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Politics Before Pupils?
Electoral Cycles and School Resources in India

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ABSTRACT

Politics Before Pupils?
Electoral Cycles and School Resources in India*

Primary education in India is a development question of a unique magnitude, and the delivery of education by Indian states is often suspected to be marred by political haggling and corruption. Using rich administrative school-level panel data across Indian states, we test for electoral cycles in the provision of school resources. The effects are identified using staggered timing of state elections. We find that rulers allocate more primary school resources in the years preceding and following elections, but there is only weak evidence that resources are targeted to marginal constituencies. The resources affected are visible ones, namely free school uniforms, classrooms, toilets, ramps for the disabled and medical inspections. We also show that around election years, teachers spend more time on "non-teaching" activities. The political cycles are not inevitable, as they are present only in districts characterised by low voter turnout and low female literacy. Finally, we show that electoral cycles affect human capital accumulation: The phase of the electoral cycle in which pupils begin their primary schooling, affects their learning outcomes.

JEL Classification: H75, I25, O15, P16

Keywords: institutions, school resources, political cycle, public goods, voter turnout, India

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1 Introduction

A long standing debate exists on the relevance of specific school resources for learning. Studies tend to find some school infrastructure and pedagogical materials to be more important than others for learning outcomes in developing countries (see e.g. Glewwe et al., 2014 for a review). According to Glewwe et al., for example in South Asia, real government expenditure on education increased by almost eight-fold since the 1980s. Despite increases in resources, learning outcomes can remain poor.

Current literature suggests that the main bottlenecks to improved learning outcomes are not individual school resources *per se*, but rather a poor institutional framework - a mixture of poor incentives for teachers and administrators, lack of accountability, and corruption (for a summary of recent research and key issues in the Indian context, see e.g. Muralidharan 2013).

In this study, we examine the existence of election cycles and political manipulation in the distribution of primary education resources in India. Our analysis focuses on state assembly elections. Despite some prominent national programs, the states of India continue to carry the main responsibility for the provision of basic education.[1] As implied above, the significance of such cycles would not limit itself to the role of school resources in the educational production function. Political involvement in such a basic service as primary schooling is likely to proxy for poor political and administrative institutions in general.

Our study uses school-level data from a register database of Indian public primary schools (District Information System of Education, 'DISE'). The dataset used covers lower primary schools across entire India over 7 recent years, between 2005-2011. We supplement the analysis with a household level survey dataset on learning outcomes for literacy and numeracy (Annual Status of Education Report, 'ASER').

Identification of the effects of the political cycle is based on a number of features available to us. Firstly, the possibility of using school-level fixed effects allows us to control for any potential school-specific omitted time-invariant variables. Secondly, the pre-determined, staggered schedule of the state assembly elections provides an exogenous measure of political cycles. Given that in a few cases the election year departs from the schedule, we further use the intended electoral cycle as an instrumental variable for the actualised electoral cycle. Overall, this study produces credibly measured effects of the electoral cycle on resources in the context of a public service that is crucial for economic development.

This study contributes to several strands of literature in Economics. Firstly, the study tests for the presence and of electoral budget cycles in the context of a key public service for development. In contrast with other similar studies, we use micro data and measure the actual deliverables, as opposed to the flow of funds. The importance of this distinction has been emphasized for example by Khemani (2004) and Ablo and Reinikka (1998). We study whether the key theories on tactical distribution, the 'core-voter' and 'swing voter' models, are supported by the data. We also take the literature on the electoral cycles forward by utilising district level variation in political and educational factors across the country to study what determines the magnitude of the cycles. Further research in this area has been called for by Shi and Svensson (2006) for example. Secondly, our

1 Over the period we cover, a large national education programme, the Sarva Shiksha Abhiyan (SSA) has been rolled out, aiming to guarantee a minimum level of educational provision across the country (see http://mhrd.gov.in/schemes). Note that the Right to Education Act, which sets ambitious minimum level of educational provision, came into force on April 2010, which is towards the end of our sample.

results indicate that politically induced volatility in the administration of school resources can be large enough to matter for learning outcomes. This finding contributes broadly to the literature on the role of governance factors in the production of human capital (see e.g. Kremer and Holla, 2009 for some discussion on learning outcomes and school governance). Finally, our results suggest a need for further research on the role of voter turnout in the determination of political cycles.

We find robust political cycles in the distribution of lower primary school resources. In particular, politicians allocate more school resources in the year preceding elections and on the election year. The resources affected are visible ones, such as free school uniforms, girl's toilets, classrooms, ramps for the disabled and medical examinations. Importantly, this cycle in school infrastructure and materials emerges in districts with lower levels of voter turnout and female literacy. These areas tend to be concentrated in the Northern parts of the country, where development outcomes are generally weaker. In addition, in these areas, teachers spend more time on "non-teaching" activities around election years; a result that is likely to reflect political involvement and staffing of election booths. On the contrary, arguably one of the most important school resources, the number of teachers, rises in areas with high voter turnout after elections.

We do not find strong evidence that these increases in resources would be targeted towards politically aligned or tightly contested districts in a state. The pre-election effect is slightly larger in tightly contested areas and the post-election effect slightly larger in politically aligned areas offering some support for the swing voter and the core voter theories, but these differences are not statistically significant.

Lower voter turnout and female literacy are likely to proxy for lower levels of political awareness and engagement. These are factors suggested to give rise to opportunistic budget cycles for instance by Shi and Svensson (2006), given that voters are less able to distinguish between pre-electoral vote buying and competence.

Finally, using the ASER household survey that tests children, we find that the phase of the electoral cycle during which pupils begin their primary schooling, affects their learning outcomes. These results are relative effects between cohorts, and the negative net effects of the political cycles may well be larger. Overall, the presence of political cycles can have longer term implications for educational inequalities both across cohorts, as well as across districts due to cycles being concentrated to areas that lag behind in educational outcomes to begin with.

We begin by reviewing some earlier findings from relevant literature (Chapter 2), and continue with a description of the data (Chapter 3) and econometric identification (Chapter 4). The key results and extensions are covered in Chapter 5. In Chapter 6, we use a separate dataset to assess the impact of the electoral cycle on learning outcomes. Chapter 7 concludes.

2 Relevant literature

Political budget cycles refer to increases in government spending in the election year, generally aimed at increasing the chances of re-election for the incumbent (see e.g. Drazen, 2008). There is a considerable literature on political budget cycles (see e.g. Nordhaus 1975; Rogoff and Sibert 1988; and Drazen, 2001 for a review). Elections might trigger higher or lower public expenditure, depending on what the politicians believe to be valued by the electorate. The electoral cycle might also alter the composition of public expenditure, depending on the items of expenditure that are perceived as more important, or visible to the electorate (e.g. roads, health care) (see e.g. Keefer and Khemani, 2005, Drazen and Eslava, 2010).

Various theories exist on how and when public resources might be distributed by politicians. The 'core voter' model (Cox and McCubbins, 1986) suggests that vote-maximizing parties allocate benefits primarily to their core supporters. The 'swing voter' theory (Lindbeck and Weibull, 1987) suggests that politicians would target possible swing voters or swing districts with previously tightly contested elections. Benefits could be given to voters before elections or benefits could be promised upon victory (see e.g. Cox, 2002)). Empirically, studies have tended to analyse the allocation of resources across electoral districts. Such studies can be found both in support of the core voter and the swing voter theories. Recent studies on the latter include for instance Stokes (2005), Dahlberg and Johansson (2002), and Case (2001) and of the former, Ansolabehere and Snyder (2006), Levitt and Snyder (1995) and Cole (2009).

Studies indicate that political budget cycles can be more prevalent in developing than developed countries, due to institutional weaknesses (such as democracy, government transparency) and a poorly informed electorate (see e.g. Akhmedov and Zhuravskaya, 2004 and Shi and Svensson, 2006). A poorly informed, or myopic electorate may be susceptible to short-term pre-election vote buying. They may also be less efficient in voting out politicians who serve them poorly during their terms, as they are unable to distinguish between competence and pre-electoral manipulation (see Shi and Svensson, 2006).

