Teachers, Electoral Cycles and Learning in India

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Abstract

Teachers are central for learning, but if they are civil servants, their management and hiring can be affected by the political cycle. Using an administrative school-level panel data set across India, we show that teacher transfers and the hiring of new teachers increase significantly after State Assembly elections. The identification relies on the staggered and pre-determined timing of elections across states. The restructuring can be harmful; test scores are up to 0.15 standard deviations lower for children whose schooling coincides with the post-election phase. We conduct various checks to establish the link between the two findings.

Keywords: Teacher turnover, Political cycle, Education, India

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1 Introduction

It is well acknowledged that teachers are a key input in the educational production function (see e.g. Glewwe et al., 2014). There is also growing recognition of the importance of the quality of management of schools and other public services within Economics (see e.g. Bloom et al., 2015). The relevance of management can extend to school systems as a whole, and can also be influenced by external factors, such as the political process. While the role of political factors in the provision and management of education is recognised, there is limited rigorous quantitative evidence for developing countries, in particular with respect to teachers (see e.g. Kingdon et al., 2014 for a systematic review). In contexts where teachers are civil servants, political processes and changes can affect the management of personnel.

In this study, we demonstrate that the electoral cycle affects the transfer and recruitment of Indian public primary school teachers. In addition, we show that the electoral cycle affects learning. Further analysis indicates that the two effects are connected, implying that the political cycles in the management of teachers can have performance implications. Our data source for teachers is an India wide administrative school records database (District Information System for Education, DISE). For the analysis on learning we rely on child-level data from the Annual Status of Education Report (ASER), given that the DISE does not include reliable indicators on learning.

Regular primary school teachers in India are civil servants, generally on permanent contracts. They are hired by Indian states and core recruitment decisions are made at this level (see e.g. Ramachandran et al., 2008 and Sharma and Ramachandran, 2009). Teachers can also be directly involved in the political process for instance due to their role in staffing election booths.[1]

Our study focuses on State Assembly Elections; the timing of which is pre-determined and staggered across states, and can thus be considered exogenous. We find increases in teacher turnover, the number of teachers and new hires after the elections in relation to other years, but no evidence of electoral cycles in reported days spent on non-teaching assignments. The findings can be compatible with bottlenecks created by a rule banning transfers in the pre-election period, but also with increased administrative and political momentum of the incoming government.

The Election Commission of India's Model Code of Conduct imposes a ban on the transfers of all government employees, who are connected with election duties in the run-up to elections

1 A few studies claim that teachers may be used as agents for political support during election times and that political connections affect the possibility of obtaining a transfer (see e.g. Béteille, 2009, Kingdon and Muzammil, 2009 and 2013). The database that we use does not include data on the political connections of teachers.

(from the announcement of the elections). It also bans the appointment and promotion in Government/Public undertakings during the period.[2] Its aim is to reduce the capacity of politicians to influence the electorate during elections. Singh (2012) provides a useful overview on its content and development, including how political parties perceive to be constrained by it. Anecdotal evidence suggests that the ban is relevant for how teachers are managed. According to Jha et al. (2008), over the 2002-2005 period, the "Imposition of model code of conduct for assembly elections had also delayed teacher recruitment in Bihar and Haryana" (p.332).

To the authors' knowledge, the effects of electoral cycles on teacher recruitment have not been studied rigorously previously. A few studies have been conducted on bureaucrats in India. In an unpublished study, Iyer and Mani (2007) find evidence of an increase in bureaucrat transfers around election years, influenced by the incoming government. Other studies on Indian bureaucrats include Iyer and Mani (2012), which finds that political interests play a role in the transfers of Indian bureaucrats and a study by Bertrand et al. (2015) on the determinants of the effectiveness of Indian bureaucrats. On the other hand, there is a large literature on electoral cycles in public sector resources (see e.g. Nordhaus, 1975, Khemani, 2004).

After establishing an electoral cycle in the management of teachers, we proceed to test whether the electoral cycle also affects learning. We find that fourth grade pupils perform better in Reading and Mathematics tests if their schooling does not coincide with the post-election phase that is characterised by higher teacher turnover and new hiring. The role of the electoral cycle in learning itself has been little researched.

As evidence of a connection between the effects of the electoral cycle on learning and on the reorganisation of teachers, we show that 1) the timing of the two effects coincides and 2) the negative effects of the post-election period on learning are stronger in districts[3] that have a higher degree of excess teacher turnover in the post-election period. Further, we estimate models to rule out some potential competing explanations for the learning effects. Over the period studied, there is little connection between the electoral cycle and various types of reported crime or communal unrest, which might intensify during election periods and disrupt schooling. Furthermore, the electoral cycle has either no effect on learning (Reading), or a much milder effect (Mathematics) on pupils in private schools, indicating that the causes of the cycles lie within the public sector. Finally, we find no connection between the electoral cycle and pupil composition, when the latter is

2 For example, Election Commission's letter No. 464/INST/2007-PLN-I dated 7.1.2007 and titled "Code of Conduct: Do's and Dont's", specifies a ban on transfers, appointments and promotions (p. 2-3) http://eci.nic.in/eci_main/CurrentElections/ECI_Instructions/MCC_%20Do_and_Do'nt.pdf

3 A district is an administrative sub-unit of a state in India.

measured as the tendency of fourth grade pupils to be enrolled in a private school.

We therefore propose that our findings on learning are compatible with our findings on the teacher reorganisation process. This process incorporates the increased turnover of teachers, but the entire process cannot be captured with a single indicator in our data.[4] It is likely that such reorganisation can be disruptive and reduce effective teaching time, or the quality of teaching.

At a broader level, the results on the electoral cycles in teachers and learning can be considered symptomatic of impairments in the management of these services. Given that learning in private schools is largely unaffected by the electoral cycles, our findings also provide a new angle to the literature on the relative effectiveness of private versus public schooling (see e.g. Muralidharan and Sundararaman, 2015 and Singh, 2015).

We begin with a description of the data set used and the summary statistics (Section 2). The analysis on teachers, including a discussion of the identification and estimation, is presented in Section 3. Section 4 focuses on the effects of the electoral cycle on learning and Section 5 concludes.

