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Election cycles and electricity provision: Evidence from a quasi-experiment with Indian special elections

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ABSTRACT

We present evidence from India showing that state governments induce electoral cycles in electricity service provision. Our data and research strategy allow us to build on models of political business cycles and targeted distribution in two important ways. First, we demonstrate that by manipulating the flow of critical inputs into economic activity like electricity, elected leaders can influence economic outcomes even in contexts where they have constrained fiscal capacity. Second, we identify the effect of elections on electricity provision by focusing on special elections held for exogenous reasons. Our results show that state governments induce substantive increases in electricity service to constituencies that hold special elections. Manipulation of the power supply is stronger in contested constituencies and during special elections held in states where the government commands only a small majority. Overall, we find no evidence of positive welfare effects from the electoral manipulation of electricity supply.

1. Introduction

An influential literature asserts that democratic governments have strong incentives to use their leverage over economic policies to improve their electoral prospects, generating political business cycles in the process. Most empirical research evaluating the existence of political business cycles draws on the industrialized world, where government control over economic policies can have significant impact on the national economy.[2] In much of the developing world, however, governments face constraints on fiscal capacity and budget shortfalls which may limit their ability to influence economic conditions in ways that are meaningful to the majority of voters. By focusing on special elections held to fill unexpected vacancies in Indian state legislatures, this paper demonstrates a means by which elected leaders in developing countries can influence economic outcomes through an alternative channel: by manipulating the flow and quality of public services that are critical inputs into economic activity.

Our data and research strategy allow us to build on the existing literature on political business cycles in several ways. First, using a dataset of some 4000 state-level assembly constituencies observed from 1992 to 2009, we examine whether Indian state governments manipulate the provision of electricity before elections. Electricity is the lifeblood of the modern economy and a basic input into many productive activities (Dinkelman, 2011; Rud, 2012; Lipscomb et al., 2013). Yet, persistent power shortages in many developing countries require governments to actively manage the supply and distribution of power through load shedding and power outages. In India, electricity service provision is highly valued by citizens and power shortages are known to significantly reduce firm output and revenues (Allcot et al., 2014). As a result, electricity often features as one of the top priorities of Indian voters in election surveys (Chhibber et al., 2004). At the same time, state governments can apply significant pressure on public utilities regarding how, when, and where electricity is provided (Min, 2015; Min and Golden, 2014; Nagavarapu and Sekhri, 2014). They can thereby influence economic activity and welfare even when they have limited capacity to manipulate broader levers of fiscal and monetary policy that are more common objects of study in the industrialized world.

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[1] All errors are ours.

[2] Previous empirical contributions on political business cycles include Alesina (1989), Akhmedov and Zhuavskaya (2004), Brender and Drazen (2005), and Shi and Svensson (2006). See Drazen (2000) for a survey of the literature.

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Second, given the lack of disaggregated data on electricity provision, we use time series data on the emission of night lights as an indicator of electricity service provision. Using satellite imagery of night lights offers several advantages in our context. Given its high spatial resolution, estimates can be constructed for a variety of jurisdictional units. In addition, the data are automatically recorded, providing an objective and consistent measure resistant to human biases in reporting. Administrative data, in contrast, is often unreliable or poorly measured in developing countries.

Third, we employ a strategy that credibly identifies the effect of elections on government policy and economic outcomes, overcoming concerns in existing studies regarding the potential endogenous timing of elections. Our identification strategy exploits the exogenous timing of special (or bye-) elections in India's states — state assembly (*Vidhan Sabha*) elections that are held to fill seats that become vacant in between two general elections due to the death of a sitting incumbent. These special elections due to death are credibly exogenous to economic conditions and electricity supply since death is a natural phenomenon. Moreover, special elections take place in different constituencies at different times. Previous studies of electoral business cycles at the local level, for example Baleiras and Costa (2004), and Drazen and Eslava (2010), typically face the problem that elections are held in all localities at the same date, making it difficult to separately identify electoral cycles from other contemporaneous shocks.

Fourth, we explore whether the extent of manipulation depends on constituency or state-level political variables. One plausible hypothesis that follows from the theoretical literature on tactical redistribution is that state governments will target swing constituencies in their pursuit of re-election (Lindbeck and Weibull, 1987; Dixit and Londregan, 1998; Golden and Min, 2013). Similarly, manipulation should be stronger if the state government commands only a narrow majority in the legislature and seeks to increase its party strength. Given our data and empirical framework, we can explicitly test these hypotheses.

Overall, our research identifies significant increases in the provision of electricity in years in which a constituency holds a special election, which we interpret as efforts by political leaders to boost service provision and improve the economic climate prior to elections. We also find that manipulation is more pronounced in constituencies that are both closely contested (swing constituencies) and aligned with the state government. We observe furthermore that manipulation is more pronounced in states where the government holds only a weak majority. These findings are meaningful because they demonstrate a pathway by which politicians can engage in electorally-motivated manipulation of state resources even in contexts of limited policy flexibility and budget constraints.

Finally, our results indicate that the increase in electricity supply to special election constituencies is due to diversion from non-election constituencies rather than due to the creation of additional electricity. This observation and other pieces of evidence suggest that the overall welfare effects of manipulation around special elections are not positive and may even be negative.

A previous contribution close to our paper is Khemani (2004), who studies electoral business cycles in India at the state level using within-state variation in fiscal policy. The author finds that Indian state governments do not manipulate aggregate fiscal variables such as total spending or deficits in the run-up to an election, but that they manipulate individual budget items and public investment projects. Similarly, Cole (2009) observes electoral cycles in agricultural credit provided by public sector banks in India. Our paper differs from Khemani (2004) and Cole (2009) in that they use state-level and district-level data, respectively, while we use smaller assembly constituency-level data, which allows us to study electoral manipulation at a more disaggregated level and for politically relevant units in which elections are actually held. Min and Golden (2014) find electoral cycles in the incidence of electricity theft and line losses in the state of Uttar Pradesh. More specifically, the discrepancy between power supplied and billed increases in periods immediately prior to general elections. However, their study does not account for the potential endogeneity in the timing of elections.

The remainder of this paper is structured as follows. In the next section, we provide institutional background regarding electricity provision and elections in India, and discuss our main theoretical hypotheses. Sections 3 and 4 describe the data and introduce our empirical model. Section 5 discusses the main results. We examine how the competitiveness of an election affects manipulation before special elections in Section 6 and discuss welfare implications in Section 7. We conclude in Section 8.

2. Background

2.1. Politics of electricity in India

As in much of the developing world, India's power sector is primarily owned and managed by the state. The public sector controls about 90% of generation and almost all transmission and distribution in India (Lal, 2005).[3] Electricity provision is primarily a state-level responsibility, overseen by public power corporations that are led by political appointees and staffed by some 600,000 public employees.

