OPPORTUNISTIC POLITICAL CYCLES:
TEST IN A YOUNG DEMOCRACY SETTING[*]

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Abstract

This paper tests the theory of opportunistic cycles in a decade-old democracy–Russia–finds strong evidence of cycles, and provides an explanation for why previous literature often found weaker evidence. Using regional monthly panel data, we find that: (1) the budget cycle is sizable and short-lived; public spending shifts towards direct monetary transfers to voters; (2) the magnitude of the cycle decreases with democracy, government transparency, media freedom, voter awareness, and over time; and (3) pre-electoral manipulation increases incumbents' chances for reelection. The short length of the cycle explains underestimation of its size by previous literature because of low frequency data used in previous studies.

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"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)

I. 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 the pioneering work of Nordhaus [1975]. The evidence from developed countries is particularly weak.[1] The 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 theoretical works argued that opportunistic cycles should be most sizable in countries with immature democratic regimes (Gonzalez 2000 and Shi and Svensson 2002a). The evidence has been strongly supportive of this view: studies of country-level panels with large presence of young democracies and within-country studies of imperfect democracies have shown robust significant fiscal cycles almost exclusively.[2] Many of these tests, however, suffer from severe data limitations, in particular, insufficient frequency and, often, too high level of aggregation of fiscal spending. This paper sheds further light on the puzzle: we provide strong evidence of opportunistic cycles using regional monthly panel from a decade-old democracy–Russia, demonstrate that use of lower frequency data leads to underestimation of cycles explaining weakness of results in previous studies, and document the link between cycles, on the one hand, and democracy and transparency, on the other.

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[1] Little empirical support for opportunistic cycles in developed countries was found in Klein [1996], Berger and Woitek [1997], Reid [1998], and Alesina and Roubini [1992]. Alesina, Roubini, and Cohen [1997], Drazen [2000], and Franzese [2002] provide detailed surveys of theoretical and empirical literatures.

[2] See evidence and discussion in Brender and Drazen [2003] as well as evidence produced by Ames [1987], Gonzalez [2002], Krueger and Turan [1993], Drazen and Eslava [2003], Block [2001, 2002], Block, Ferree, and Singh [2003], Shi and Svensson [2002a, 2002b], Schuknecht [2000], and Drazen and Eslava [2003].

Russia's regional elections of executives provide an ideal case for an empirical test of opportunistic political cycle theory. First, Russia is a typical immature democracy: it is characterized by dependent media, nontransparent government, and a large fraction of an uninformed and myopic electorate. Since the theory predicts sizable cycles in immature democracies, one should expect to find evidence of cycles on Russian data. Simple plots of actual series of regional spending in many cases suggest vivid increases in spending prior to elections (examples are given in Figure I). Second, high uniformity in electoral rules and scope for policymaking among regions combined with high variation in the level of democracy and government transparency help to identify the effect of the latter on the magnitude of political cycles. Third, detailed monthly regional panel data allow more powerful tests of the theory than the ones done so far.

We find sizable and short-lived cycles in public expenditure and its composition and no cycles in growth or inflation controlling for region-specific characteristics, federal trend, seasonality, and ideology. Monthly panel data allow us careful measurement of even very short cycles. This turned out to be important because most sizable shifts in spending happen within a month or two from elections. We show that use of quarterly data leads to severe underestimation of cycles because the opposite-sign shifts in public expenditure around elections cancel out in low frequency data. Previous empirical studies of cycles in developing countries used quarterly or lower frequency data.[3] Short length of the cycle, however, does not undermine its economic significance. First, the cycle is primarily targeted at poor voters who have the worst possibilities for consumption smoothing: the largest fluctuations are observed in repayment of wage arrears to public workers and spending on welfare and other public assistance programs. Second,

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[3] To the best of our knowledge, the only paper that uses monthly data is Berger and Voitek [1997]; it rejects hypothesis of opportunistic cycles for the German developed democracy.

irrespective of the driving force of the cycle–asymmetric information or voter myopia–it indicates the absence of checks and balances on politicians that make politicians accountable in mature democracies (i.e., separation of powers, free press, active NGOs, etc.). These institutions should provide voters with fuller information and longer-term control over politicians and, therefore, limit possibilities for misuse of public office for private gain. Large and short-lived cycles, therefore, provide evidence of poor long-term accountability of politicians.

In line with recent theoretical results, we find that proxies for informational symmetry, voter awareness, and the level of regional democracy significantly reduce cycles. In addition, cycles get smaller over time. The latter could be an indication of a general phenomenon: as democracy matures, voters learn and independent media and civil society develop, or of a Russia-specific effect of an informal change in federal control over regional budgets from one Russian president to the other (data are insufficient to distinguish between these two explanations of the time effect). Finally, we find that cycles in fiscal policy instruments significantly increase the popularity of incumbents and help them win.

The remainder of the paper is organized as follows. In the next section, we formulate testable hypotheses motivated by theoretical literature. Section III describes the data. Section IV presents results. Conclusions follow in Section V.

II. HYPOTHESES

First, we test predictions of the classic opportunistic political cycle models. Nordhaus [1975] built the first formal model of opportunistic political cycles based on Phillips curve with adaptive expectations of voters. In the model, naïve voters get consistently fooled by a pre-electoral inflationary boom despite the recession following election. The model predicts political cycles in growth, unemployment, and inflation. Rogoff and Sibert [1988], Rogoff [1990], and

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Persson and Tabellini [1990] reconciled rational expectations with opportunistic political cycles in dynamic signaling models with asymmetric information about politicians' competence. In particular, Rogoff and Sibert [1988] and Rogoff [1990] focus on political budget cycle in which voters reward pre-electoral distortion in public spending because it signals about an incumbent's ability to provide public goods. Rogoff and Sibert's model predicts a pre-electoral fall in tax collection along with increases in deficit and inflation; Rogoff's model focuses on the pre-electoral shift in composition of government spending away from investment, observed with a lag, towards more visible public consumption. We look at the dynamics of a wide range of public expenditure items, composition of the budget, growth and inflation to test predictions of these theories.

Second, we study determinants of the magnitude of the budget cycle. Asymmetry of information about politicians' competence plays a central role in rational opportunistic cycles. Recent literature extends the basic set up of Rogoff's model to illustrate that transparency modeled as the probability that voters get correct exogenous signal about an incumbent's competence (Gonzalez 2000) and awareness of voters defined as a share of perfectly informed voters (Shi and Svensson 2002a) reduce the magnitude of the cycle. Furthermore, Gonzalez [2000] introduces the level of democracy directly into the model. Democracy has a non-monotone effect on the cycle: First, if the cost of enforcing political turnover is prohibitively high for voters (as is the case in dictatorships), then politicians do not have an incentive to engage in costly signaling, and therefore, there are no cycles. Second, when change in office can be enforced, the cycle decreases with democracy because it ensures institutional checks and balances on government that allow voters to observe politicians' competence directly with higher probability (i.e., free media). We test whether these theoretical predictions are consistent with the data by looking at how cross-sectional variation in voter awareness (measured by education and

urbanization), transparency (measured by indices of government transparency and media freedom), and democracy influence the cycles.

In young democracies, like Russia, emerging NGOs and independent media accumulate experience in collecting, packaging, and disseminating information about politicians and their policies while voters learn how to process this information. Thus, cycles should get smaller over time as learning proceeds and civil society develops (Drazen and Brender 2003). We test for the effect of time on the cycle. Apart from learning, however, the time effect may also be attributed to the difference in scope for fiscal manipulation by regional governments under Yeltsin and Putin's administrations (Rogoff [1990] discusses the effects of restraining fiscal policies on political cycles). We attempt to separate these hypotheses.

All opportunistic cycle theories predict that pre-electoral manipulations are rewarded by voters. In rational signaling models, a cycle has costs (policy distortions) and benefits (transmitting information about politicians' competence). If voters learn politicians' type directly, for instance, through well-functioning independent media, signaling has no value to voters. In fact, in developed democracies voters reward politicians for restrained fiscal policies (Alesina, Perotti, and Tavares 1998 and Brender 2003). As a last step of our analysis, we investigate whether it pays to pursue pre-electoral fiscal expansion in a maturing democracy.

III. THE DATA

The comprehensive list of regional governor elections that took place in Russia between August 1995 and December 2003 consists of 194 electoral events.[4] Data on most policy instruments and outcomes are available for 159 elections between September 1996 and July 2003. Four regions had three rounds of elections, sixty-five regions had two rounds of elections,

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[4] This list covers all the regions but Dagestan, the only region where there have not been any governor elections. We excluded Chechnya and Ingushetia from the sample because fluctuations in fiscal policies of these regions have been driven by war rather than elections.

and seventeen regions had just one round of elections during this period. The source of the data on elections is *Tsentrizbirkom*, the Central Elections Committee of the Russian Federation.

Regional monthly series of fiscal instruments and outcomes come from two sources: *Goskomstat*, the State Committee of Statistics, provided data on wages and income, wage arrears from the regional budgets, price level, and industrial output between 1995 and 2003; the Ministry of Finance of the Russian Federation provided detailed data on the execution of regional budgets for the period between 1996 and 2003.

To test for the determinants of cycle magnitude, we use cross-section data. Data on urbanization and education come from *Goskomstat*. The data on freedom of media in the regions were provided by the Institute of Free Media (www.freepress.ru). Data on transparency of the regional government come from "Media-Soyuz," an independent professional association of Russian journalists. Data on the scope of regional democracy are from the Carnegie Moscow Center, Carnegie Endowment for International Peace (Petrov 2001). The appendix presents descriptive statistics of the data.

