]	-0.036 [0.72]	0.023 [0.25]	0.003 [0.05]	0.037 [1.09]	-0.053 [1.16]	-0.017 [0.72]	-0.008 [0.38]	0.003 [0.19]
<b>month 9</b>	-0.025 [1.04]	0.006 [0.16]	-0.017 [0.72]	-0.066** [2.24]	-0.018 [0.72]	0.074 [1.46]	0.054 [0.57]	0.061 [0.89]	0.028 [0.82]	0.097** [2.08]	-0.002 [0.07]	0.009 [0.44]	-0.019 [1.11]
<b>month 10</b>	0.016 [0.67]	0.079** [2.08]	0.004 [0.19]	0.023 [0.79]	-0.006 [0.24]	0.105** [2.10]	0.048 [0.51]	0.031 [0.46]	0.064* [1.90]	0.062 [1.35]	-0.042* [1.77]	-0.012 [0.59]	0.030* [1.69]
<b>month 11</b>	0.03 [1.30]	0.043 [1.13]	0.01 [0.43]	0.068** [2.34]	0.01 [0.38]	-0.059 [1.16]	-0.016 [0.17]	0.024 [0.35]	0.011 [0.33]	-0.048 [1.03]	0.032 [1.35]	0.021 [0.98]	-0.005 [0.31]
<b>month 12</b>	0.034 [1.32]	0.051 [1.24]	0.038 [1.50]	0.035 [1.09]	0.039 [1.42]	-0.019 [0.35]	-0.024 [0.22]	0.046 [0.62]	0.007 [0.20]	-0.07 [1.38]	0.019 [0.71]	0.047** [2.04]	0.01 [0.60]
<b>Constant</b>	-0.021 [1.04]	-0.038 [1.28]	0.021 [1.13]	-0.038* [1.65]	-0.029 [1.45]	-0.156*** [3.64]	-0.128* [1.68]	-0.199*** [3.70]	-0.036 [1.26]	-0.129*** [3.26]	-0.021 [1.12]	-0.116*** [6.74]	0.013 [0.91]
<b>Observations</b>	5579	5729	5739	5722	5732	5548	5326	5632	5471	5445	5733	5746	5808
<b># of regions</b>	86	86	86	86	86	86	86	86	86	86	86	86	86
<b>R<sup>2</sup></b>	0.77	0.49	0.73	0.7	0.6	0.54	0.51	0.42	0.27	0.35	0.72	0.59	0.01

Note: All dependent variables are de-trended and in logs. They are measured in real terms per capita. Absolute values of t-statistics are in parentheses. \*\*\*, \*\* and \* denote significance at 1, 5 and 10% level, respectively. Regional Fixed effects included.