Demand for electricity far outpaces available supply in much of India, resulting in regular and frequent power cuts, especially in rural areas. Many states provide so-called rostering schedules listing the times during which the power is scheduled to be shut off, though power cuts often exceed even these hours. In the World Bank Enterprise Survey of Indian businesses in 2006, a large fraction of firms (35%) cited access to reliable electricity as the number one obstacle facing their business.[4] Indian firms estimated losing 6.6% of sales as a result of power outages. Given that electricity is so important to social and economic welfare, access to electrical power is an important issue for voters. In a 2001–02 national survey of public attitudes, three-quarters of Indians ranked electricity as an important problem in their lives and 93% said governments were primarily responsible for electricity service provision (Chhibber et al., 2004).

Given the constraints on overall electricity supply and the political salience of electricity, state governments routinely intervene in the operation of state power utilities, from patronage transfers of employees, interventions in the selection of villages for electrification projects, and influence over the location and length of power outages. Min (2015) documents how power officials in Uttar Pradesh are pressured to meet requests for uninterrupted electricity supply from different political interests. Badiani et al. (2012) argue that parties court rich farmers by promising them cheap or even free electricity. Min and Golden (2014) further note that the agriculture sector in Uttar Pradesh receives preferential supply of electricity, resulting in increased losses at the utility and degraded service for other sectors. Further, reforms to alleviate problems with electricity provision have hit roadblocks as constituents are reluctant to pay increased power tariffs (Lal, 2005).

Because of the influence it has over the distribution of electrical power and the selection of villages for electrification projects, state governments can manipulate public service provision and can do so in electorally motivated ways. Yet estimating whether governments manipulate delivery of public services before elections is difficult because in India, as in many parliamentary settings, incumbent governments also exercise influence over the timing of elections. India's state legislatures (*Vidhan Sabhas*) are required to hold elections at least every five years. But an opportunistic government may resign early to force early

[3] The Electricity Act of 2003 sought to reform the power sector in India by introducing more competition in electricity provision by allowing entry of private firms. Nagavarapu and Sekhri (2014) and Allcot et al. (2014) note, however, that private entry into the sector, especially in distribution, has remained limited even after the reforms.

[4] World Bank Enterprise Survey, http://www.enterprisesurveys.org/ accessed in June 2014.

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elections when it thinks the winds are in its favor. Similarly, governments may try to delay elections as long as it is constitutionally possible. For instance, a government that is likely to lose a no-confidence motion may make an unexpected alliance with another party to sustain the government. Thus, the timing of elections is unlikely to be exogenous to economic conditions.

To credibly estimate the impact of elections on public service delivery, we examine how electricity provision varies due to exogenously scheduled special elections following the unexpected death of a legislator. Special elections are required to be scheduled within six months of any unplanned vacancy in India's legislatures.[5] A seat however is allowed to remain vacant until the next general election if the remaining term of the assembly is a year or less. There were 613 cases of special elections during our sample period between 1992–2009, of which 223 were due to the death of a legislator.[6] Special elections were also called due to resignation in 337 cases, and disqualification in 22 cases. We were unable to verify the reason for 27 special elections.

The outcome of a special election could affect the political calculus in important ways leading an incumbent government to manipulate the availability of electricity before the election. Special elections are perceived as an important test for the popularity of the incumbent government, especially for a government that enjoys a weak majority in the legislature, and are often labeled as litmus tests for the chief minister's leadership in the popular media. Further, losing badly in a special election might weaken the chief minister in his own party and, in turn, precipitate a decline in his or her authority or even energize rival factions within the party.[7]

2.2. Theories of political business cycles and tactical redistribution

Two prominent sets of theories explain electoral manipulation by incumbent governments: models of political business cycles and models of targeted distribution. The more recent theoretical contributions to the literature on political business cycles, rational expectation models, assume that voters, who are unsure about a politician's competence, believe that competent politicians can affect economic outcomes more than incompetent ones. Hence, competent politicians will want to reveal their type through electoral manipulation.

In view of this literature, we expect state governments to manipulate public service provision, such as electricity supply, leading up to a special election. A bump in electricity supply can showcase the ruling party's competence and reduce voter uncertainty about its ability to provide critical resources. In addition, manipulation by the state government can dispel uncertainty about the candidates' ability to acquire resources for their constituency, and in particular about their political influence with the state government.

Another major branch of the political economy literature is concerned with the targeted distribution of resources by governments (Cox and McCubbins, 1986; Lindbeck and Weibull, 1987; Dixit and Londregan, 1998). Since allocating resources to a given constituency has opportunity costs, state governments may favor constituencies where the political rewards are highest. One strand of this literature, also known as the patronage hypothesis, predicts that governments will allocate more resources to voters who strongly support the incumbent government, i. e. to core supporters. Another strand of the literature, the swing voter hypothesis, argues that the state government may target resources towards marginal constituencies where a small change in the vote share can make a large difference in the electoral outcome.

Our paper allows us to test these competing predictions in the context of special elections. If state governments direct resources towards swing constituencies, manipulation before special elections should be more pronounced in swing than in safe (or core) constituencies. Moreover, we further distinguish between swing and core constituencies that are aligned or unaligned with the ruling party or coalition. Since the government seeks to resolve voters' uncertainty about its competence, the incentive to manipulate is greater where its efforts can be more clearly attributed to the state government. Thus, we expect manipulation to be more salient in constituencies that are both swing and aligned with the state government. When a special election constituency is aligned with the opposition, there is additional uncertainty about whom should be given credit for improved public service provision as voters may interpret a bump before a special election as a signal about the opposition's competence.

For a ruling government, the expected payoffs from manipulation depend not only on the probability of winning an election but also on the benefits that flow from the strength of the government majority. Since governments with slim majorities face greater uncertainty about retaining power, we expect that they should expend more effort at securing victory in special elections. We therefore test if manipulation before special elections is stronger in states in which the government commands a smaller seat majority.

3. Data

3.1. Night lights as proxy for electricity consumption

Building on a large body of research, we estimate changes in access to electricity service by analyzing satellite imagery of the earth at night and recording the level of light output annually from 1992 to 2009.[8] These images record average light output at the 30 arcsecond level, equivalent to about 1 km^2 at the equator. We aggregate light output to the state assembly constituency level to examine the effect of electoral cycles on electricity provision.

We create three different constituency-level variables in each year. The first measure is *Light Output Per Capita*, computed as the sum of light values from all pixels within the boundaries of each constituency divided by the number of registered voters as reported by the Election Commission. Since variation in light output is correlated with electricity service provision, this measure tracks the availability and use of electricity within a constituency, a political unit for which few economic indicators are reported. To help account for the skewed distribution of light output towards lower values, we use the natural logarithm of light output per capita.[9] The second measure is *Growth of Per Capita Light*, which is computed by taking the difference of the log light output per capita in the current year and in the previous year. The third measure is the *Proportion of Lit Villages* within a constituency, which is the proportion of villages with detectable levels of light output in a given year. We consider this measure a useful alternative way of quantifying the breadth of

[5] The exact timing of special elections within that window of six months depends on various logistical issues and other factors such as weather, the agricultural cycle, school examination schedules, and religious festivals and public holidays. See http://www.eci.nic.in/eci_main1/the_function.aspx#whendoelections accessed on May 15, 2013.