2 Data

Our data source on teachers is an administrative school records database, the District Information System for Education (DISE), managed by the National University of Educational Planning and Administration (NUEPA), Delhi. From the year 2005 onwards, the database has full, or nearly full coverage of government administered primary schools in India. The data are reported on an annual basis and form a panel dataset of schools. The database includes a rich set of variables on school resources, management and pupils. For each school, it also includes a teacher level file with information on each teacher and key characteristics. These include name, age, caste, gender, date of birth, starting point of career as a teacher and indicators on educational qualifications.[5] There are no other comparable India-wide, annual data sources on schools. In most estimations we use a panel data set of schools for seven years between 2005-2011.

4 It is worth noting that in a developed country context some research (e.g. Ronfeldt et. al., 2013) shows a negative effect of teacher turnover on student achievement.

5 The database was originally introduced for the purpose of planning and monitoring of national education programmes in India, as such information systems were not available. The responsibility of reporting lies with schools. The consistency of the DISE data is checked annually at the state level with 5% re-sampling, and should involve independent monitors. More details can be found from http//:www.dise.in, http://schoolreportcards.in/SRCNew/AboutDISE/AboutDISE.aspx. (Last accessed 2 January 2016). There are no other comparable large, or accurate data sources on Indian schools.

In terms of timing, the year 2005 refers to the academic year 2005-06[6], and the data are collected in the Autumn of 2005, and similarly for the other years as well. To focus on a unified group of teachers, the sample is restricted to lower primary schools, which in most states spans grades 1-5.[7] Most of our analysis focuses on public sector schools, with a few robustness checks for private schools. For schools that include both lower and upper primary schools, the variables in this study relate only to lower primary students, and teachers who teach such students. The ASER survey, which is the source for the learning data is described in more detail in Section 4.

Our key outcomes of interest are the numbers of teachers, whether teachers leave a school in a particular year (transfer) and the numbers of new teachers hired per year in a district. We also analyse the effects on the number of days spent on non-teaching assignments per teacher in a school.[8] The number of teachers includes both regular teachers and contract teachers. The latter are hired on fixed term contracts with lower salaries. With transfers, we focus only on regular teachers, given that contract teachers are by nature temporary.

The database does not include a teacher identifier. For the indicator on teacher transfers, we need to uniquely identify teachers within schools. We construct an identifier based on the gender and the date of birth of the teachers within schools.[9] A teacher is considered to have transferred after year *t* if he, or she is no longer present in the school in the following year (*t*+1). We focus on teachers between the age of 18 and 55 to exclude the possibility of retirement. In general, teacher departures from schools can be interpreted as transfers. Moving out of the teaching profession is unusual due to the relatively high salaries and job security. Thus, when a teacher leaves a school, this can in most cases be interpreted as a transfer to another school, or to an administrative position in some cases (see e.g. Ramachandran et al. 2008). Changing school can be based on a transfer application or be forced.[10] The database does not allow us to track teachers across schools in a credible manner.[11]

We can define a dummy variable for leaving a school for all teachers up to the year 2010.

6 The school year tends to begin around June/July in most Indian states.
7 In some states, lower primary schools cover grades 1-4.
8 Non-teaching assignments can include tasks such as staffing of polling booths, revising electoral rolls, immunisation campaigns and the provision of information on welfare schemes and family planning (see e.g. Ramachandran et al., 2005).
9 The data includes names of teachers, but their spelling may vary and some surnames are too common to be used for identifying people.
10 Transfer rules are often not clear, and discretionary transfers and even mass transfers are reported in the literature (see e.g. Sharma and Ramachandran, 2009, pp. 161-166). While we have obtained documents relating to teacher transfers from specific Indian states, it has proven challenging to codify them or obtain comprehensive coverage of any rules over the time period studied.
11 This is due to the lack of a unique teacher identifier across schools.

The mean for our final sample is 0.171, suggesting a 17.1 percent likelihood of a transfer for each teacher-year observation. This 'exit indicator' cannot be computed for 2011 since that is the final year in our sample. Schools for which we cannot uniquely identify all teachers based on gender and date of birth are excluded.[12] Table 1 shows the summary statistics for the variables on teachers that are relevant for this study.

TABLE 1

Summary statistics for the variables used in the school level analysis are shown in Table 2. The selection of the sample and the data cleaning procedure are documented more precisely in Appendix 1. The final sample covers schools in 29 states or union territories and in 600 districts. It includes about 6 million observations of about 1.3 million unique primary schools in the form of an unbalanced panel for seven years.

TABLE 2

The data on the Indian Assembly Elections for the years 1999-2012 are supplied by the Election Commission of India. The timetable for the elections can be found in Appendix 1, 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.

The DISE database does not contain reliable, or comparable data on levels of learning.[13] We use the survey data on literacy and numeracy skills of children collected by the ASER Centre, which are representative across India. This will be described in more detail in Section 4.

12 In the raw data for 2005-2011 we have a panel of 12,596,621 teacher observations between the age of 18-55, who teach in lower primary non-private schools. The number falls by 266,489 (2.1%) due to the exclusion of schools in which teachers who cannot be uniquely identified by their date of birth and gender. After excluding year 2011 and some outliers we are left with with 10,182,861 observations. Table 1 reports the 9,546,949 observations for which also the phase of election is defined. This selection does not appear to lead to a substantial bias in terms of key characteristics such as gender, caste or education of the teachers.

13 DISE has information on the proportions of pupils that pass, or obtain a grade of more than 60% in the year 5 final exam. However, these are not based on a standardised test and cannot be considered comparable across schools.

3 Teachers and the electoral cycle

The following model is used to estimate the effects of the timing of elections on a set of teacher outcomes:

$$(1)\quad Outcome_{it}=\sum_{y}\beta_y D_{ys}+\lambda_t+\tau_s t+\alpha_i+u_{it} \qquad t\in[2005,2011] \qquad y\in[1,5]$$

where i refers to school, s to states and t to years. D_{ys} are a set of dummies corresponding to the election phases and y denotes the number of years from the latest election, one being the post-election year, and 5 being the (next) election year. The phase three years after the elections (y = 3) is set as the reference category. The coefficients of interest are the β coefficients, which measure the effect of the political cycle on the teacher variables. Finally, λ_t refers to year effects, τ_s to state trends and α_i to school fixed effects. Standard errors are clustered at the state level.