**Table 3. Cycles in regional economic outcomes and wage arrears.**

	<b>Growth</b>	<b>Level of Industrial Output</b>	<b>Inflation</b>	<b>Price Level</b>	<b>Wage level</b>	<b>Total Wage Arrears</b>	<b>Regional Wage Arrears</b>	<b>Money Income</b>
<b>Lag</b>	-0.489*** [17.03]	0.640*** [47.07]	-0.210*** [7.49]	0.922*** [162.51]	0.721*** [60.07]	0.918*** [115.96]	0.809*** [42.44]	0.660*** [39.41]
<b>Term in power</b>	0.001 [0.22]	-0.005 [0.72]	0 [0.55]	-0.003*** [4.40]	0.001 [0.35]	0.016** [2.19]	-0.03 [0.75]	-0.021*** [4.16]
<b>Left wing party</b>	-0.01 [0.58]	-0.009 [0.46]	-0.001 [0.57]	0.002 [0.68]	-0.008 [1.21]	0.119 [1.38]		-0.029** [2.49]
<b>month -12</b>	-0.006 [0.40]	-0.01 [0.54]	0.004*** [2.78]	0.006*** [2.70]	0 [0.03]	-0.015 [1.08]	-0.017 [0.29]	0.034*** [2.69]
<b>month -11</b>	-0.01 [0.62]	-0.029 [1.51]	0.001 [0.75]	0.005** [2.35]	-0.001 [0.15]	-0.027** [1.99]	-0.061 [1.01]	-0.002 [0.16]
<b>month -10</b>	-0.012 [0.79]	-0.025 [1.35]	0 [0.24]	0.002 [1.13]	-0.006 [1.06]	-0.024* [1.77]	-0.053 [0.89]	0.005 [0.39]
<b>month -9</b>	-0.017 [1.13]	-0.025 [1.34]	0 [0.08]	0.001 [0.36]	-0.001 [0.14]	-0.040*** [3.00]	-0.101* [1.71]	-0.008 [0.70]
<b>month -8</b>	-0.029* [1.85]	-0.055*** [3.00]	0 [0.21]	0 [0.17]	-0.003 [0.58]	-0.039*** [2.88]	-0.088 [1.55]	-0.029** [2.43]
<b>month -7</b>	-0.007 [0.45]	-0.028 [1.55]	-0.001 [0.84]	-0.001 [0.40]	0.001 [0.22]	-0.052*** [3.89]	-0.126** [2.24]	-0.025** [2.04]
<b>month -6</b>	-0.002 [0.11]	-0.035** [1.97]	-0.001 [0.73]	-0.001 [0.70]	-0.003 [0.49]	-0.034** [2.53]	-0.06 [1.05]	-0.02 [1.64]
<b>month -5</b>	0.019 [1.26]	0.017 [0.97]	0 [0.35]	0.003 [1.54]	-0.009 [1.52]	-0.043*** [3.32]	-0.133** [2.53]	-0.031** [2.55]
<b>month -4</b>	-0.003 [0.19]	0.02 [1.14]	0 [0.12]	0.002 [1.19]	0.007 [1.27]	-0.032** [2.34]	-0.140** [2.50]	-0.023* [1.81]
<b>month -3</b>	0.011 [0.72]	0.025 [1.41]	0 [0.32]	0.002 [0.83]	0 [0.00]	-0.017 [1.22]	-0.126** [2.12]	-0.014 [0.88]
<b>month -2</b>	0.009 [0.57]	0.024 [1.29]	-0.002 [1.25]	0 [0.17]	-0.003 [0.55]	-0.01 [0.76]	-0.141** [2.51]	0.041*** [3.24]
<b>month -1</b>	-0.013 [0.86]	-0.031* [1.66]	-0.002 [1.23]	-0.002 [1.20]	0.020*** [3.32]	-0.045*** [3.31]	-0.280*** [4.96]	0.082*** [6.20]
<b>month 0 – elections</b>	0.014 [0.88]	-0.006 [0.34]	-0.001 [0.63]	0 [0.06]	0.004 [0.60]	-0.055*** [3.80]	-0.275*** [4.64]	0.107*** [8.26]
<b>month 1</b>	-0.026 [1.55]	-0.043** [2.12]	0.001 [1.16]	0.002 [1.25]	-0.006 [1.00]	-0.041*** [3.00]	-0.079 [1.38]	0.018 [1.39]
<b>month 2</b>	0.005 [0.29]	-0.027 [1.33]	0 [0.08]	0.003* [1.72]	-0.007 [1.12]	-0.008 [0.60]	0.01 [0.18]	-0.004 [0.34]
<b>month 3</b>	-0.02 [1.15]	-0.017 [0.82]	0.001 [0.55]	0.004** [2.17]	-0.016*** [2.73]	0.021 [1.53]	0.003 [0.05]	0.018 [1.39]
<b>month 4</b>	0.006 [0.34]	-0.016 [0.76]	0.001 [0.62]	0.004** [2.32]	-0.012** [2.01]	0.005 [0.33]	0.034 [0.58]	0.022* [1.71]
<b>month 5</b>	-0.016 [0.94]	-0.041** [1.97]	-0.001 [1.16]	0.002 [0.97]	-0.003 [0.51]	-0.006 [0.43]	-0.033 [0.58]	-0.003 [0.27]
<b>month 6</b>	0.005 [0.29]	-0.025 [1.21]	0.001 [0.72]	0.003 [1.49]	-0.007 [1.09]	-0.015 [1.03]	-0.006 [0.09]	0.029** [2.23]
<b>month 7</b>	0.005 [0.30]	-0.004 [0.22]	0 [0.18]	0.003 [1.57]	-0.009 [1.56]	-0.023 [1.61]	-0.064 [1.06]	0.018 [1.38]
<b>month 8</b>	-0.011 [0.66]	-0.01 [0.52]	-0.001 [0.97]	0.001 [0.62]	0.005 [0.75]	-0.003 [0.18]	-0.049 [0.81]	-0.024* [1.86]
<b>month 9</b>	0.007 [0.42]	0.002 [0.09]	0 [0.19]	0.002 [1.10]	0.001 [0.21]	-0.012 [0.86]	0.047 [0.76]	0.006 [0.44]
<b>month 10</b>	0.018 [1.17]	0.014 [0.73]	0.001 [0.77]	0.003 [1.64]	0.004 [0.66]	-0.022 [1.39]	0.022 [0.29]	0.034*** [2.69]
<b>month 11</b>	-0.001 [0.05]	-0.02 [1.07]	0.002* [1.69]	0.005*** [2.68]	-0.003 [0.43]	-0.037*** [2.69]	-0.078 [0.94]	-0.011 [0.89]
<b>month 12</b>	0.004 [0.22]	-0.007 [0.33]	0.001 [0.47]	0.005** [2.30]	-0.003 [0.43]	-0.051*** [3.72]	-0.195** [2.15]	0.063*** [5.06]
<b>Constant</b>	0.005 [0.53]	-0.125*** [10.96]	0 [0.21]	-0.001 [0.41]	-0.017*** [4.69]	-0.057 [1.54]	0.014 [0.24]	-0.035*** [5.04]
<b>Observations</b>	5974	6235	5968	5986	6700	3920	2141	4617
<b># of regions</b>	86	86	86	86	86	86	86	86
<b>R<sup>2</sup></b>	0.12	0.26	0.013	0.828	0.26	0.80	0.37	0.33