IV. RESULTS

IV.A. Test for Opportunistic Cycles

Three groups of policy instruments and outcomes are considered: budgetary expenditures (total spending as well as levels and shares of expenditures on social programs, education, culture, healthcare, mass media, and industrial subsidies), budget revenues and deficit (total revenues, tax revenues, deficit, and federal transfers) and such outcomes as growth, inflation, regional budgetary wage arrears, wage level, and income. We test for political cycles in these

variables treating election time as exogenous.[5] The following equation is estimated on regional monthly panel data:

(1) $$y_{it} = \sum_{j \in \{-12;12\}} \alpha_j m_{jit} + \beta(L) y_{it-1} + \gamma_1 Term_{it} + \gamma_2 Left_{it} + \tau_t + f_{is} + \varepsilon_{it},$$

where i identifies regions; t–real time in months. y stands for a logarithm of instrument or outcome of regional policy (all monetary variables are expressed in real terms per capita). To control for the federal trend and macroeconomic shocks, we include the complete set of time fixed effects τ_t–one for each month t. To control for region-specific fixed effects and region-specific seasonality, we include fixed effects f_{is} for each of the 12 calendar months $s(t)$ in each region i. Results are robust to using alternative methods of accounting for seasonality and trend (Alesina and Roubini 1992).

m_{jit} is a dummy that equals 1, if t is j months away from elections ($j=0$ in the month of elections; negative j means that t is before and positive–that t is after the election month). Henceforth, we refer to the m_{jit}s as cycle dummies. Positive estimates of α_j before and negative estimates of α_j after elections serve as evidence of opportunistic political cycles.

Lag polynomial $\beta(L) y_{it-1}$ accounts for autocorrelation in y. We tested for optimal lag structure using Akaike criterion. Four lags turned out to be optimal for the vast majority of the series. For the sake of uniformity, we report regressions with four lags for all the series, but results do not change if we include optimal number of lags separately for each series. Lags in panel regressions with fixed effects produce biases that converge to zero when the time

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[5] Ito [1990], Reid [1998], and Heckelman and Berument [1998] among others pointed out that opportunistic cycles can occur as a result of setting election date at a time of a boom. Although almost 19 percent of Russia's regional elections happened a month or more of their expected date; in vast majority of these cases, the time was shifted for exogenous reasons. There were few cases, however, when there was no exogenous reason for the shift of election time even though it is illegal to shift the date of regional elections. To make sure that our results are not driven by presence of endogenous elections, we repeated all tests on the subsample of elections that had exogenously pre-determined timing and got virtually identical results.

dimension of a panel goes to infinity (Nickell 1981, Hansen 1982, and White 1982). Our panel

covers more than eighty months; therefore, asymptotic properties apply. Nonetheless, we verified

that results are robust to using the Arellano-Bond procedure (Arellano and Bond 1991).[6]

Term controls for incumbent's political horizon that can be an important determinant of

fiscal policies (Besley and Case 1995). It equals 0, 1, 2, or 3 depending on the term that the

incumbent serves in office: 0 means that the governor is appointed and has not been elected

before; 1 indicates that he was elected for the first time, etc. *Left* is a dummy that equals 1 if the

incumbent governor is supported by the Communist coalition. It controls for partisan cycles.[7]

The results of estimation of equation (1) are presented in Tables I and II. Figure II plots

the predicted political budget cycle measured in percentage deviations from trend around

elections. Total budget expenditures experience the first significant jump up of 7 percent nine

months before elections.[8] After that there are no significant changes until one month before

elections when expenditures rise significantly by 13 percent. The cumulative increase in budget

spending during the year prior to elections amounts to 18 percent (five percent of monthly

regional product). The election month and two months right after elections are characterized by a

significant fall in budget expenditures of 17 percent. Spending on education, culture, and

healthcare exhibit significant increases during the two months prior to elections of 14 percent on

[6] We tested for residual autocorrelation in the panel (1). The results showed that the null hypothesis of no serial correlation in residuals cannot be rejected for all dependent variables. p-values for this test are presented in the Appendix. The null cannot be rejected for each dependent variable at 5 percent significance level and for each dependent variable but wages at 25 percent significance level. In addition, we tested for unit roots in each series for each region using augmented Dickey-Fuller test. The null hypothesis of a unit root was rejected for all series in all regions with the exception of wage arrears. Thus, we run regression (1) for the log change rather than level of wage arrears because growth of wage arrears is stationary.

[7] Alesina, Roubini, and Cohen [1997] review the partisan theory and evidence. Electoral campaign platforms of Russia's governors are polarized into the "communist left" and the "liberal-democratic ideology." In practice, the variation in the data is insufficient to have a proper test of partisan theory because there are only few cases when a new governor of the opposite ideological platform replaced an incumbent; therefore, ideology is almost perfectly collinear with fixed effects. Thus, we do not put emphasis on interpretation of coefficients of the "left-wing" dummy.

[8] Henceforth, the changes in expenditures are calculated by comparisons of fitted values net of the federal trend and seasonality (see Figure II for the illustration). This is because the estimates of coefficients of the 25 cycle dummies indicate the shift in the underlying autoregressive process rather than the shift in expenditures themselves.

average and significant decreases during the two months after elections of about 18 percent. Social expenditures rise five and one month before elections by 9 and 24 percent, respectively, reaching the level of 31 percent above the trend. In the two months after elections, social expenditures come back to the trend level.[9] Cycles in public expenditure are supported by intensive use of mass media. Media spending increases by 23 percent during half a year preceding elections and drops by 32 percent in the month of elections and two post-electoral months.[10] We also find significant budget composition effects of elections: the share of social expenditures in total spending rises by 14 percent and the share of media expenditures by 23 percent during a half a year preceding elections. In the pre-electoral month, shares of spending on social programs and media reach the levels that are 14 and 19 percent higher than their levels in the middle of the term.

Table II presents the effect of elections on budget revenues, growth, inflation, income, and wage arrears growth. Repayments of wage arrears to public workers exhibit strong cyclical pattern. Regression results for the growth of wage arrears imply the following level dynamics: Wage arrears drop by 32 percent in the three pre-election months. The cumulative decline in

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[9] We verified that standard errors of electoral dummies are estimated consistently in the dynamic panel (1) and that the results are not driven by a specific seasonal structure of elections in our sample. Following Bertrand, Duflo, and Mullainathan [2004], we conduct a series of estimations of the effect of randomly generated placebo elections (with and without holding their seasonal structure equal to the seasonal structure of the actual elections) on total and social regional expenditures. Significant cycles (i.e., increases in budget spending before elections and decreases–after) were found in less then one percent of the cases.

[10] To understand the kinds of publicity associated with budget cycle, we read local newspapers and press releases of regional authorities in a few regions. For a half a year before elections, most local newspapers actively praise incumbent governors. When few months are still left before elections, they report promises to pay out arrears in wages and child benefits in the near future (see, for instance, *"SeverInform"* [Oct 21 1999] for elections in Kirov Oblast in Mar 2000 and *"Agenstvo Informatsii Udmurtii"* [Mar 24 2000] for elections in Udmurtiya in Oct 2000). Right before elections articles usually claim that all arrears are paid off (see *"Krasnoyarsky Rabochy"* [Aug 20 2002] for elections in Krasnoyarsky Krai in Sep 2002; *Press Release of Tomsk Oblast Administration* [Aug 10 1999] for elections in Tomsk Oblast in Sep 1999). Occasionally, one comes across articles in the federal press that criticize incumbent governors and point to sources of funds used for repayment of arrears (e.g., *"Kommersant"* [Mar 24 2000] for elections in Pskovskaya Oblast in Nov 2000). The cycle in publicity around governors is vivid: we counted the number of press releases that were issued by administration of Sverdlovskaya Oblast. On average, controlling for trend and seasonality, the number of press releases in the four months before elections is 40 percent higher than throughout the electoral term.

wage arrears during the nine months before elections amounts to 42 percent. For a half a year after elections, wage arrears gradually rise but do not reach their initial level. The pre-electoral rise in public spending is financed in part by increases in non-tax and tax revenues, in part by an increase in deficit and in part by increased federal transfers (in regions-recipients of federal transfers). Budget cycle, and in particular, sizable fluctuations in government wage arrears and social spending drive dynamics of wages and income. During the four months prior to elections, wages and income rise significantly reaching the levels 5 and 1 percent above the trend, respectively, and return to the trend level in the three subsequent months.

Regional growth does not have a cyclical pattern. Inflation significantly decreases for half a year prior to elections and gradually (insignificantly) rises for half a year after elections.[11] Therefore, contrary to Nordhaus's prediction, politicians do not explore growth-inflation tradeoff to increase their chances of reelection.[12]

Budget cycles are short-lived: the largest shifts in expenditures occur within a month or two from the election date. The use of quarterly and annual data by the previous literature most likely led to underestimation of cycles because the opposite-sign shifts in fiscal policies around elections cancel out in the data with low frequency. To illustrate this point we collapsed monthly series at the quarterly level and estimated equation analogous to (1) on quarterly data. First, statistical significance in quarterly panel is reduced: few expenditure items show significant fluctuations around elections. Second, the magnitude of these fluctuations is considerably smaller. Figure III presents cycles estimated with monthly and quarterly data. For the vast

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[11] The fall in inflation before elections is much smaller in magnitude than fluctuations in fiscal policies or wages and income. Thus, our results hold irrespective of whether we take real or nominal variables. As a baseline, we report dynamics of real spending and income; the results remain the same when we test for cycles in nominal expenditures and income disregarding the price differences between regions.

[12] Keller and May [1984] were the first to argue that one needs to look at the political actions rather than the real economic outcomes to find evidence of opportunistic cycles based on analysis of President Nixon's election campaign. Drazen [2000] surveyed empirical literature to show that models with adaptive expectations à la Nordhaus [1975] are inconsistent with results of virtually all empirical tests: cycles if found affect fiscal and monetary policies rather than growth or unemployment.

majority of expenditure items, deviation from the trend near elections predicted using quarterly data is less than one third of the one predicted by monthly panel.