The information on teachers and schools for the academic year is collected in the Autumn. Suppose that elections take place in the (calendar) year 2008. We will interpret changes from 2007 to 2008 in schools as 'Election year' effects, and changes from 2008 to 2009 as 'Post-election year' effects. Post-election effects have in practice taken place under the new state government, while this is unlikely to hold for the 'Election year' changes.[14] In equation 1, D_{5s} and D_{1s} refer to the 'Election' and the first 'Post-election' years. Figure 1 illustrates the timing of the elections in relation to the school data, assuming that elections are held in the calendar year 2008.

FIGURE 1

The outcomes of interest are as follows: the total number of teachers in schools, the likelihood of a teacher leaving in a particular year (transfer) and the average number of non-teaching assignments per teacher as the dependent variable. The last outcome is of interest as teachers are used in election-related duties, and therefore elections might matter for reported non-teaching days. The variable for transfers is a dummy variable, reflecting the last year in which the teacher is observed in the school. For this outcome, a linear probability model with teacher level

14 As the school data are annual, but the election month varies, it is inevitably impossible to cleanly divide all effects into (pre-)election and post-election effects. 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 roughly similar results, while being less transparent.

data is estimated. The other two models are estimated with school level data.

Finally, we use the same framework, but a district level panel data set, to estimate the effect on the number of new teachers hired per year in a district. This model includes district fixed effects.

The identification of the β coefficients relies on the staggered timing of state elections across the states in the sample. In each state, the assembly elections are held every five years. In our sample there are a few exceptions, where 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).[15]

Table 3 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 '5') for each election year, whereas the instrument follows a five year cycle that begins again after each election.

TABLE 3

Table 4 presents the results for core teacher outcome variables for instrumental variable estimations. The results in column 1 show that there is a statistically significant effect on the likelihood of teacher transfers in the post-election year. The dependent variable is a dummy variable for the year during which the teacher is observed for the last time in the particular school. The magnitude of the coefficient is noteworthy: in the post-election year, teacher turnover, or transfers rise by 9 percentage points, while the average turnover rate is 17 percent.

The timing of the effect suggests a spike in transfer decisions in the immediate post-election year and that these changes will have materialised by the following year. Therefore, these changes can signal new policies, or momentum by the incoming government, but they could also be a result of the bottlenecks that have built up from pre-election restrictions. Our data are annual and do not allow us to distinguish credibly between different potential explanations.

The practices and policies on teacher transfers do vary by state, and the different factors and characteristics associated with turnover are beyond the scope of this study. Our purpose here is to

15 As the overwhelming majority of elections are held according to the schedule, the results with OLS or IV are very similar, and the overall conclusions of the paper do not hinge on them. The first stage, where the realised election dummies are instrumented with the scheduled dummies results in estimates that are close to one, with very high statistical significance, and are thus not reported here.

confirm that the likelihood of transfers is associated with the state election cycle, and the effect is not trivial in size.[16]

The results in column 2 of Table 4 refer to the total number of teachers in the schools, and the model is estimated using the school level panel data set. There is a statistically significant effect on the number of teachers two years after the election year. The magnitude of the effect is rather small, since with an average of about 3 teachers per school, an increase of 0.05 would translate into less than 2%.

The final column in Table 4 shows the numbers of non-teaching assignments per teacher per school, averaged across all lower primary teachers in the school. The non-teaching days reported in DISE refer to the previous academic year. Nevertheless, there does not appear to be any substantial correlation between the numbers of days spent on non-teaching assignments and the election cycle. It is worth emphasizing that these formally recorded non-teaching assignments are not indicative of informal teacher absences, which are likely to be much larger (see e.g. Kremer et al., 2005). The DISE does not include indicators on teacher absence, nor periods during which a school may be missing a teacher due to a transition period (on absence shocks, see e.g. Das et al., 2007).

TABLE 4

Table 5 shows the results for the numbers of new teachers hired in a district. A district level data set is compiled from the teacher level data, given that the hiring and posting of teachers are often managed at the level of the district (see e.g. Sharma and Ramachadran, 2009). The results in the first column suggest that two years after the elections, there are 130 more new hires compared with the reference year, and the effect is significant at the 5% level. As the size of the districts varies, column 2 shows the same estimation using a logarithmic transformation of the new hire variable. Here, the effect is no longer significant, although the coefficient of 0.376 suggests an increase of 30-40% in new hires compared to other years.

TABLE 5

16 We have tested whether the size of the election cycle effects is dependent on the political alignment of the district, but found that not to be the case. Political alignment of districts can be measured as the proportion of the constituencies in the district which were won by the same party as the leading party in the state. The DISE database does not include information on which constituency each school belongs to.

Overall, the results indicate that elections are followed by a clear increase in teacher transfers, the total number of teachers and possibly increases in new hiring. One would expect the immediate post-election year to be characterised by anticipation of change, and possible uncertainty about future transfers.

4 The electoral cycle and learning

Since teachers are considered a crucial resource in schooling, it is relevant to ask whether the observed post-election re-organisation of teachers can disrupt the school system enough to affect learning. As the DISE database does not include reliable data on learning, we are only able to study the question indirectly. We begin by demonstrating that learning is also characterised by an electoral cycle that coincides with that for teacher transfers and recruitment. We use pupil-level data from the ASER survey. Subsequently, we analyse the differences in these effects by districts with above and below median rates of teacher turnover. As a robustness check, we explore other potential channels.

4.1 Data

The ASER survey is an annual survey of rural children, carried out since 2005. The sampling is representative at the district level. It is a repeated cross-section of household surveys, which includes a test of Reading and Numerical skills of children, carried out at home. We use the child-level data from 2005 to 2012, and combine it with data on the timing of state elections. The ASER 2005 covered 6-14 year olds, and in later surveys the coverage was expanded to 3-16 year olds.[17]

The tests on learning are categorical. There are five categories for the pupils' reading skills: ability to read a story (5), paragraph (4), sentence (3), a word (2), or nothing (1). There are four categories for numerical skills: ability to divide (4), subtract (3), recognise a number (2), or nothing (1). From these indices, we construct age-specific z-scores for each pupil in both Reading and Mathematics, normalised with respect to ASER 2005. The z-score for each level of attainment is based on the distribution in the 2005 data for each age group between 6 to 14, and applied to all consecutive years. This results in a unified outcome variable that is comparable across time. The

17 ASER also includes a school survey of one government school in each village. However, the school survey has not been carried out every year, and the survey does not have a panel dimension, making it mostly unhelpful for our analysis.

main purpose for normalising the scores is to facilitate interpretation, given that the estimated coefficients can be interpreted as standard deviations.