In the Indian context, with the use of state-level data from 14 states over a thirty year period, Khemani (2004) finds some evidence of political cycles in the composition of state budgets in the case of Indian State Assembly Elections. She does not find evidence of broad based budget increases in election years, but of more targeted increases, such as small increases in capital spending, targeted tax breaks, and increases in road construction. Saez and Sinha (2010) use state level data to show that the timing of elections and a range of other political variables affect the level of expenditure.[2] Cole (2009) finds that government-owned banks provide more agricultural credit in the election year, targeting particularly those districts with tightly contested elections.

3 Data

The data on school resources come from an administrative school records database, the District Information System for Education (DISE), collected by the National University of Educational Planning and Administration (NUEPA).[3] From the year 2005 onwards, the database has full, or nearly full coverage of government administered primary schools in India. Our dataset is a panel of schools for seven years between 2005-2011. In terms of timing, the year 2005 refers to the academic year 2005-06, and the data are collected in the Autumn of 2005. The sample is restricted to lower primary schools, which in most states spans grades 1-5.[4] For schools that include both lower and upper primary schools, the resource variables in this study will only relate to lower primary students if possible.[5] The source for the learning outcomes data is the ASER survey, which will be covered in Chapter 6.

Summary statistics of the variables used in the analysis for the main regression sample are shown in Table 1. Outliers and observations of poor quality have been excluded. The selection of the sample

2 Crost and Kambhampati (2010) focus specifically on education spending using village level data for 120 villages in two Northern Indian states in late 1990s. They consider a range of variables, including political ones and find for instance that party fractionalisation, party turnover and political reservations matter for specific school resources. Their study has no time dimension or an attempt at econometric identification.

3 http://www.dise.in/

4 In some states, lower primary schools cover grades 1-4.

5 Building related variables (such as boundary wall, ramp) cannot be separated for lower and upper primary students.

and the data cleaning procedure are documented more precisely in the Appendix. The final sample covers schools in 29 states**,** or union territories and in 600 districts. It includes about 4.5 million observations of nearly 1 million unique primary schools in the form of an unbalanced panel of seven years.

The data on Indian Assembly Elections are supplied by the Election Commission of India[6]. Our study uses data on election outcomes of constituencies for the years 2000-2012. The timetable for the elections can be found in the Appendix, Table A2. Each state is divided into a number of election constituencies. The winning candidate in each constituency gets a seat in the State Assembly, from which the state government is formed. By constitution, the Assembly Elections are carried out in each state every five years, but the cycle is different across states, so that every year sees elections being held in some states.

Table 1 Summary statistics, pooled school data 2005-2011

	Obs.	Mean	S.D.	Min	Max
# Teachers	4461840	2.796	1.812	0	59
Days of non-teaching per teacher	4461768	2.32	11.02	0	365
Pupils per teacher	4316215	44.44	35.51	0.14	305.50
Enrolment	4479855	119.93	101.73	1	905
School Resources					
# of Free textbooks per pupil	4479855	.824	.398	0	3.167
# of Free uniforms per pupil	4479855	.239	.335	0	1.430
Number of classrooms per 100 pupils	4479855	3.95	3.57	0	36.36
Toilet	4479855	.535	.499	0	1
Girls' toilet	4479855	.467	.499	0	1
Electricity	4479855	.269	.443	0	1
Water index	4479855	1.839	.880	0	3
Building quality index	4479855	3.706	.799	0	4
Boundary wall	4479855	.435	.496	0	1
Book bank	4479855	.520	.500	0	1
# of Computers per 100 pupils	4479855	.139	.710	0	10
Ramp	4479855	.453	.498	0	1
Medical examinations	4479855	.556	.497	0	1
Playground	4479855	.469	.499	0	1
District level data					
% seats in aligned with state	4446424	.493	.269	0	1
Voter turnout	4445712	.653	.116	.051	.999
% seats tightly contested	4446424	.165	.169	0	1
% females literate	4437760	.613	.113	.303	.977
% urban	4437760	.239	.176	0	1

Notes: Teachers, pupils, textbooks, uniforms refer to classes 1-5 (or lower primary schools). Water index takes values between 0-3, depending on the quality of water provision (0-none, 1-well, 2-hand pump, 3-tap). Building quality index is a similar index ranging between 0-4 with the quality of the school building (0-none, 1-tent, 2-hay or mud-based, 3-partly solidly built, 4-fully solid). The district level variables are described in Chapter 5.

Since political constituencies cannot be matched directly to school-level data, the matching is done at the level of districts, which represent a sub-state administrative level. Each district is composed of a number of political constituencies. The median number of constituencies per district is six, and the number is between 2-14 for 95% of the district-years.[7] Overall, the final sample includes schools

6 http://eci.nic.in/eci/eci.html

7 The number of constituencies per district varies somewhat over time due to a delimitation exercise in 2008, which

in 4201 assembly constituencies.

The study focuses on school resources. Many of these are resources that functioning schools would be expected to have and are also specified in the 2009 Indian Right to Education (RTE) Act[8]. The RTE Act of 2009 specifies for instance that a school should include an "all-weather" building, a safe source of drinking water, separate toilets for boys and girls, barrier free access, a boundary wall or fence, a library, a playground and at least one classroom per teacher. Overall, the effects of elections on 14 resources, the number of teachers and their engagement in non-teaching assignments are studied. The summary statistics in Table 1 indicate that between 2005-2011, less than a half of the schools had a girl's toilet, a boundary wall, a book bank or a playground. The political variables summarised in Table 1 will be discussed in more detail in Chapter 5.

States in India are the main decision makers on educational spending, but are supported by the central government (by up to 60% in 2009-10). The pattern of spending varies by state. Although the cost of teachers is typically the largest single cost item, some states spend more on school infrastructure.[9]

It is apparent from the data that the concept of 'school resources' is multifaceted. As mentioned already, states may have different spending priorities. For the identification of the effect of the electoral cycle on resources, this poses the problem that the cycle may have effects on different resource items in different parts of the country. For example, the quality of the building may have a different priority in different climates. To account for the possibility that different school resources may matter more in different polities and climates, we created an index of the 14 resource variables in Table 1 by taking the first principal component (PC1) of their normalised transformations, and normalising the PC1 to have a mean of zero and a standard deviation of unity.[10] Alternatives to the PC1 will be presented as a robustness check. We present most of the analysis using the first principal component as the dependent variable, but also produce the key results for all 14 components separately. Teachers are analysed separately in chapter 5.2.

4 Econometric models and identification

The following model is used to estimate the effects of the timing of elections on school resources:

$$(1)\quad Resource_{it} = \beta_y D_{ys} + \lambda_t + \tau_s t + \alpha_i + u_{it} \qquad t \in [2005, 2011] \qquad y \in [0,4]$$

where i refers to school and s to state. Years are denoted by t, and y denotes the number of years from the latest election, zero being an election year, and 4 being the year before elections. The coefficients of interest are the β coefficients, which measure the effect of the political cycle on the resources. In the case that the dependent variable is a dummy variable, linear probability models are estimated. Correct standard errors require clustering at the state-year level, but due to potential autocorrelation in the dependent variable(s), the clustering will be done at the state level. All models control for year effects (λ_t), state trends (τ_s) and school fixed effects (α_i).

Correct temporal matching of the political data to the school resource data is important. For example, the information on school resources for the academic year 2008-09 (DISE 2008) is

changed the boundaries of the assembly constituencies. However, these boundaries or boundary changes do not cross district borders, apart from few exceptions.