Overall, we find very strong evidence of sizable opportunistic cycles in fiscal policies and no evidence of cycles in economic growth. The most vivid increases in budget spending occur a month or two before elections. This implies that instruments of pre-electoral manipulation are observed by voters almost immediately. This could be achieved only with direct monetary payments to voters. Indeed, the two main instruments are repayment of wage arrears and social expenditures comprised of welfare, child benefits, veteran allowances, social insurance, and other public assistance programs. Our analysis of local and federal newspapers and regional governments' press releases provides numerous anecdotes suggesting that cycles in total, healthcare, education, and cultural spending also reflect manipulation with compensation of medical workers, teachers, and other government employees. Government wage bill is sufficiently large to show cycles in aggregate spending. For instance, in education and healthcare spending, it takes up about two thirds of the total. Therefore, we observe the shift of public expenditure (both over time and across items) towards the most visible to voters as predicted by the Rogoff's model [1990].[13]

IV.B. Determinants of Opportunistic Cycles

Methodologically, the best way to test for the determinants of cycle magnitude is to run specification (1) with additional regressors: proxies for possible determinants of the cycle and their interaction with the cycle dummies m_{jit}. The following potential determinants are considered: level of democracy (measured by a regional democracy index), voter awareness

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[13] A shift of expenditure towards more visible items was also found by Block [2003]. Using an annual panel of developing countries, he found that budget composition shifted away from public investment towards current consumption in the face of elections. In contrast, Khemani [2000] and Gonzalez [2002] find significant pre-electoral increases in public investment in Mexico and India. Khemani, however, shows that it is the road construction–the most visible type of investment–that is cyclical.

(measured by logs of regional shares of population with higher education and of urban population), and transparency (measured by region-level indices of media freedom and government transparency), all of which are positively correlated. These measures are available only as cross-section; we denote them by R_i. Proxies for voter awareness reflect both the possibilities of electorate to get access to information (i.e., internet and TV) which is easier in urban areas and the ability of electorate to process this information that comes with education. We also test how cycle magnitude changes with time. For ease of interpretation of coefficients, we rescale time to be measured in years (*Time*=t/12). As discussed below, *Time* is a proxy for voter learning and development of independent media and civil society since the country's democratization, but it could also reflect increased federal control over Russian regions under Putin's compared to Yeltsin's presidency. For presentation purposes, we report results of estimation of the short specification (1`) that preserves the main results of estimating specification with the full set of 25 cycle dummies.

(1`) $$y_{it} = \sum_{j\in\{-3;3\}}\alpha_j m_{jit} + \sum_{j\in\{-3;0\}}\eta_j m_{jit}R_i^D + \sum_{j\in\{-3;0\}}\xi_j m_{jit}Time_t^D + \xi Time_t + \beta(L)y_{it-1} + \gamma_1 Term_{it} + \gamma_2 Left_{it} + \tau_t + f_{is} + \varepsilon_{it}.$$

Here we look only at budgetary expenditures that exhibit cyclical dynamics. Superscript *D* indicates that we subtract means from *R* and *Time* before taking cross-terms; in this case, coefficient of the respective *m* is equal to the full effect evaluated at the mean values of *R* and *Time*. Negative significant coefficients at $m_{jit}R_i$ and $m_{jit}Time_t$ (given that coefficients at respective m_{jit} are positive) serve as evidence that *R* and *Time* decrease the cycle magnitude.

Table III presents results of estimation of equation (1`) for social expenditures and expenditures on culture. Results for the total budget expenditures and revenues, share of social expenditures, and spending on healthcare and education are very similar. There are no results for media spending and industrial subsidies. The interaction terms of voter awareness, democracy,

transparency and time with cycle dummies two months before elections usually have negative and often significant coefficients. Thus, these variables reduce the magnitude of the cycle. For instance, the results imply that a one standard deviation increase in our measures of education, urbanization, democracy, government transparency, and media freedom leads to a significant reduction in the jump of social expenditures prior to elections of 7, 9, 10, 7, and 8 percentage points, respectively.

The fact that R_i does not vary across time and for two of the five measures (viz., education and urbanization) varies only a little across regions potentially creates a problem in estimation of this panel because regressors $m_{jit}R_i$ and m_{jit} are correlated. To make sure that our results are not driven by this correlation we carry out a cross-section test. We construct the following measure of cycle magnitude: The cycle amplitude in a particular policy instrument for a particular election is defined as the residual corresponding to the last month before the election from estimation of the following equation run separately for each region and each instrument:

$$(2) \qquad\qquad y_{it} = \beta(L)y_{it-1} + \sum_{j\in\{1;12\}}\alpha_j s_{jt} + \gamma t + \varepsilon_{it}.$$

s_{jt} stands for dummies corresponding to 12 calendar months. t is the real time. $\beta(L)y_{it-1}$ is the lag polynomial of the same order as in (1). Summary statistics for constructed amplitudes are presented in Table IV. For all fiscal policy instruments, mean amplitudes are positive and, for eight out of nine, they are significantly different from zero. In addition, we constructed an aggregate measure of the magnitude of the political budget cycle as the first principal component of the amplitudes in individual fiscal policies with the most profound cycle. To test for the determinants of the cycle, we run the following equation on the pooled cross section of elections:

$$(3) \qquad\qquad A_i = \beta_0 + \beta_1 R_i + \beta_2 Time_i + \beta_3 Budget_i + \beta_4 Duration_i + \varepsilon_i,$$

where i is the ordinal number of elections. A_i is a measure of cycle amplitude. As above, R_i is a proxy for awareness, democracy, and transparency, and $Time_i$–real time measured in years.[14] Richer regions may have higher financial slack for manipulation of fiscal policies because the necessary fixed expenditure takes up a smaller part of the whole budget. We control for this effect with the regional mean of the size of per capita budget over the whole period– $Budget_i$.[15]

Persson and Tabellini [2003] show that differences in electoral rules explain a part of cross-country variation in political cycles. Generally, regional electoral rules in Russia are uniform. There are potentially significant differences only in the duration of governor's electoral term that varies from 4 to 5 years with the exception of one region with a seven-year term. The duration of term may affect "ego-rents" (Rogoff 1990) increasing the politicians' incentives for cycles or may reflect the fact that incumbent had more time for getting rid of political opposition decreasing the need for cycle. We use dummy indicating regions with the term above four years as a control ($Duration_i$).[16]

Table V presents results of estimation of equation (3). Cross-section test confirms findings from the panel regressions. Voter awareness, democracy, transparency, and time reduce cycles. In all regressions, coefficients of these measures are negative and in more than two

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[14] In cross-section regressions, instead of the entire index of democracy, we use dummy that indicates if a region has a value of democracy that is above the median (this proxy gives us better fit, possibly because of poor cardinal properties of the index). In estimation of equation (3), we excluded elections that took place in nine *Autonomous Okrugs* that are subdivisions of other larger regions because for the most part R data are unavailable for them, elections with a single candidate, and elections in which an incumbent did not run for reelection and did not name his successor. Equation (3) has regional-level regressors; thus, we allow error terms to be clustered within regions.

[15] Including *Term* as a control in cross-section regressions does not change any of the results. It is highly correlated with *Time*; when both are included as regressors, *Term* comes out insignificant, but the effect of *Time* does not change. The cycle magnitude could also be affected by world oil price because in times of high oil prices governors may be less constrained in pre-electoral fiscal manipulations. A large chunk of budgetary revenues at all levels of government depends on taxation of oil rents. As a robustness check, we have controlled for the oil price and oil revenues in all regressions and for their interaction with the cycle dummies in panel regressions and found that none of our baseline results are driven by the dynamics of oil prices.

[16] There is one other difference in electoral rules across regions: Vast majority of regions have two round elections, i.e., a runoff follows the first round in the case when none of the candidates receive more than a half of the votes; a few regions, however, have single-round elections. The number of single-round elections is insufficient to test its influence on cycles. Controlling for the number of rounds does not have any effect on the results.

thirds–significant. A 10 percent increase in the share of educated population decreases cycle amplitude in education, cultural, and healthcare expenditures by about 2 percentage points. A 10 percent increase in urbanization leads to a decrease in cycles in total, healthcare, and cultural spending of 2 and in social spending of 5 percentage points. The difference in magnitudes of cycles in total, social, cultural, and healthcare spending between regions with democracy above and below the median is about ten percentage points. A standard deviation increase in the index of government transparency leads to a decrease of cycles in social and total spending of 9 and 3 percentage points while a standard deviation increase in the media freedom index leads to a decrease in cycles in social and cultural expenditure of 8 and 6 percentage points. The first principal component of cycle amplitudes is significantly affected by all the measures.

Panel and cross-section results show that time negatively affects the size of the budget cycle. As shown in Table V, cycles fade away relatively fast: each additional year on average decreases cycle magnitude by about 3 percentage points. We also estimated the effect of the number of previous elections as was done by Block, Ferree, and Singh [2003]: An additional election in a region significantly reduces the cycle amplitude by 6 to 16 percent depending on the expenditure item considered. We consider two possible interpretations of the negative influence of time on the magnitude of the cycle: the emergence of civil society and learning by voters as democracy matures, and the disciplining role of increased central control over regions during Putin's compared to Yeltsin's administration. Although, there were no formal changes in electoral rules or authority of regional governments over spending from Yeltsin's to Putin's time, one could argue that Putin has monitored regional governments more closely than Yeltsin and increasingly closely throughout his term. In an attempt to separate the two hypotheses, we repeat the analysis separately for the two time periods when each of the presidents was in power. In each subsample, budget cycle is significant. The difference in magnitude of the cycle between

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the two subsamples is 2.5 times. Under Putin, the effect of time on the cycle is negative but for the majority of expenditure items insignificant; under Yeltsin, it is essentially zero. Thus, most of the variation in the cycle magnitude over time comes from comparison of the two waves of elections that took place under different presidents. We cannot rule out the voter learning explanation of the effect of time, however, because insignificance of time within subsamples could be attributed to the reduction in the number of observations or clustering of election dates.[17]

IV.C. Do Cycles Help Wining?