*Note: All dependent variables are in logs and de-trended. All except inflation and prices are measured in real terms per capita. Absolute values of t-statistics are in parentheses. \*\*\*, \*\* and \* denote significance at 1, 5 and 10% level, respectively. Regressor “Left wing party” dropped out of the regression with regional wage arrears because it is collinear with fixed effects over the period for which we have regional wage arrears data. Regional Fixed effects included.*

**Table 4. Determinants of the magnitude of the cycles.**

	The first component of amplitudes of the cycles in six fiscal policies with the largest cycles				Amplitude of the cycles in:								
					total budgetary expenditures		social expenditures		educa-tion exp.	expenditures on culture			health-care exp.
<b>Log of share of population with higher education in 1995</b>	-0.969*** [3.53]								-0.207*** [3.69]	-0.234*** [2.91]			-0.186*** [2.64]
<b>Log of share of urban population</b>	-0.989** [2.15]				-0.214** [2.16]		-0.511** [2.40]			-0.200* [1.74]			
<b>Log of # of computers per capita</b>	-0.105 [0.47]				-0.069* [1.92]								
<b>Index of freedom of mass media production</b>	-0.009 [1.41]						-0.008** [2.42]			-0.003* [1.68]			
<b>Time</b>	-0.014*** [4.03]	-0.014*** [3.73]	-0.013*** [3.64]	-0.002** [2.61]	-0.002** [2.61]	-0.002*** [2.85]	-0.003 [1.55]	-0.002 [1.39]	-0.002** [2.42]	-0.004*** [3.95]	-0.004*** [3.85]	-0.004*** [3.75]	-0.003*** [3.54]
<b>Left</b>	-0.212 [1.35]	-0.152 [0.90]	-0.161 [0.96]	-0.073** [2.14]	-0.073** [2.14]	-0.073** [2.10]	0.041 [0.55]	0.029 [0.39]	-0.064** [2.15]	0.031 [0.75]	0.046 [1.03]	0.036 [0.84]	-0.054 [1.38]
<b>Constant</b>	3.217*** [3.83]	4.668** [2.33]	0.829** [2.38]	1.085** [2.54]	1.085** [2.54]	0.350*** [3.40]	2.409** [2.59]	0.537*** [3.06]	0.745*** [4.60]	0.833*** [3.56]	1.020** [2.07]	0.289*** [3.23]	0.718*** [3.40]
<b>Observations</b>	117	118	117	123	123	123	125	124	124	124	125	124	124
<b>R<sup>2</sup></b>	0.176	0.156	0.128	0.122	0.122	0.109	0.086	0.083	0.135	0.171	0.154	0.144	0.13