[6] The Election Commission's files do not indicate cause of death. In order to affirm exogeneity of death from economic factors, we researched a large sample of cases and confirmed that almost all were due to natural causes. In our online searches, we did find that 6 legislators were murdered. However, these cases were rare and unrelated to our outcome variables nonetheless.

[7] It is not unusual to find multiple changes in chief ministerships during the reign of the same party in a single term. See for example http://articles.economictimes.indiatimes.com/2014–08-18/news/52941982_1_chief-minister-anandi-patel-prime-minister-narendra-modi-the-bjp.

[8] The nighttime satellite imagery comes from the U.S. Air Force Defense Meteorological Satellite Program's Operational Linescan System (DMSP-OLS) and are processed by the National Oceanic and Atmospheric Agency's (NOAA) National Geophysical Data Center (NGDC). Due to space constraints, please refer to Section A in the Online Appendix for details of the light data.

[9] Since some constituencies emit no light above the threshold of detection, we add a constant of 1 before taking the natural logarithm. Other alternatives, such as adding a constant less than the minimum value of the light variable, do not change our substantive results.

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access to electricity within a constituency compared to measures based on the level of light output alone.

Our data span all states in India with the exception of the northern state of Jammu and Kashmir and the smaller union territories, totaling close to 4000 constituencies over the period from 1992 to 2009.[10] In all, we have about 68,500 constituency–year observations over this period. Our analysis below focuses on special elections following death of a legislator, which we consider are credibly exogenous to electricity supply and economic conditions.

3.2. Event study plots

For a first impression of how electricity provision varies around special elections, we construct event study plots for the three outcome variables. The top panels in Fig. 1 plot the raw averages of our three light variables in constituencies that experienced a special election and those that did not in the three years before and after a special election, whereas the lower panels show the differences in the raw averages.[11]

The event plots in Panels (a) and (d) suggest that light output in special election constituencies is lower than in non-election constituencies in the pre-election period. In the special election year, there is a steep increase in light output in special election constituencies with light output jumping to the level in non-election constituencies. In non-election constituencies, in contrast, there is a small dip. In the post-election period, both series follow similar trajectories. Panels (b) and (e) show that the growth rates of light output in special election and non-election constituencies are similar in the pre-election period, but jump in special election constituencies during the election year.

Finally, Panels (c) and (f) show that the proportion of lit villages increases in special election constituencies in the election year but not in non-election constituencies. In the first post-election period, the proportion of lit villages in special election constituencies drops to the level in non-election constituencies. There is also some divergence in both series after the second post-election year.

Overall, the event study plots suggest that constituencies holding special elections experience a substantial increase in electricity service provision. We interpret this as evidence of electorally-motivated efforts by the state government to boost public service delivery. The patterns further suggest, as we discuss below, that this increase is likely due to diversion from other areas in which elections are not being contested, rather than due to the generation of additional electricity.[12]

4. Empirical model

To quantitatively establish the effect of special elections on light output, we specify the following model:

$$Y_{ist} = \alpha_i + \gamma_t + \theta_s \times t + \beta \times \text{Special election}_{ist} + \delta \times X_{ist-1} + \mu_{ist}. \quad (1)$$

where Y_{ist} is one of the three light output measures (*Log Light Output Per Capita*, *Growth of Light Per Capita*, *Proportion of Lit Villages*) for constituency i in state s in year t.

Special election$_{ist}$ is an indicator variable capturing the incidence of a special election taking place after the death of an incumbent. The parameter α_i denotes constituency fixed effects and accounts for time-invariant constituency-specific factors that may affect night lights, such as distance to power stations or attitudes regarding electricity consumption. γ_t denotes year fixed effects that account for secular trends in light output that are common to all constituencies, such as changes in electricity production technology, satellite technology and so on. We also include state-specific linear time trends $\theta_s \times t$ to account for secular trends in light output across different states and for other state-level changes.

X_{ist-1} are lagged values of time-variant constituency-specific control variables. These variables include the following dummy variables: *State government constituency*, which is 1 if a constituency is represented by the ruling party or a party in the ruling coalition and 0 otherwise; *Central government constituency*, which is 1 if a constituency is represented by the national ruling party or a party in the national ruling coalition and 0 otherwise; and *Central and state government constituency*, which is 1 if a constituency is represented by a party in the ruling coalition at both the central and state government levels and 0 otherwise. We also control for the number of registered voters (ln(*Electorate Size*)), and percentage voter turnout (ln(*Turnout*)). We also use indicator variables for the presence of a *Coalition government* and for *Female legislators*. The reason for using lagged values is that most of the above variables are likely to be simultaneously determined with special elections.

In this setup, identification comes from within constituency variation in our outcome variables. We thus compare the change in the outcome variables in special election constituencies in the year of the special election with the change in the outcome variables in non-special election constituencies after partialling out the effects of common time trends, state-specific linear time trends, and constituency-specific time-varying covariates. The implicit assumption here is that treatment (special election) is independent of unobserved variables, conditional on the observed covariates, fixed effects, and state-specific trends. This is the standard selection-on-observables assumption. Furthermore, in the presence of serial correlation, ordinary least squares (OLS) standard errors may be severely understated. This is true in our case because light output in a constituency is correlated over time. We therefore cluster standard errors at the constituency level.

5. Results

5.1. Quasi-randomness of special elections

As discussed above, our identification strategy depends on the exogenous timing of special elections after death. We check the quasi-exogeneity assumption by relating special elections to pre-treatment characteristics of constituencies. Since these variables are supposed to have been determined before treatment assignment, any evidence of a relationship would cast doubt on the exogeneity of special elections. Columns (1)–(2) of Table 1 report mean and standard deviation of the light variables and the predetermined covariates, mainly the lagged values of the independent variables, in special and non-special election constituencies. Column (3) tests whether the differences in means between the two groups are significant. Additionally, in column (4) we report the results from a regression of the pre-determined covariates on the special election dummy after partialling out constituency and year fixed effects, state-specific time trends and other covariates. The column is populated with the coefficient estimates for the special election dummy in each regression.

Neither the lagged values of covariates nor the within-variation in the lagged values differ significantly between constituencies in which special elections are held after the death of a legislator and the

[10] The geographic boundaries of state assembly constituencies remained unchanged from 1976 till 2008. For 2008 and 2009, our analysis includes only elections held before new boundaries took effect.