To test whether cycles helped incumbent governors to get reelected, we estimate how the share of votes for incumbent and the probability to win depend on the cycle magnitude controlling for an incumbent's ideology, performance in the last term, and differences in electoral rules. The following equation is estimated on the pooled cross section of elections:

$$(4) \quad P_i = \gamma_0 + \gamma_1 A_i + \gamma_2 A_i^D Time_i^D + \gamma_3 Time_i + \gamma_4 Left_i + \gamma_5 Urban_i + \gamma_6 Perform_i + \gamma_7 Duration_i + \varepsilon_i.$$

P_i is the popularity of incumbent measured by the ratio of votes for the incumbent to the sum of votes for the incumbent and the most popular challenger. A (cycle amplitude), *Time*, *Left*, and *Duration* are described above. Again, we subtract means before taking the cross-term to make interpretation of γ_1 easier. Positive coefficient at A is an indication that cycles are associated with an increase in incumbents' popularity; negative coefficient at the cross-term indicates that this relationship weakens over time. We control for the incumbent's past performance (*Perform*) which can be important in the case of retrospective voting (Alesina and Rosenthal 1995). In reported regressions, we control for the relative share of social expenditures, relative per capita regional industrial product and relative inflation measured as net-of-federal-

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[17] Clustering of election dates is particularly severe during Yeltsin's time: 53 percent of governor elections covered by our sample during Yeltsin's presidency occurred within four months of each other. In the Putin's subsample, elections are more spread out over time.

trend means over the last term relative to the overall regional means net of federal trend. We verified that the main results do not depend on presence or the choice of proxies for past performance. In particular, we also controlled for relative income, relative taxation level, relative total public spending, as well as relative growth rates in all of these performance indicators. We also control for the regional share of urban population, *Urban*, which has a very strong direct effect on popularity. Cook's distance (Cook 1977) and DFITS statistic (Belsley, Kuh, and Welsch 1980) indicate that two observations have excessively strong effect on the estimates (these are elections with very popular incumbents); we exclude them from the sample.[18] In addition to OLS regressions for incumbents' popularity, we estimate similar binary and ordered probit regressions with the following outcomes for an incumbent: wins/losses and wins/is the first runner up/is below the second place.

There is an endogeneity problem in these regressions. If an incumbent is certain about the results of an upcoming election because he is either extremely popular or extremely unpopular, he has little incentive for pre-electoral manipulations. The tighter the electoral competition, the higher the cycle. OLS underestimates the causal relationship between cycles and popularity when incumbents are confident of reelection and overestimates when incumbents are sure of losing. We do not have a good instrument for the cycle amplitude because all variables that sufficiently strongly correlate with it have an independent-of-the-cycles effect on the popularity of incumbents. In the vast majority of cases, however, incumbents were the most popular candidates to win the next election, and therefore, overall we are likely to underestimate the effect. To make sure that we do not overestimate the effect of cycles on popularity, we re-run the regressions on the subsample of elections in which incumbent was at least as popular as the main

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[18] As above, we exclude from the sample elections with a single candidate, elections in which an incumbent did not run for reelection and did not name his successor, and elections in nine *Autonomous Okrugs*. Error terms are allowed to cluster within regions.

opponent (for OLS) and on the subsample where the opponent is at most 1.3 times as popular as the incumbent (for OLS and probits) and got very similar results.

Table VI presents the results. Cycles in social, healthcare, education, and cultural spending generate significant political benefits for incumbent governors while pre-electoral expansion of total spending has no effect. A one standard deviation increase in the cycle amplitude in social, education, and cultural expenditures leads to growth in incumbents' popularity of 4, 5, and 5 percentage points, respectively. In the table we report coefficients of the ordered probit regressions which translate into the following marginal effects: A one standard deviation increase in the cycle in social and education spending leads to an increase in the probability of incumbent's win of about 10 and 13 percentage points, respectively. Binary probit results are analogous. We do not find any significant weakening of political benefits of cycles over time: in all regressions, the coefficient of the interaction term of time and cycle magnitude is insignificant.

V. CONCLUSION

We tested for existence of opportunistic political cycles and studied whether voter awareness, democracy, transparency, and time affect cycles. We also examined if cycles increase governors' chances of reelection. The monthly regional panel data allowed us to define timing of the cycle more precisely than it has been previously done in the literature. The key findings are as follows. 1) We find significant political cycles in budget spending and its composition. The average pre-electoral increase in total regional expenditure amounts to about five percent of monthly gross regional product and in welfare and other public assistance programs to 0.43 percent of monthly gross regional product. Consistent with Rogoff [1990], we observe a shift of public spending towards direct monetary payments to voters. 2) Previous studies likely

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underestimated the budget cycle because quarterly frequency of the data is insufficient to measure the cycle precisely. The underestimation is particularly strong when cycles are short-lived. Most sizable manipulations in our sample occur within a month or two away from an election date. We show that use of quarterly as opposed to monthly data results in estimates that are one third of the actual deviations from the trend around elections. 3) The magnitude of the cycle decreases with education, urbanization, level of democracy, transparency of the government, and freedom of media. Thus, information symmetry and development of democratic institutions are important factors influencing cycles. 4) Cycles have become smaller over time. This is consistent with the view that voters and independent media learn as democracy matures, but could also be explained by a change in Russia-specific institutional factors. 5) The scale of pre-electoral manipulations increases the popularity of incumbent governors and probability to get reelected.

We show that maturity of democracy is an important factor determining the scope for effective use of political cycles: Cycles are smaller in more democratic regions. This result contrasts with the finding of Gonzalez [2002] that Mexican political cycles got stronger with democratization. The evidence suggests that, unlike in Mexico during the PRI domination, democracy in Russia in 1996-2003 reached the point where enforcement of political turnover at the regional level was not prohibitively costly and in some regions, it even gave rise to an embryo of free press and institutions of civil society allowing voters to get information about politicians in a less costly way than by means of political cycles.

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TABLE I
CYCLES IN BUDGET SPENDING AND ITS COMPOSITION

	Total expenditures	Social expenditures	Education expenditures	Expenditures on culture	Healthcare expenditures	Media expenditures	Expenditures on industry	Share of social exp.	Share of media exp.
month −12	-0.034	-0.018	-0.001	0.007	-0.013	0.019	0.003	0.045	0.070
	(0.026)	(0.043)	(0.026)	(0.032)	(0.028)	(0.059)	(0.111)	(0.040)	(0.056)
month −11	0.000	0.003	-0.038	-0.051	-0.013	0.013	0.081	-0.007	-0.002
	(0.026)	(0.041)	(0.025)	(0.031)	(0.027)	(0.057)	(0.106)	(0.039)	(0.054)
month −10	-0.013	0.028	0.011	-0.011	0.019	0.033	0.012	0.029	0.026
	(0.026)	(0.041)	(0.024)	(0.031)	(0.027)	(0.055)	(0.105)	(0.039)	(0.053)
month -9	0.047*	0.053	0.027	0.037	0.017	0.077	0.188*	0.004	0.011
	(0.026)	(0.041)	(0.025)	(0.031)	(0.027)	(0.055)	(0.107)	(0.039)	(0.053)
month -8	0.015	0.041	0.001	0.005	0.027	0.036	0.122	0.064	0.013
	(0.026)	(0.041)	(0.024)	(0.031)	(0.027)	(0.055)	(0.104)	(0.039)	(0.054)
month -7	0.020	-0.044	-0.015	-0.033	0.003	-0.034	-0.028	-0.020	-0.047
	(0.025)	(0.039)	(0.023)	(0.029)	(0.025)	(0.055)	(0.098)	(0.039)	(0.053)
month -6	0.007	-0.026	0.031	0.028	0.022	0.002	0.125	-0.011	0.008
	(0.025)	(0.039)	(0.023)	(0.029)	(0.025)	(0.055)	(0.100)	(0.038)	(0.051)
month -5	0.025	0.074*	0.033	0.006	0.003	0.062	-0.114	0.016	0.011
	(0.025)	(0.038)	(0.023)	(0.029)	(0.025)	(0.055)	(0.099)	(0.038)	(0.051)
month -4	0.009	0.059	-0.046*	-0.020	-0.041	0.081	-0.122	0.038	0.088*
	(0.026)	(0.040)	(0.024)	(0.030)	(0.026)	(0.057)	(0.105)	(0.040)	(0.054)
month -3	0.024	0.038	0.001	-0.001	0.010	0.032	-0.096	-0.001	0.018
	(0.025)	(0.040)	(0.024)	(0.030)	(0.026)	(0.056)	(0.105)	(0.038)	(0.052)
month -2	-0.001	0.030	0.031	0.020	0.045*	0.170***	0.017	0.049	0.159***
	(0.025)	(0.040)	(0.024)	(0.030)	(0.026)	(0.055)	(0.105)	(0.038)	(0.052)
month -1	0.117***	0.226***	0.121***	0.140***	0.130***	0.106*	0.200*	0.103***	-0.049
	(0.025)	(0.040)	(0.024)	(0.030)	(0.026)	(0.054)	(0.105)	(0.037)	(0.051)
month 0: elections	-0.053**	0.030	-0.077***	-0.061**	-0.081***	-0.127**	-0.008	0.080**	-0.091*
	(0.026)	(0.041)	(0.025)	(0.031)	(0.027)	(0.057)	(0.107)	(0.039)	(0.053)
month +1	-0.046*	-0.092**	-0.039*	-0.116***	-0.031	-0.164***	-0.147	-0.042	-0.122**
	(0.024)	(0.038)	(0.023)	(0.029)	(0.025)	(0.053)	(0.099)	(0.036)	(0.049)
month +2	-0.043*	-0.062	-0.016	-0.034	-0.019	-0.170***	-0.095	-0.031	-0.140***
	(0.024)	(0.038)	(0.023)	(0.028)	(0.025)	(0.052)	(0.098)	(0.035)	(0.049)
month +3	0.004	0.016	-0.005	-0.037	0.006	0.004	0.029	0.028	-0.014
	(0.023)	(0.038)	(0.023)	(0.029)	(0.025)	(0.052)	(0.097)	(0.035)	(0.049)
month +4	-0.005	-0.006	0.022	-0.007	0.038	0.060	0.108	-0.011	0.050
	(0.023)	(0.038)	(0.023)	(0.028)	(0.025)	(0.052)	(0.096)	(0.035)	(0.048)
month +5	0.011	0.019	0.022	0.013	0.012	0.046	-0.039	0.009	0.038
	(0.023)	(0.037)	(0.022)	(0.027)	(0.024)	(0.051)	(0.092)	(0.035)	(0.048)
month +6	-0.007	0.061*	0.033	-0.005	-0.019	-0.002	0.211**	0.067*	-0.016
	(0.023)	(0.037)	(0.022)	(0.027)	(0.024)	(0.051)	(0.094)	(0.035)	(0.049)
month +7	0.039*	-0.012	0.013	0.014	0.020	0.086*	-0.147	-0.053	0.033
	(0.023)	(0.037)	(0.022)	(0.028)	(0.024)	(0.052)	(0.095)	(0.036)	(0.049)
month +8	0.019	0.014	-0.007	0.026	0.002	0.015	-0.022	0.007	-0.006
	(0.024)	(0.038)	(0.022)	(0.028)	(0.025)	(0.052)	(0.097)	(0.036)	(0.049)
month +9	-0.018	-0.005	0.031	-0.010	0.012	0.047	-0.056	0.023	0.065
	(0.023)	(0.038)	(0.023)	(0.029)	(0.025)	(0.052)	(0.097)	(0.036)	(0.049)
month +10	-0.009	-0.002	0.007	0.040	-0.004	-0.036	0.017	0.008	-0.013
	(0.023)	(0.038)	(0.022)	(0.028)	(0.025)	(0.052)	(0.098)	(0.035)	(0.049)
month +11	0.042*	0.016	0.023	0.075***	0.031	0.004	-0.018	-0.007	-0.015
	(0.023)	(0.038)	(0.022)	(0.028)	(0.025)	(0.051)	(0.100)	(0.035)	(0.048)
month +12	-0.004	-0.016	0.015	0.004	-0.019	0.024	0.059	-0.003	0.050
	(0.024)	(0.039)	(0.023)	(0.029)	(0.025)	(0.054)	(0.102)	(0.036)	(0.050)
Term in power	-0.012	-0.006	-0.010	-0.017	-0.001	-0.026	-0.037	-0.002	-0.028
	(0.010)	(0.016)	(0.010)	(0.012)	(0.010)	(0.022)	(0.041)	(0.015)	(0.021)
Left	0.030	-0.003	0.028	-0.001	0.005	-0.073	-0.011	-0.009	-0.101
	(0.034)	(0.048)	(0.028)	(0.036)	(0.031)	(0.077)	(0.122)	(0.052)	(0.070)
Obs.	6767	7004	6989	6921	6966	6600	5825	6388	6241
R²	0.574	0.561	0.548	0.481	0.511	0.235	0.611	0.349	0.131