8 http://mhrd.gov.in/rte

9 See e.g. Accountability Initiative, Budget Brief, Education Sector, Vol. 4, Issue 5.

10 Correlations of the 14 resource items with PC1 are shown in Table A3 of the Appendix.

collected in the Autumn of 2008, which means that any improvements in resources will have taken place between the Autumn of 2007 and the Autumn of 2008. The improvements were likely to have been carried out at the latest by the end of the fiscal and school years ending around April-June 2008. An opportunistic politician, aiming to influence election results in 2008, would be likely to target such improvements to 2007-2008. In our dataset, the election results for the calendar year 2008 are matched to the DISE 2009 data. Therefore, an effect of the 2008 elections on resources in DISE 2008 would be considered a pre-election effect. In turn, an effect of the 2008 elections on the DISE 2009, would be considered a post-election effect, because improvements are more likely to have been carried out by the newly elected government. In equation 1, D_{0s} and D_{1s} refer to the first two post-election years and D_{4s} to the pre-election year.[11]

Figure 1 Timing of the school resource data and elections, assuming elections in 2008

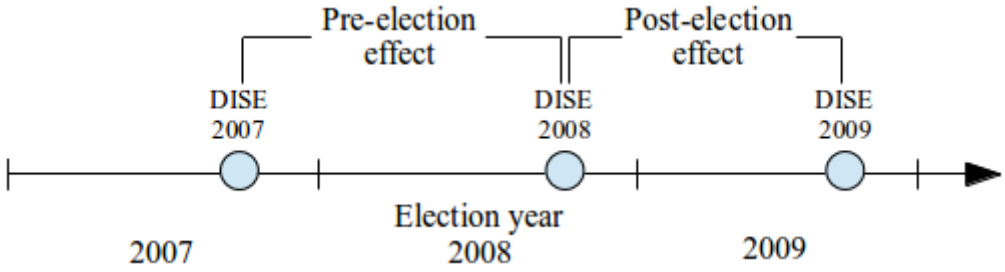


Figure 1 illustrates the timing of the data, assuming that elections are held in the calendar year 2008. As the school data are annual, but the election month election varies, it is inevitably impossible to cleanly divide all effects into pre- and post-election effects, and this should be kept in mind when interpreting the results.

The identification of the β coefficients relies on the staggered timing of state elections across the 29 states and territories in the sample. In each state, the assembly elections are held every five years. In our sample there are a few exceptions, in which the elections have been held early, and one case in which the elections were held six years apart. Since early (or late) elections may be correlated with the political process, we instrument the timing of the elections with the original, scheduled election cycle. For example, the instrument for the next election year is the fifth year after the previous election. This is identical to the identification strategy used by Khemani (2004) and Cole (2009).

Table 2 Definition of timing dummies for OLS and IV estimations, hypothetical

Year	93	94	95	96	97	98	99	00	01	02	03	04	05	06	07	08	09	10	11	12
Election	E	-	-	-	-	E	-	-	E	-	-	-	-	-	E	-	-	-	-	E
Spacing		normal				short				long					normal					
OLS dummies	0	1	2	3	4	0	1	4	0	1	2	2	3	4	0	1	2	3	4	0
Instruments	0	1	2	3	4	0	1	2	0	1	2	3	4	0	0	1	2	3	4	0

Definitions:
E = Election year
0 = 0 years from election ("Post-Election year")
1 = 1 year from election
2 = 2 years from election (Control year)
3 = 3 years from election
4 = 4 years from election ("Pre-election year")

11 We have also tried alternatives in which we utilise the month of the election more precisely, and define the election year to run from April to March or October to September, but these alternatives would lead to similar results.

Table 2 illuminates the difference between the electoral cycle dummies with OLS and their instrumental variables for a hypothetical set of election cycles. For example, with OLS, there will be a dummy (labelled '4') for each pre-election year, whereas the instrument follows a five year cycle that begins again after each election. [12]

5 Results

5.1 Physical school resources

The results of model (1) on the effects of the electoral cycle on school resources, using the first principal component as the outcome variable, are shown in columns 1 and 2 of Table 3. Column 1 refers to an OLS model, and column 2 to an instrumental variables (IV) estimation.[13] All models include school fixed effects, so that the election effects refer to within school deviations. Only the coefficients and standard errors of the electoral cycle dummies are displayed (β_y coefficients in model 1).

The results indicate that resource provision is characterised by a political cycle; the coefficient estimates become larger closer to the elections, and become statistically significant just before and after the elections. The point of comparison in these models is the year which is furthest away from the elections. As the dependent variable is normalised, the estimates can be interpreted as standard deviations. The estimates for the 'pre-election' year effect suggest an increase of .081 to .088 standard deviations, and slightly lower increases of .077 to .084 standard deviations for the 'post-election' year. The instrumental variable estimates point to somewhat larger effects. Overall, the results suggest that increases in resources take place just prior and right after the elections. One should note that 2005-2011 has been a period of rapid improvement in school resources in India. As the models control for trends for each state, the effects of the electoral cycle capture deviations from these trends. It is important to bare in mind that since the school resource data are annual, but elections can run in nearly any month of the year, a clean-cut division into pre and post effects is not possible.

When the model in column (2) is estimated separately for each of the 14 resources as the outcome variables, we find statistically significant positive pre-election effects for uniforms, girls' toilets, and ramps. Significant positive post-election effects are found for textbooks, uniforms, classrooms per pupil, ramps and medical examinations. These results are shown in Table A4 in the Appendix. As can be seen from the Table, the effects on resources are not strictly confined to the pre and post election years. Some of the resources fall or rise in other years as well. However, for a large subset of the resources, the increases tend to be largest in the pre and post election years. Some of the resource outcomes are measured per pupil, but a separate regression on the number of pupils does not suggest that the electoral cycle would have an effect on the number (results not shown). The results on individual resources will reflect partly the heterogeneity of states, which is why a composite measure of resources is preferable for most of the analysis.

The results of some robustness checks for the model specification in column 2 of Table 3 are presented in the Appendix, Table A5. Firstly, we create two alternative indices of school resources. 'Index 1' is based on a simple addition of the 14 normalised resource variables. This is a linear combination equivalent to PC1, but with an alternative weighting of the items. To address the

12 The results with OLS or IV are similar, and the overall conclusions of the paper do not hinge on them. The first stages of the IV estimates, where the realised election dummies are instrumented with the scheduled dummies are close to one, with very high statistical significance.

13 The numbers of observations across models vary slightly due to IV dropping some observations due to collinearity.

possibility that extreme values affect the index, we also create 'Index 2'. In this case all resource variables are transformed into dummy variables, based on whether the value is above the average for public primary schools, and the values are then added up, so that the index for each school is an integer between 0 and 14. The result on the electoral cycles is robust to both of these alternatives. We also test for any effects on private non-aided schools, which are also in the DISE database. We find no effects of electoral cycles of a similar magnitude.

Table 3 Effect of electoral cycle on school resources

Dependent: Normalised 1st PC of school resources				
	[1] OLS	[2] IV	[3] IV	[4] IV
Years from Election				
[3]	.021	.032	.033	.033
	[.029]	[.034]	[.034]	[.034]
[4] 'Pre-Election'	.081	.088	.084	.088
	[.028] **	[.029] **	[.031] **	[.030] **
[0] 'Post-Election'	.077	.084	.084	.079
	[.030] *	[.031] **	[.031] **	[.037] *
[1]	.028	.034	.035	.035
	[.029]	[.027]	[.027]	[.027]
Interactions				
Pre-Election			.026	
x Competed			[.039]	
Post-Election				.011
x Aligned				[.038]
Competed			-.013	
			[.045]	
Aligned				-.009
				[.028]
Marginal effects				
Pre-Election | Not competed			.084	
			[.031] **	
Pre-Election | Competed			.109	
			[.044] *	
Post-Election | Not aligned				.079
				[.037] *
Post-Election | Aligned				.090
				[.036] *
Obs	4476106	4367424	4336007	4336007
R2	.304	.304	.303	.303

Notes: All models include school fixed effects, state trends and year effects. Standard errors (in brackets) are adjusted for state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

