Do Criminally Accused Politicians Affect Economic Outcomes? Evidence from India

Nishith Prakash
University of Connecticut*

Marc Rockmore
Clark University†

Yogesh Uppal
Youngstown State University‡

October 22, 2015§

Abstract

We study the casual impact of electing criminally accused politicians to state legislative assemblies in India on the subsequent economic performance of their constituencies. Using data on the criminal background of candidates running for state assembly elections and a constituency-level measure of economic activity proxied by intensity of night lights, we employ a regression discontinuity design that controls for unobserved heterogeneity across constituencies and find 22-percentage point lower economic activity arising from the election of a criminally accused politician. These effects are driven by serious, financial and the number of criminal charges and are concentrated in the less developed and more corrupt Indian states. Similar findings emerge for the provision of public goods using data on India's major rural roads construction program.

JEL Classification: D72, D73, O40, O12
Keywords: Criminal Accusations, Politicians, Night Lights, Regression Discontinuity, India

*Nishith Prakash: Assistant Professor, Department of Economics and Human Rights Institute, University of Connecticut, IZA, CReAM, HiCN & INSIDE, Storrs, CT 06269, USA (e-mail: nishith.prakash@uconn.edu).

†Marc Rockmore: Assistant Professor, Department of Economics, Clark University & HiCN, Worcester, MA 01610, USA (e-mail: mrockmore@clarku.edu).

‡Yogesh Uppal (Corresponding Author): Associate Professor, Department of Economics, Youngstown State University, Youngstown, OH 44406, USA (e-mail: yuppal@ysu.edu).

§We thank Chris Barrett, Thushyanthan Baskaran, Sonia Bhalotra, Prashant Bharadwaj, Areendam Chanda, Alex Cohen, Eric Edmonds, Irma Clots Figueras, Teevrat Garg, Clement Imberts, Lakshmi Iyer, Tarun Jain, Dean Karlan, Daniel Keniston, Erzo Luttmer, Vikram Maheshri, Mushfiq Mobarak, Dilip Mookherjee, Ameet Morjaria, Karthik Muralidharan, Sriniketh Nagavarapu, Stephen Ross, Nick Ryan, Sandip Sukhtankar, Chris Udry, Gergely Ujhelyi and seminar participants at Carlton University, Cornell University, City College of New York, Dartmouth College, Delhi School Winter School 2015, Georgia Tech University, Indian School of Business, Penn State Harrisburg, University of Connecticut, University of Michigan, University of North Dakota, Wellesley College, Wesleyan University, Yale University, 12th Midwest International Economic Development Conference, 10th Annual Conference on Economic Growth and Development, and BIGD Conference on Political Economy, Accountability and Governance for helpful comments and discussions. We especially thank to Raymond Fisman, Dan Keniston, and Brian Min for sharing data and Rajyabardhan Sharma (ex-Indian Police Service) for helping us understand the Indian Penal Code and the ADR Criminal accusation data. We also thank Tasneem Ahmed, Maxwell Aliapoulios, Kirtan Bhavsar, Doga Akar Bilgin, Rachel Borden, Avijit Ghosh, Kelsey Barton-Henry, Sungoh Kwon, Christopher McCormack, Sharon Pallier, Andrew Pitt, Brain Tiedt, Qinghan Yan who provided excellent research assistance. We are responsible for any errors that may remain. A previous version of the paper was circulated with the title: "Do Criminal Representatives Hinder or Improve Constituency Outcomes? Evidence from India".

"They may protest the administrative machinery and thereby break the law, but they are seen as local heroes who are trying to help poor people by different means" - (NY Times, 2014)

"Earlier politicians used criminals. Now the criminals themselves have entered politics" - (Associated Press, 2014)

1 Introduction

Despite a history of widely contested and transparent elections, and the presence of a vibrant and open media, India elects an increasing number of politicians facing criminal charges. This share has risen from 24 percent of members of the Indian Parliament in 2004 to 34 percent in 2014 (NY Times 2014).[1] While the election of criminally accused candidates to public office is concerning in any context, this especially pertains to India. Large quantities of funds are distributed by the government through a wide variety of interventions and programs, which have been plagued by costly scandals with losses in the hundreds of billions of dollars (Sukhtankar and Vaishnav 2015).[2] This problem is exacerbated by a severely understaffed judiciary and police force, resulting in an extremely slow judicial system.[3] Taken together, these realities create a context in which an influx of criminally accused politicians could be especially costly for an economy.

In this paper, we examine the aggregate economic costs of electing criminally accused politicians to State Legislative Assemblies (Vidhan Sabhas) in India for elections held during the 2004 to 2008 period. A Supreme Court of India order in 2003 required all candidates seeking election to the Parliament of India or to State Legislative Assemblies to disclose information on their criminal background in a sworn affidavit submitted to the Election Commission of India (ECI). The data from these affidavits not only allow us to identify criminally charged candidates, but also allow us to differentiate between the types of charges, and by the number of outstanding cases against a

[1]`http://india.blogs.nytimes.com/2014/05/23/in-the-newly-elected-indian-parliament-worrying-trends/?_r=0`.

[2]Looking at a statutory wage increase for participants in India's employment guarantee scheme (NREGS), Niehaus and Sukhtankar (2013) estimate marginal leakage of almost 100%. Similarly, Baskaran et al. (2015), Nagavarapu and Sekhri (2013), and Min and Golden (2014) find that the allocation of electricity is distorted by political incentives.

[3]For instance, Suhkatankar and Vaishnav (2015) note that nearly 60 percent of police positions are unfilled in Uttar Pradesh. Nationwide, 20 to 30 percent of district, sub-ordinate and High court seats are unfilled and close to a quarter of all cases have been pending for 5 years or more and there is a backlog of over 31 million cases.

candidate. We utilize this information to estimate the causal effect of electing criminally accused on economic activity in state Assembly constituencies in India.

We measure constituency-level economic activity using satellite data on the intensity of night lights, since no GDP data is available for the state assembly constituencies; satellite data have been used as a proxy for economic activity (Bleakley and Lin 2012, Henderson et al. 2012, Hodler and Rashky 2014, Storeygard 2014) at both the national and sub-national levels and offer several advantages. First, unlike large household and economic surveys in India, these data are available at a highly disaggregated geographical level and can be aggregated to compile constituency-level measures. Second, these data are available annually allowing for a more credible time series analysis. Third, night-lights data are collected automatically by the satellites and thus are less prone to biases that may be present in administrative data.

Using a regression discontinuity design that credibly identifies the effect of electing criminally accused politicians by comparing constituencies which elect criminally accused with those which elect non-accused politicians in close elections, we find that electing a criminally accused politician has a large negative effect on economic activity in their constituency. On average, constituencies in which a criminally accused candidate barely won experienced roughly 22 percentage point lower yearly growth in the intensity of night-lights than constituencies that barely elected a non-accused candidate. Moreover, these effects are larger when the charges include serious or financial charges. Using existing estimates from the literature on the elasticity of GDP growth to night light growth, this is roughly equivalent to roughly 2 to 7 percentage point lower GDP growth.

We find very similar effects using a proxy for overall public good delivery, the number of incomplete road projects. The number of incomplete road projects is significantly higher in constituencies in which an accused politician barely won compared to constituencies in which a non-accused barely won. Overall, these results highlight the high aggregate economic costs of electing lower quality politicians (i.e. criminally accused) and point to likely significant individual costs in foregone access to public services.

Our paper contributes to several related literatures. Most narrowly, our findings contribute to the emerging literature on criminally accused politicians in India. Existing studies typically focus on the selection of these candidates; some examine the response of voters to information on criminal status or criminal charges and the potential mitigating effect of caste politics (Banerjee et al. 2014, Charchard 2014). Others examine the selection of these candidates by political parties (Aidt et al. 2012, Tiwari 2014, Vaishnav 2011b, 2011c, 2011c). However, despite a presumption of a negative effect of electing criminals, the actual effect is unknown. Our study provides the first evidence of the aggregate economic cost of electing criminally accused politicians.[4] Since criminally accused politicians are not limited to India, this indicator can also be applied more broadly to other contexts (for e.g. Brazil and Pakistan).[5,6]

Additionally, we contribute to the discussion regarding whether criminal accusations are 'too noisy' a signal to be useful for research or policy. In part, this concerns arises since political rivals may have the incentive and means to fabricate charges against opponents. Similarly, there are examples of political activists being charged while participating in democratic protests (Jaffrelot and Verniers 2014). We indeed find that accusations provide useful information regarding the criminality of politicians and the economic costs vary with the type and the number of crimes.

We also contribute to the literature on the quality of politicians which typically uses proxies such as education (Alcantara 2008, Besley et al. 2005, De Paola and Scoppa 2010, Martinez-Bravo 2014) and, more recently, personality (Callen et al. 2015). As our results demonstrate, whether or not a politician is criminally accused can have an important effect on the constituency level economic outcomes. Consequently, we believe that this may represent a novel pre-election indicator of politician quality.

Finally, although our study focuses on India, it contributes to our broader understanding of the costs of electing lower quality politicians in clientelistic democracies. We find that the costs of electing criminally accused politicians are concentrated in those states which are less developed and have higher levels of corruption. These findings are consistent with work that suggests that low quality politicians (such as criminals) may provide targeted benefits to certain voters (Chandra 2004, Vaishnav 2011a, Banerjee and Pande 2007) based on certain salient factors, such as caste, ethnicity or class. Robinson and Verdier (2013) and van de Walle (2005) further suggest that less developed countries are associated with this type of clientelist or patronage politics. Bardhan and

[4] Chemin (2012) shows that the election of accused politicians has distributional impacts by reducing the per capita expenditure of SC, ST, and OBC members. However, he does not investigate the overall effects.