All regressions include four lags, full set of time dummies, and fixed effects for each region-calendar month combination. All dependent variables are in logs and measured in real terms per capita. Robust standard errors are in parentheses. ***, ** and * denote significance at 1, 5 and 10% level, respectively. Regression for federal transfers is run on subsample where regions received strictly positive federal aid.

TABLE II

CYCLES IN BUDGET REVENUES, INCOME, WAGE ARREARS, GROWTH AND INFLATION

	Total revenues	Tax revenues	Deficit	Transfers	Growth	Inflation	Wage	Money income	Growth in wage arrears
month –12	0.039	0.017	-0.053**	-0.057	0.007	-0.006**	0.003	0.003	-0.060
	(0.028)	(0.024)	(0.021)	(0.085)	(0.014)	(0.003)	(0.005)	(0.007)	(0.071)
month –11	0.003	-0.008	-0.019	0.012	-0.006	-0.007***	-0.011**	0.003	0.063
	(0.027)	(0.023)	(0.020)	(0.081)	(0.013)	(0.003)	(0.005)	(0.007)	(0.064)
month –10	-0.038	-0.046**	0.009	0.055	-0.004	0.001	-0.001	-0.004	0.022
	(0.026)	(0.023)	(0.020)	(0.078)	(0.013)	(0.002)	(0.005)	(0.007)	(0.063)
month -9	0.019	0.036	0.011	0.161**	-0.005	-0.007***	0.010*	-0.004	0.084*
	(0.026)	(0.023)	(0.020)	(0.075)	(0.013)	(0.002)	(0.005)	(0.007)	(0.051)
month -8	0.014	0.028	0.002	0.094	-0.008	-0.004	-0.006	-0.007	-0.067
	(0.026)	(0.023)	(0.020)	(0.075)	(0.013)	(0.002)	(0.005)	(0.007)	(0.050)
month -7	-0.014	-0.023	0.017	-0.101	0.024*	0.000	0.004	-0.003	-0.056
	(0.025)	(0.022)	(0.020)	(0.075)	(0.013)	(0.002)	(0.005)	(0.007)	(0.047)
month -6	-0.014	0.016	0.028	-0.062	0.010	-0.001	-0.005	-0.008	0.046
	(0.025)	(0.022)	(0.020)	(0.074)	(0.013)	(0.003)	(0.005)	(0.007)	(0.047)
month -5	0.036	0.053**	-0.014	0.053	0.005	0.002	-0.013***	-0.006	-0.217***
	(0.024)	(0.022)	(0.020)	(0.075)	(0.013)	(0.003)	(0.005)	(0.007)	(0.047)
month -4	-0.014	-0.004	0.000	-0.137*	-0.003	-0.004	0.008	0.002	0.023
	(0.026)	(0.023)	(0.021)	(0.081)	(0.013)	(0.003)	(0.005)	(0.007)	(0.055)
month -3	0.008	0.035	0.042**	-0.036	0.016	-0.007***	0.008	0.014**	0.052
	(0.026)	(0.022)	(0.020)	(0.078)	(0.014)	(0.003)	(0.005)	(0.007)	(0.058)
month -2	0.008	0.027	0.003	-0.072	0.007	-0.002	0.005	0.018***	-0.012
	(0.026)	(0.022)	(0.020)	(0.078)	(0.013)	(0.003)	(0.005)	(0.007)	(0.060)
month -1	0.080***	0.000	0.052***	0.145*	-0.013	-0.005**	0.017***	0.036***	-0.207***
	(0.026)	(0.022)	(0.019)	(0.079)	(0.013)	(0.003)	(0.005)	(0.007)	(0.066)
month 0: elections	-0.050*	-0.024	0.010	-0.148*	0.000	-0.005*	0.006	0.006	-0.073
	(0.026)	(0.023)	(0.020)	(0.084)	(0.014)	(0.003)	(0.005)	(0.007)	(0.064)
month +1	-0.063**	-0.008	-0.007	-0.035	-0.009	0.001	-0.012**	-0.022***	0.043
	(0.025)	(0.022)	(0.019)	(0.080)	(0.014)	(0.003)	(0.005)	(0.007)	(0.059)
month +2	-0.032	-0.047**	-0.009	0.042	-0.003	0.001	0.003	-0.028***	0.057
	(0.024)	(0.022)	(0.019)	(0.077)	(0.015)	(0.003)	(0.005)	(0.007)	(0.056)
month +3	0.010	-0.001	0.000	0.028	0.012	0.004	0.004	-0.005	0.098*
	(0.024)	(0.022)	(0.018)	(0.078)	(0.015)	(0.003)	(0.005)	(0.007)	(0.052)
month +4	-0.005	0.015	0.029	-0.170**	0.004	0.004	-0.007	0.006	-0.024
	(0.024)	(0.021)	(0.018)	(0.079)	(0.015)	(0.003)	(0.005)	(0.007)	(0.050)
month +5	-0.023	-0.013	0.020	-0.138*	0.016	0.001	0.001	0.010	0.050
	(0.024)	(0.021)	(0.019)	(0.079)	(0.014)	(0.003)	(0.005)	(0.007)	(0.048)
month +6	-0.012	0.009	0.013	0.055	0.003	-0.002	-0.004	-0.008	0.055
	(0.024)	(0.021)	(0.019)	(0.079)	(0.014)	(0.003)	(0.005)	(0.007)	(0.048)
month +7	0.038	0.060***	-0.015	0.018	0.007	-0.002	-0.013**	0.003	-0.114**
	(0.024)	(0.021)	(0.019)	(0.077)	(0.014)	(0.003)	(0.005)	(0.007)	(0.049)
month +8	0.005	-0.004	0.005	0.031	-0.022	-0.003	0.000	0.002	0.044
	(0.025)	(0.021)	(0.019)	(0.076)	(0.014)	(0.003)	(0.005)	(0.007)	(0.051)
month +9	-0.009	-0.021	-0.014	-0.024	0.000	0.000	0.003	-0.001	0.064
	(0.025)	(0.021)	(0.019)	(0.076)	(0.014)	(0.003)	(0.005)	(0.007)	(0.052)
month +10	-0.028	-0.018	0.011	-0.055	-0.013	0.002	0.004	-0.001	-0.015
	(0.025)	(0.021)	(0.019)	(0.076)	(0.015)	(0.003)	(0.005)	(0.007)	(0.057)
month +11	0.055**	0.008	-0.004	-0.125*	-0.009	0.005*	-0.006	-0.009	-0.101*
	(0.024)	(0.021)	(0.018)	(0.074)	(0.014)	(0.003)	(0.005)	(0.007)	(0.061)
month +12	-0.008	0.020	-0.005	-0.087	-0.018	-0.001	-0.003	-0.003	-0.066
	(0.025)	(0.022)	(0.019)	(0.075)	(0.014)	(0.003)	(0.005)	(0.007)	(0.072)
Term in power	-0.001	-0.002	0.002	-0.029	0.005	-0.004***	0.001	0.003	0.013
	(0.011)	(0.009)	(0.008)	(0.033)	(0.005)	(0.001)	(0.002)	(0.003)	(0.024)
Left	0.025	0.071***	-0.038	0.227	-0.009	-0.001	-0.003	-0.012*	0.000
	(0.030)	(0.027)	(0.026)	(0.160)	(0.012)	(0.002)	(0.005)	(0.006)	(0.000)
Obs.	6498	7060	5916	4115	6946	10731	7970	8002	2823
R²	0.656	0.584	0.334	0.631	0.351	0.818	0.928	0.851	0.864

All regressions include four lags, full set of time dummies, and fixed effects for each region-calendar month combination. Robust standard errors are in parentheses. ***, ** and * denote significance at 1, 5 and 10% level, respectively.