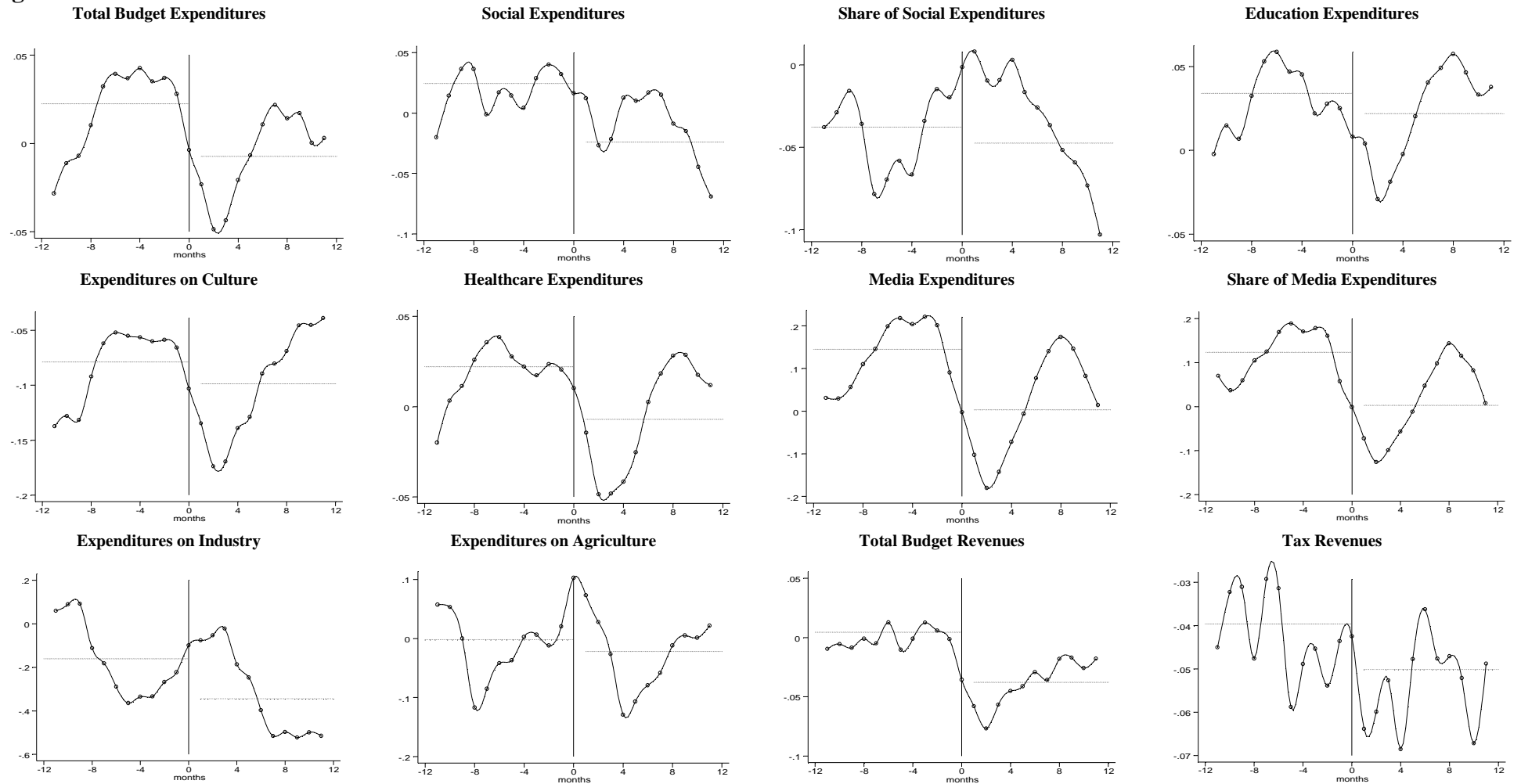
Note: Absolute values of t-statistics are in parentheses. \*\*\*, \*\* and \* denote significance at 1, 5 and 10% level, respectively.