[5] Almost 200 legislators, or a third of Brazil's Congress, are facing charges in trials overseen by the Supreme Federal Tribunal (see: `http://www.law360.com/articles/457373/brazil-has-had-it-with-corporate-bribery`).

[6] Criminality and politics in Pakistan (see: `http://www.dawn.com/news/1200870`).

Mookherjee (2012) note that patronage democracies can lead to excessively short-term payoffs and a lack of general public goods and other long-run investments. In such democracies, criminally accused politicians may be especially detrimental for economic development and public goods delivery.

The remainder of this paper is organized as follows. In Section 2 we provide the background on the elected representatives in India and discuss the corruption and criminality in Indian politics. Section 3 discusses the empirical strategy, followed by the data description and the validity of the regression discontinuity design in Section 4. We present our results in Section 5. We calculate the estimated effect on GDP in Section 6 and conclude the paper in Section 7.

2 Background

2.1 Elected Representatives in India

India is a federal republic with a parliamentary system of government. The Parliament of India, the national level legislature, consists of two Houses–an Upper House (also called the Rajya Sabha or Council of States) and a Lower House (also called the Lok Sabha or House of the People). Those elected or nominated to either house of the Parliament are referred to as Members of Parliament (or MPs). State assemblies in India follow a similar structure where the Upper House is called the Legislative Council (or Vidhan Parishad) and the Lower House is called the Legislative Assembly (or Vidhan Sabha). Those elected to the state Legislative Assembly are referred to as Members of the Legislative Assembly (or MLAs). Similar to the national level, the election system at the state level is a "first-past-the-post" system and constituencies are divided into single member constituencies. The term of each MLA is 5 years, although it is possible to have elections before the 5-year term mostly due to shifting of political alignments. The focus in this paper is on the members elected to the state Legislative Assemblies.

The Indian Constitution grants elected representatives certain responsibilities. In particular, MLAs hold legislative, financial, and executive power. In addition to these constitutional powers, they also have control over the state bureaucracy (e.g., promotions, and job assignment/transfers) (Asher and Novosad 2015, Iyer and Mani 2012, Krishnan and Somanathan 2013, Nath 2014, Sukhtankar and Vaishnav 2015).[7], which allows them to play a key role in the allocation of funds for various development projects, the distribution of licenses, and facilitate access to governmental schemes. Collaboration with or control of the bureaucracy also allows politicians to act as mediators between the private sector and the government, and to lobby political allies and business contacts to bring projects to their constituencies (Bussell 2012, Chopra 1996, Jensenius 2013). Finally, MLAs also have access to discretionary development funds, known as the Member of Legislative Assembly Constituency Development Scheme, which they can spend on development projects within their constituencies. Therefore, elected representatives can both directly and indirectly affect economic activity in their constituencies.

2.2 Corruption and Criminality in Indian Politics

Elected officials are widely reputed to be involved in corruption, mostly the graft and embezzlement of public funds (BBC News India 2012, India Today 2012). A recent paper by Sukhtankar and Vaishnav (2015) compiled an inventory of the biggest public corruption scandals uncovered after 2003, and found amounts totaling hundreds of billions of dollars.[8] Fisman et al. (2014) utilize the asset disclosures of candidates for Indian state legislators and compare the asset growth of election winners versus runners-up to calculate the financial returns from holding public office relative to private sector opportunities available to political candidates. They find that the estimated annual growth rate of the winners' assets is 3-5 percent higher than those of the runners-up. Similarly, Bhavnani (2012) compares the change in winners' and losers' self-declared family assets in India's two most recent state and national elections, and finds that the average election winner increased their assets by 4-6 percent a year.[9]

The issue of criminally accused candidates contesting elections in India is not new and has

[7] The nexus between politicians and bureaucrats, and in particular, the possibility of bribes involving job assignments/transfer of bureaucrats was recently alleged in a press conference on May 22, 2015 by an incumbent Chief Minister of Delhi (`http://www.ndtv.com/video/player/news/kejriwal-says-centre-has-betrayed-people-of-delhi-by-siding-with-lieutenant-governor/368367`).

[8] Table 1 of Sukhtankar and Vaishnav (2015) estimates a mean scam "value" of Rs. 36,000 crore (about 5.6 billion USD), and the median was Rs. 12,000 crore (about 1.9 billion USD).

[9] According to Banerjee et al. 2011, in the case of Uttar Pradesh state legislators, the 287 elected MLAs in 2007 who ran for elections again in 2012 witnessed an increase in their average asset value from $220,613 to $658,804, over their 5 year term in office. At that time, the average annual salary of MLAs in Uttar Pradesh was approximately $12,000. The political affiliation was especially important as MLAs who belonged to the political party heading the state government (or the ruling party) saw their asset value increase by an average of $500,000. For opposition party members, this increase amounted to less than $300,000.

been debated at the highest level by the Election Commission of India and the Indian Parliament. Even the Supreme Court of India showed serious concern about the increasing "criminalization" of politics, which led to its landmark judgment about making the criminal backgrounds of politicians public. In 2003, the Supreme Court of India required candidates seeking election to the Parliament or a Legislative Assembly to file sworn affidavits detailing information on their professional and educational qualifications, their assets and liabilities and those of their immediate family, as well as regarding their criminal convictions and charges. In particular, the affidavits require candidates to report prior convictions and any pending accusations for which the offence is punishable with imprisonment for two years or more, and in which a charge is framed or cognizance is taken by the Court of Law (that is, any criminal indictment). These charges are limited to those framed prior to the six-month period preceding an election. Since candidates face penalties for lying on the sworn affidavits and rival candidates (and the media) have incentives to verify information contained in these affidavits, any deliberate misreporting should be minimized.[10]

The Association for Democratic Reforms (ADR), an election watchdog, along with the National Election Watch have conducted so-called Election Watches for all state and federal elections since 2003 in India.[11] The surprising finding is that the percentage of MPs facing criminal charges has increased between the 2009 and 2014 elections for the national Parliament. The findings are similar for the state assembly elections. According to the ADR report, over 30 percent of the MLAs currently face criminal cases in India.[12]

[10]These are sworn affidavits and there is a penalty for filing incorrect affidavit (e.g. disqualification, imprisonment for a term which may extend to six months, or with fine, or both). The affidavits can be accessed from the ECI's website (`http://eci.nic.in/eci/eci.html`) and its website on candidate affidavit (`http://eci.nic.in/eci_main1/LinktoAffidavits.aspx`).

[11]An Election Watch comprises of background reports based on Criminal, Financial, Educational and Income Tax details of Candidates and Winners (MPs, MLAs and Ministers) who have contested Elections to State Assemblies, the Parliament and a few local bodies.

[12]For example, in one of most populous and politically important state, Uttar Pradesh, 575 of the candidates for the 403 assembly seats had criminal backgrounds or faced criminal charges during the 2007 state legislative assembly elections. Out of these, 140 won the assembly seats. Unsurprisingly, following this success, an even greater number of criminally accused candidates (759) ran in the next elections in 2012. Of these, 189 won seats in the state assembly (ADR, 2012a).

3 Regression Discontinuity Design

A key contribution of this paper is the identification of the causal effect of electing criminally accused politicians to state assemblies in India on local economic activity. The main challenge in doing this is that the victory of criminally accused politicians is not necessarily random; for example, criminally accused candidates may be more likely to run and win from certain constituencies than others in ways that are unobservable to us. As a result, average differences in economic activity between constituencies that elected an accused MLA and those that elected a non-accused MLA will result in a biased estimate of the effect of electing criminally accused candidates.

A regression discontinuity (RD) design (Lee 2008, Imbens and Lemieux 2008) allows us to exploit a discontinuity in the treatment assignment to identify the causal effect of a treatment variable.[13] In our setting, the assignment of treatment, whether a candidate is criminally accused or not ($ACCUSED$), is determined solely on the basis of a cutoff value, c=0, of the forcing variable, the victory margin ($MARGIN$). The treatment assignment follows a known deterministic rule, $ACCUSED = 1\ (MARGIN \geq c)$, where 1(.) is the indicator function. The constituencies which fall below the cutoff ($MARGIN < 0$), the control group ($ACCUSED = 0$), elect a non-accused candidate who won against an accused runner-up, and victory margin in these elections is the difference in the vote shares of the accused runner-up and the non-accused winner. Constituencies that fall above the cutoff ($MARGIN \geq 0$), the treatment group ($ACCUSED = 1$), elect a criminally accused candidate who won against a non-accused runner-up, and the victory margin in these elections is the difference in vote shares of the accused winner and the non-accused runner-up. Therefore, at the victory margin of zero, the accusation status of a politician changes discontinuously from non-accused to criminally accused. Thus, as the victory margin becomes arbitrarily small (i.e. as we move closer to the cutoff), the outcome of an election is as good as random. As a result, constituencies that barely elected a non-accused politician in a close election serve as a valid counterfactual for constituencies that barely elect a criminally accused politician.