TABLE III

DETERMINANTS OF THE MAGNITUDE OF THE BUDGET CYCLE, PANEL ESTIMATION

Dependent variable:	Social expenditures	Expenditures on culture	Social expenditures	Expenditures on culture	Social expenditures	Expenditures on culture	Social expenditures	Expenditures on culture	Social expenditures	Expenditures on culture	Social expenditures	Expenditures on culture
R stands for:	Education		Urbanization		Democracy		Transparency		Media freedom		Time	
R * Month -3	0.092	-0.107	0.121	0.051	-0.035	0.003	-0.048	0.004	0.002	0.000	-0.019	0.000
	(0.155)	(0.115)	(0.136)	(0.106)	(0.042)	(0.032)	(0.039)	(0.030)	(0.003)	(0.002)	(0.017)	(0.013)
R * Month -2	-0.263*	-0.154	-0.031	-0.086	-0.002	0.001	-0.020	-0.028	-0.006**	-0.004*	-0.025	-0.031**
	(0.160)	(0.120)	(0.145)	(0.114)	(0.043)	(0.033)	(0.040)	(0.030)	(0.003)	(0.002)	(0.017)	(0.013)
R * Month -1	-0.081	-0.232**	-0.312**	-0.014	-0.076*	-0.034	-0.089**	-0.014	-0.007**	-0.002	-0.063***	-0.032**
	(0.145)	(0.109)	(0.141)	(0.111)	(0.043)	(0.033)	(0.039)	(0.030)	(0.003)	(0.002)	(0.017)	(0.013)
R * Month 0	-0.085	-0.193*	0.060	-0.073	0.008	0.016	0.038	0.015	-0.001	-0.002	-0.003	0.012
	(0.145)	(0.108)	(0.143)	(0.106)	(0.043)	(0.033)	(0.039)	(0.030)	(0.003)	(0.002)	(0.017)	(0.013)
Month -3	0.022	-0.002	0.033	0.002	0.025	0.008	0.027	0.008	0.029	0.002	0.035	0.006
	(0.037)	(0.028)	(0.039)	(0.029)	(0.038)	(0.028)	(0.038)	(0.028)	(0.038)	(0.028)	(0.039)	(0.029)
Month -2	0.024	0.012	0.028	0.011	0.026	0.012	0.028	0.012	0.027	0.014	0.026	0.003
	(0.037)	(0.028)	(0.039)	(0.029)	(0.038)	(0.029)	(0.038)	(0.029)	(0.038)	(0.029)	(0.039)	(0.029)
Month -1	0.223***	0.127***	0.227***	0.121***	0.216***	0.121***	0.218***	0.121***	0.227***	0.126***	0.197***	0.107***
	(0.037)	(0.028)	(0.038)	(0.029)	(0.037)	(0.028)	(0.037)	(0.028)	(0.038)	(0.028)	(0.039)	(0.029)
Month 0	0.038	-0.067**	0.036	-0.063**	0.035	-0.066**	0.036	-0.065**	0.036	-0.067**	0.040	-0.059**
	(0.037)	(0.028)	(0.038)	(0.029)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)	(0.038)	(0.029)
Month +1	-0.097***	-0.115***	-0.094**	-0.110***	-0.102***	-0.106***	-0.101***	-0.106***	-0.107***	-0.108***	-0.090**	-0.108***
	(0.037)	(0.027)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)
Month +2	-0.069*	-0.034	-0.069*	-0.034	-0.069*	-0.025	-0.069*	-0.025	-0.071*	-0.022	-0.069*	-0.034
	(0.037)	(0.027)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)
Month +3	0.003	-0.052*	0.005	-0.045	0.011	-0.051*	0.010	-0.051*	0.001	-0.045	0.005	-0.046
	(0.037)	(0.027)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)	(0.037)	(0.028)
Left	-0.003	0.000	-0.003	0.000	-0.003	-0.001	-0.004	-0.001	-0.003	0.000	-0.003	-0.001
	(0.045)	(0.034)	(0.048)	(0.036)	(0.046)	(0.035)	(0.046)	(0.035)	(0.046)	(0.035)	(0.047)	(0.035)
Term in power	-0.011	-0.012	-0.010	-0.012	-0.012	-0.014	-0.011	-0.014	-0.011	-0.016	-0.007	-0.010
	(0.015)	(0.011)	(0.015)	(0.011)	(0.015)	(0.011)	(0.015)	(0.011)	(0.015)	(0.011)	(0.015)	(0.011)
Time											0.058***	0.041***
											(0.010)	(0.007)
Observations	6410	6356	7004	6921	6593	6563	6593	6563	6671	6632	7004	6921
Number of regions	76	76	86	86	80	80	80	80	81	81	86	86
R-squared	0.596	0.497	0.560	0.479	0.584	0.479	0.584	0.479	0.581	0.482	0.561	0.480

All regressions include four lags, full set of time dummies, and fixed effects for each region-calendar month combination. All dependent variables are in logs and measured in real terms per capita. Robust standard errors are in parentheses. ***, **, and * denote significance at 1, 5, and 10% level, respectively. The results are robust to the number of pre-electoral dummies included and to accounting for R and Time influence jointly or in separate specifications.

TABLE IV
SUMMARY STATISTICS FOR THE CONSTRUCTED MEASURES OF THE
AMPLITUDE OF THE CYCLES

	Obs.	Mean	Median	S.E.	Min	Max
Amplitude of the cycle in:						
Total budget expenditures	132	0.075 *	0.073	0.018	-0.713	0.653
Social expenditures	136	0.158 *	0.100	0.030	-0.624	1.442
Education expenditures	136	0.088 *	0.082	0.017	-0.517	0.581
Expenditures on culture	134	0.078 *	0.093	0.020	-0.669	0.642
Healthcare expenditures	135	0.089 *	0.091	0.020	-0.734	0.692
Media expenditures	131	0.089 *	0.068	0.039	-1.219	1.346
Expenditures on industry	113	0.171 *	0.240	0.062	-1.662	1.793
Deficit	127	0.035 *	0.038	0.014	-0.389	0.440
Negative of wage arrears growth	44	0.035	0.020	0.037	-0.738	0.631

Asterisks mark mean amplitudes that are significantly different from zero.

TABLE V
DETERMINANTS OF THE MAGNITUDE OF THE BUDGET CYCLE, CROSS-SECTION

	The first component of cycle amplitudes					Amplitude of total expenditure cycle					Amplitude of social expenditure cycle				
Education	-1.269** (0.621)					-0.016 (0.081)					-0.139 (0.117)				
Urbanization		-1.706*** (0.515)					-0.175*** (0.065)					-0.508*** (0.094)			
Democracy			-0.801*** (0.252)					-0.076** (0.038)					-0.164*** (0.057)		
Gov. transparency				-0.272* (0.145)					-0.038** (0.018)					-0.098*** (0.027)	
Media freedom					-0.019* (0.010)					0 (0.001)					-0.006** (0.002)
Time	-0.314*** (0.068)	-0.339*** (0.067)	-0.323*** (0.066)	-0.323*** (0.066)	-0.321*** (0.067)	-0.034*** (0.008)	-0.035*** (0.009)	-0.034*** (0.008)	-0.034*** (0.008)	-0.034*** (0.009)	-0.043*** (0.016)	-0.049*** (0.017)	-0.043*** (0.016)	-0.044*** (0.016)	-0.045*** (0.016)
Av. regional budget	0.726*** (0.268)	0.886*** (0.279)	0.765*** (0.275)	0.610** (0.298)	0.607** (0.271)	0.083** (0.037)	0.110*** (0.037)	0.096*** (0.035)	0.082** (0.037)	0.082** (0.035)	0.102** (0.050)	0.169*** [0.052]	0.123** (0.052)	0.086 (0.058)	0.094* (0.047)
Length of term	-0.261 (0.338)	-0.281 (0.348)	-0.241 (0.367)	-0.311 (0.352)	-0.319 (0.369)	-0.017 (0.042)	-0.029 (0.041)	-0.023 (0.043)	-0.037 (0.040)	-0.014 (0.047)	-0.005 (0.066)	-0.033 [0.063]	-0.019 (0.065)	-0.051 (0.061)	-0.05 (0.070)
Constant	2.814*** (0.592)	3.160*** (0.581)	2.923*** (0.584)	2.907*** (0.585)	2.900*** (0.594)	0.392*** (0.075)	0.420*** (0.076)	0.395*** (0.075)	0.395*** (0.074)	0.394*** (0.076)	0.557*** (0.151)	0.648*** [0.158]	0.566*** (0.151)	0.570*** (0.153)	0.585*** (0.155)
Observations	117	117	117	117	117	121	121	121	121	121	125	125	125	125	125
R²	0.242	0.259	0.252	0.235	0.234	0.154	0.182	0.174	0.179	0.153	0.086	0.155	0.109	0.133	0.121