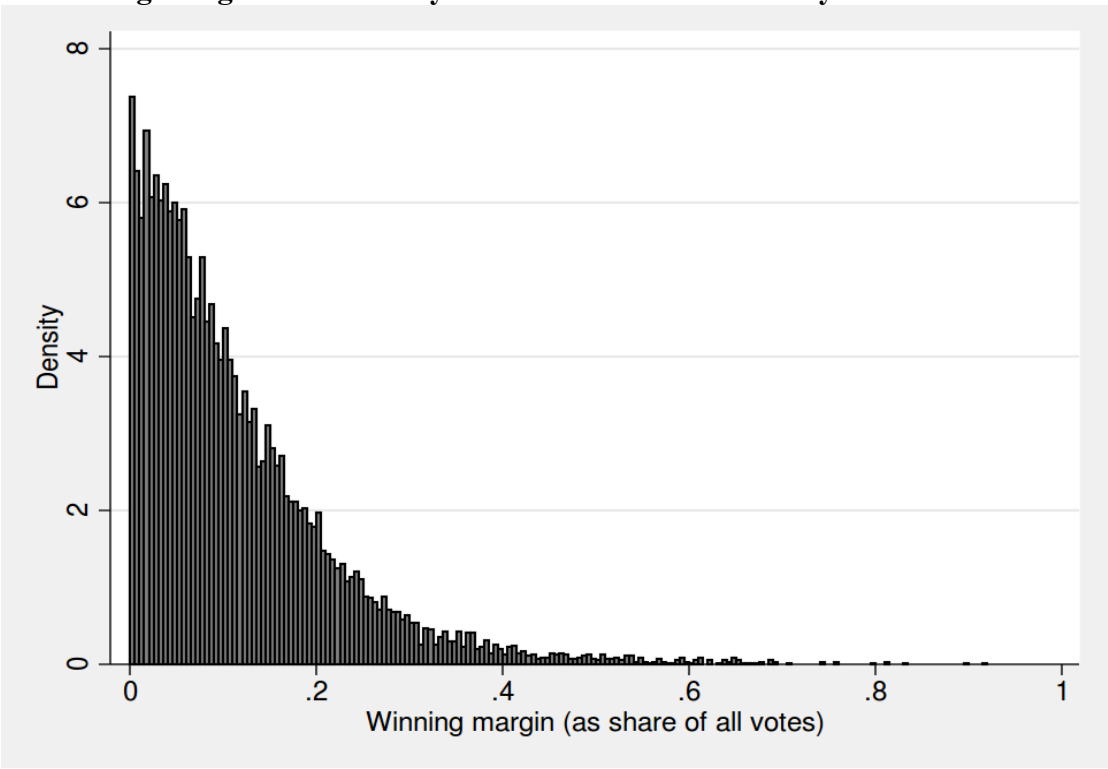
5.1.1 Wooing swing voters or rewarding core voters?

The 'swing voter' theory would suggest that in an attempt to buy votes, politicians may wish to increase spending in tightly contested or marginal constituencies prior to the elections. The 'core voter' theory in turn would suggest that politicians reward their supporters. Testing for these theories requires some discussion on the political variables summarised at the bottom of Table 1.

As the electoral outcomes are matched to schools at the district level, the electoral outcomes are

averaged across the electoral constituencies of each district.[14] We rely on two key district level political variables. Firstly, we measure political alignment of a district (A_{dt}) as the proportion of electoral constituencies within a district, where the winning candidate comes from the party that won most seats within the whole state in the latest election. Secondly, possible 'swing' voter areas will be measured as the proportion of tightly contested constituencies in the district (T_{dt}). In calculating (T_{dt}), we follow Clots-Figueras (2011) and define a 'marginal victory' or a 'tight contest' as one where the winner won by a margin of 3.5% or less. Figure 2 shows the distribution of the margins of victory for Indian Assembly elections over the period 2000-2012. Out of the 10,611 constituency elections in 2000-2012 in our sample, 2386 (22.4%) were won within the 3.5% margin.

Figure 2 Winning margin for Assembly elections at the constituency level over 2000-2012.



Notes: Each pillar is 0.005 wide, thus the first 7 pillars, adding up to 0.035 margin, are counted as 'marginal victories'.

To test for the swing voter theory, we study whether schools in districts where the last elections were tightly contested, receive more resources in the immediate pre-election period. For this, we estimate equation (2) below. Then, we test for whether the increases in resources in the immediate post-election period favour those who voted for the winner of the election, as suggested by the core voter model.[15] For this, we estimate equation (3) below. We simply interact the pre or post election dummy with the political variable of interest.

$$(2)\quad Resource_{it} = \beta_3 D_{3s} + \beta_4 D_{4s} + \beta_0 D_{0s} + \beta_1 D_{1s} + \delta D_4 T_{dt} + \gamma T_{dt} + \lambda_t + \tau_s t + \alpha_i + u_{it}$$

Here, the marginal effects of interest are β_4, which measures the size of the pre-election effect when none of the constituencies in the district were tightly contested in the last election ($T_{dt} = 0$)[16], and

14 Unweighted averages can be taken since constituencies are designed to be of roughly equal size.

15 We have also for estimated the role of tightly contested elections in the post-election period and political alignment in the pre-election period, but found no significant effects.

16 Elections are defined here as being tightly competed if the winner's margin of victory was less than 3.5% of the vote. The average in the estimation sample is 0.165, or 16.5% of the constituencies in the district.

secondly, $\beta_4+\delta$, which refers to the case where all constituencies in the district were tightly competed ($T_{dt} = 1$). The interaction parameter δ measures the difference between the two extremes. One should keep in mind that T_{dt} varies over the entire range [0,1], and thus the effects of intermediate values can be interpreted as fractions of δ. When estimated with instrumental variables, the interaction term between T_{dt} and the schedule-based instrumental variable for D_4 is used as an additional exclusion restriction.[17]

$$(3)\quad Resource_{it} = \beta_3 D_{3s} + \beta_4 D_{4s} + \beta_0 D_{0s} + \beta_1 D_{1s} + \sigma D_0 A_{dt} + \theta A_{dt} + \lambda_t + \tau_s t + \alpha_i + u_{it}$$

Here, the marginal effects of interest are β_0, which measures the size of the post-election effect when none of the constituencies in the district were aligned with the state government ($A_{dt} = 0$), and $\beta_0+\sigma$, which refers to the case where all constituencies in the district were aligned ($A_{dt} = 1$). The interaction parameter σ measures the difference between the two. Again, A_{dt} varies in the range [0,1]. The main alignment effect, measured by θ, captures whether political alignment leads to higher or lower resources outside the election period in general. Again, the interaction term between A_{dt} and the instrumental variable for D_0 is used as an additional instrumental variable.

The results of equation (2) are shown in column 3 of Table 3. We find that the interaction parameter (δ) has the expected sign, but is not statistically significant. The marginal effect of the pre-election year is somewhat larger for the tightly contested areas (.109 vs .084 SDs). The general effect of tight competition, as measured by the main effect of competition (γ), suggests that tightness does not matter in other years.

Column 4 displays the results for equation (3). As with the previous column, the interaction effect has the expected sign, but again it is not statistically significant. The marginal effects at the bottom of the table suggest that the null hypothesis for the absence of the political cycle can be rejected irrespective of whether the constituencies in the district are politically aligned with the party that received the largest share of the votes in the recent election. The general effect of being aligned, as measured by alignment alone, suggests that there are no benefits from being aligned with the state government.

Overall, the results on of this section indicate that while there is clear evidence of a political resource cycle, there is less evidence of the increases in resources being targeted specifically at swing voters and core voters.

5.1.2 Determinants of the electoral cycle

The analysis so far has overlooked the regional diversity of India. This is somewhat inevitable since a sufficient number of states are required to estimate the effects of the electoral cycle accurately with our identification strategy. However, different parts of India can differ substantially with respect to development outcomes such as literacy, sophistication of the electorate and the levels of graft in politics, for example.

In this section we report results on the possible determinants of the electoral cycle. We build on the basic IV estimations in Table 3, column 2, by adding interaction terms between district level fixed factors and the election cycle dummies. We incorporate dimensions of voter sophistication and engagement, in the spirit of previous work (see Akhmedov and Zhuravskaya, 2004 and Shi and Svensson, 2006). Specifically, we interact the election cycle dummies with district-level averages

17 T_{dt} is determined prior to the dependent variable, so that simultaneity as a source of endogeneity can be ruled out.

for voter turnout in the state elections, female literacy rates and the rate of urbanisation.[18] Lower voter turnout can reflect a weaker democracy or a lower degree of voter engagement. It is also possible that calculating politicians see more potential for 'vote buying' in areas of low turnout, simply due to a larger pool of potential new voters. Nichter (2008) for instance distinguishes between "vote buying" and "turnout buying", where the latter is defined as rewarding unmobilized supporters for showing up at the polls, and therefore a way for parties to 'activate their passive constituencies'.

Table 4 presents the results of the models, where the election cycle dummies are interacted with district level fixed factors.[19] Note that the main effects of district level variables are absorbed by the school fixed effect, and are therefore not included as separate variables in the models. Column 1 shows the basic IV model of Table 3, with a slightly restricted sample, given that there are some missing observations for a few districts. Separate models are estimated for each district variable.