We consider the following specification for estimating the RD treatment effect of electing a

[13]The seminal paper by Lee (2008) exploits a regression discontinuity design using electoral data. Studies using similar design in the context of India and elsewhere include Asher and Novosad (2014), Bhalotra et al. (2013), Bhalotra and Clots-Figueras (2014), Broockman (2014), Clots-Figueras (2011, 2012), Fisman et al. (2014), and Uppal (2009).

criminally accused candidate to state legislative assemblies relative to a non-accused candidate:

$$GROWTH_{i,s,t+1} = \alpha_s + \beta_{t+1} + \gamma ACCUSED_{i,s,t} + f(MARGIN_{i,s,t}) + \mu_{i,s,t+1} \tag{1}$$

$\forall\ MARGIN_{i,s,t} \in (c-h,\ c+h)$

where $GROWTH_{i,s,t+1}$ is the outcome of interest, i.e. yearly growth in night lights [$Log(Y_{i,s,t+1})$-$Log(Y_{i,s,t})$]. α_s is the state fixed effects and control for any time-invariant state characteristics. β_{t+1} is the time fixed effects and control for any macroeconomic shocks or national policies that affected all states uniformly, and for changes in satellite technology over time. The variable $ACCUSED_{i,s,t}$ is the treatment, $MARGIN_{i,s,t}$ is the forcing variable, and h is the neighborhood around the cutoff c=0, also referred to as the bandwidth. The control function $f(MARGIN_{i,s,t})$ is some continuous function, usually a n-order polynomial in the forcing variable on each side of c. Finally $\mu_{i,s,t+1}$ is the error term. The coefficient of primary interest γ estimates the causal impact of electing criminally accused politicians to state assemblies in India on economic activity as proxied by the growth of night lights. The identification of this causal effect relies on fairly weak conditions on the conditional distribution of the error term μ, which is assumed to be a continuous function of the forcing variable ($MARGIN$).

We estimate a local linear regression (Hahn, Todd, and Van der Klaauw 2001, Porter 2003, Imbens and Lemieux 2008) as it allows for a suitable bandwidth with a linear control function.[14] We follow the algorithm proposed by Imbens and Kalyanaraman (2012) to calculate an optimal bandwidth (referred to as h) for each regression. As a robustness check, we also estimate the local linear regression using the optimal bandwidth proposed by Calonico, Cattaneo and Titiunik (2014), half the optimal bandwidth (h/2), and twice the optimal bandwidth (2h). Since growth in night lights is likely to be correlated over time within a constituency, the standard errors are clustered at the constituency level.

[14]Different variations of equation (1) with different bandwidths and control function have been used in the literature. For example. Lee, Moretti and Butler (2004) use parametric regression-based higher order polynomials in the control function (second-order, third-order, and fourth-order polynomials), thus allowing all the observations to be used in the RD estimation. However, this method puts equal weight on observations far from the cutoff, which can be misleading (Gelman and Imbens 2014).

4 Data Description and Validity of the RD Design

4.1 Night Lights as a Measure of Economic Activity

To study the costs associated with electing criminally accused candidates, we need a measure of economic activity at the State Assembly constituency level, our unit of analysis. To the best of our knowledge, no such data exist in India. Large surveys, such as the National Sample Survey, the India Human Development Survey and the Economic Census of Firms, are only available at the district level. In theory, it is possible to make use of the above mentioned data sets, however, on average, there are approximately 6 to 7 constituencies per district. Since the number of constituencies varies across districts and there is no logical way to weight constituencies within districts, the district level data cannot be easily matched to various constituencies.[15] Moreover, even if a measure of state constituency level economic activity could be derived, the above mentioned surveys are not available annually.

We use data on the intensity of night lights as a proxy for economic activity. Although the intensity of night lights is not a perfect measure of economic activity, Henderson et al. (2012), Hodler and Rashky (2014), and Storeygard (2014) find a strong relationship between GDP and night light intensity at the sub-national level using data on cross-sections of countries.[16] These data offer several advantages including that pixels can easily be aggregated to the constituency level and the availability of annual frequency allows for more detailed temporal analysis. Moreover, whether through increased electrification or higher incomes levels, it should arguably be related to local economic activity.

The satellite data are collected by the National Aeronautics and Space Administration's (NASA)

[15] Based on the Delimitation Order of 1976, the constituency boundaries remained fixed till 2008. As a result, there were 4,120 state assembly constituencies. According to the Delimitation Order of 2008, the number of Assembly constituencies are 4,033. There were 593 districts in India according to the 2001 Census, while according to the 2011 Census, there are 640 districts.

[16] Henderson at al. (2012) shows that night lights can also identify short run fluctuations, including the Asian Financial Crisis in Indonesia between 1997 and 1998 and the Rwandan Genocide between 1993 and 1994. Thus satellite night lights data are a useful proxy for economic activity at temporal and geographic scales for which traditional data are of poor quality or are unavailable (Henderson et al. (2012). Additionally, prior research shows that the Defense Meteorological Satellite Program's Operational Linescan System (DMSP-OLS) can reliably detect electrified villages in developing countries and that night lights output is a useful proxy for electricity provision (Doll et al. 2006; Min et al. 2013; Baskaran et al. 2015). Recent papers have used night lights data to study growth of cities in sub-Saharan Africa (Storeygard 2014), production activity in blockaded Palestinian towns of the West Bank (Abrahams 2015), van der Weide et. al (2015), and urban form in China (Baum-Snow and Turner 2012) and India (Harari 2015).

Defense Meteorological Satellite Program's Operational Linescan System (DMSP-OLS) uses a set of military weather satellites that have been orbiting the earth since 1970. The satellites record high resolution images of the entire earth each night typically between 8:30 and 10:00 pm local time. The images, captured at an altitude of 830 km above the earth, record concentrations of outdoor lights, fires, and gas flares at a fine resolution of 0.56 km and a smoothed resolution of 2.7 km. These images are used to produce annual composites during a calendar year, dropping images where lights are shrouded by cloud cover or overpowered by the aurora or solar glare, and removing ephemeral lights like fires, other temporary lighting phenomenon and noise.

The result is a series of images covering the globe for each year from 1992 onwards (Elvidge et al. 1997, 2001). Images are scaled onto a geo-referenced 30 arc-second grid (approximately 1 km^2). Each pixel is encoded with a measure of its annual average brightness on a 6-bit scale from 0 to 63. Thus, it is top-coded at 63 and censored at 0 (i.e. the brightest areas are not well measured and areas require some minimum level of light to be captured). In our baseline RD sample, there are 63 fully lit constituencies. Top-coding is a concern since we are unable to observe increases in lighting above 63. However, this should not be systematically related to close elections of criminally accused candidates. That said, we subsequently replicate our analysis with and without the top coded constituencies to verify the robustness. Another potential issue is blooming, which occurs when light from a brightly lit area spills over into neighboring areas. While this is a concern around large cities, our sample is predominately rural.

Lastly, the levels of light output are relative brightness values. There is no onboard radiance calibration on the satellite sensors, thus there is no way to convert the relative brightness values to an actual level of illumination. This complicates time series analysis because changes in observed brightness in different annual composites may be due to real changes in light output on the ground or due to technical factors related to gain levels and sensor properties. We follow Henderson et al. (2012) and Chen and Nordhaus (2011), and account for this limitation by including year fixed effects in our estimating equation to control for contemporaneous shocks affecting all units in a year, including any factors that may affect the overall brightness detected by a sensor in any given year. Finally, we utilize the data available on stable night lights that drop light values from pixels with unstable light signatures over time.

Our primary dependent variable is the annual growth in night lights. This is the difference in the natural log of night lights intensity for the constituency between the current $(t+1)$ and the previous period (t). As discussed earlier, this has been widely accepted in the literature as a proxy for economic activity. Another advantage of specifying the dependent variable in this form (i.e. the difference in natural logs) is that it allows us to roughly estimate the impact on GDP using estimates from the literature of the elasticity of GDP growth to night light growth. In addition, we also show results using three alternate dependent variables: the natural log of night lights, the proportion of lit villages within a constituency, and the growth of night lights averaged over the entire election term.[17] These additional dependent variables are likely to be less sensitive when comparing growth of night lights over time. We report the descriptive statistics of the key dependent variables in Table 1-A.

Finally, we also use another outcome variable that is a proxy for public good provision at the constituency level: the number and share of incomplete roads projects. In particular, we use the official data from Pradhan Mantri Gram Sadak Yojana (PMGSY) that was launched in December 2000. PMGSY is a fully funded centrally sponsored scheme that aims to provide all weather road connectivity in rural areas in India. We match the roads data to the state assembly constituency level data for the empirical analysis.[18]

4.2 Election Results and Affidavit Data

We use the Election Commission of India (ECI) Statistical Reports on General Elections to State Legislative Assemblies for election data.[19] These reports provide important information related to elections, such as the name and code of the constituency, candidates and their vote shares, electorate size (number of registered voters), number who turned out to vote (number of voters) gender and

[17]The proportion of lit villages within each constituency is the number of villages with detectable stable light output observed at the village center divided by the total number of villages, which is the proportion of villages with positive light output within each constituency. A typical constituency had a total light output of 1,956 "brightness" units in 1992, and this increased to 3,745 by 2008. The proportion of lit villages was just under half in 1992 but increased to 69-percent by 2008 [Baskaran et al. (2015)].

[18]The PMGSY data is available at the census block level. There is no one-to-one matching between census blocks and Assembly constituencies. For example, a block can span more than one constituency. We however match a block to a specific constituency if at least 50 percent of the villages in the block fell in that constituency.

[19]The reports are available at `http://eci.nic.in/eci_main1/ElectionStatistics.aspx`, accessed in May 2014.

constituency type (whether reserved for SCs (STs) or non-reserved).[20] For criminal accusations, we rely on the data from affidavits that have been collected and digitalized by the Election Watch, in collaboration with the Association for Democratic Reforms (ADR).[21] The ADR data provide information on the number of criminal cases against each candidate, the charges associated with each criminal case, a classification of each accusation as serious or not, the asset and liabilities of each candidate and each candidate's level of education.