	Amplitude of education expenditure cycle					Amplitude of cycle in expenditure on culture					Amplitude of expenditure spending cycle				
Education	-0.202** (0.079)					-0.213*** (0.061)					-0.164** (0.072)				
Urbanization		-0.108 (0.074)					-0.167* (0.086)					-0.216*** (0.078)			
Democracy			-0.064* (0.034)					-0.117*** (0.043)					-0.081** (0.039)		
Gov. transparency				-0.017 (0.019)					-0.015 (0.022)					-0.029 (0.023)	
Media freedom					-0.002 (0.001)					-0.004*** (0.001)					-0.002 (0.001)
Time	-0.031*** (0.008)	-0.032*** (0.008)	-0.031*** (0.008)	-0.031*** (0.008)	-0.032*** (0.008)	-0.042*** (0.011)	-0.043*** (0.010)	-0.042*** (0.010)	-0.042*** (0.011)	-0.042*** (0.010)	-0.035*** (0.011)	-0.037*** (0.011)	-0.035*** (0.011)	-0.035*** (0.011)	-0.036*** (0.011)
Av. regional budget	0.077** (0.030)	0.071** (0.035)	0.068** (0.032)	0.054 (0.033)	0.056* (0.031)	0.066 (0.047)	0.069 (0.051)	0.068 (0.051)	0.042 (0.052)	0.045 (0.048)	0.105** (0.046)	0.122*** (0.043)	0.105** (0.047)	0.087* (0.048)	0.090** (0.044)
Length of term	-0.103** (0.042)	-0.102** (0.046)	-0.102** (0.048)	-0.104** (0.044)	-0.109** (0.046)	-0.013 (0.049)	-0.013 (0.052)	-0.016 (0.050)	-0.011 (0.055)	-0.033 (0.051)	-0.094** (0.048)	-0.102** (0.046)	-0.097* (0.049)	-0.104** (0.046)	-0.107** (0.052)
Constant	0.388*** (0.072)	0.402*** (0.072)	0.388*** (0.071)	0.385*** (0.072)	0.391*** (0.071)	0.451*** (0.090)	0.472*** (0.085)	0.452*** (0.088)	0.443*** (0.090)	0.454*** (0.088)	0.424*** (0.096)	0.462*** (0.094)	0.429*** (0.095)	0.428*** (0.093)	0.435*** (0.096)
Observations	126	126	126	126	126	123	123	123	123	123	125	125	125	125	125
R²	0.236	0.186	0.191	0.18	0.186	0.195	0.166	0.184	0.148	0.19	0.176	0.181	0.166	0.16	0.162

Robust and adjusted for clusters within regions standard errors are in parentheses. ***, ** and * denote significance at 1, 5 and 10% level, respectively.

TABLE VI

EFFECT OF CYCLES ON POPULARITY OF INCUMBENTS AND PROBABILITY TO WIN

	Incumbents' populatiry, OLS						Incumbent wins/first runner up/loses, ordered probit					
First component of amplitudes	0.031**						0.155*					
	(0.015)						(0.088)					
Amplitude of total exp. cycle		0.065						0.087				
		(0.129)						(0.702)				
Amplitude of social exp. cycle			0.125**						0.760*			
			(0.057)						(0.394)			
Amplitude of healthcare exp. cycle				0.112						0.804		
				(0.097)						(0.539)		
Amplitude of education exp. cycle					0.234*						1.801**	
					(0.127)						(0.702)	
Amplitude of cycle in exp. on culture						0.201***						0.814
						(0.074)						(0.505)
Time	0.045***	0.034***	0.035***	0.031***	0.036***	0.040***	0.258***	0.192**	0.235***	0.217***	0.247***	0.234***
	(0.013)	(0.012)	(0.010)	(0.011)	(0.011)	(0.011)	(0.082)	(0.076)	(0.067)	(0.069)	(0.072)	(0.069)
Time * Respective cycle amplitude	-0.003	0.01	0.024	0.012	0.015	-0.036	-0.007	0.065	0.241	-0.013	0.156	-0.198
	(0.007)	(0.057)	(0.026)	(0.050)	(0.064)	(0.043)	(0.034)	(0.257)	(0.169)	(0.249)	(0.263)	(0.256)
Left	-0.073*	-0.078*	-0.099**	-0.081*	-0.080*	-0.091**	-0.398*	-0.395*	-0.499**	-0.391*	-0.373*	-0.444**
	(0.042)	(0.047)	(0.045)	(0.046)	(0.045)	(0.041)	(0.225)	(0.221)	(0.220)	(0.219)	(0.219)	(0.220)
Length of term	0.152**	0.11	0.116*	0.129**	0.146**	0.150**	0.874***	0.610**	0.730**	0.806***	0.973***	0.908***
	(0.064)	(0.069)	(0.063)	(0.062)	(0.061)	(0.059)	(0.337)	(0.309)	(0.310)	(0.312)	(0.316)	(0.338)
Relative product	0.366*	0.199	0.318	0.163	0.307	0.309	0.843	0.18	0.95	0.822	1.138	0.719
	(0.218)	(0.221)	(0.213)	(0.214)	(0.218)	(0.195)	(1.192)	(1.118)	(1.107)	(1.265)	(1.219)	(1.107)
Relative social expenditure share	0.051	0.092	0.063	0.043	0.04	0.04	-0.026	0.15	0.262	0.145	0.009	0.091
	(0.109)	(0.115)	(0.104)	(0.107)	(0.102)	(0.104)	(0.617)	(0.613)	(0.544)	(0.574)	(0.600)	(0.551)
Reltive inflation	-10.826	-1.495	-2.754	-2.828	-4.437	-13.678	1.06	34.164	26.274	22.728	10.952	-19.452
	(13.206)	(12.396)	(11.139)	(11.971)	(12.291)	(12.786)	(53.504)	(47.893)	(49.212)	(49.525)	(50.594)	(52.491)
Urbanization	0.306***	0.286***	0.321***	0.309***	0.310***	0.305***	1.685***	1.553***	1.725***	1.680***	1.670***	1.625***
	(0.079)	(0.081)	(0.079)	(0.081)	(0.079)	(0.075)	(0.454)	(0.443)	(0.443)	(0.445)	(0.430)	(0.455)
Constant	0.194	0.297**	0.288***	0.320***	0.263**	0.230**						
	(0.118)	(0.117)	(0.096)	(0.118)	(0.112)	(0.105)						
Observations	115	119	123	123	124	121	115	119	123	123	124	121
R-squared	0.269	0.197	0.234	0.2	0.232	0.276						

Robust and adjusted for clusters within regions standard errors are in parentheses. ***, ** and * denote significance at 1, 5 and 10% level, respectively.
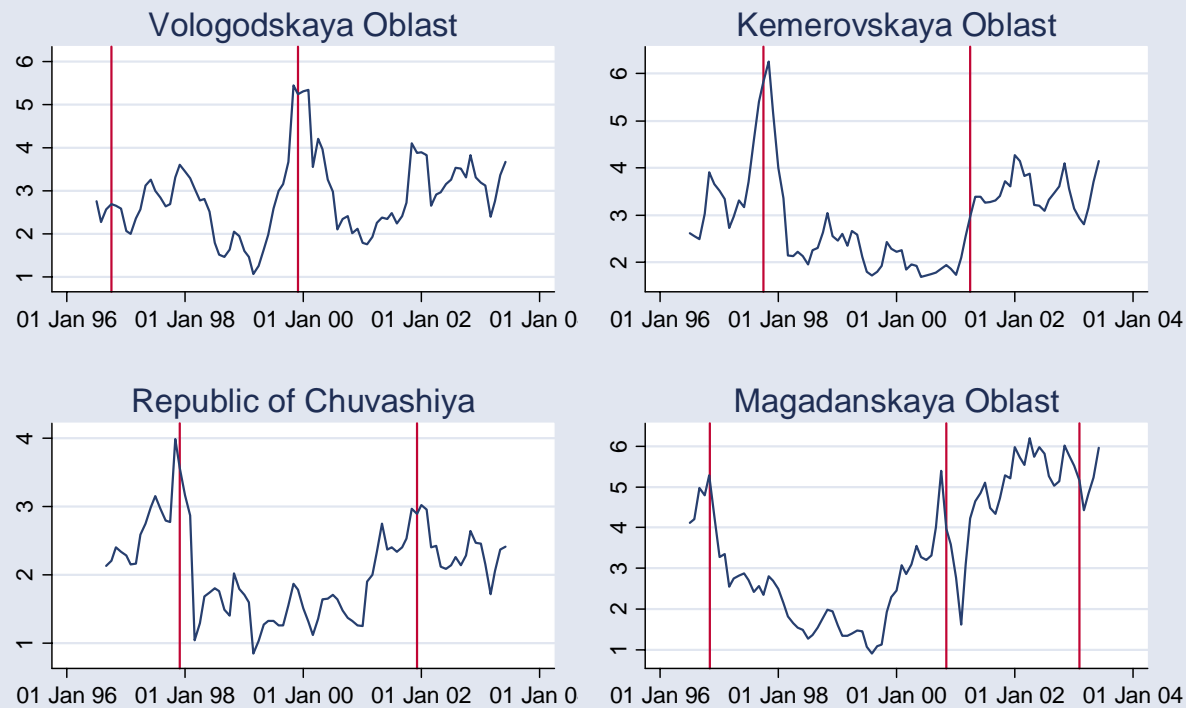
FIGURE I