Table 4 Determinants of the electoral cycle, IV estimates

	[1]		[2]		[3]		[4]	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Years from Election								
[3]	.033	.034	.358	.116 **	.131	.103	.056	.033 +
[4] 'Pre-Election'	.089	.030 **	.437	.083 **	.345	.083 **	.115	.033 **
[0] 'Post-Election'	.084	.032 **	.275	.104 **	.256	.100 *	.085	.038 *
[1]	.036	.027	-.064	.113	.136	.071 +	.041	.024 +
Interactions								
[3] x Voter Turnout			-.504	.193 **				
[4] x Voter Turnout			-.537	.132 **				
[0] x Voter Turnout			-.298	.179 +				
[1] x Voter Turnout			.143	.191				
[3] x Fem. Literacy					-.163	.141		
[4] x Fem. Literacy					-.418	.138 **		
[0] x Fem. Literacy					-.281	.157 +		
[1] x Fem. Literacy					-.166	.098 +		
[3] x Urbanisation							-.100	.038 **
[4] x Urbanisation							-.112	.069
[0] x Urbanisation							-.002	.072
[1] x Urbanisation							-.023	.046
Observations	4326260		4326260		4326260		4326260	
R2	.3036		0.3072		.3047		.3039	

Notes: All models include school fixed effects, state trends and year effects. Standard errors are adjusted for state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively. The estimations include schools in 28 states.

The results in Table 4 suggest that the electoral cycle is stronger in districts with lower voter turnout and lower female literacy.[20] Higher levels of literacy and voter turnout reduce the pre and post-election effects. Additionally, these indicators appear to moderate the pre-election effect somewhat

18 The source for female literacy and the urbanisation rate of districts is the Indian Census of 2011. Histograms showing the distribution of district averages for turnout and female literacy are shown in the Appendix, Figure A1. The summary statistics are shown at the bottom of Table 1. Turnout is measured as the average turnout in a district for the State Assembly Elections between 2000-2012. The results would be robust to using only the pre-period turnout from 2000-2004.

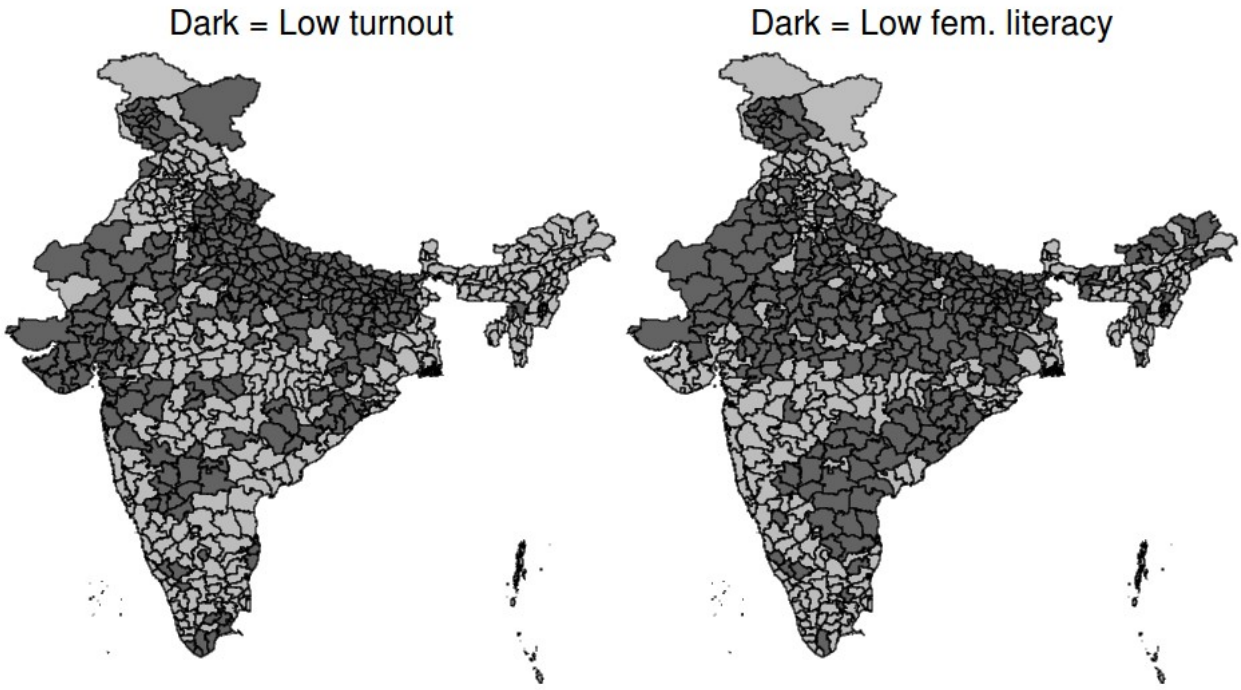
19 We have also interacted the election cycle dummies with the proportion of scheduled castes and scheduled tribes in the districts, as caste identity is considered to play a role in voting behaviour in India, but these do not to play a role in explaining the cycles we observe.

20 We also estimated the models using average male literacy, which has a higher mean and a lower variance than female literacy. The results were similar, but not as strong as with female literacy, as measured by the R-squared.

more strongly; an effect which could be argued to be generated by rulers intending to impress a myopic electorate. The link with urbanisation is not as strong.

Figure 3 tells us more about the geographical distribution of areas with lower rates of voter turnout and female literacy. The districts with a turnout below the median (67.3%) are scattered across the country, but tend to be much more prevalent in the Northern States around the Ganges river, particularly in the states of Uttar Pradesh and Bihar. This division of the sample is likely to correlate with the political culture of the states and potentially a range of other variables, such as education (see e.g. Diwakar, 2010 for a state-level analysis of the correlates of voter turnout in India). Figure 3 also divides the districts to those with lower and higher female literacy.

Figure 3 Districts with low voter turnout and low female literacy



Notes: Left figure: Dark denotes below median turnout (< 67.3%). Right figure: Dark denotes districts with female literacy rates below the median (< 63.6%). Sources: State Assembly Election turnout rates from the Election Commission for 2000-2012 and Census 2011.

To our knowledge, the role of voter turnout in the political cycles is an under-explored question. As mentioned above, voter turnout could reflect the functioning of democracy in general. However, it is possible that lower degrees of voter turnout merely act as a proxy for specific features of the political system that create the electoral cycles. These 'deep causes' behind the electoral cycles may be operating largely at the level of the state, since that is the level at which the primary school system is operated in India. The fact that we failed to find substantial targeting of resources in Section 5.1 suggests that the cycles are at least as much a manifestation of incompetence, than simply rational calculus of politicians. A further possible interpretation of the relevance of voter turnout is a more mechanical one – that cycles are an attempt by politicians to mobilise new voters

at the polls, and with lower levels of turnout, the cycles would be larger. Discriminating between these and other possible explanations is beyond the scope of this study.

5.2 Teachers and their time use

The literature on human capital production functions indicates that teachers are the most important school resource (see e.g. Glewwe et al., 2014). In the Indian context, depending on the state, teacher salaries contribute an average of 40% of the education budget (see footnote 9 for a reference). Regular primary school teachers in India have traditionally been civil servants on permanent contracts and thus there is a degree of inflexibility in their recruitment. It might be more difficult for states to manipulate the numbers of teachers, at least with respect to the ones who are already working.

On the other hand, teachers can be politically active, and it has been reported elsewhere that during election years, the work of teachers may be disturbed by duties related to the organisation of the elections, or personal political activism (see for instance Kingdon and Muzammil 2009 and Béteille, 2009).

Using the same estimation framework as for physical school resources, we test whether the number of teachers per school and the days spent on non-teaching assignments per teacher are affected by the political cycle. These non-teaching assignments exclude training.

Table 5 Effects of the electoral cycle on the number of teachers, IV estimates

Dependent: Number of teachers in school						
	[1]		[2]		[3]	
	Coef	SE	Coef	SE	Coef	SE
Years from Election						
[3]	.075	.053	.214	.318	.038	.180
[4] 'Pre-Election'	.021	.073	-.405	.450	-.244	.331
[0] 'Post-Election'	.014	.064	-.630	.287 *	.429	.465
[1]	.049	.025 *	.161	.157	.150	.078 +
Interactions						
[3] x Voter Turnout			-.215	.479		
[4] x Voter Turnout			.647	.650		
[0] x Voter Turnout			.994	.426 *		
[1] x Voter Turnout			-.162	.234		
[3] x Fem. Literacy					.061	.249
[4] x Fem. Literacy					-.238	.282
[0] x Fem. Literacy					.411	.393
[1] x Fem. Literacy					-.163	.120
Observations	4308609		4308609		4308609	
R2	.0379		.0412		.0388	

Notes: All models include school fixed effects, state trends and year effects. Standard errors are adjusted for state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

Starting with the number of teachers, the results in Table 5 (column 1) indicate that there is no statistically significant pre or immediate post-election effect on the number of teachers. However, there is a positive effect a year after the elections. This suggests that teachers may not be a strategic resource that can be manipulated immediately pre or post the election period. The positive impact observed a year after the elections could arise from the hiring of new teachers, as part of an education strategy designed by the new government.