[11] The event study plots for special election constituencies trace the unweighted average of each light measure around the election year within the group of special election constituencies. For non-election constituencies, there are no obvious pre- and post-election periods. We therefore construct synthetic averages for non-election constituencies by matching to each special election constituency the average of the relevant light measure in all non-election constituencies in a given year, and then collapsing this average across all special election constituencies. A more detailed description of our approach is available in Section B in the Online Appendix.

[12] In Panels (a) and (c), there is some indication that the increase in electricity around elections persists into the post-election period. However, we find in Table C-6 in the Appendix that this persistence is limited to constituencies that would hold a state-wide election in the near future.

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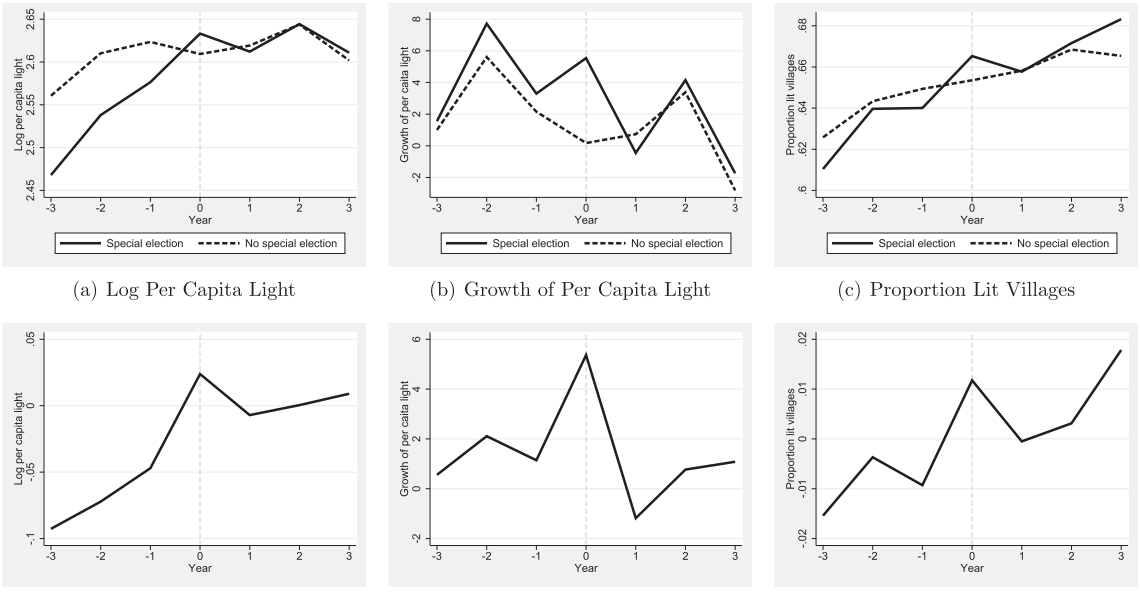

(a) Log Per Capita Light (b) Growth of Per Capita Light (c) Proportion Lit Villages

(d) Difference in Log Per Capita Light (e) Difference in Growth of Per Capita Light (f) Difference in Proportion Lit Villages

Fig. 1. Event study plots.

Table 1
Descriptive statistics: special elections vs non-special election years.

	(1)	(2)	(3)	(4)
	Mean		Difference	FE coefficient
	Special Election = 1	Special Election = 0		
Panel A: Main dependent variables				
Log per capita light$_t$	2.68	2.64	0.040	
	[1.08]	[1.09]	[0.077]	
Growth of per capita light$_t$ (%)	6.15	2.71	3.44	
	[38.6]	[35.3]	[2.49]	
Proportion of lit villages$_t$	0.66	0.65	0.011	
	[0.36]	[0.36]	[0.026]	
Panel B: All predetermined variables				
Log per capita light$_{t-1}$	2.62	2.61	0.0057	−0.008
	[1.09]	[1.10]	[0.077]	[0.019]
Log electorate size$_{t-1}$	11.8	11.7	0.030	0.000
	[0.77]	[0.80]	[0.056]	[0.005]
Log turnout$_{t-1}$	4.19	4.19	0.0052	0.001
	[0.22]	[0.24]	[0.017]	[0.009]
Margin$_{t-1}$	12.4	13.0	−0.52	−1.079
	[11.7]	[11.8]	[0.83]	[0.678]
State govt. constituency$_{t-1}$	0.60	0.61	−0.0094	0.004
	[0.49]	[0.49]	[0.034]	[0.024]
Coalition government$_{t-1}$	0.59	0.54	0.047	0.023
	[0.49]	[0.50]	[0.035]	[0.017]
Central govt. constituency$_{t-1}$	0.39	0.36	0.033	0.006
	[0.49]	[0.48]	[0.034]	[0.019]
Central and state govt. constituency$_{t-1}$	0.26	0.24	0.016	−0.008
	[0.44]	[0.43]	[0.030]	[0.015]
Female legislator$_{t-1}$	0.025	0.056	−0.031*	−0.034***
	[0.16]	[0.23]	[0.016]	[0.009]
Observations	223	68,306		68,045

Columns (1) and (2) report means and standard deviations of special elections (Special Election = 1) and other years (Special Election = 0). Column (3) reports the difference in the means of special elections and other years. Column (4) reports the coefficient on a dummy for special elections from a fixed effect regression of each predetermined covariate on the special election dummy, other covariates, year fixed effects and state-specific time trends. The values with *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively. Per capita light is light per 1000 registered voters.

remaining constituencies except for the gender of the legislator who died.[13] These checks suggest that there are no systematic differences between constituencies that hold special elections after death of a legislator and other constituencies, and imply that special elections due to death are indeed quasi-random. We exploit this quasi-random variation in occurrence of an election to identify the causal effect of an election on light output in a constituency.[14]

5.2. Baseline results

Table 2 reports the first set of results, which we treat as our baseline. In these regressions, we relate special elections held after death of a current legislator to the three outcome measures.[15] The results in column (1) suggest that per capita light output in constituencies that hold a special election is significantly higher than in other constituencies, on average 4% higher in special election years than otherwise. Column (2) adds the additional political control variables. While control variables are in principle not necessary to obtain consistent causal estimates if special elections are quasi-random, it is nonetheless useful to include them since they improve efficiency. The inclusion of pre-determined control variables may also serve as an informal test of our identification strategy. Following (Altonji et al., 2005), the argument is that if the inclusion of observable control variables does not significantly change the estimated coefficients for the variable of interest, it is unlikely that the estimates are biased due to omitted and possibly unobservable variables. We indeed find that the estimated coefficient is almost identical to the estimates in column (1).

[13] Constituencies that are held by a female legislator are less likely to have special elections due to the death of a legislator. This may, however, reflect the natural discrepancy in life expectancy of males and females.