Table 5. Effect of cycles on probability to get re-elected.

Proxy for Rationality and Awareness:	Log of share of population with higher education in 1995		Log of share of urban population		Log of # of computers per capita		Index of freedom of mass media production	
	Incumbent's popularity	Probability of incumbent's win (ordered probit)	Incumbent's popularity	Probability of incumbent's win (ordered probit)	Incumbent's popularity	Probability of incumbent's win (ordered probit)	Incumbent's popularity	Probability of incumbent's win (ordered probit)
<b>Amplitude</b>	0.445*** [3.28]	0.347** [2.16]	0.394*** [2.90]	0.333** [2.13]	0.419*** [3.38]	0.311** [2.06]	0.389*** [2.92]	0.345** [2.13]
<b>Amplitude*Rationality</b>	-0.151 [0.26]	0.436 [0.62]	-0.655 [0.89]	0.474 [0.48]	-0.782* [1.96]	0.035 [0.06]	-0.011 [1.23]	0.003 [0.26]
<b>Amplitude*Time</b>	0.006 [1.04]	0.002 [0.26]	0.005 [1.00]	0.003 [0.38]	0.007 [1.27]	0.002 [0.26]	0.006 [1.10]	0.002 [0.33]
<b>Rationality</b>	0.679 [1.38]	0.124 [0.21]	0.57 [0.64]	0.503 [0.57]	0.377 [1.17]	0.424 [1.28]	0.018* [1.91]	0.013 [1.20]
<b>Time</b>	0.018*** [3.23]	0.018** [2.40]	0.016*** [2.64]	0.017** [2.49]	0.017*** [3.12]	0.017** [2.42]	0.016*** [2.83]	0.017** [2.47]
<b>Left</b>	-1.025*** [4.35]	-1.011*** [3.94]	-1.042*** [4.20]	-0.970*** [3.74]	-1.008*** [4.16]	-0.907*** [3.52]	-1.009*** [4.66]	-0.955*** [3.80]
<b>Relative Share of Soc. Exp.</b>	0.028 [0.66]	0.037 [0.51]	0.013 [0.31]	0.033 [0.47]	0.021 [0.56]	0.032 [0.47]	0.017 [0.45]	0.032 [0.47]
<b>Relative Productivity</b>	1.101 [0.88]	0.727 [0.44]	1.043 [0.80]	0.921 [0.54]	0.73 [0.57]	1.042 [0.61]	1.105 [0.93]	0.763 [0.47]
<b>Relative Price</b>	0.003 [0.00]	0.704 [0.16]	-0.984 [0.22]	1.06 [0.24]	-0.62 [0.16]	1.199 [0.27]	-0.273 [0.07]	1.133 [0.26]
<b>Constant</b>	0.872*** [5.24]		0.900*** [5.12]		0.851*** [4.96]		0.876*** [5.47]	
<b>Observations</b>	107	107	107	107	107	107	107	107
<b>R<sup>2</sup></b>	0.303		0.297		0.322		0.319	

Note: Absolute values of t-statistics are in parentheses. \*\*\*, \*\* and \* denote significance at 1, 5 and 10% level, respectively.