In columns 2 and 3, the election cycle dummies are interacted with district averages for voter

turnout and female literacy. Now the results indicate that at higher levels of voter turnout, that the number of teachers is more likely to rise right after the elections. Conditional on all the control variables, a 10 percentage points higher voter turnout rate would lead to about 0.1 more teachers per school in the post-election year, compared to the control year (which is the one two years from the election). With a low rate of turnout, say 50%, the marginal effects in column 2 would suggest close to no electoral effects. The post-election increase in teachers in districts with a high level of turnout might represent the educational strategy of the new government or the fulfilment of an electoral promise. The interaction terms with female literacy however, are not statistically significant.

Table 6 Effects of the electoral cycle on the number of non-teaching days, IV estimates

Dependent: Number of non-teaching days per teacher						
	[1]		[2]		[3]	
	Coef	SE	Coef	SE	Coef	SE
Years from Election						
[3]	.184	.288	1.238	1.553	.363	1.280
[4] 'Pre-Election'	.351	.282	2.830	1.254 *	1.429	.761 +
[0] 'Post-Election'	.490	.422	6.887	1.540 **	3.214	1.416 *
[1]	.614	.357 +	2.356	1.181 *	2.919	1.519 +
Interactions						
[3] x Voter Turnout			-1.620	2.528		
[4] x Voter Turnout			-3.781	2.098 +		
[0] x Voter Turnout			-9.905	2.354 **		
[1] x Voter Turnout			-2.703	1.939		
[3] x Fem. Literacy					-.353	1.967
[4] x Fem. Literacy					-1.782	1.413
[0] x Fem. Literacy					-4.469	1.933 *
[1] x Fem. Literacy					-3.786	2.165 +
Observations	4308572		4308572		4308572	
R2	.0117		.0129		.0120	

Notes: All models include school fixed effects, state trends and year effects. Standard errors are adjusted for state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

In Table 6, the same estimations are repeated for the days that teachers spend on non-teaching assignments (excluding training), averaged over all lower primary school teachers in the school. The non-teaching days reported in DISE refer to the previous academic year. Supposing that elections are held in April 2008, and teachers are used for staffing the polling stations and counting the votes, these assignments would be reported in the DISE 2008, leading to a significant pre-election effect. But if the elections were in November 2008, they would be reported in the DISE 2009, leading to a significant post-election effect. Thus election-related activity can be picked up by both pre and post-election dummies.

The results in the first column suggest a positive, but only marginally significant effect for the year after the elections. However, the differences are again sharper, depending on the level of voter turnout and in this case also female literacy. Pre-election effects are positive in districts with lower turnout and literacy and moderated by higher turnout and literacy. The fact that we observe both pre and post-election effects here is consistent with the hypothesis that teachers do get significant duties during elections, but these tend to be confined to areas with less informed and engaged voters. This may reflect differences in teacher's political involvement.

The magnitudes of the estimates for non-teaching assignments are not large. The post-election effect for every additional 10 percentage point increase in voter turnout is about 1 day per teacher per academic year, which, if correct, should not have a major effect on learning. On the other hand, these duties are more concentrated to certain schools and certain individuals, meaning that the

effects can be very different for different pupils. Secondly, the days reported to the formal register database may or may not fully reflect the reality. The evidence on teacher absences in India (e.g. Kremer et al. 2005) suggests that days spent in non-teaching activities can well be grossly under reported.

6 Effect of electoral cycle on learning outcomes

The fact that school resources are muddled by political interests in areas with lower literacy and civic disengagement can perpetuate geographical inequalities from a longer term perspective. As a final exercise, we study the effects of the electoral cycle on learning outcomes.

The DISE data does not have good data on learning outcomes.[21] We assess the effects on learning outcomes using the literacy and numeracy data collected by the ASER Centre[22]. ASER has carried out an annual survey of rural school children since 2005, which is representative at district level. We use the data from 2005 to 2010, and merge it directly to the data on state electoral cycle.

It is not *ex ante* clear how electoral cycles in school resources should affect learning. The first problem is that physical school resources during the period have risen quickly, and that electoral cycles, while real, contribute only to variations around this improvement. Secondly, since the lower primary school lasts five years in most states, all pupils will experience the potential effects of the electoral cycle during their schooling. Thus the relevant question is; what stage of the electoral cycle is the best or the worst time to begin school? Here we should emphasise that if the net effect of electoral cycles on learning outcomes is negative, but the timing of the cycle does not matter, we should not be able to find any effect on learning outcomes. In other words, we will not be able to identify any net effect of the electoral cycle, only whether it is worse or better to begin school during a particular point of the electoral cycle.

All children in the households sampled by ASER were tested for their literacy and numeracy skills. In 2005, 6-14 year olds were covered, and in later surveys the coverage was expanded to 3-16 year olds. We restrict the sample to 6-14 year olds, leaving us with a sample of about two million tested children for the years 2005 to 2010. The learning outcome measures in the ASER surveys are based on categorical tests. Reading skills of pupils are categorised into five levels: ability to read a story (5), paragraph (4), sentence (3), a word (2), or nothing (1). Numeracy is measured in four similar categories: ability to divide (4), subtract (3), recognise a number (2), or nothing (1). To simplify the measure of outcomes, we compute an age-specific z-score for each pupil in both reading and numeracy. Each pupil gets assigned the median z-score in his/her age-specific skill category.[23]

We begin by taking all 6-14 year olds in the ASER survey, and compute their predicted year of starting school. Determining the year of starting school is not entirely straightforward. For each child, their age and class in school are provided. Both age and class are subject to potential measurement error (possibly with larger errors in class), and we also know that progression in

21 DISE has information on proportions of pupils that pass or obtain a grade of more then 60% in year 5 final exam. However, these are not based on a standardised test or comparable across schools and not necessarily indicative of skills.

22 www.asercentre.org/

23 For example, if 10% of 6-year old children can read a paragraph (the second highest category) and 5% can read a story (highest), then the ones who can read a story are assigned z = 1.96 as $\Phi^{-1}[1-(.05/2)] = \Phi^{-1}[.975] = 1.96$, while those who can read a paragraph are assigned z = 1.28 as $\Phi^{-1}[1- .05 - (.10/2)] = \Phi^{-1}[.90] = 1.28$. The z-score for each level is based on distributions in the 2005 data, and applied to all consecutive years. The advantage of this is that we do not need to model each level of achievement separately, but instead make an educated assumption about the relative distance between the levels of achievement.

school may not be automatic, and dropping out is common. Since the modal age of first graders is six, it is reasonable to assume that the year of starting school is 'Current year – age + 6'. This will be the preferred assumption. The alternative is to compute 'Current year – class + 1' as the year of starting school. Since class contains more missing observations, the determination of a starting year based on age has the additional advantage that it also covers individuals who have quit school and do not report class.

The starting year of school is then matched to the timing of the elections. Here, using the electoral cycle as an instrumental variable for school resources is not appropriate, since elections may affect other factors besides school resources, which in turn affect learning. Teachers' absences during the election year due to electoral duties is one possibility. As such, the estimates we provide are reduced form ones, and we cannot claim that any possible effects would be driven exclusively by school resources. In fact, despite vast improvements in school resources, the skills measured by the ASER have been stagnating over the period we observe.

The precise OLS model to be estimated is

$$(4)\quad zscore_{it} = \beta_y D_{ys} + \Lambda_t + \Omega_d + u_{it} \qquad t \in [2005, 2011] \qquad y \in [0,4] \quad ,$$

where *i* refers to pupils, *s* to state and *d* to district. As before, *y* measures the number of years from the latest election, zero being an election year, and 4 being the year before elections. The coefficients of interest are the β coefficients. The age of the pupils is controlled for with age-specific scores. Λ_t controls for survey year effects. District fixed effects are controlled for with Ω_d. Standard errors are clustered at the state level.