In the measurement of the learning effects it is important to take into consideration that since the lower primary school lasts for five years in most states, fifth graders will have experienced all phases of the election cycle during their time in school. However, as we are interested in understanding more about the consequences of a school system specific election effect, we wish to focus on pupils who have been in the education system for a sufficiently long enough period. Otherwise, we may be capturing effects on learning that pre-date formal schooling.

Therefore, we focus on pupils who are currently in their fourth grade. They have all avoided a specific election phase.[18] After pooling fourth graders across all states and years, roughly one fifth of the pupils have not experienced elections (or another phase of the cycle) during their time in school, since their 4 years of education fall in between the 5-year election cycle. This will be the source of identifying variation in the main estimation that we discuss below.

We restrict the sample to those who state that they are currently in grade 4 in a government school, leaving us with a sample of about 400,000 tested children for the years 2005 to 2012. Each ASER survey is conducted in the Autumn term, so that the survey is typically collected in November and December of the same year. We match the data on test scores to the timing of the elections by calendar year. As before, the identification of the effects of the electoral cycle relies on the staggered timing of state elections across the 28 states and territories in the sample. The summary statistics for the ASER sample are shown in Table 6.

TABLE 6

4.2 Estimation and results

In practice, all pupils enter into one of five exogenously determined treatments, based on the phase of the election cycle that they begin their schooling in. Abstracting from the possible grade repetition, Table 7 lays out the potential electoral phases experienced by the pupils. The rows in the Table refer to years, showing pupils progressing from a lower to a higher grade. The columns refer to the potential treatments, which depend on the election cycle. Pupils in treatment 1 (T1, column 1)

18 Focusing on grade 4 pupils simplifies the interpretation of the estimates, but similar estimations could be carried out for grade 3 or grade 2 pupils with similar results. Grade 1 pupils on the other hand, have been in school for less than half a year at the time of testing.

begin their schooling in the post-election year (phase 1), and enter grade 4 in phase 4. Among pupils in grade 4, those in treatment 1 have not experienced elections (phase 5) during their schooling by the time of testing (Autumn of their fourth grade). Due to the availability of eight consecutive ASER survey years and the staggered timing of elections across the states, the five lines of treatments are distributed relatively evenly across states, cohorts and years in our data.

TABLE 7

In principle, all phases of the election cycle could have different effects on learning, with differential effects by grade, and decay over time. In practice, not all of these effects would be identifiable from the data. Thus, for the sake of tractability, our working hypothesis is that certain phases of the electoral cycle are worse or better for learning, they affect all pupils irrespective of grade, but the effects on learning may or may not be persistent.[19] We begin by estimating the following model:

(2) $zscore_{itd} = A_i + Female_i + \Lambda_t + \Omega_d + \beta Miss_y + u_{it} \qquad t \in [2005, 2012] \qquad y \in [1,5]$.

The dependent variable is the level of skills by pupil *i* in year *t* and district *d,* as measured by the age-specific z-score (normalised to 2005) in either Reading or Mathematics. The sample is restricted to grade 4 pupils. The variable of interest is the $Miss_y$ dummy. Assuming that no grade repetition took place, this dummy variable indicates whether a pupil was not attending school in the school year that begins over a certain phase of the election cycle (*y*). For example, $Miss_1$ refers to missing the school year that begins in the post-election year.

A set of dummies (A_i) are used to control for the number of years that the pupil is over or under aged for the grade. The model also includes gender, survey year effects (Λ_t), and district fixed effects (Ω_d).[20] Again, the actual election phases are instrumented with the intended election phase.

It is worth noting that in India there is substantial variation in age at all grade levels. For example, fourth graders are typically 9 or 10 years old, and 93% are between 8 and 12 years old. We do not have information on grade repetition, and deal with this variation by controlling for age.

To provide another angle to the question, we also estimate the effect of the *current* election

19 The persistence of the effects will be discussed in the interpretation of the results.
20 This is the level at which the data are representative and it is the lowest geographical denominator with a panel dimension.

phase on grade 4 pupils:

(3) $zscore_{itd} = A_i + Female_i + \Lambda_t + \Omega_d + \sum_y \beta_y D_{ys} + u_{it} \quad t \in [2005, 2012] \quad y \in [1,5]$.

This specification is similar to the estimated models on teachers, with similar election year dummies (D_{ys}) and instrumentation. The election year (D_{5s}) is the excluded category. Essentially, models (2) and (3) will convey the same message, but are framed differently. The ASER surveys are also conducted in the Autumn and the matching with the timing of the elections is done similarly as with the regressions on the teacher outcomes.

Table 8 shows the results for model specification (2). We estimate separate models for pupils in government (public) and private schools and separate models for Reading and Mathematics. The rows refer to a missed phase of the election cycle. It is important to note that each row-column cell represents a coefficient from a separate estimation.

The results in column 1 show that not being in school in the school year beginning during the post-election year is beneficial for Reading outcomes. Avoiding this year increases Reading scores on average by .084 standard deviations for those currently in grade 4. With respect to the treatments in Table 7, the result implies that fourth graders would be best off if they experience 'Treatment 2'; they begin their schooling in phase 2, and enter fourth grade in phase 5 (the election year).

TABLE 8

Those who miss election phase 3; three years after elections, appear to do worse than others. Referring back to Table 7 we can see that fourth graders who miss phase 3 are in 'Treatment 4'; they experience elections in the same year that they begin grade 2, and reach grade 4 two years after the elections. Out of all the treatments linked with experiencing the post-election year in Table 7 (T1, T3, T4 and T5), those in Treatment 4 are in the 'worst' position since the election phases 1 and 2, which were associated with teacher movements, have just taken place prior to the testing of the children in grade 4. Those in treatments T1 and T5, who experienced the turbulent year earlier during their schooling, have better skills. This result suggests that there is a degree of decay in the effect of the post-election shock.

Column 2 shows the results for similar models for Mathematics. The results are broadly similar to those for Reading, but the estimated coefficients are somewhat larger. A fourth grade pupil scores .115 standard deviations higher, if she has missed the school year starting in the post-election year during her primary schooling.

In columns 3 and 4 of Table 8, the same estimates are repeated for children who attend private schools. If the effects on learning derive from the government school system, we would not expect to see effects on learning for private school pupils. For Reading, there is no evidence that the phases of the election cycle would make a difference for learning; all coefficients in the Table are statistically insignificant. The same is true for Mathematics in column 4, with phase 1 being only marginally statistically significant. Overall, the results for private schools suggest that the findings in columns 1 and 2 are not mere statistical artefacts, but represent variations in the quality of how government schools are run across the electoral cycle.