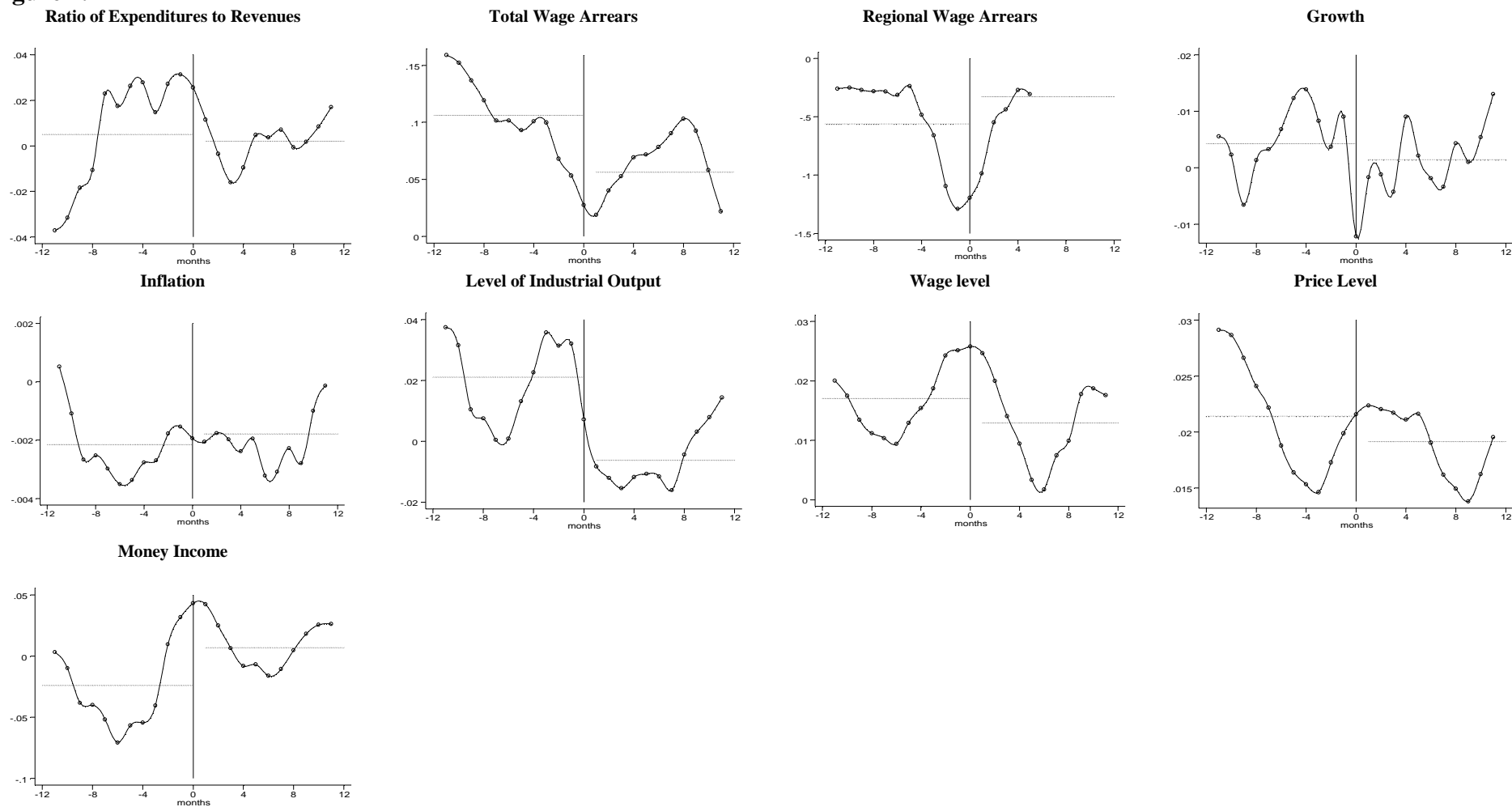
**Figure 1.**



*Note: Graphs show the dynamics of the moving average, MA(4), of the aggregate of logs of seasonally adjusted de-trended policy instruments from a year before to a year after elections. Zero-month is the month of elections. The policy instruments are normalized, so that zero level on each graph represents the middle of the term level. Two dotted horizontal lines on each graph represent the average values of the instrument in a year before and in a year after elections.*



**Figure 2.**



*Note: Graphs show the dynamics of the moving average, MA(4), of the aggregate of logs of seasonally adjusted de-trended policy instruments and outcomes from a year before to a year after elections. Zero-month is the month of elections. The policy instruments and outcomes are normalized, so that zero level on each graph represents the middle of the term level. Two dotted horizontal lines on each graph represent the average values of the instrument in a year before and in a year after elections.*