[14] We have also tested for differences in constituencies that ever held a special election due to death and constituencies that never held a special election for a battery of characteristics that are available for each village in the 1991 census. Overall, the differences between special election after death and non-election constituencies are insignificant. We report these results in Table C-1 in the Online Appendix.

[15] Table C-2 in the Online Appendix provides comparable results for all special elections including those due to resignation and other causes.

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Table 2
Special elections and electricity provision.

	(1)	(2)	(3)	(4)	(5)	(6)
	Log Per Capita Light		Growth of Per Capita Light		Proportion of Lit Villages	
Special election	0.040**	0.041**	4.973**	4.758**	0.011	0.011*
	[0.017]	[0.018]	[2.233]	[2.257]	[0.006]	[0.006]
Log electorate size$_{t-1}$		−0.505***		16.856***		−0.040***
		[0.061]		[2.502]		[0.011]
Log turnout$_{t-1}$		−0.020		1.120		−0.014***
		[0.016]		[0.819]		[0.004]
State govt. constituency$_{t-1}$		−0.015***		−0.295		−0.002
		[0.005]		[0.262]		[0.002]
Central govt. constituency$_{t-1}$		−0.031***		−1.457***		−0.008***
		[0.005]		[0.324]		[0.002]
Central and state govt. constituency$_{t-1}$		0.043***		2.493***		0.010***
		[0.006]		[0.418]		[0.003]
Coalition government$_{t-1}$		−0.012***		3.593***		−0.005***
		[0.004]		[0.353]		[0.002]
Female legislator$_{t-1}$		−0.026***		−0.342		−0.013***
		[0.010]		[0.407]		[0.005]
R^2	0.37	0.39	0.36	0.37	0.34	0.35
N	68,529	67,962	64,358	64,055	65,123	64,579
Method	FE	FE	FE	FE	FE	FE

Log Per Capita Light is the natural log of total light output divided by the size of electorate. Growth of Per Capita Light is the annual growth rate of per capita light. Proportion of Lit Villages is the proportion of villages in a constituency that have positive light output. Special Election is 1 for years in which a special election is held to fill a vacancy after the death of a legislator and 0 otherwise. All control variables are lagged by one period. All regressions include year fixed effects and state-specific time trends. Standard errors are clustered at the constituency level and given in parentheses. The values with *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Column (3) relates the special election dummy to the annual growth rate of per capita light output. The estimates suggest a significant and positive effect of special elections. The growth rate of light output is, on average, 5 percentage points higher for a constituency in the special election year. Column (4) adds the political controls. The estimate for the special election dummy is again almost identical to the estimate in the model without control variables.

Column (5) relates the third output measure, the proportion of lit villages, to special elections.[16] The estimated coefficient is positive, even though it is not significant. However, once we add additional control variables in column (6), we observe about 1 percentage point increase in the proportion of lit villages during special elections and the increase is significant at the 10% level.

Overall, these estimates confirm the findings in the event study plots. They suggest that state governments induce electoral cycles in electricity provision during special elections. We conjecture that the bump in electricity supply is a way for ruling parties and their candidates to signal competence. The evidence suggests that the state government applies effort to reduce outages and to provide additional electricity to villages that were unlit in the pre-electoral period, either of its own accord or at the insistence of influential candidates.

Some control variables also have a significant effect on the outcome measures, though we should be cautious with a causal interpretation of these results. Seats with larger electorates seem to have lower levels of light output and fewer lit villages, but higher growth rates in light output. These findings may be related to our normalization of the level of light output by the size of the electorate. Turnout in the last election is negatively related to the proportion of lit villages. Constituencies with a legislator aligned with both the state *and* the central government display higher values for the output measures. On the other hand, constituencies aligned with only one of the two higher tiers of government have lower values for the outcome measures. Finally, constituencies with female legislators tend to have lower outcome measures.

5.3. Robustness

5.3.1. Redefined special election dummies

A problematic aspect of the baseline specification is that the special election dummy is set to one in the year of the special election, irrespective of whether the election took place early or late in the calendar year. If a special election is held early in the year, governments might have already begun to influence electricity provision in the year preceding the special election.

To check for sensitivity of our results, we estimate three sets of regressions in which we treat the timing of special elections differently than in the baseline models. Columns (1), (4), and (7) of Table 3 present regressions for each of the three outcome variables where the special election dummy is set to one only for those elections that are held in the second half of the year. We know that special elections held in the second half of a year were the result of a vacancy in the same year due to the constitutional requirement of filling the vacancy within 6 months. Given this definition of special elections, the effect of special elections is consistent with what we find above. Light outcomes, on average, are larger in special election years than other years.

Columns (2), (5), and (8) of Table 3 present regressions where the special election dummy is set to 1 for those special elections held after January. For special elections held in January, the previous year is coded as the special election year.[17] While this is a more conservative recoding of the special election dummy than the previous one, the estimates are again in line with the baseline results. The special election dummy is consistently positive and significant.

Finally, columns (3), (6), and (9) of Table 3 present regressions where the special election variable is defined to be 0 if a special election was held in January, 1 if it was held in February, and so on until July, where it assumes a value of 6. From July onward, the variable remains at 6. The idea is that if the special election was held in January, none of the manipulation should have taken place in the special election year; if the special election was held in February, there was at least one month where manipulation could have taken place in the special election year. This redefined special election variable remains at 6

[16] There are a few constituencies that are completely urbanized and contain no villages. Given the definition of this outcome, samples in columns (5) and (6) do not include such constituencies.

[17] The results are insensitive to the choice of the month and available upon request.