Figure 2 presents a time line to assist in the interpretation of the findings so far. The indicator for observing teachers for the last time in their school peaks in the Autumn of election phase 1. This implies that turnover itself peaks over the academic year that starts in that Autumn. By the start of the following academic year, we observe increases in the numbers of new hires and teachers. Therefore, the school year that begins during election phase 1 and ends in election phase 2 is the most turbulent year in terms of teacher turnover and recruitment. Pupils who begin their primary schooling right after this period (Treatment T2), do better than others. Those in treatment T4, who are tested right after the most turbulent school year, have the worst test scores. Those reaching grade 4 in the beginning of the turbulent year (Treatment T3) do not fare worse than others.

FIGURE 2

Table 9 presents the results of model (3), separately for Reading and Mathematics. In this case, each column refers to a single regression, and shows the relationship between grade 4 test scores and the *current* election cycle, at the point when the pupils are tested.

TABLE 9

The baseline group in Table 9 is Treatment T2, or those who are in grade 4 in the election year. All estimated coefficients in column 1 are negative, suggesting that pupils in all other treatments fare worse in Reading. Again, the most harmful treatment is T4. This refers to the group of pupils tested right before the turbulent personnel management year. The estimate shows that the difference in relation to the best treatment is 0.127 standard deviations.

A comparison of the results in columns 1 and 2 shows that the persistence of the negative shock from the turbulent year is larger for Mathematics than for Reading. For treatment T5, the turbulent year coincides with the second grade, and for treatment T1, it coincides with the first grade. The effect of being in these treatments is significantly negative for Mathematics, but not for Reading.

There is no association between the electoral cycle and Reading in private schools in Table 9. For Mathematics, there is one marginally significant coefficient for children in private schools. Here, it is worth emphasizing that even if the cause of the election effects lies with the public sector, there may still be positive effects on private sector schools, since we do not know the educational history of the tested pupils. Further, the private schools in the ASER data include government aided schools, and can therefore be dependent on the electoral cycle as well.

Taken together, the results in Tables 8 and 9 encompass three key findings: 1) Government school pupils who miss the school year characterised by higher turnover and new hiring during their first years of schooling do on average better than others in both Reading and Mathematics. The pupils who experience this year towards the end of their primary schooling do worse in tests than those who experience it in earlier grades, suggesting that the effects are not fully persistent. 2) These election cycle effects are largely absent for private sector pupils, indicating that features, or events associated with the public system are likely to explain the effects. 3) The magnitude of the effects is not trivial. The average difference in test scores between those in the best and worst election phases is .151 standard deviations for Mathematics, and .127 standard deviations for Reading.

4.3 Is the reorganisation of teaching connected with the variations in learning?

Learning in the post-election period can evidently be affected by a number of factors. In this Section we provide suggestive evidence for the claim that the reorganisation of teaching resources after the elections is associated with the observed variations in learning. The 'reorganisation' of teachers may have multiple effects; the anxiety related to the anticipation of the potential transfers, as well as the more concrete disruption to teaching as some teachers move and new teachers are hired. Any effect on learning could be composite of these factors, which cannot be easily measured with our data. Below we provide indirect evidence to validate the link between the findings on teachers and learning. We also explore the role of alternative channels in explaining the learning effects.

The exogenous source of variation in this study stems from the timing of the elections. However, the election cycle cannot be used as an instrumental variable for the teacher turnover. This is because it is possible that the election cycle affects learning via various mechanisms, for which data are not available.

As the first robustness check, we estimate a measure for the 'excess teacher turnover' caused by the post-election year for each Indian district, and show that the size of this excess turnover is related to the magnitude of the electoral cycles in learning across Indian districts.

Next, we explore whether the effects on learning could be explained by changes in pupil composition. Finally, we test whether multiple measures of communal upheaval and crime, which could be disruptive for schooling, follow the election cycle.[21]

In the first robustness check, we estimate the extent to which teacher turnover is higher in the post-election year separately for *each* district, using the following simple OLS model:[22]

$$(4) \qquad Turnover_t = \alpha + \beta D_{1t} + u_t \qquad t = 2005 \ldots 2010 \quad .$$

The coefficient β captures the magnitude of the turnover in the district in the post-election year (phase 1) in comparison to other years on average. Since each regression is based on only 6 observations, statistical significance is ignored and no other controls are added. Unsurprisingly, the estimate for β varies substantially by district. It has a mean of .061, a median of .042, and a standard deviation of .125. The mean is in line with the post-election effect on turnover estimated from the individual level teacher data in Table 4.

As the next step, we divide the districts in two groups; those where β is below and those where it is above the median (0.04205). We then re-estimate the effect of the electoral cycle on learning as in Table 8. The results of this exercise are shown in Table 10. They reveal that in districts where post-election turnover is greater, avoiding the post-election year during primary schooling is associated with 0.111 standard deviations higher Reading scores and 0.148 standard

21 In a working paper, Fagernäs and Pelkonen (2014) show that there is a modest increase in physical school resources in the election year and the post-election year, compared with other years in the election cycle. However, the effect varies by individual school resources. Such increases cannot explain the post-election slump in test scores.

22 Estimated only for districts for which all year observations are available (569 districts).

deviations higher Mathematics scores. For those districts with lower turnover, the effects are lower at .0612 standard deviations and .0911 standard deviations, respectively. A cautious interpretation of these results would be that the post-election excess teacher turnover is at least a partial proxy for the turbulence that leads to lower learning outcomes.

TABLE 10

Private schooling is very common in India, and roughly a quarter of primary school children attend a private school. If the year after the elections are characterized by impaired functioning of public sector schools, and this is recognized locally, parents may respond by moving their children to private schools.

In Table 11, we have estimated the effect of the electoral cycle on the likelihood of a child being in a private school for the sampled households in ASER in grade 4. The results show that there is no connection between the likelihood of attending a private school and the election cycle. Therefore, changes in pupil composition cannot lie behind the effects of the electoral cycle on the learning of fourth graders.