Table 7 Summary statistics, ASER (6-14 year olds)

	Obs.	Mean	S.D.	Min	Max
Year (ASER)	1865352	2007.6	1.6	2005	2010
Read nothing	1803846	.082	.275	0	1
Read word	1803846	.157	.364	0	1
Read sentence	1803846	.175	.380	0	1
Read paragraph	1803846	.191	.393	0	1
Read story	1803846	.395	.489	0	1
Reading z-score	1803846	.048	.857	-3.15	2.51
Maths nothing	1793597	.103	.304	0	1
Maths number	1793597	.363	.481	0	1
Maths subtract	1793597	.241	.428	0	1
Maths divide	1793597	.294	.455	0	1
Maths z-score	1793597	.102	.846	-2.34	3.08
Age	1865352	9.66	2.35	6	14
Class	1751716	4.08	2.08	1	8
Years from elections at age 6					
0	1865352	.203	.402	0	1
1	1865352	.191	.393	0	1
2	1865352	.191	.393	0	1
3	1865352	.208	.406	0	1
4	1865352	.208	.406	0	1
Coverage: 563 districts in 27 states					

Notes: The mean of z-scores is above zero and S.D lower than unity due to normalisation being made using the 2005 data only.

Table 7 presents the summary statistics for the ASER sample used. The results of the regression models are presented in Tables 8 and 9, for both OLS and IV models and for both reading and

mathematical skills respectively. The results of separate IV models with interaction terms for both voter turnout and female literacy are also shown.

Table 8 Electoral cycle and reading skills in ASER data, years 2005-2010 pooled

Reading: Age-specific z-score								
	[1]		[2]		[3]		[4]	
	OLS		IV		IV		IV	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Years from Election								
[3]	-.005	.005	-.004	.005	-.008	.017	-.013	.022
[4] 'Pre-Election'	-.004	.006	.001	.004	-.006	.017	.021	.018
[0] 'Post-Election'	-.011	.004 **	-.008	.004 *	-.006	.027	-.003	.019
[1]	-.010	.006	-.016	.006 **	-.051	.034	-.027	.029
Interactions								
[3] x Voter Turnout					.006	.029		
[4] x Voter Turnout					.010	.027		
[0] x Voter Turnout					-.004	.042		
[1] x Voter Turnout					.053	.048		
[3] x Fem. Literacy							.016	.035
[4] x Fem. Literacy							-.033	.029
[0] x Fem. Literacy							-.009	.030
[1] x Fem. Literacy							.016	.041
Observations	1833691		1803846		1803846		1780281	
R2	.0009		.0009		.0009		.0009	

Notes: District fixed effects, survey year controls, state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

Table 9 Electoral cycle and mathematics skills in ASER data, years 2005-2010 pooled

Mathematics: Age-specific z-score								
	[1]		[2]		[3]		[4]	
	OLS		IV		IV		IV	
	Coef	SE	Coef	SE	Coef	SE	Coef	SE
Years from Election								
[3]	-.004	.004	-.006	.003 +	-.031	.015 *	-.033	.024
[4] 'Pre-Election'	-.004	.005	-.006	.007	-.046	.017 **	.010	.019
[0] 'Post-Election'	-.009	.003 **	-.009	.003 **	.007	.022	-.011	.021
[1]	-.007	.006	-.014	.005 **	-.061	.016 **	-.035	.019 +
Interactions								
[3] x Voter Turnout					.039	.024		
[4] x Voter Turnout					.062	.026 *		
[0] x Voter Turnout					-.024	.036		
[1] x Voter Turnout					.073	.023 **		
[3] x Fem. Literacy							.045	.039
[4] x Fem. Literacy							-.025	.030
[0] x Fem. Literacy							.003	.033
[1] x Fem. Literacy							.034	.029
Observations	1823293		1793597		1793597		1770246	
R2	.0032		.0031		.0031		.0030	

Notes: District fixed effects, survey year controls, state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

The OLS and IV estimates for reading in Columns 1 and 2 suggest that starting primary school during or right after elections is detrimental to learning, and the results for numeracy in Table 9 are similar. The effects are small, approximately 0.01 standard deviations. Further, as emphasised, the effect can be driven by other factors than the type of school resources we are able to measure in this study. In this context, the 'Post-Election' effect would refer to a case where elections are held in the same year as pupils enter school in the Autumn, and as such it could also be interpreted as the 'Election year' effect.

With respect to reading skills, voter turnout or female literacy are not statistically significant determinants of the election effects, even though there is a marked difference in the coefficients for the year after elections between high and low turnout areas. However, in the case of Maths scores, the electoral cycle dummies before and after the election year are negative for children in districts with lower turnout. This negative effect is not present with high levels of turnout. The results indicate that in areas of low turnout, the year after the election year is the worst time to enter primary school, relative to those who start in the middle of the cycle.[24] Those who start a year after the elections, will experience the next elections in their fifth, or last year of lower primary school. Roughly the same applies to those who start in the pre-election year and experience elections in their second grade. As such, the results show that the point of the electoral cycle in which pupils start school, affects their final learning outcomes.

The fact that the electoral cycle has an impact on learning outcomes in a dataset unrelated to the DISE, and that the results vary geographically in a similar fashion, gives credence to the findings on school resources and activities of teachers from the earlier chapters. It is worth re-emphasising that since all primary school pupils are affected by the electoral cycles, we are unable to estimate their net effect on learning outcomes. What we have shown, is that children's learning outcomes may be particularly adversely affected their last year of lower primary school (the fifth year) coincides with elections. The overall negative effects of electoral cycles can be much larger than what these relative effects suggest.

Given that the literature on school resources generally finds only small effects of school resources on learning outcomes, it is very much possible that the effects on learning outcomes we find here, result from the behaviour and number of teachers across the election cycle, but this cannot be confirmed with our data.

7 Conclusions

This study has used an administrative panel database on schools for 2005-2011 to demonstrate that the provision of school resources in India is characterised by a political cycle. There are improvements in school resources both prior and post state elections across Indian states, although it should be acknowledged that the nature of the data does not allow for a clean-cut division into pre and post-election effects. The effects are not limited to physical resources, but are also evident in teachers' time use. We do not find strong evidence that the increases in infrastructure and materials would be targeted towards politically aligned or tightly contested districts within states.

The identification of the results rests on the predetermined and staggered timing of state elections across Indian states. The estimations also account for school level fixed effects and state specific time trends. Similar cycles in resources are not found for private non-aided schools.

24 The results were similar in nature, but weaker in statistical significance if the school starting year was computed using the current reported class.

The cycle in school infrastructure and materials emerges in districts with lower levels of voter turnout and female literacy. In addition, in these areas, teachers spend more time on “non-teaching” activities around election years. These areas tend to be concentrated in the Northern parts of the country. Lower voter turnout and female literacy are considered as proxies for lower levels of political awareness and engagement. These types of factors are suggested to give rise to opportunistic budget cycles for instance by Shi and Svensson (2006) given that voters are less able to distinguish between pre-electoral vote buying and competence. In areas of high voter turnout, the number of teachers in turn increases in the immediate post-election year.

The fact that resource investments are muddled by political cycles in areas with lower literacy and civic disengagement can perpetuate geographic inequalities. With a separate household survey data set that measures children's skills, we show that electoral cycles affect human capital accumulation. The phase of the electoral cycle during which pupils enter primary school affects their learning outcomes: those who experience state elections during their fifth (and last) year of lower primary school, are at the largest disadvantage. As the electoral cycle is a questionable instrumental variable for observable school resources, we have provided only reduced form estimates on the effect of the electoral cycle on learning outcomes. Importantly, the estimates on learning outcomes only measure the impact of the relative timing of elections on pupils' skills; the overall net effect of the electoral cycle on learning remains to be unknown. However, the existing correlation between cycles and low female literacy across districts suggests that the politically induced turbulence in primary school resources and functioning is one of the factors aggravating regional inequalities in human capital accumulation.

Anecdotal evidence on the influence of politics on schooling in India is plentiful, but this study provides a more systematic set of results. While there are many studies showing how the political cycle affects the provision of public goods, this study is among the few that use micro data, and measures the effect on actual public goods, not just the flows of funds. It is notable that the effects of the upcoming elections on physical school resources concentrate on visible, but mostly relatively cheap items.