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Table 3
Special elections and electricity provision: alternative definitions of special elections.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Log Per Capita Light			Growth of Per Capita Light			Proportion of Lit Villages		
Special Election Alternative I	0.061**			3.942			0.017*		
	[0.030]			[5.036]			[0.010]		
Special Election Alternative II		0.039**			4.627**			0.012*	
		[0.018]			[2.249]			[0.006]	
Special Election Alternative III			0.012***			0.981*			0.004**
			[0.004]			[0.594]			[0.001]
Log electorate size$_{t-1}$	−0.504***	−0.504***	−0.504***	16.854***	16.856***	16.856***	−0.040***	−0.040***	−0.040***
	[0.061]	[0.061]	[0.061]	[2.502]	[2.502]	[2.502]	[0.011]	[0.011]	[0.011]
Log turnout$_{t-1}$	−0.020	−0.020	−0.020	1.122	1.120	1.122	−0.014***	−0.014***	−0.014***
	[0.016]	[0.016]	[0.016]	[0.819]	[0.819]	[0.819]	[0.004]	[0.004]	[0.004]
State govt. constituency$_{t-1}$	−0.015***	−0.015***	−0.015***	−0.298	−0.294	−0.297	−0.002	−0.002	−0.002
	[0.005]	[0.005]	[0.005]	[0.262]	[0.262]	[0.262]	[0.002]	[0.002]	[0.002]
Central govt. constituency$_{t-1}$	−0.031***	−0.031***	−0.031***	−1.455***	−1.455***	−1.456***	−0.008***	−0.008***	−0.008***
	[0.005]	[0.005]	[0.005]	[0.324]	[0.324]	[0.324]	[0.002]	[0.002]	[0.002]
Central and State Govt.	0.042***	0.042***	0.042***	2.490***	2.491***	2.494***	0.009***	0.009***	0.009***
Constituency$_{t-1}$	[0.006]	[0.006]	[0.006]	[0.418]	[0.418]	[0.417]	[0.003]	[0.003]	[0.003]
Coalition government$_{t-1}$	−0.011**	−0.011**	−0.011**	3.596***	3.593***	3.592***	−0.005***	−0.005***	−0.005***
	[0.004]	[0.004]	[0.004]	[0.353]	[0.353]	[0.353]	[0.002]	[0.002]	[0.002]
Female legislator$_{t-1}$	−0.026***	−0.026***	−0.026***	−0.357	−0.343	−0.348	−0.013***	−0.013***	−0.013***
	[0.010]	[0.010]	[0.010]	[0.407]	[0.407]	[0.407]	[0.005]	[0.005]	[0.005]
R^2	0.39	0.39	0.39	0.37	0.37	0.37	0.35	0.35	0.35
N	67,791	67,791	67,791	64,055	64,055	64,055	64,408	64,408	64,408
Method	FE	FE	FE	FE	FE	FE	FE	FE	FE

Log Per Capita Light is the natural log of total light output divided by the size of electorate. Growth of Per Capita Light is the annual growth rate of per capita light. Proportion of Lit Villages is the proportion of villages in a constituency that have positive light output. Special Election Alternative I is 1 for years in which a special election is held to fill a vacancy after the death of a legislator in July or later and 0 if earlier. Special Election Alternative II counts a special election in January as held in the previous year. Special Election Alternative III takes a value of 0 if the election is in January, 1 if in February, 2 if in March, 3 if in April, 4 if in May, 5 if in June and 6 if election is held in July or later. All control variables are lagged by one period. All regressions include year fixed effects and state-specific time trends. Standard errors are clustered at the constituency level and given in parentheses. The values with *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

even after July because of the constitutional requirement that special elections must be held within six months.

The results for this specification, too, are in line with both the baseline results and those from the other redefinitions. While the magnitude of the estimates is naturally different as we now use a count variable to indicate special elections, they are significantly positive for all outcome variables. An additional month of campaign time within the special election year increases light output in that year by about 1 percent. Similarly, an additional month of campaign time increases the growth of light output by about 1 percentage point and the proportion of lit villages by about 4 percentage points.

5.3.2. Neighbor sample

Another robustness check for the baseline estimates is to compare special election constituencies with geographically neighboring constituencies that did not have a special election. If the bump observed during special elections is due to some unobserved effect correlated with the timing of special elections, it is plausible that this unobserved effect will affect the neighbors of special election constituencies as well. Thus, if we observe a similar increase in light output in neighboring constituencies that do not hold a special election, we would question our claim that manipulation is electorally motivated.

We examine this in columns (1)–(3) of Table 4, where our sample is restricted to special elections constituencies and their neighbors that share a border over the sample period. The results suggest that per capita light output is on average about 4% higher in constituencies that had a special election than in neighboring constituencies. Similarly, growth in per capita light output in special election constituencies compared to their neighboring constituencies is about 4% higher. Finally, the estimate for lit villages is also of the same magnitude as in the baseline estimates. These tests provide further evidence that constituencies that hold a special election experience increased electricity provision due to elections alone.

5.3.3. Special election constituencies sample

Yet another check is to restrict the sample to only constituencies that ever had a special election. If special election constituencies are not significantly different from non-election constituencies, i. e. if special

Table 4
Special elections and electricity provision: alternative samples.

	(1)	(2)	(3)	(4)	(5)	(6)
	Neighbor sample			Special election constituencies only		
	Log per capita light	Growth of per capita light	Proportion of lit villages	Log per capita light	Growth of per capita light	Proportion of lit villages
Special election	0.042**	4.702**	0.011*	0.041**	4.764**	0.011*
	[0.018]	[2.254]	[0.006]	[0.017]	[2.293]	[0.006]
Controls	X	X	X	X	X	X
R^2	0.38	0.36	0.34	0.40	0.36	0.35
N	19,671	18,563	18,607	3630	3425	3371
Method	FE	FE	FE	FE	FE	FE

In columns (1)–(3), the neighbor sample consists of constituencies with special elections and constituencies that border them. In columns (4)–(6), we consider only constituencies that ever had a special election. All control variables are included and lagged by one year. All regressions include year fixed effects and state-specific time trends. Standard errors are clustered at the constituency level and given in parentheses. The values with *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

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elections are indeed quasi-random, the estimated coefficients for the special election dummy should be similar to those in the baseline models. We report such regression results in columns (4)–(6) of Table 4. The estimated coefficients are indeed similar to those in the baseline regressions. Light output in special election years is about 4% higher, the growth rate is about 4 percentage points higher, and the proportion of lit villages increases by 1 percentage point.

6. Manipulation and targeted redistribution

The previous results imply that, on average, state politicians induce electoral cycles during special elections. However, it is plausible that the incentives of the state government for electoral manipulation depend on the political characteristics of a constituency. In particular, state governments might focus on improving electricity supply to swing constituencies where the probability of swaying the election through manipulation is high. We hence examine in Table 5 whether special election constituencies that were more closely contested in the last general election witness a relatively larger increase in light output.

Pure swing voter models would predict that all swing constituencies will receive the same amount of electricity from state governments. However, as discussed above, this prediction should be qualified since some seats are represented by members of opposition parties. Hence, state governments may only target aligned swing constituencies in which the previous MLA belonged to the ruling party or coalition as manipulation in these constituencies can be clearly attributed to the ruling party.

We define dummy variables to identify both swing constituencies and constituencies that are aligned with the ruling party or coalition. The variable *Special election <= 5% and Ruling Party* is 1 for special elections that have a margin of victory of 5% or less in the previous election and are aligned with the ruling party or coalition and 0 otherwise. This dummy identifies constituencies that were closely won by the incumbent party in the previous election. *Special elections > 5%* and *Ruling Party* is 1 for special elections that have a margin of victory of greater than 5% in the previous election and are aligned with the ruling party or coalition and 0 otherwise. This dummy identifies constituencies that were won more comfortably by the incumbent party in the previous election. *Special election <= 5%* and *Non-ruling Party* and *Special elections > 5%* and *Non-ruling party* are constructed accordingly.