We consider all state elections held between 2004 and 2008. While the light data is available from 1992 onwards, we are limited by the data on affidavits, which became mandatory only after the Supreme Court order in 2003. Further, ADR data are available only for elections held after 2004. As a result, we have a sample of 20 states out of a total of 28 covering approximately 90 percent of India's total electorate.[22] The constituency boundaries changed in 2008 meaning constituencies before and after delimitation are not comparable.[23] Thus, between the Court order to file affidavits in 2003 and the redrawing of boundaries in 2008, we observe at most 1 election per state. However, we utilize the light data until 2012 for some states.[24] Appendix Table A-1 reports the information on the number of assembly constituencies and information on year in which the elections were held in each state after the Supreme Court order from the ADR website.

Our main variable of interest is the criminal accusations.[25] A potential concern with accusations

[20]According to the Indian Constitution, certain seats are reserved for Scheduled Castes (SCs) and Scheduled Tribes (STs), the two historically disadvantaged minority groups. While registered voters from all social groups can vote, only an SC (ST) candidate may contest election from the seats reserved for SC (ST).

[21]The ADR data is available for public use at `www.myneta.info`.

[22]The included states are Arunachal Pradesh, Assam, Bihar, Goa, Gujarat, Haryana, Himachal Pradesh, Jharkhand, Kerala, Maharashtra, Manipur, Meghalaya, Nagaland, Odisha, Punjab, Tamil Nadu, Tripura, Uttar Pradesh, Uttarakhand and West Bengal. The states excluded from our analysis are Andhra Pradesh, Chhattisgarh, Jammu and Kashmir, Karnataka, Madhya Pradesh, Mizoram, Rajasthan and Sikkim. Note that these states are excluded from the analysis based on the pre-determined timing of their elections. Consequently, there is no reason to believe that there are any systematic differences between included and excluded states (particularly with respect to the growth of night lights or criminally accused candidates).

[23]The boundaries for constituencies were fixed in 1976 until the Delimitation Act of 2002. This Act constituted a Delimitation Commission to redraw the constituency boundaries based on the 2001 census figures. Based on the delay in compiling the necessary data and in creating the new boundaries, the first election with redrawn boundaries was only held in Karnataka in 2008. Consequently, the period between 1976 and 2008 had fixed constituencies boundaries allowing for the comparison of satellite imagery across time. Once the new boundaries were implemented, it is not possible to make a comparison between the two periods.

[24]The affidavits are available starting first election held after the Supreme Court order in 2003. For example, the order was first effective in 2004 in Arunachal Pradesh and the first election after the boundaries changed was held in 2009. So for Arunachal Pradesh, our (post-treatment) sample period is from 2004-2009. However, for Uttar Pradesh the first election after the order, took place in 2007 and the first election after the changes in the boundaries was held in 2012. As a result, our sample period for Uttar Pradesh is from 2007-2012.

[25]It is not possible to examine convictions since there are only a handful of cases in which criminal proceedings lead to a conviction. Ideally, we would be able to use earlier accusations and examine whether candidates are subsequently convicted. However, the data on accusations are only recent and the Indian judicial process frequently takes years or

is that political rivals may file false cases to gain electoral advantages. Unfortunately, it is not possible to distinguish between "true" and "false" criminal accusations. Despite this limitation, these data have been widely used to measure criminal accusations (Aidt et al. 2015, Asher and Novosad 2014, Banerjee et al. 2014, Fisman et al. 2014). There is some evidence to suggest that false cases are not as frequent as might be believed. Looking at a sub-sample of states, Vaishnav (2011a) finds that accusations are unrelated to prior electoral performance (a proxy for popularity), incumbency, and the timing of elections. In addition, only cases filed 6 months prior to elections need to be reported, therefore it is less likely the criminal charges are electorally motivated.

In our baseline specification, we define a binary variable for whether or not a candidate is currently accused in any criminal case. Specifically, this variable takes a value of 1 if MLA faces any current criminal cases and 0 otherwise. Given the setup in a RD design, we only consider races in which, among the top-2 candidates, one is accused and the other is non-accused. This is because the RD design implicitly assumes that voters are faced with a choice between two types of candidates (accused and non-accused). In practice, the top 2 (or even all the candidates) might be of the same type. We therefore restrict the sample to constituencies in which the top two candidates represent each type. This restricts our sample from the full 2633 constituencies for which we have data to a smaller sample of 941 constituencies, which are observed annually during our sample period in between elections totaling over 3600 observations.[26]

While we are not the first study to use the data on criminal accusations, few studies, Vaishnav (2011a) being a notable exception, consider the type of charges. This is particularly important since not all charges are of the same severity or relevance in assessing a candidate's aptness for public office. We consider whether a specific charge associated with a criminal case is serious or not, and whether it is related to any financial wrongdoing or not. Since any definition of a serious criminal charge is inherently arbitrary, we rely on a classification used by the ADR, such as the maximum punishment under the law, their violent nature, and offenses under the Prevention of Corruption Act.[27] Since ADR does not classify charges into financial and non-financial categories, we define a

even decades to resolve cases. According to Sukhtankar and Viashnav (2015), of the 76 MPs serving in the 15th Lok Sabha (2009 national elections) who faced ongoing criminal action, the case had been pending for an average 7 years.

[26]Note that our results are robust to using the full sample. We choose this restricted sample since this is the implicit comparison in a RD design.

[27]ADR compiles the detailed data on each candidate's criminal cases and the type of charges farmed in each case. Thus it reports the exact criminal charge(s) for each candidate as defined under the Indian Penal Code (IPC). IPC is the main criminal code of India that covers all substantive aspects of criminal law. ADR defines serious criminal

charge as financial if the corresponding IPC refers to a crime resulting in a loss to public exchequer.[28] Similar to the variable for any criminal case, we create binary variables for whether or not a candidate is accused of a serious or financial criminal charge.

Our baseline definition of criminal accusations is whether a candidate has any criminal case against him/her. We refine our definition of criminally accused further by considering candidates who face multiple criminal cases. Insofar as there is a cost to framing false cases against politicians, we might expect that a higher number of cases might be a more reliable indicator of a politician's true type. Additionally, this also provides a measure of the "intensity of the treatment". Accordingly, we should expect the treatment effects to become larger as the treatment intensity increases. We consider two thresholds: a candidate is criminally accused if he/she has 2 or more cases or if he/she has 5 or more cases.

Table 1-C reports descriptive statistics of the data on criminal accusations. Approximately 54-percent of the winners in our sample report at least one criminal case, while 40-percent are facing at least one serious charge, and 20-percent at least one financial charge. Although some of the differences between the winners and the runners-up are statistically significant in the full sample in Table 1-B, these become insignificant when we look at close elections in our sample. Further, Appendix Figure A-6 depicts the distribution of criminally accused MLAs across Indian States.

4.3 Validity of the RD Design

There are two standard tests to show the validity of the RD design (Imbens and Lemieux 2008). The first is the McCrary (2008) density test for a jump around the cutoff in the density of the forcing variable. In our context, this tests for whether criminally accused candidates disproportionately win close elections. For instance, criminally accused politicians might be able to manipulate elections and therefore be more likely to win close elections, thereby violating the identifying assumption that

charges using eight criteria. They are: (1) Whether the maximum punishment for the offence committed is of five years or more, or; (2) Whether the offence is non-bailable, or; (3) Offences pertaining to the electoral violation (IPC 171E or bribery), or; (4) Offence related to the loss to exchequer, or; (5) Offences the nature of which are related to assault, murder, kidnap, rape, or; (6) Offences that are mentioned in Representation of the People Act, or; (7) Offences under Prevention of Corruption Act, or; (8) Offences related to the Crimes against women. The following is a link to an online Appendix on ADR criteria for coding serious crimes:`http://adrindia.org/content/criteria-categorization-serious-criminal-cases`.

[28] This classification is based on consultations with several high level Indian police officers and we labels the following IPCs as financial crimes: 171B, 171E, 230–262, 272, 273, 274, 275, 276, 378–420, and 466–489D.

treatment is randomly assigned. If this were the case, we would find a larger frequency of criminally accused candidates compared to non-accused candidates in the neighborhood of the cutoff. This would imply that the density of the margin of victory, the forcing variable, would show a discontinuity at the cutoff. Panels (a)-(b) of Figure 1 shows that the density of the victory margin above and below the cutoff is not statistically significant.

The second test of the validity of the RD design is whether the observed characteristics of candidates and constituencies are continuous around the cutoff. That is, while the characteristics for criminally accused and non-accused candidates may be different over the entire sample, with the exception of the treatment, no other variable should jump around the cutoff. For instance, recent papers (Caughey and Sekhon 2011, Grimmer et al. 2011) have shown that in the context of U.S. elections, the incumbent party tends to have systematically greater chances of winning even when elections are close. However, Eggers et al. (2014) use data on 40,000 closely contested races in different electoral settings, including India, and do not find any systematic evidence of sorting or imbalance around the electoral thresholds.

We formally check for continuity of various constituency and candidate characteristics in Figure 2. The variable on the y-axis is net of state and year fixed effects. The dots in the scatter plot depict the averages over each successive interval of 0.5% of margin of victory. The curves are local linear regressions fit separately for positive and negative margins of victory using a triangular kernel and an optimal bandwidth calculator as suggested in Imbens and Kalayanaraman (2012). The confidence intervals are the 95% confidence intervals plotted using standard errors that are clustered at the constituency level.