TABLE 11

As a final check, we test whether the electoral cycle is connected with communal upheaval or crime, that could disrupt schooling. Evidence for earlier periods suggests that there could be a connection between elections and violence (e.g. Wilkinson, 2006). For this analysis, we have obtained panel data on the reported numbers of murders, rapes, kidnappings, riots and arson by Indian police districts (Summary statistics are in Appendix 1, Table A3). Matching the data to the timing of elections across the years and states allows us to test whether the incidence of these crimes varies by the electoral cycle. Table 12 reports the results.

TABLE 12

The results in Table 12 provide no support for the alternative hypothesis that communal violence could explain the disturbances in teaching or learning outcomes. We observe only slightly elevated (8%) numbers of riots two years after the elections. In a similar robustness check, we have tested whether the election cycles in learning are similar in districts with and without Naxalite activity, and they are.[23]

5 Conclusions

In this study, we show evidence of a reorganisation of the teaching force after State assembly elections in India. Indian teachers are more likely to be transferred a year after elections, and the numbers of teachers as well as new hires rise two years after the elections. These findings can be consistent with the Indian Election Commission's Model Code of Conduct, which imposes a ban on the transfers of government employees in the run-up to elections and also restricts hiring, creating a backlog of transfers. Alternatively, they can reflect the added 'political momentum' of a new government. In any case, it is evident that the post-election period is characterised by the restructuring of personnel in schools.

Using a rich household survey of children for 2005-2012, we show that pupils who avoid the turbulent phase starting a year after the elections, perform significantly better than others in both Reading and Mathematics. Such effects are not found for pupils in private primary schools, confirming that the causes of the cycles in learning lie within the government schooling sector.

There is growing recognition of the importance of the management of schooling and other public services within Economics. For instance, in the context of Indian schooling, evidence shows that private schools can function more efficiently (e.g. Muralidharan and Sundararaman, 2015), and the suggested explanations to this range from school autonomy to teacher accountability. Further, Bloom et. al. (2015) show that for eight countries ranging from the US to India, 20 basic management practice measures are strongly associated with better learning outcomes. The impact of the political processes on the functioning of schools as seen in this study, can be considered an additional management dimension, or a consequence of impaired management practices.

The potential learning premium associated with private schools in India has been estimated to range from zero, or moderate (see e.g. Muralidharan and Sundararaman, 2015) to more than 0.5 standard deviations (Singh, 2015). In our study, experiencing specific phases of the electoral cycle can lead to up to 0.15 standard deviation differences in test scores by grade 4 in government schools. Therefore, such variations in the functioning of schools due to the election cycle can well be a component of the public-private difference in school quality in India. As the electoral cycles in

23 The results are available on request from the authors.

learning are largely not observed in the private sector, the evidence in this study would provide support for a degree of school autonomy in personnel management.

Our results indicate that one potentially interesting avenue for future research would be to collect more detailed data on teacher attendance, effort, outside activities or teachers' role in the political process around the election years across Indian states. Another avenue would be to study the presence of electoral cycles in the management of public sector employees also in other countries or in other public services.

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Table 1 Summary statistics for formal government school teachers, pooled 2005-2010

	Obs.	Mean	S.D.	Min	Max
Teachers exits school (transfer)	9546949	.171	.376	0	1
Female	9546949	.411	.492	0	1
Age	9546949	38.5	8.8	18	55
Newly hired teacher	9546949	.047	.211	0	1
Election phase:					
1 – Post-election year	9546949	.205	.404	0	1
2	9546949	.215	.411	0	1
3	9546949	.192	.394	0	1
4	9546949	.198	.399	0	1
5 – Election year	9546949	.189	.391	0	1

Source: DISE 2005-2010. Observations for 2011 are excluded as the teacher exit variable cannot be calculated for the final year (as it is defined as the last year that a teacher is observed in a school).

Table 2 Summary statistics for schools, pooled 2005-2011

	Obs.	Mean	S.D.	Min	Max
# of Teachers	4929221	2.76	1.80	0	59
# of Formal teachers	4929221	2.31	1.83	0	59
Days on non-teaching assignments	4929147	2.3	11.1	0	365
Election phase:					
1 – Post-election year	4929221	.200	.400	0	1
2	4929221	.209	.406	0	1
3	4929221	.203	.402	0	1
4	4929221	.203	.402	0	1
5 – Election year	4929221	.185	.388	0	1

Source: DISE 2005-2011

Table 3 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	5	1	2	3	4	5	1	2	5	1	2	3	3	4	5	1	2	3	4	5
Instrument	5	1	2	3	4	5	1	2	3	1	2	3	4	5	1	1	2	3	4	5

Definitions:
- E = Election year
- 1 = 1 years from election ("Post-election year")
- 2 = 2 years from election
- 3 = 3 years from election (Control year)
- 4 = 4 years from election
- 5 = 5 years from election ("Election year")

Table 4 Effects of the electoral cycle on the turnover of teachers and number of teachers in government schools, IV estimates

	[1] Turnover	[2] # of Teachers	[3] Non-teaching assignments (days)
[4]	.0697	.0717	.1330
	[.0418]	[.0482]	[.28]
[5] 'Election year'	.0207	.0209	.3130
	[.0185]	[.0703]	[.286]
[1] 'Post-Election year'	.0917**	.0165	.4710
	[.0208]	[.0601]	[.404]
[2]	.0065	.0476*	.5940
	[.00903]	[.023]	[.337]
Data	Teacher-level	School-level	School-level
Observations	9507638	4813102	4813054
R-squared	.022	.040	.011

Notes: All models include school fixed effects, state trends and year effects. In column [1] the model is estimated using individual teacher data and the dependent variable is a dummy indicating that the teacher is being observed in the school for the last year. The sample includes formal teachers in non-private schools who are between 18-55 years old. Column [2] is based on school-level data and includes para-teachers. Standard errors are clustered at the state level. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

Table 5 Political cycle and number of newly hired teachers, district panel 2005-2011, IV estimates

	[1]	[2]
	# New teachers	
	Linear	Log
[4]	97.1	.00126
	[72.8]	[.223]
[5] 'Election year'	36.2	.116
	[43.6]	[.298]
[1] 'Post-Election'	25.1	-.0831
	[33.8]	[.235]
[2]	130*	.376
	[65.1]	[.303]
Observations	4103	4103
R-squared	.148	.151
Number of Districts	598	598

Notes: All models include district fixed effects, state trends and year effects. In the logarithmic transformation a 1 is added to all numbers to avoid losing log(0) observations. Standard errors are adjusted for state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