The results in columns (1)–(3) show that the effect on light output is much larger in magnitude and statistically significant if special elections are held in constituencies that are both aligned and swing. While Wald tests for the equality of the coefficients on close aligned and non-close aligned constituencies are insignificant for per capita light output and growth of light output, the Wald test for proportion of lit villages is significant and the one for per capita light output displays a relatively low p-value. The results hence suggest that state governments only manipulate light output in constituencies that are both swing and aligned. These findings are consistent with (Cole, 2009), who also finds that manipulation of agricultural loans before elections is targeted towards

Table 5
Special elections and electricity provision: targeted manipulation.

	(1)	(2)	(3)	(4)	(5)	(6)
	Close elections			Weak majority		
	Log per capita light	Growth of per capita light	Proportion of lit villages	Log per capita light	Growth of per capita light	Proportion of lit villages
Election <= 5% Margin and Ruling party	0.105**	6.366*	0.041***			
	[0.044]	[3.726]	[0.013]			
Election > 5% Margin and Ruling party	0.032	3.667	0.008			
	[0.027]	[4.113]	[0.010]			
Election <= 5% Margin and Non-ruling party	0.037	6.615	0.001			
	[0.053]	[5.398]	[0.019]			
Election > 5% Margin and Non-ruling party	0.017	4.581	0.007			
	[0.027]	[3.483]	[0.012]			
Election with Seat margin < = 5%				0.085***	11.866***	0.020**
				[0.029]	[3.381]	[0.010]
Election with 5% < Seat margin <= 10%				0.006	−2.268	−0.002
				[0.047]	[4.692]	[0.016]
Election with 10% < Seat margin < = 15%				0.014	4.390	−0.005
				[0.049]	[6.302]	[0.016]
Election with 15% < Seat margin				0.017	0.542	0.016
				[0.028]	[4.137]	[0.011]
Margin of victory	−0.000**	−0.002	0.000			
	[0.000]	[0.011]	[0.000]			
Government's Seat Margin				−0.002***	−0.090***	−0.001***
				[0.000]	[0.017]	[0.000]
Controls	X	X	X	X	X	X
Method	FE	FE	FE	FE	FE	FE
Wald p-value	0.16	0.63	0.04	0.28	0.05	0.46
R^2	0.39	0.37	0.35	0.39	0.37	0.35
N	67,791	64,055	64,408	67,444	63,768	64,083

Election <= 5% Margin and Ruling party is 1 for a special election constituency if the margin of victory in the last election was within 5% and if it was represented by the ruling party or coalition and 0 otherwise. *Election > 5% Margin and Ruling party* is 1 for a special election constituency if the margin of victory in the last election was above 5% and if it was represented by the ruling party or coalition and 0 otherwise. *Election <= 5% margin and non-ruling party* is 1 for a special election constituency if the margin of victory in the last election was within 5% and if it was not represented by the ruling party or coalition and 0 otherwise. *Election > 5% margin and non-ruling party* is 1 for a special election constituency if the margin of victory in the last election was above 5% and if it was not represented by the ruling party or coalition and 0 otherwise. *Margin of victory* is difference between vote shares of the winner and runner-up party in the previous election. *Government's Seat Margin* is equal to (*seats*−50%), where seats is the percentage of seats won by the ruling party or coalition. Election with Seat Margin < = 5% is 1 for special elections in which Government's Seat Margin is less than 5% and 0 otherwise. Election with 5% < seat margin <= 10% is 1 for special elections in which Government's Seat Margin is between 5% and 10% and 0 otherwise. Election with 10% < seat margin < = 15% is 1 for special elections in which Government's Seat Margin is between 10% and 15% and 0 otherwise. Election with 15% < = seat margin is 1 for special elections in which Government's Seat Margin is greater than 15% and 0 otherwise. The Wald p-values are from H_0: Election <= 5% Margin and Ruling party = Election > 5% Margin and Ruling party in columns (1)–(3) and from H_0: Election with Seat Margin <= 5% = Election with 5% < Seat Margin < = 10% = Election with 10% < Seat Margin <= 15% = Election with 15% < Seat Margin in columns (4)–(6). All other control variables are included and are lagged by one year. Standard errors are clustered at the constituency level and given in parentheses. The values with *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

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districts that are more closely contested. Similarly, we find no evidence of a patronage effect whereby state governments reward constituencies where it enjoys strong support.[18]

Another aspect of competitiveness that we explore is whether the state government enjoys a safe majority in the legislature. It is plausible that the majority party or coalition has stronger incentives to manipulate public service delivery to key constituencies if it enjoys only a fragile majority.

Conversely, states in which the government's majority is large may not have the extra motivation to increase its majority, all else equal. To explore this issue, we calculate the *Government's Seat Margin* as (*Seats* − 50), where *Seats* is the percentage of seats in the legislature that the ruling party or coalition won in the last election. Given that a state government needs at least 50% of seats to rule, this measure reflects the strength of a government's majority. We create a dummy variable which is one if a constituency holds a special election and the state government's seat margin is less than or equal to 5% (Election with Seat Margin < = 5%) and 0 otherwise. We create three more dummy variables in similar fashion: Election with 5% < Seat Margin < = 10%; Election with 10% < Seat Margin < = 15%; and Election with Seat Margin > 15%.

The results are reported in columns (4)–(6) of Table 5. The light output variables are significantly and inversely related to the size of a government's majority suggesting that governments with a larger majority supply less electricity on average. The light variables are larger in magnitude and statistically significant only for those special elections in which a state government has a narrow majority. While the Wald test for the equality of the coefficients on the dummy variables for different majority levels is only significant for one output measure, growth of light output, the results suggest that the state government only engages in manipulation before special elections if it commands a narrow majority in the Assembly.[19]

7. Welfare implications

The previous results suggest that the state government increases the provision of electricity supply to constituencies as a result of special elections. What remains unclear is whether this manipulation has broader welfare implications. Is the increase in light output in special election constituencies a result of an overall increase in electricity supply in the state, or is it simply a diversion of electricity from other areas into the seat? If the increase in special election constituencies is due to diversion of power and a decrease of electricity supply in other areas, then the net welfare effect of manipulation is unlikely to be positive.

Several pieces of evidence suggest that the increase in special election constituencies is due to diversion rather than an increase in power supply. First, adding power generating capacity is a costly time-consuming endeavor, and many Indian states have struggled to increase their power supply. For example, in Uttar Pradesh, power generation totaled 21 terawatt-hours in 2010, a figure no higher than it was in 1995. During that period, numerous power plant proposals have stalled due to environmental protests and other political obstacles.