In Panels (a)-(j), we compare accused winners to non-accused winners on growth of night lights in the prior years (t-1 and t-2) and on several other constituency and candidate characteristics: constituency characteristics are electorate size, number of voters, whether a constituency was aligned with the ruling party in the state, in the previous election, and whether a constituency is reserved for Scheduled Caste (SC) or Scheduled Tribes (ST); the candidate characteristics are the MLA's incumbency status, gender, asset, liabilities, and education. The results indicate that there are no statistically significant differences in the observed covariates around the cutoff. Therefore, the results from the McCrary test and the continuity of covariates strongly suggest that the assumptions underlying the RD design are valid in this setting and that the outcome of a close election is as good as random.

5 Criminally Accused Politicians and Economic Activity

5.1 Main Results

We present the main results with the graphical illustration of the RD effect of electing criminally accused candidates in Figure 3 which plots the yearly growth of night lights against the margin of victory for the criminally accused candidates. The yearly growth of night lights is the residual from the regression of yearly growth of night lights on state and year dummies. The scatter plot contains the local averages of the residuals in each successive interval of 0.5 percent of margin of victory. The solid curves are plotted non-parametrically using local linear regression which uses a triangular kernel and the optimal bandwidth criterion proposed by Imbens and Kalyanaraman (2012). Positive margins of victory indicate a constituency in which a criminally accused candidate won against a non-accused candidate (the runner-up), while a negative margin shows that she/he was the runner-up and that the winner was not criminally accused. The RD figure shows a sharp difference in the average yearly growth of night lights at the cutoff ($MARGIN = 0$). The vertical difference between the red and blue lines reflects the estimated causal effect of electing a criminally accused candidate on yearly growth of night lights. In particular, at the cutoff, there is a statistically significant and negative effect of electing a criminally accused candidate.

We use a local linear regression with an optimal bandwidth (h) calculated using the criterion proposed by Imbens and Kalyanaraman (IK) algorithm to estimate the RD effect (equation 1) in column (1) of Table 2. This is our main specification and is analogous to the Figure 3. The rest of the columns present these results using alternate bandwidths. We find a statistically negative effect of electing criminally accused candidates: the annual growth of night lights is approximately 22 percentage points lower in constituencies that barely elect a criminally accused candidate as compared to those constituencies that barely elected a candidate without accusations. In column (2), we use a bandwidth calculated using the Calonico, Cattaneo and Titiunik (2014) (CCT) algorithm, while in columns (3)-(4) we halve and double the IK bandwidth, respectively. The results in column

(2) is quantitatively identical to those in column (1). Halving the bandwidth in column (3) results in a slightly larger estimate, while doubling the bandwidth in column (4) leads to a smaller estimate. Overall, the results remain statistically significant and similar in magnitude.

As discussed in Section 4, we use the intensity of night lights as a proxy for economic activity. It is, however, unclear what is driving the changes in night lights. For example, growth in night lights could represent changes in the supply of electricity. Alternately, it could be attributable to changes in demand. Irrespective of whether these changes are supply or demand-side driven, economic activity and the intensity of night lights should be correlated (although certainly not one-to-one). While we currently interpret these results in terms of night light intensity, we compute the corresponding decline in GDP using existing estimates of the elasticity of growth of night lights to the growth of GDP in Section 7.

5.2 Robustness

In this section, we perform further robustness checks using alternate functional forms, alternate definitions of the dependent variable. We also re-estimate our main result after controlling for covariates in the RD regression (similar to Meyersson 2014), and finally examine the impact of top coding on our results.

5.2.1 Sensitivity Analysis of RD Specification

While earlier researchers emphasized the analysis of different bandwidths (Imbens and Lemieux 2008), recent studies broaden the focus to include alternate control functions (Dell 2010, Lee and Lemieux 2009, Meyersson 2014). We address this in Appendix Table A-2 which reports the RD effects for quadratic, cubic, and quartic functions using the IK (h), CCT, h/2, and 2h bandwidth choices.[29] Variations in the polynomial order in the control function are ordered by row and bandwidth choices by column in this table.

By and large, we find that the RD estimates are negative and statistically significant and qualitatively similar to the effect estimated in Table 2. Statistical significance, however, is lost with larger

[29] In results we do not present here, we also re-calculate the RD estimates using alternate bandwidth choices, specifically $0.9h$, $1.3h$, $1.7h$, $2.1h$, where $h(IK) = 6.35$. We further repeat the same exercise for four additional bandwidths (i.e. 2.5, 5.0, 7.5, and 10.0) and find similar results. Both tables are available upon request.

bandwidth and/or polynomial order of the control function.

5.2.2 Alternate Dependent Variables

Since night lights and their distribution can be measured in several ways, we explore here three alternate definitions of the dependent variable: the intensity of night lights in levels [$Log(Night\ Lights)$], the proportion of lit villages ($Proportion\ of\ Lit\ Villages$), and the growth of night lights averaged over the election term of the candidate (*Average Growth over the Election Term*). We present the estimates from RD effect for the three dependent variables in Appendix Table A-3.[30]

While we focus on the yearly growth of night lights (for better comparison across constituencies), both growth rates and levels are used when researchers talk about growth. Moreover, large percentage changes due to a small initial level can mask very small absolute changes. We therefore estimate the local linear RD regression using IK bandwidth for night lights in levels and present the results in column (1).

Our second 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. This measure provides an alternative way of quantifying the breadth of access to electricity within a constituency as compared to measures based on the level of light output alone. We present the estimate from local linear regression using IK bandwidth in column (2).

Finally, we consider the growth of night lights averaged over the election term of the candidate. This is because our main dependent variable uses the year to year variation in the growth of night lights in a constituency which could potentially be influenced by year to year volatility. We present this estimate in column (3).

Results from Appendix Table A-3, columns (1)-(3) suggest that the point estimates remain statistically significant and negative using the alternate definitions of the dependent variable.

5.2.3 Controlling for Covariates

In an RD framework, it is not necessary to control for various pre-determined covariates as the treatment is independent of these covariates at the cutoff. However, it is possible to directly control

[30]The graphical illustration of the RD effect, and the validity of the balance test are available upon request.

for the covariates and estimate the local linear RD regression. We present the results in Appendix Table A-4, where additional controls are added as we move from column (1) to (2).

In column (1), we present the RD regression result with state and year fixed effects but without any controls [similar to Table 2, column (1)], and add constituency and candidate's characteristics (for e.g. growth of night lights in t-1, growth of night lights in t-2, electorate size, numbers voted, total turnout, ruling party, SC constituency, ST constituency, gender, education, asset, and liabilities) in column (2).

Our results, both statistically and substantively, do not change significantly after inclusion of these covariates. This provides further reassurance about the validity of the RD design in our context.

5.2.4 Top Coding

As previously noted, the night light data are censored at 63. While this is not an issue in less developed areas, this could be an issue in the wealthiest and most populated areas where we cannot observe any changes above an intensity of 63. Although this is unlikely to be systematically correlated with the accusation status of elected candidates, we directly address this in Appendix Table A-5. In Panel A, we drop any observations where the constituency-year pixel average is 63. In Panel B, we drop any constituency in which the average pixel intensity for any year is 63. In both cases, the results are both qualitatively and quantitatively similar to our earlier results.

5.3 Types and the Number of Charges

In this subsection we begin by examining whether the costs of electing accused politicians vary based on the underlying charges. If economic costs associated with electing criminal politicians are attributable to accused politicians, we would expect that both the type of charge and the number of underlying cases would matter. As noted in Section 4, we define the types of charges, first, by their seriousness, and second, if the charges are related to any loss to public exchequer (financial crimes).

We report the results by the type of charges in Table 3. In columns (1)-(4) of Panel A, we estimate the RD effect of electing candidates accused of financial charges on the yearly growth of night lights. In particular, we compare constituencies with a winner who is accused of at least 1 financial charge (and a loser who is not accused) to constituencies with a non-accused winner (and a loser who has at least 1 financial charge). Similarly, in columns (5)-(8), we examine the effect of electing candidates accused of only non-financial crimes where we compare constituencies with a winner who is accused of at least 1 crime but has no financial charge (and the runner up is not accused) to constituencies with a non-accused winner (and a runner up with at least 1 accusation but no financial accusation). We perform a similar analysis with serious charges in Panel B. We find consistent results: the type of charges matters. The coefficients for both financial and serious crimes are consistently significant and larger in magnitude (in absolute terms) than those estimated for any charge in column (1) of Table 2. In contrast, non-financial and non-serious charges are uniformly insignificant.

In Table 4, we examine the effect of the number of criminal cases. A larger number of cases can be viewed as a "higher intensity treatment" and, insofar as there are costs to filing false charges, may be more likely to represent "true" accusations. In columns (1)-(4) we present the results of the impact of electing a candidate with two or more charges, while in columns (5)-(8) present the results for candidates accused of five or more charges. Similar to the results with financial and serious charges, the number of criminal cases has a clear effect. The estimated coefficients are consistently significant and greater than the effect for any charge [column (1) in Table 2].[31] Taken together, the results from Tables 3 and 4 demonstrate that the characteristics of the candidate, specifically the candidate's accusation record, underlie the earlier results and the costs are potentially much higher of electing candidates accused of more severe types of crimes.

5.4 Timing of the Negative Effects

We next examine whether the effect of electing accused politicians varies across the years of an accused politician's term. That is, is the negative effect instantaneous (the first year of the term) and then remains constant throughout the term or does it rather accumulate over time?

In Table 5, we estimate the effect of elected an accused candidate separately for each year of the term. We also present the graphical illustration of the RD effect in Figure 4. The results show that the negative effect does not appear instantaneously. There are no effects in the first and second

[31]We estimate similar models using 2 and more financial (serious) charges and 5 or more financial (serious) charges. The results are qualitatively similar to those in Table 4. The table is available upon request.

year. In contrast, the estimated coefficients are significant in years 3 and 4, and the magnitude and significance of the coefficient increase over time.