Table 6 Summary statistics of ASER 2005-2012, grade 4 pupils

	Obs.	Mean	S.D.	Min	Max
Read nothing	408677	.034	.182	0	1
Read word	408677	.105	.306	0	1
Read sentence	408677	.187	.390	0	1
Read paragraph	408677	.283	.451	0	1
Read story	408677	.390	.488	0	1
Reading z-score	408677	.103	.924	-3.15	2.51
Maths nothing	406532	.044	.205	0	1
Maths number	406532	.363	.481	0	1
Maths subtract	406532	.346	.476	0	1
Maths divide	406532	.247	.431	0	1
Maths z-score	406532	.104	.900	-2.34	3.08
Female	423629	.456	.498	0	1
Age	427218	9.60	1.37	6	14
Private school	422740	.211	.408	0	1
Current election phase					
1 – Post-election year	427218	.195	.396	0	1
2	427218	.191	.393	0	1
3	427218	.196	.397	0	1
4	427218	.216	.411	0	1
5 – Election year	427218	.203	.402	0	1
Coverage: 562 districts in 28 states					

Notes: The mean of z-scores is above zero and the standard deviation lower than unity due to the normalisation being with respect to ASER 2005.

Table 7 The five 'treatments' induced by the election cycle

	[T1]	[T2]	[T3]	[T4]	[T5]
	Experienced phases of the cycle				
Grade 1	1	2	3	4	5
Grade 2	2	3	4	5	1
Grade 3	3	4	5	1	2
Grade 4	4	5	1	2	3

Notes: Treatment T1 means that the pupil begins school, and enters grade 1 in phase 1 of the election cycle, or one year after the election year.

Table 8 Learning outcomes of grade 4 pupils, by missed election phase during primary school, IV estimates

	[1]	[2]	[3]	[4]
	Government		Private	
	Reading	Maths	Reading	Maths
Treatment / Election phase missed by grade 4				
T2 / Miss school year beginning in the post-election year	.0843*	.115**	.0133	.0481+
	[.0362]	[.0409]	[.0221]	[.0273]
T3 / ..phase 2	-.0130	-.0131	-.0017	-.0114
	[.0263]	[.0278]	[.0139]	[.0162]
T4 / ..phase 3	-.0719**	-.0703**	-.0188	-.0320
	[.026]	[.0267]	[.024]	[.0287]
T5 / ..phase 4	.0056	-.0191	-.0108	-.0047
	[.025]	[.022]	[.017]	[.0171]
T1 / Miss school year beginning in the election year	.0064	.0004	.0164	-.0020
	[.0254]	[.0302]	[.0191]	[.0199]
Observations	317762	316104	83699	83261
Number of districts	562	562	562	562

Notes: Each row-column cell represents the coefficient from a separate regression model. Each model includes district fixed effects, survey year controls, age and gender controls. Standard errors are clustered at the state level. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels

Table 9 Learning outcomes of grade 4 pupils by current election phase, IV estimates

	[1]	[2]	[3]	[4]
	Government		Private	
	Reading	Maths	Reading	Maths
Treatment / Years from election:				
T3 / 1 year from elections	-.0803**	-.105**	-.0143	-.0485*
	[.0214]	[.0315]	[.0206]	[.0232]
T4 / 2	-.127**	-.151**	-.0298	-.0678
	[.0478]	[.052]	[.0379]	[.0456]
T5 / 3	-0.0655	-.109*	-0.0226	-0.045
	[.0473]	[.0472]	[.028]	[.0347]
T1 / 4 years from elections	-.0693	-.101+	-.0007	-.0420
	[.0447]	[.0521]	[.0257]	[.0295]
Observations	317762	316104	83699	83261
R-squared	.116	.136	.118	.129
Number of districts	562	562	562	562

Notes: The excluded category in election phases is Treatment 2, or the election year (phase 5). All models include district fixed effects and year effects, and age and gender controls. Sample includes 562 districts. Standard errors are adjusted for state level clustering. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels, respectively.

Table 10 Reading outcomes of grade 4 pupils, by missed election phase during primary school. Split samples by the intensity of teacher turnover in the Post-election year, IV estimates

	[1]	[2]	[3]	[4]
	Reading		Mathematics	
	Low β districts	High β districts	Low β districts	High β districts
Treatment				
T2	.0612+	.111**	.0911*	.148**
	[.0357]	[.0401]	[.0393]	[.0468]
T3	-.0068	-.0100	-.0125	-.0093
	[.0196]	[.0409]	[.0271]	[.0429]
T4	-.063**	-.0642*	-.0597**	-.0617*
	[.0229]	[.0279]	[.0179]	[.0312]
T5	-.0028	.0004	-.0129	-.0378
	[.0263]	[.0303]	[.0269]	[.0252]
T1	.0114	-.0151	-.0047	-.0133
	[.0225]	[.0438]	[.0315]	[.0491]
Observations	139679	174137	139030	173176
Number of districts	274	280	274	280

Notes: β in the table refers to the coefficient in a district-specific regression model, where the annual teacher turnover rate is explained by the phase 1 (post-election year) dummy only. 'High' and 'Low' refer to above and below median values (.04205). Each row-column cell represents a separate estimation. Each model includes district fixed effects, survey year controls, age and gender controls. Standard errors are clustered at the state level. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels.

Table 11 Effect of election cycle on private school enrolment in ASER, pupils in grade 4, IV estimates

Dependent: Attend private school	
[4]	.0008
	[.00789]
[5] 'Election year'	.0058
	[.0061]
[1] 'Post-Election'	.0046
	[.00529]
[2]	-.0058
	[.00564]
Observations	424889
R-squared	.012
Number of districts	562

Source: ASER pupil level data for 2005-2012. The dependent variable is a dummy variables. Model controls for gender, district fixed effects and year effects. Standard errors are clustered at the state level. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels.