Second, we run regressions to explore manipulation of electricity output during general elections to evaluate the ability of the state government to generate additional electricity during elections. If electricity supply does not increase in general election years, it would suggest that state governments lack the resources to create additional electricity during election periods and may be similarly constrained in special election years. The results are presented in columns (1)–(3) of Table 6. To identify the effect of general elections in these regressions, we use the identification strategy of Khemani (2004) and Cole (2009) by instrumenting the general election dummy with a dummy for scheduled general elections. The results suggest that governments are unable to increase light output during general elections and any manipulation would likely involve a redistribution of existing supply.[20]

Third, the event study plots in Fig. 1 show a small but noticeable dip in non-election constituencies for log light and growth of light in the special election year. These dips also suggest that the bump in light output in special election constituencies is due to redistribution of power from other areas. The event study plots for the levels of light output indicate that manipulation persists in the post-election period. Given the constraints on overall supply, this persistence may come at the expense of other constituencies.[21]

To further explore whether increases in electricity supply result in increased economic output, we run regressions where we relate GDP at the district level — which is the smallest administrative unit for which GDP data is available — to the incidence of special elections in constituencies located within a given district's boundaries. As Table 4 suggests that the neighboring constituencies do not experience supply disruptions and electricity is diverted from the state at large, any increases in light output should show up in district level GDP measure if it has positive effects on economic output. We analyze 587 districts for which we have GDP data from 2001–2007 out of a total of 640. We consider three district-level output measures: total district GDP, per capita GDP, and per worker GDP and present the results in columns (4)–(6) of Table 6. We find no evidence of increased economic output as a result of special elections on any of these production measures.

Overall, our results are consistent with Cole (2009) who finds that increases in short-term agricultural loans prior to elections have no discernible effect on agricultural yield or output in Indian states. The implication is that such loans are inefficient and reflect political distortions designed to target votes rather than to actually improve agricultural output. Similarly, electorally motivated increases in electricity supply may not be enough to boost production in the short run. Businesses and firms rely on stable and predictable electrical power for production, and increases in the hours of service may not sway businesses to abandon generators, time-shifting of workers, and other strategies to cope with the unpredictable nature of electrical power supply. Instead, the election-induced increases in electricity supply are likely to result in increased consumption, primarily in the residential and agricultural sectors (Min and Golden, 2014).

8. Conclusion

We examine in this paper whether Indian state politicians induce electoral cycles in electricity provision, a critical input for economic activity and crucial determinant of welfare. Our results provide strong evidence that electricity supply increases in constituencies during exogenous special elections held due to the death of a legislator. In addition, we also uncover significant interactions between the prevailing political climate in a state and the state government's incentives to manipulate electricity supply during special elections. The state government

[18] We get similar results when we define a core constituency by the number of years a candidate (or party) has held a constituency. These results are reported in Table C-5 in the Online Appendix.

[19] We also find that special elections that are held earlier in a term have a larger effect on light output than special elections held later in the term (i. e. closer to a state-wide general election). This may be because newer state governments are more motivated to demonstrate competence in special elections. We report these results in columns (1)–(3) of Table C-4 in the Online Appendix.

[20] Given that there are no overall effects in general elections and that there are added constraints on manipulative redistribution in general elections, the effect of special elections is likely to be an upper bound to the effect of general elections.

[21] Standard models of the political business cycle do not predict persistence, but there are plausible explanations for persistently higher electricity supply in the post-election period in our context. In particular, the bump in electricity supply may persist if there are political costs to reversing the pre-election increase. This could be true for constituencies that will again go to election soon. In line with this idea, we find in Table C-6 in the Online Appendix that the bump in electricity supply only persists for special elections that are held close to a general election.

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Table 6
Elections and electricity provision: redistribution and welfare effects.

	(1)	(2)	(3)	(4)	(5)	(6)
	State-wide general elections			District-level GDP measures		
	Log per capita light	Growth of per capita light	Proportion of lit villages	Log GDP	Log GDP per capita	Log GDP per worker
Election year	−0.021	−5.227*	0.014			
	[0.019]	[2.891]	[0.010]			
Special election				0.010	0.012	0.008
				[0.007]	[0.008]	[0.008]
Log electorate size$_{t-1}$	−0.529***	9.838**	−0.021	0.026	0.012	0.026
	[0.101]	[4.950]	[0.028]	[0.018]	[0.016]	[0.017]
Log turnout$_{t-1}$	−0.025	−0.199	−0.011	−0.173***	−0.180***	−0.055
	[0.029]	[3.547]	[0.010]	[0.032]	[0.032]	[0.035]
State govt. constituency$_{t-1}$	−0.015	−0.240	−0.002	0.006	0.004	0.024**
	[0.012]	[0.459]	[0.005]	[0.010]	[0.010]	[0.010]
Central govt. constituency$_{t-1}$	−0.031**	−1.653**	−0.007	−0.003	−0.004	−0.002
	[0.015]	[0.820]	[0.006]	[0.012]	[0.013]	[0.013]
Central and state govt. constituency$_{t-1}$	0.042	2.458*	0.009	0.001	0.002	−0.010
	[0.028]	[1.408]	[0.011]	[0.010]	[0.010]	[0.011]
Coalition government$_{t-1}$	−0.012	3.179	−0.004	0.003	0.003	0.007
	[0.019]	[2.368]	[0.009]	[0.006]	[0.006]	[0.007]
Female legislator$_{t-1}$	−0.027**	−0.454	−0.013**	0.018	0.013	0.013
	[0.012]	[0.343]	[0.006]	[0.028]	[0.029]	[0.030]
R^2	0.39	0.37	0.34	0.78	0.67	0.44
N	67,791	64,055	64,408	3747	3747	3747
Method	FE	FE	FE	FE	FE	FE

Columns (1)–(3) examine the effect of state-wide general elections on constituency-level light variables. Election year is 1 if a state held a general election in the year and 0 otherwise. Columns (4)–(6) examine the effect of a special election in a district on the district-level GDP measures. Special election is 1 if a constituency in a district held a bye-election after death of a legislator in the year and 0 otherwise. All control variables are lagged by one year. All regressions include year fixed effects and state-specific time trends. Standard errors are clustered at the state level in columns (1)–(3) and at the district level in columns (4)–(6) and given in parentheses. The values with *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

increases electricity supply during special elections more strongly in swing and aligned constituencies. We also find suggestive evidence that increases in electricity before elections are due to diversion of electricity from non-election areas and hence have no net positive welfare effects.

Besides the opportunity to credibly identify political business cycles, the Indian experience offers an interesting contrast to the results in previous studies of the political business cycle. One theme that pervades the existing empirical literature is that electoral cycles emerge in developing countries mostly because voters have little experience with democratic politics and are more easily manipulated by politicians (Brender and Drazen, 2005; Akhmedov and Zhuavskaya, 2004). Many studies argue that as a democracy matures, electoral cycles become less pronounced. India has been a stable and vibrant democracy for over half a century, and yet we continue to observe evidence of electorally motivated cycles. The results of this paper suggest that electoral cycles are not necessarily dampened with increasing experience with democracy.

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Appendix A. Supplementary material

Supplementary data to this article can be found online at http://dx.doi.org/10.1016/j.jpubeco.2015.03.011.

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