We interpret these findings to mean that the election of criminally accused politicians does not instantaneously result in lower economic activity and the economic costs show up after a lag. For politicians to engage in corrupt behavior, they require collaboration with local bureaucrats (Iyer and Mani 2012). Consequently, a certain amount of time is necessary for corrupt politicians and bureaucrats to form a nexus and to engage in corrupt activity. For example, the effect of neglected public infrastructure, such as roads etc., may take some time to slow down economic activity.

6 Potential Mechanisms

6.1 State Characteristics

We now examine how the effect of electing criminally accused varies by different state characteristics. First, we consider the so-called "BIMAROU" states of Bihar, Jharkhand, Odisha, Uttar Pradesh, and Uttarakhand (The acronym BIMAROU is formed using the first letters of the word 'sick': in Hindi). These states are widely considered to be lagging behind in terms of economic development and have been singled out for being corrupt and generally dysfunctional. Second, we consider the set of states that the Ministry of Finance has classified as "Least Developed": Arunachal Pradesh, Assam, Bihar, Jharkhand, Odisha and Uttar Pradesh. Finally, using measures of corruption created by Transparency International India's (TII), drawn from the India Corruption Study of 2005, we classify states into "High-Corruption" and "Low-Corruption" states.

We present the results in Table 6, Panel A. The RD estimate for BIMAROU states are reported in column (1), while column (2) reports the estimate for Least Developed states, and finally column (3) reports the estimate for High-Corruption states. These effects are negative and statistically significant. The size of the coefficients for BIMAROU and Least Developed states are roughly one and a half times larger than our main result [Table 2, column (1)] and slightly more than double for High-Corruption states [The coefficient is -21.73 for our baseline result (all states), -49.47 for Least Developed states, and -51.29 for High-Corruption states]. However, the results are statistically insignificant for Non-BIMAROU, Relatively Developed and Low Corruption states as shown in Panel

B.[32]

Since there is substantial overlap between the states in the BIMAROU, Least Developed and High Corruption classifications, it is difficult to isolate one particular factor. Rather, we see that the effects of electing accused politicians are largely confined to certain states. One common feature of these states is the relatively weaker institutions whether judicial, police or political. Anecdotal evidence suggests that in states with stronger institutions, the actions of criminal politicians are more constrained. In contrast, those states with lower quality institutions have reputations (arising from many examples) of lawless behavior and general impunity for politicians and bureaucrats. Thus, a criminally accused politician is more likely to compromise governance where institutions are less developed.

6.2 Public Good Provision

Our above results are based on night lights as a measure of economic activity. We next examine an alternate measure of economic development and a proxy for public good provision: the number of incomplete road projects in the constituency. Highway construction in India is widely believed to be plagued with rampant corruption (WSJ, 2012) and frequently involves manipulation of the tenders and the process of procurement, often leading to lower quality and unfinished road projects.[33] For instance, a senior official reported that "road mafias" of contractors, engineers, the local police, civil servants, "and last but not least local politicians", conspire to keep prices on road contracts above market rates (Singh 2005).[34] In explaining corruption in the roads sector in the state of Jharkhand (a so-called BIMAROU state), a civil society activist told the New York Times that "the nexus of politicians, contractors and bureaucrats is very strong" (Polgreen 2010).[35]

We use data from the Pradhan Mantri Gram Sadak Yojana (PMGSY) program, a rural roads construction program which forms an integral part of the Government's poverty reduction strategy. Under PMGSY about 360,000 kms of rural roads are being constructed with a projected investment

[32] The "Relatively Developed" states defined by the Ministry of Finance are Goa, Haryana, Kerala, Maharashtra, Punjab, Tamil Nadu, and Uttarakhand. According to TII, "Low-Corruption" states are Kerala, Himachal Pradesh, Gujarat, Maharashtra, Punjab, West Bengal, Odisha, and Uttar Pradesh.

[33] `http://blogs.wsj.com/indiarealtime/2012/05/04/road-building-still-tarred-with-corruption/`.

[34] Eynde and Lehne (2015) finds evidence for political corruption in PMGSY. In particular, by matching local politicians to the contractors active their constituencies-based on their last names, they find that politicians appear to be intervening in the allocation of contracts on behalf of members of their own network.

[35] `http://www.nytimes.com/2010/06/29/world/asia/29india.html`.

of approximately US $14 billion for construction and US $9 billion for "upgradation" of existing tracks.

We present the RD effects from the local linear regression using an IK bandwidth by the type of accusation and state characteristics in Table 7. In Panel A, we report the results for the number of incomplete road projects for any accusation [similar to Table 2, column (1)], while Panel B reports the result by serious accusation, and Panel C by financial accusation. We find that the number of incomplete road projects increases in constituencies represented by criminally accused candidates and that the magnitude of the RD estimate is larger when we consider candidates accused of serious and financial charges. When we consider the impact of electing criminally accused candidates and candidates accused of serious and financial charges in constituencies belonging to BIMAROU, Least Developed, and High Corruption states, we find that the number of incomplete projects is approximately *three* times higher in High Corruption and Least Developed states as compared to the sample consisting of all states [columns (3)-(4) compared to column (1) in Panels A-C].[36]

These results complement our findings above with the night lights and are consistent with the view that criminal politicians are detrimental to economic development and public good delivery.

7 Rough Calculation of the Effect on GDP

In the previous sections, we interpret the change in the intensity of night lights as a proxy for economic activity. It is possible, however, to obtain a *rough* estimate of the direct effect on GDP by using the existing estimates elasticity of the GDP with respect to night lights. We use two alternate estimates of this elasticity: 0.3 estimated by Henderson et al. (2012) for a global sample of low and medium income countries and Bickenbach et al.'s (2013) India-specific district-level estimate of elasticity of 0.107. Since there are no reliable and systematic figures for growth at the constituency level, the latter represents the most disaggregated analysis of the elasticity for India. These two estimates provide us with an upper-and lower-bound respectively for our rough estimate of the effect on GDP growth.

[36]We present the graphical illustration of the RD effect for the number of incomplete road projects, and results from the balance test in Appendix Figure A-5. We repeat the same analysis for share of incomplete road projects and find similar result. The graphical illustration of the RD effect, the balance test, and the regression result are available upon request.

We present the "back of the envelop calculations" of GDP loss in Table 8. In column (1), we calculate the impact of electing an accused candidate in terms of GDP loss [i.e. for our main result as reported in Table 2, column (1)]. We present the same estimate for candidates accused of at least one financial charge in column (2), and finally for candidates accused of at least one serious charge in column (3).

Depending on the type of criminal accusation, we find estimates ranging from 2.3-6.5 percentage point lower GDP growth per year for our baseline estimates. India experienced very high growth during this period, ranging from 7.9-percent in 2003 to 9.8-percent in 2007. Since these are estimates of the yearly cost, the foregone growth over the entire term is larger as these losses compound over the full 5 year term. Using a more conservative estimate of 6-percent GDP growth as a measure of the average yearly constituency growth, this would imply that, on average, electing an accused candidate would result in a 5.61 to 5.86-percent GDP growth per year (as compared to the 6-percent otherwise).

These rough estimates of the cost in terms of GDP growth raise parallel questions in terms of the foregone poverty reduction and the effects on distribution. While the data do not exist to verify this, it is important to highlight that these are not just aggregate constituency-level costs; they are likely commensurate micro-level costs.

8 Conclusions

In this paper, we estimate the aggregate economic costs of electing criminally accused politicians at the constituency level using data on the intensity of night lights and sworn affidavits of candidates on their criminal background. We find a large negative and causal impact as the yearly growth of the intensity of night lights declines by roughly 22 percentage points for constituencies that barely elect a criminally accused candidate as compared with those that do not. The estimated effect is not just statistically significant but it is also economically meaningful; this effect translates into roughly a 2.3-percentage point lower GDP growth per year [Table 8, column (1)]. In addition, we also find important evidence of lower public good provision (insofar as road projects are a good proxy). While we only have aggregate constituency level outcomes, this forgone growth must also impact poverty reduction and other micro-level development outcomes. Consequently, the election of accused politicians and, more generally, lower quality politicians can have adverse effects along a variety of dimensions.

At the same time, we find large variation in the effects of electing accused politicians based on the nature of the criminal accusation of the elected representative (for example, serious or financial criminal charge vs. any criminal charge). In contrast, the election of candidates with only non-serious or non-financial charges have no impact on the subsequent growth. Additionally, this effect increases with the number of underlying criminal cases, which may serve as an indicator of 'intensity of treatment'. This variation not only highlights the importance of the precise accusations but also more reliably captures the criminality of politicians. This possibility is further underlined by the gradual accumulation of the costs over time. This result is consistent with prior studies which highlight the high turnover of bureaucrats after elections and the need for corrupt politicians and bureaucrats to identify each other.

Lastly, we find that the effects of electing accused politicians vary strongly across states. In particular, these costs are more pronounced in states with high corruption levels, lower levels of development and those which plausibly have weaker institutions ('BIMAROU' states). This suggests that even if lower quality politicians, such as accused politicians, are elected, the local context can substantially limit their detrimental effect. While we cannot identify the precise channels, we believe that institutions can play important role in constraining the actions of elected politicians. Additionally, in the spirit of Kremer's 'O-Ring' theory (1993), local growth might depend on complementary pieces including effective leadership. In certain contexts, such as less developed areas, there is no redundancy in leadership making the effects of inferior politicians more serious.