The literature on school resources has a long tradition of comparing the relative impacts of school resources and teachers on learning outcomes. This study highlights that this 'static' view of the educational production function is largely blind to the general quality of school system administration, which may be deeply entangled with political sea changes and corruption. In particular in the context of such a basic public service as primary schooling, the evidence on political cycles can be considered a reflection of poor governance.

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Appendix

Cleaning of the DISE school data

The sample of interest covers lower primary schools governed by the Department of Education, Tribal/Social Welfare Department or another local body. The raw database for 2005-2011 includes roughly 6 million school observations.

The size of the sample used in the analysis is smaller for three reasons. Firstly, we have excluded schools for which there is some doubt about the robustness of the school code across time. This procedure excludes 8.7% of observations. On average, the excluded schools are slightly smaller than others (3.1 versus 3.3 teachers per school). In practice, we have excluded schools that go through a 'substantial' name change as defined by a simple algorithm, while keeping the same school code. This can lead to the exclusion of schools, which have genuinely simply changed name, but since the analysis uses school fixed effects throughout, we need to ensure that all school panels are genuine. Secondly, from the remaining sample, 9% are deemed to be outlier observations with respect to some variables. Typically outlier status is assigned to variables with unrealistic values. In uncertain cases, the top (and/or bottom) 0.5% of the values are regarded as outliers for each variable. Outliers, on average, relate to larger schools than others (4.0 versus 3.3 teachers). Finally, 9.7% of the remaining observations include missing values for some variables of interest, and these schools tend to be slightly smaller in size than others (2.9 versus 3.3 teachers). The initial and final samples in terms of year and school management are showed in Table A1 below. The 'regression sample' is defined as the set of observations for which there are no missing values for any of the 14 school resource variables.

Table A1 Sample selection in the DISE school-level data

Panel A: Initial raw data

	School management			
Year	Department of Education	Tribal/Social Welfare Dept.	Local body	Total
2005	564405	41947	212393	818745
2006	569568	40554	229863	839985
2007	589311	42546	231209	863066
2008	607042	37295	238745	883082
2009	604608	45979	235918	886505
2010	604137	45942	236515	886594
2011	590381	55451	236047	881879
Total	4129452	309714	1620690	6059856

Panel B: Regression sample

	School management			
Year	Department of Education	Tribal/Social Welfare Dept.	Local body	Total
2005	366040	19537	182590	568167
2006	453547	30678	196016	680241
2007	470225	33812	208162	712199
2008	498533	30289	214394	743216
2009	431725	35095	198776	665596
2010	386217	33419	176311	595947
2011	374765	36775	168167	579707
Total	2981052	219605	1344416	4545073

Table A2 Election dates 1999-2012

2012	2007	2002
Gujarat Dec	Gujarat Dec	Gujarat Dec
Himachal Pradesh Dec	Himachal Pradesh Nov	Jammu & Kashmir Oct
Goa Jan	Goa Jun	Goa May
Manipur Jan	Uttar Pradesh Apr	Manipur Feb
Punjab Jan	Manipur Feb	Punjab Feb
Uttar Pradesh Jan	Punjab Feb	Uttar Pradesh Feb
Uttarakhand Jan	Uttarakhand Feb	Uttarakhand Feb
2011	**2006**	**2001**
Assam Apr	Assam Apr	Assam May
Kerala Apr	Kerala Apr	Kerala May
Tamil Nadu Apr	Pondicherry May	Pondicherry May
West Bengal Apr	Tamil Nadu May	Tamil Nadu May
Pondicherry Apr	West Bengal Apr	West Bengal May
2010	**2005**	**2000**
Bihar Oct	Bihar (re-election) Oct	Bihar Feb
2009	Bihar Feb	Haryana Feb
Arunachal Pradesh Oct	Jharkhand Nov	Manipur Feb
Jharkhand Oct	Haryana Feb	Orissa Feb
Haryana Oct	**2004**	**1999**
Maharashtra Oct	Maharasthra Oct	Arunachal Pradesh Oct
Andhra Pradesh Apr	Arunachal Pradesh Oct	Andhra Pradesh Oct
Orissa Apr	Andhra Pradesh Apr	Karnataka Oct
Sikkim Apr	Karnataka Apr	Maharashtra Oct
2008	Orissa Apr	Sikkim Oct
Chattisgarh Nov	Sikkim May	Goa June
Madhya Pradesh Nov	**2003**	
Mizoram Dec	Chattisgarh Nov	
NCT of Delhi Nov	Delhi Nov	
Rajasthan Dec	Madhya Pradesh Nov	
Jammu and Kashmir Nov	Mizoram Nov	
Karnataka May	Rajastan Nov	
Nagaland Mar	Himachal Pradesh Feb	
Meghalaya Mar	Meghalaya Feb	
Tripura Mar	Nagaland Feb	
	Tripura Feb	

Source: Election Commission of India (http://eci.nic.in/eci/eci.html)

Table A3 Correlation of resources with the first principal component in the panel 2005-2011

	Correlation with 1st PC
# of Free textbooks per pupil	.166
# of Free uniforms per pupil	.437
# of classrooms per 100 pupils	.167
Toilet	.291
Girls' toilet	.493
Electricity	.595
Water index	.496
Building quality index	.508
Boundary wall	.438
Book bank	.450
# of Computers per pupil	.245
Ramp	.515
Medical examinations	.522
Playground	.403

Table A4 Effect of electoral cycle on individual resources, IV estimates

Years from election:	3 years		4 years "Pre-Election"		0 years "Post-Election"		1 year			
	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Obs.	R2
School Resources										
# of Free textbooks per pupil	.009	.029	.049	.035	.058	.032 +	.001	.031	4808546	.099
# of Free uniforms per pupil	.015	.015	.089	.032 **	.064	.021 **	.054	.032 +	4808543	.178
# of classrooms per 100 pupils	.007	.078	.213	.145	.213	.103 *	.160	.085 +	4830619	.119
Toilet	.026	.039	.044	.039	.052	.033	-.009	.038	4837680	.129
Girls' toilet	.051	.028 +	.054	.028 +	.017	.030	-.031	.028	4837680	.143
Electricity	-.007	.010	-.019	.011 +	.011	.020	.022	.017	4768730	.179
Water index	.003	.014	.020	.015	.003	.017	-.025	.018	4821861	.051
Building quality index	-.006	.008	-.024	.015 +	-.043	.023 +	-.012	.011	4606727	.040
Boundary wall	.010	.010	.006	.008	.003	.010	.004	.009	4712086	.036
Book bank	-.022	.017	-.054	.029 +	-.034	.014 *	-.038	.016 *	4802719	.082
# of Computers per pupil	.033	.017 +	.023	.018	.016	.013	.014	.008 +	4829568	.065
Ramp	-.011	.014	.069	.021 **	.073	.015 **	.044	.010 **	4826781	.229
Medical examinations	.015	.018	.007	.019	.040	.010 **	.037	.012 **	4826218	.037
Playground	-.004	.003	-.007	.006	-.010	.009	-.021	.011 +	4831235	.009

Notes: All models include school fixed effects, state trends and year effects. Standard errors are adjusted for state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

Table A5 Sensitivity of the main result to alternative indices of school resources and private schools

	[1]	[2]	[3]
Estimator:	IV	IV	IV
Dependent:	Index 1	Index 2	Index 2
Mean(S.D):	.066(5.822)	6.64(2.60)	7.61(2.30)
Sample:	Public schools	Public schools	Private unaided
Corr. With:			
PC1	.981	.954	
Index 1		.957	
Years from election			
[3]	.232	.124	.117
	[.229]	[.110]	[.050] *
[4] 'Pre-Election'	.646	.399	.119
	[.203] **	[.105] **	[.090]
[0] 'Post-Election'	.609	.365	.192
	[.228] **	[.105] **	[.104] +
[1]	.199	.102	-.045
	[.190]	[.080]	[.060]
Observations	4367424	4838628	655226
R2	.286	.289	.027

Notes: School fixed effects, state trends and year effects. SEs (in brackets) are clustered at the state level. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively. 'Index 1' is based on a simple addition of the 14 normalised resource variables. In 'Index 2', all resource variables are transformed into dummy variables, based on whether the value is above the average for public primary schools, and the values are then added up, so that the index for each school is an integer between 0 and 14.

Figure A1 Distributions of voter turnout and female literacy across districts