**Appendix A.**

**Panel A. Descriptive statistics of policy instruments and outcomes**

Variable	No. of	Median	Mean	S.E.	Min	Max	Time span	Source	Units
Total Budget Expenditures	5787	24.32	40.92	0.73	6.21	520.91	Mar, 1996 - Nov,	MF	\$ per capita
Social Expenditures	5934	1.82	2.61	0.04	0.18	30.1	Jan, 1996 - Nov, 2001	MF	\$ per capita
Share of Social Expenditures	5673	6.9	7.68	0.05	1.13	31.83	Jan, 1996 - Nov, 2001	MF	%
Education Expenditures	5939	5.53	8.46	0.12	1.51	84.74	Jan, 1996 - Nov, 2001	MF	\$ per capita
Expenditures on Culture	5928	0.61	0.96	0.02	0.12	11.42	Jan, 1996 - Nov, 2001	MF	\$ per capita
Healthcare Expenditures	5938	3.78	5.5	0.07	0.85	51.12	Jan, 1996 - Nov, 2001	MF	\$ per capita
Media Expenditures	5763	0.08	0.16	0	0	3.41	Mar, 1996 - Nov,	MF	\$ per capita
Share of Media Expenditures	5683	0.3	0.37	0	0	1.83	Mar, 1996 - Nov,	MF	%
Expenditures on Industry	5601	0.32	1.52	0.07	0	83.78	Jan, 1996 - Nov, 2001	MF	\$ per capita
Expenditures on Agriculture	5843	0.85	1.54	0.03	0.01	19.36	Jan, 1996 - Nov, 2001	MF	\$ per capita
Total Budget Revenues	5940	23.58	38.98	0.69	6.21	474.55	Jan, 1996 - Nov, 2001	MF	\$ per capita
Tax Revenues	5941	15.71	23.93	0.41	1.45	313.91	Jan, 1996 - Nov, 2001	MF	\$ per capita
Ratio of Expenditures to Revenues	5695	101.28	104.56	0.31	48.2	281.5	Mar, 1996 - Nov,	MF	%
Growth	6331	0.83	2.19	0.27	-63.41	157.6	Feb, 1995 - Oct, 2001	GKS	%
Inflation	9831	2.8	7.46	0.13	-1.3	213.5	Feb, 1992 - Nov,	GKS	%
Level of Industrial Output	6598	92.55	0.12	0	0	1.06	Jan, 1995 - Oct, 2001	GKS	\$ per capita
Total Wage Arrears	4143	25.51	0.05	0	0	0.66	Oct, 1997 - Sep, 2000	GKS	\$ per capita
Regional Wage Arrears	2391	1.84	0.01	0	0	0.25	Jan, 1999 - Sep, 2000	GKS	\$ per capita
Wage level	6975	124.12	155.39	1.18	46.28	652.4	Feb, 1995 - Oct, 2001	GKS	\$ per capita
Price Level	4850	0.99	121.58	1.03	32.43	553.68	Jan, 1992 - Nov, 2001	GKS	relative to Apr.
Money Income	5787	101.63	40.92	0.73	6.21	520.91	Jan, 1995 - Oct, 1999	GKS	\$ per capita

**Panel B. Descriptive statistics of electoral variables**

Variable	No. of	Median	Mean	S.E.	Min	Max
Dummy for participation of incumbents	198	1	0.949	0.016	0	1
Dummy for incumbents' win	188	1	0.665	0.035	0	1
Dummy for incumbent's loss worse than the second	188	0	0.059	0.017	0	1
% of votes pro incumbent	186	56.43	53.908	1.682	4.76	99.9
% of votes pro main competitor of incumbent	173	28.5	32.51	1.587	0.71	82
% of votes pro winner	196	59.71	62.599	1.088	23.5	99.9
% of votes pro the first runner up	182	24.11	24.015	0.949	0.71	48
Number of candidates	193	5	5.523	0.211	1	16

*Note: Statistics are presented for 198 Russian regional elections between 1992 and 2002.*

**Panel C. Descriptive statistics of rationality and awareness measures**

Variable	Regions	Median	Mean	S.E.	Min	Max
Log of education in 1995	76	2.75	2.78	0.03	2.19	3.68
Log of share of urban population	86	4.23	4.17	0.03	2.91	4.61
Log of number of computers per capita in 1998	83	2.5	2.43	0.06	0.68	3.64
Index of freedom of media production	81	37	36.25	1.59	0	75