More broadly, our results are consistent with the literature on patronage democracies. One manifestation of a patronage democracy is the election of politicians who are able and willing to provide targeted benefits (Burgess et al. 2015). The benefits could be targeted based on caste, class or ethnicity. India is notable for its caste-politics (Chandra 2004). Therefore, instead of focusing on the overall outcomes (such as the delivery of public goods), voters focus on whether politicians can deliver targeted transfers to their specific group or caste. Not only are voters perhaps more likely to overlook accusations but these accusations might serve as a signal of the politician's willingness to use the office to reward fellow-group members (Chauchard 2014, Wade 1985). If true, our results suggest three consequences. First, this can result in the election of criminally accused candidates, and therefore potentially explains the ever increasing number of accused politicians who are elected in India. Second, the election of lower quality candidates in patronage democracies leads to lower aggregate growth. Third, these effects, however, are mediated by the local context (especially the institutional and legal context). In the more developed, and less corrupt states in our sample, the effects of the accused politicians were lower, perhaps due to the strength of institutions.

Although we study a particular context, lower quality politicians are believed to be pervasive in many developing countries. While the underlying cause is often context-specific and may range from caste-politics to tribal and ethnic voting, we believe that our analysis is suggestive for other contexts.

References

Abrahams, Alexei. 2015. "Hard Traveling: Redistributive Effects of Commuting Costs in the Second Palestinian Uprising." Working Paper.

Aidt, Toke, Miriam A. Golden, and Devesh Tiwari. 2012. "Criminality in the Indian National Legislature." Working Paper.

Akhmedov, Akhmed, and Ekaterina Zhuravskaya. 2004. "Opportunistic Political Cycles: Test in a Young Democracy Setting." *The Quarterly Journal of Economics*, *119* (4): 1301–38.

Alcantara, Manuel. 2008. " The Political Competence of Representatives in Latin America: An Analysis of Eleven National Cases." Annual Meeting of the American Political Science Association 2008.

Asher, Sam, and Paul Novosad. 2014. "Dirty Politics: Natural Resource Wealth and Politics in India." Working Paper.

Asher, Sam, and Paul Novosad. 2015. "Politics and Local Economic Growth: Evidence from India." Working Paper.

Association for Democratic Reform. 2012a. "Analysis of Candidates Contesting in Uttar Pradesh Assembly Elections 2012." Press Release on February 24th, 2012.

Association for Democratic Reform. 2012b. "Analysis of Criminal, Financial and other details of Newly Elected MLAs of the Uttar Pradesh Assembly Elections 2012." Press Release on March 11th, 2012.

Banerjee, Abhijit V., Selvan Kumar, Rohini Pande, and Felix Su. 2011. "Do Informed Voters Make Better Choices? Experimental Evidence from Urban India." Working Paper.

Banerjee, Abhijit V., Donald Green, Jeffrey McManus, and Rohini Pande. 2014. "Are Poor Voters Indifferent to Whether Elected Leaders are Criminal or Corrupt? A Vignette Experiment in Rural India." Working Paper.

Banerjee, Abhijit V., and Rohini Pande. 2007. "Parochial Politics: Ethnic Preferences and Politician Corruption." Working Paper.

Bardhan, Pranab, and Dilip Mookherjee. 2012. "Political Clientelism and Capture: Theory and Evidence from West Bengal." Working Paper.

Baskaran, Thushyanthan, Brian Min, and Yogesh Uppal. 2015. "Election Cycles and Electricity Provision: Evidence from a Quasi-Experiment with Indian Special Elections." *Journal of Public Economics*, *126*: 64–73.

Baum-Snow, Nathaniel and Turner, Matthew. 2012. Transportation and the Decentralization of Chinese Cities. Working paper.

Besley, Timothy, Rohini Pande, and Vijayendra Rao. 2005. "Politician Selection and Quality of Government: Evidence from South India." Economic Growth Center Discussion Paper Series. Center Paper no. 921.

Bhalotra, Sonia, and Irma Clots-Figueras. 2014. "Health and the Political Agency of Women." *American Economic Journal: Economic Policy*, *6* (2): 164–197.

Bhalotra, Sonia, Irma Clots-Figueras, and Lakshmi Iyer. 2013. "Path-Breakers: How Does Women's Political Participation Respond to Electoral Success?." IZA Discussion Paper No. 7771.

Bhavnani, Rikhil. 2012. "Using Asset Disclosures to Study Politicians' Rents: An Application to India." Paper presented at World Bank Annual Bank Conference on Development Economics, Washington, DC, May 7-8.

Bickenbach, Frank, Eckhardt Bode, Mareike Lange, and Peter Nunnenkamp. 2013. "Night Lights and Regional GDP." Kiel Working Paper No. 1888.

Bleakley, Hoyt and Jeffrey Lin. 2012. "Portage and Path Dependence." *The Quarterly Journal of Economics*, *127*: 587–644.

Broockman, David E. 2014. "Do Female Politicians Empower Women To Vote or Run for Office? A Regression Discontinuity Approach." *Electoral Studies*, (34): 1–15.

Burgess, Robin, Remi Jedwab, Edward Miguel, Ameet Morjaria, and Gerard Padro i Miquel. 2015 "The Value of Democracy: Evidence from Road Building in Kenya." *American Economic Review*, *105* (6): 1817–51.

Bussell, Jennifer. 2012. “Corruption and Reform in India: Public Services in the Digital Age.” New York: Cambridge University Press.

Callen, Michael, Saad Gulzar, Ali Hasanain, Yasir Khan, Arman Rezaee. 2015. “Personalities and Public Sector Performance: Evidence from a Health Experiment in Pakistan.” NBER Working Paper No. 21180.

Calonico, S., M. D. Cattaneo, and, R. Titiunik. 2014. “Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs.” *Econometrica*, *82* (6): 2295–2326.

Caughey, Devin, and Jasjeet Sekhon. 2011. “Elections and the Regression-Discontinuity Design: Lessons from Close U.S. House Races, 1942-2008.” *Political Analysis*, *19* (4): 385–408.

Chauchard, Simon. 2014. “Is ‘Ethnic Politics’ Responsible for ‘Criminal Politics’? A Vignette-Experiment in North India.” Working Paper.

Chemin, Matthieu. 2012. “Welfare Effects of Criminal Politicians: A Discontinuity-Based Approach.” *The Journal of Law & Economics*, *55* (3): 667–690.

Chen, Xi, and William D. Nordhaus. 2011. “Using Luminosity Data as a Proxy for Economic Statistics.” *Proceedings of the National Academy of Sciences*, *108* (21): 8589–8594.

Chopra, Vir K. 1996. “Marginal Players in Marginal Assemblies: The Indian MLA.” Hyderabad: Orient Longman.

Clots-Figueras, Irma. 2011. “Women in Politics: Evidence from the Indian States.” *Journal of Public Economics*, *95*: 664–690.

Clots-Figuerasa, Irma. 2012. “Are Female Leaders Good for Education?.” *American Economic Journal: Applied Economics*, *4* (1): 212–244.

Cole, Shawn. 2009. “Fixing Market Failures or Fixing Elections? Agricultural Credit in India.” *American Economic Journal: Applied Economics*, *1* (1): 219–250.

De Paola, Maria, and Vincenzo Scoppa. 2010. “Political Competition and Politician Quality: Evidence from Italian Municipalities.” *Public Choice*, *148*: 547–559.

Dell, Melissa. 2010. "The Persistent Effects of Peru's Mining Mita." *Econometrica*, *78* (6): 1863–1903.

Dinc, I. Serdar. 2005. "Politicians and Banks: Political Influences on Government-Owned Banks in Emerging Markets." *Journal of Financial Economics*, *77* (2): 453–479.

Doll C.N.H., Muller J.-P., Morley J.G. 2006. "Mapping Regional Economic Activity from Night-time Light Satellite Imagery." *Ecological Economics*, *57*: 75–92.

Eggers, Andrew C., Olle Folke, Anthony Fowler, Jens Hainmueller, Andrew B. Hall, and James M. Snyder, J 2014. "On The Validity Of The Regression Discontinuity Design For Estimating Electoral Effects: New Evidence From Over 40,000 Close Races." *American Journal of Political Science*, *00* (00): 1–16.

Elvidge, Christopher D., Kimberly E. Baugh, Eric A. Kihn, Herbert W. Kroehl, and Ethan R. Davis. 1997. "Mapping City Lights with Nighttime Data from the DMSP Operational Linescan System." *Photogrammetric Engineering and Remote Sensing*, *63* (6): 727–734.

Elvidge, Christopher D. et al. 2001. "Night-time Lights of the World: 1994–1995." *ISPRS Journal of Photogrammetry and Remote Sensing*, *56* (2): 81–99.

Eynde, Oliver Vanden and Jonathan Lehne. 2015. "Building Connections: Political Corruption and Road Construction in India." Working Paper.

Ferraz, Claudio. 2007. "Electoral Politics and Bureaucratic Discretion: Evidence from Environmental Licenses and Local Elections in Brazil." Working Paper.

Fisman, Raymond. 2001. "Estimating the Value of Political Connections." *The American Economic Review*, *91* (4): 1095–1102.

Fisman, Raymond, Florian Schulz, and Vikrant Vig. 2014. "Private Returns to Public Office." *Journal of Political Economy*, *122*, (4): 806–862.

Gelman, Andrew, and Guido, Imbens. 2014. "Why High-order Polynomials Should not be Used in Regression Discontinuity Designs." NBER Working Paper No. 20405.

Grimmer, Justin, Eitan Hersh, Brian Feinstein, and Dan Carpenter. 2011. "Are Close Elections Random?" Mimeograph. Harvard School of Government.