Table 12 The effect of election cycle on communal upheaval in a district-level panel, 2005-2012, OLS estimates

Dependents in logs	[1] Murder	[2] Rape	[3] Kidnapping & Abduction	[4] Riots	[5] Arson
Years from election:					
[4]	-.045+	-.008	-.021	-.007	.045
	[.0263]	[.0332]	[.036]	[.0312]	[.0578]
[5] 'Election year'	-.035	-.009	.022	.064	.045
	[.0228]	[.0359]	[.0455]	[.0418]	[.032]
[1] 'Post-Election'	-.0371+	.020	.028	.027	.046
	[.0202]	[.0442]	[.0403]	[.0384]	[.0325]
[2]	-.032	.034	.042	.0869*	.004
	[.0234]	[.0324]	[.0417]	[.039]	[.0332]
Observations	4626	4626	4626	4626	4626
R-squared	.007	.077	.276	.018	.003
Number of Districts	588	588	588	588	588

Notes: The dependent variable is in logarithmic form and a 1 is added to all numbers to avoid losing log(0) observations. Each model includes district fixed effects and year controls. Standard errors are clustered at the state level. (+, *, **) refer to statistical significance at 10%, 5% and 1% levels. Summary statistics of the data are in Appendix 1.

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

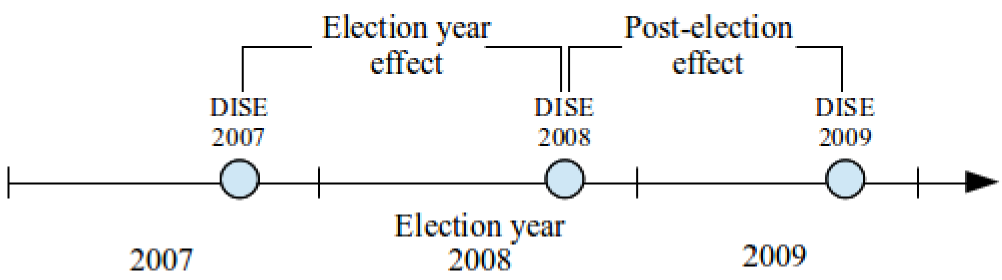
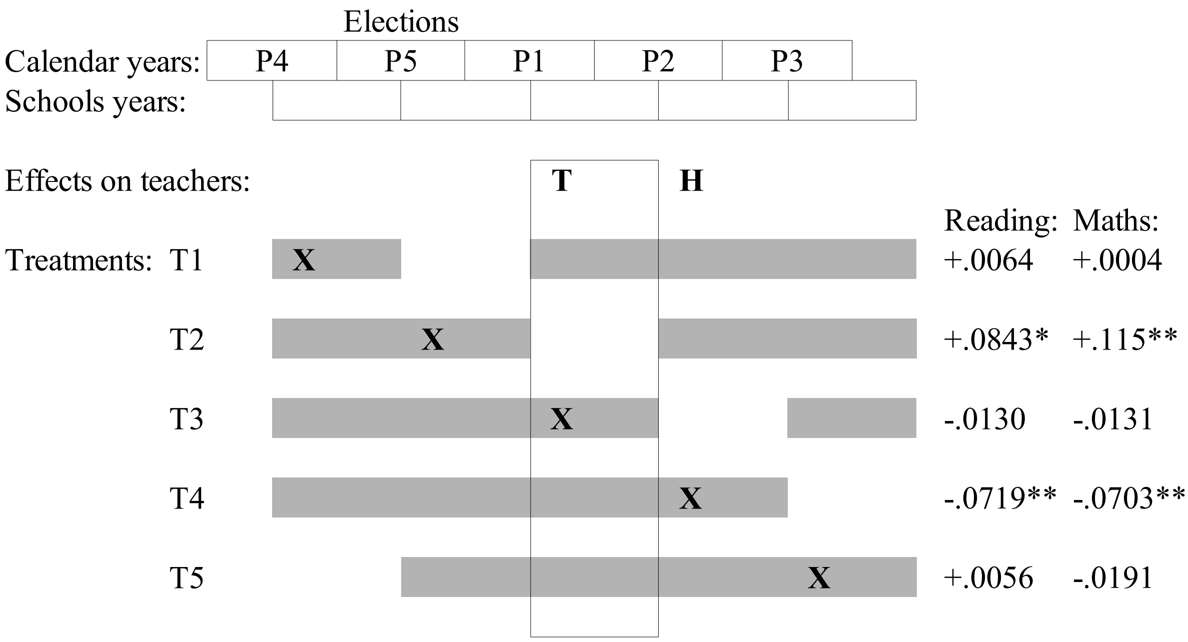


Figure 2 The key results, a time line representation



T Peak in teachers leaving/changing school
H Peak in new hires observed at district level
The most turbulent school year
Treatment pupils in school
X Point of testing
Reading/Maths: Effect on learning based on coefficients in Table 8 for missing a specific election phase

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 (2.61 versus 2.77 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 changed name, but since the analysis uses school fixed effects throughout, we strive to ensure that school panels are genuine. Secondly, from the remaining sample, 8.9% of observations are deemed to be outlier observations with respect to some key variable of interest. Outlier status is first assigned to observations with undoubtedly unrealistic values. With uncertain cases, the top (and/or bottom) 0.5% of the values are regarded as outliers. Outliers, on average, relate to larger schools than others (3.02 teachers). Finally, 2.2% of the remaining observations include missing values for some variables of interest. The initial and final samples in terms of a few characteristics are showed in Table A1 below.

Table A1 Sample selection in the DISE school-level data

	Raw data		Change Sch. Code		Outliers		Final		Regression sample	
Observations	6059856		524356		494636		5040864		4929221	
	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.	Mean	S.D.
Year	2008.1	2.0	2008.0	2.0	2007.9	2.3	2008.1	2.0	2008.1	2.0
# of Teachers	2.77	1.95	2.61	1.80	3.02	3.15	2.76	1.81	2.76	1.80
Urban school	.07	.25	.05	.22	.10	.30	.07	.25	.07	.25

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	West Bengal Apr	Pondicherry May
West Bengal Apr	Tamil Nadu May	Tamil Nadu May
Pondicherry Apr	Pondicherry May	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	Rajasthan 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 Numbers of reported crimes/communal upheaval by police district, 2005-2012

	Mean	S.D.	Min	Max
Murder	54.4	46.0	0	451
Rape	35.1	36.5	0	568
Kidnapping & Abduction	51.5	59.6	0	764
Riots	108.0	158.9	0	2818
Arson	15.4	38.3	0	2350
Election phase:				
1 – Post-election year	.187	.390	0	1
2	.197	.398	0	1
3	.208	.406	0	1
4	.213	.410	0	1
5 – Election year	.195	.396	0	1
N = 4626, 588 police districts				

Source: District-wise crimes under various sections of Indian Penal Code (IPC), Crime in India 2013, National Crime Records Bureau (NCRB).