ELECTIONS AND ACTUAL SERIES OF REGIONAL PER CAPITAL SOCIAL EXPENDITURES
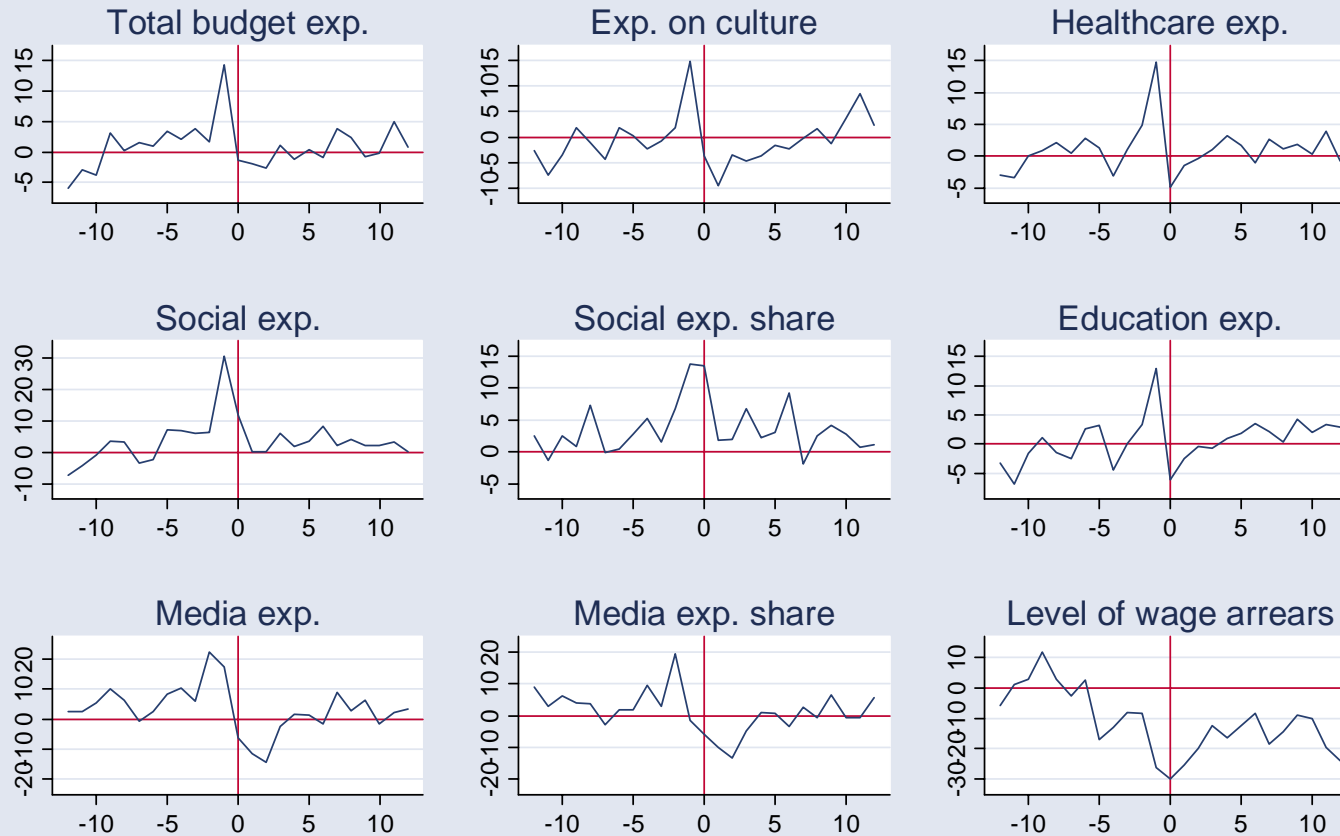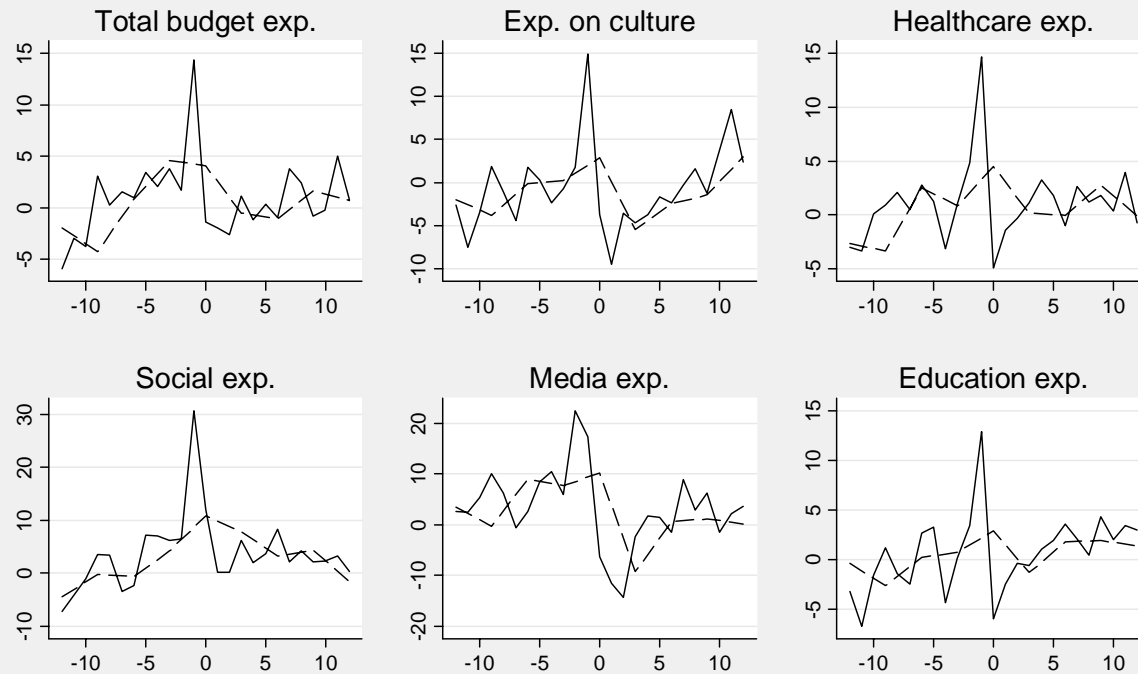(SMOOTHED BY A MOVING AVERAGE)

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FIGURE II
ESTIMATED POLITICAL BUDGET CYCLE

Predicted percentage deviation from trend
using monthly data (solid line) and quarterly data (dashed line)

Horizontal axes - months away from elections

FIGURE III
UNDERESTIMATION IN QUARTERLY DATA

Panel A. Descriptive statistics of policy instruments and outcomes

Variable	No. of obs.	Mean	Median	SD	Min	Max	Source	Units*	Time span	P-value**
Total budget expenditures	7370	45.07	26.92	61.58	5.79	606.16	MF	$ per capita	03/1996 - 07/2003	0.42
Social expenditures	7522	2.90	2.10	3.05	0.18	29.36	MF	$ per capita	01/1996 - 07/2003	0.62
Share of social expenditures	7244	7.83	7.30	3.76	1.17	29.83	MF	Percentage	01/1996 - 07/2003	0.58
Education expenditures	7528	9.28	6.12	10.24	1.29	89.40	MF	$ per capita	01/1996 - 07/2003	0.78
Expenditures on culture	7512	1.07	0.68	1.28	0.12	11.31	MF	$ per capita	01/1996 - 07/2003	0.65
Healthcare expenditures	7524	5.95	4.12	6.05	0.83	50.71	MF	$ per capita	01/1996 - 07/2003	0.65
Media expenditures	7332	0.18	0.08	0.35	0.00	3.51	MF	$ per capita	03/1996 - 07/2003	0.96
Share of media expenditures	7210	0.37	0.30	0.26	0.02	1.76	MF	Percentage	03/1996 - 07/2003	0.40
Expenditures on industry	7181	2.23	0.49	7.13	0.00	90.90	MF	$ per capita	01/1996 - 07/2003	0.93
Total budget revenues	7355	45.35	27.30	60.60	5.85	519.08	MF	$ per capita	01/1996 - 07/2003	0.27
Tax revenues	7542	25.76	16.72	34.08	1.54	320.62	MF	$ per capita	01/1996 - 07/2003	0.40
Ratio of expenditures to revenues	7094	100.10	98.47	23.83	43.47	266.37	MF	Percentage	03/1996 - 07/2003	0.62
Growth	7826	0.0003	0.0005	0.0256	-0.17	0.15	GKS	Percentage	02/1995 - 07/2003	0.56
Inflation	11246	0.19	0.12	0.23	-0.18	1.68	GKS	Percentage	02/1992 - 07/2003	0.26
Regional wage arrears	3870	0.0064	0.0012	0.0220	0.0000	0.25	GKS	$ per capita	01/1999 - 07/2003	0.76
Wage level	8410	164.61	131.09	104.05	48.21	671.72	GKS	$ per capita	02/1995 - 07/2003	0.09
Money income	8463	125.24	102.65	76.32	32.00	566.03	GKS	$ per capita	01/1995 - 07/2003	0.38

* "$ per capita" stands for constant 1997 US dollars per capita. ** P-values are for the test of residual autocorrelation in equation (1); see footnote 7.

Panel B. Descriptive statistics of elections

Variable	No. of obs.	Mean	Median	SD	Min	Max
Dummy for participation of incumbent	214	0.90	1	0.31	0	1
Dummy for incumbents' win	192	0.65	1	0.48	0	1
Dummy for incumbent coming the third or worse	192	0.06	0	0.23	0	1
Percentage of votes for incumbent	192	53.98	56.23	22.84	4.76	99.9
Percentage of votes for main competitor of incumbent	181	32.30	28.50	20.87	0.71	82
Percentage of votes for winner	214	62.28	59.28	15.06	23.5	99.9
Percentage of votes for the first runner up	202	24.09	24.23	12.98	0.71	48
Number of candidates	214	5.69	5	2.96	1	16

Panel C. Descriptive statistics of determinants of cycle magnitude

Variable	Regions	Mean	Median	SD	Min	Max
Log of education in 1995	76	2.78	2.75	0.26	2.19	3.68
Log of share of urban population	86	4.17	4.23	0.28	2.91	4.61
Index of freedom of media production	81	36.25	37	14.31	0	75
Index of democracy	77	2.96	3	0.87	1	5
Index of government transparency	77	2.99	3	0.93	1	5
Dummy for length of term above 4 years	88	0.26	1	0.44	0	1