Hahn, J., Todd, P., and, Van Der Klaauw, W. 2001. "Identification and Estimation of Treatment Effects with a Regression Discontinuity Design." *Econometrica*, *69*: 201–209.

Harari, Mariaflavia. 2015. "Cities in Bad Shape: Urban Geometry in India." Working Paper.

Henderson, J. Vernon, Adam Storeygard, and David N. Weil. 2012. "Measuring Economic Growth from Outer Space." *American Economic Review*, *102* (2): 994–1028.

Hodler, Roland and Paul A, Raschkyl. 2014. "Regional Favoritism." *Quarterly Journal of Economics*, *124* (2): 995–1033.

Imbens, Guido W., and Thomas Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics*, *142* (2): 615–635.

Imbens, Guido, and Karthik Kalyanaraman. 2012. "Optimal Bandwidth Choice for the Regression Discontinuity Estimator." *The Review Of Economic Studies*, *79* (3): 933–959.

Iyer, Lakshmi, and Anandi Mani. 2012. "Traveling Agents: Political Change and Bureaucratic Turnover in India." *The Review of Economics and Statistics*, *94* (3): 723–739.

Jaffrelot, Christophe and Gilles Verniers. 2014. "Indian Elections : Reaching the End of the Democratic Tether." *Esprit*, *7*:75–87.

Jensenius, Francesca. 2013. "Who are Indian MLAs" in "Power, Performance and Bias: Evaluating the Electoral Quotas for Scheduled Castes in India." 55–71.

Khemani, Stuti. 2004. "Political Cycles in a Developing Economy: Effect of Elections in the Indian States." *Journal of Development Economics*, *73* (1), pp. 125–154.

Khwaja, Asim and Atif Mian. 2005. "Do Lenders Favor Politically Connected Firms? Rent Provision in an Emerging Market." *The Quarterly Journal of Economics*, *120* (4): 1371–1411.

Kremer, Michael. 1993. "The O-Ring Theory of Economic Development." *The Quarterly Journal of Economics*, *108* (3): 551–575.

Krishnan, K.P. and T.V. Somanathan . 2013. "The Civil Service in the 21st Century." in Devesh Kapur and Pratap Bhanu Mehta, eds., Building an Indian State for the 21st Century.

Lee, David S. 2008. "Randomized Experiments from Non-Random Selection in US House Elections." *Journal of Econometrics*, *142* (2): 675–697.

Lee, David and Thomas Lemieux. 2010. "Regression Discontinuity Designs in Economics." *Journal of Economic Literature*, *48* (2): 281–355.

Lee, David, Enrico, Moretti, and Matthew, Butler. 2004. "Do Voters Affect or Elect Policies? Evidence From the U.S. House." *The Quarterly Journal of Economics*, *119* (3): 807–859.

Martinez-Bravo, Monica. 2014. "Educate to Lead? The Local Political Economy Effects of School Construction in Indonesia." Working Paper.

McCrary, Justin. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics.*, *142* (2): 698–714.

Meyersson, Erick. 2014. "Islamic Rule and the Empowerment of the Poor and Pious." *Econometrica*, *82* (1): 229–269.

Brain, Min and Miriam Golden. 2014. "Electoral Cycles in Electricity Losses in India." *Energy Policy*, *65*: 619–625.

Nath, Anusha. 2014. "Bureaucrats and Politicians: How Does Electoral Competition Affect Bureaucratic Performance?" Unpublished Manuscript.

Nagavarapu, Sriniketh, and Sheetal Sekhri. 2014. "Less Is More? Implications of Regulatory Capture for Natural Resource Depletion." Working Paper.

Niehaus, Paul and Sandip Sukhtankar. 2013. "Corruption Dynamics: The Golden Goose Effect." *American Economic Journal: Economic Policy*, *5* (4): 230–269.

Porter, Jack. 2003. "Estimation in the Regression Discontinuity Model." Working Paper, Department of Economics, University of Wisconsin.

Robinson, James A., and Thierry Verdier. 2013. "The Political Economy of Clientelism." *The Scandinavian Journal of Economics*, *115* (2): 260–291.

Schady, Norbert R. 2000. "The Political Economy of Expenditures by the Peruvian Social Fund (FONCODES), 1991-95." *The American Political Science Review*, *94* (2): 289–304.

Singh, Sanjeet. 2005. "Fighting Corruption in Developing Countries: Dimensions of the Problem in India." Paper prepared for Good Governance, Institutional Integrity and Human Resources Management for Road Administrations, Session 2, Integrity in the Road Sector, World Road Association.

Storeygard, Adam. 2014. "Farther on Down the Road: Transport Costs, Trade and Urban Growth in Sub-Saharan Africa.", Working Paper.

Sukhtankar, Sandip, and Milan Vaishnav. 2015. "Corruption in India: Bridging Research Evidence and Policy Options." *Brookings-NCAER India Policy Forum 2014.*

Tiwari, Devesh. 2014. "Quality is in the Eye of the Beholder: Electoral Competition and Candidate Quality in India ." Unpublished Manuscript.

Uppal, Yogesh. 2009. "The Disadvantaged Incumbents: Estimating Incumbency Effects in Indian State Legislatures." *Public Choice*, *138* (1-2): 9–27.

Vaishnav, Milan. 2011a. "Caste Politics, Credibility and Criminality: Political Selection in India." Working Paper, South Asia Program, Carnegie Endowment for International Peace.

Vaishnav, Milan. 2011b. "The Market for Criminality: Money, Muscle and Elections in India." Typescript, Department of Political Science, Columbia University.

Vaishnav, Milan. 2011c. "Doing Good by Doing Bad: Why Indian Voters Support Criminal Politicians." Typescript, Department of Political Science, Columbia University.

van der Weide et. al 2015. "How Valuable is Market Access? Evidence from the West Bank." Unpublished Manuscript.

Wade, Robert. 1982. "The System of Administrative and Political corruption: Canal Irrigation in South India." *Journal of Development Studies*, *18* (3): 287–328.

TABLE 1-A

Descriptive Statistics of Dependent Variables

	RD Sample			Close Elections (difference in share <5%)		
Variables	Accused	Non-Accused	Difference	Accused	Non-Accused	Difference
Growth of Night Lights in t+1	2.04	2.43	-0.39	-1.79	1.36	-3.15
	(75.60)	(98.60)	(2.90)	(83.00)	(88.00)	(4.88)
Log of Night Lights in t+1	11.00	11.00	0.02	10.80	11.00	-0.13
	(1.84)	(1.83)	(0.06)	(1.75)	(1.83)	(0.10)
Proportion of Lit Villages in t+1	0.72	0.71	0.02	0.68	0.71	-0.035*
	(0.33)	(0.34)	(0.01)	(0.34)	(0.36)	(0.02)
No. of observations	1915	1701		611	616	
Number of Incomplete Road Projects	0.62	0.71	-0.10	0.60	0.68	-0.08
	(2.91)	(3.11)	(0.11)	(3.23)	(2.88)	(0.19)
No. of observations	1591	1396		518	549	

Notes: Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 1-B

Descriptive Statistics of Pre-determined Characteristics

	RD Sample			Close Elections (difference in share <5%)		
Variables	Accused	Non-Accused	Difference	Accused	Non-Accused	Difference
Growth of Light in Previous Year	16.00	30.80	-14.9**	27.90	35.40	-7.50
	(75.50)	(140.30)	(7.23)	(91.60)	(133.90)	(12.90)
Number of Incomplete Road Projects in Previous Year	0.091	0.1	-0.012	0.058	0.083	-0.024
	(0.48)	(0.52)	(0.04)	(0.35)	(0.33)	(0.04)
Log of Night Lights in Previous Year	10.90	10.80	0.04	10.70	10.80	-0.13
	(1.61)	(1.65)	(0.11)	(1.41)	(1.46)	(0.16)
Proportion Lit Villages Previous Year	0.72	0.72	0.00	0.67	0.73	-0.06
	(0.34)	(0.34)	(0.02)	(0.35)	(0.34)	(0.04)
Log Electorate Size in Previous Election	12.00	12.10	-0.072**	12.10	12.10	0.00
	(0.49)	(0.42)	(0.03)	(0.43)	(0.38)	(0.05)
Log Number Voted in Previous Election	11.50	11.60	-0.071***	11.60	11.60	-0.02
	(0.45)	(0.38)	(0.03)	(0.37)	(0.35)	(0.04)
Turnout in Previous Election	64.30	64.40	-0.10	63.10	64.40	-1.24
	(10.50)	(11.40)	(0.71)	(10.30)	(11.00)	(1.20)
Log Winners Assets	14.90	14.90	0.00	14.80	15.00	-0.25
	(2.10)	(1.90)	(0.13)	(2.27)	(1.56)	(0.22)
Log Winners Liability	7.46	6.87	0.59	8.27	7.33	0.94
	(6.59)	(6.44)	(0.43)	(6.50)	(6.34)	(0.72)
Log Runners-up Assets	14.85	14.71	0.14	14.78	14.92	-0.14
	(2.11)	(2.15)	(0.14)	(2.30)	(1.77)	(0.23)
Log Runners-up Liability	7.31	6.92	0.39	6.94	6.88	0.06
	(6.43)	(6.42)	(0.42)	(6.56)	(6.48)	(0.73)
Winners Gender in Previous Election	0.07	0.05	0.02	0.08	0.07	0.01
	(0.26)	(0.22)	(0.02)	(0.26)	(0.25)	(0.03)
Runners-up Gender in Previous Election	0.07	0.05	0.02	0.06	0.04	0.01
	(0.25)	(0.21)	(0.02)	(0.23)	(0.21)	(0.03)
Winners Education	2.22	2.48	-0.26***	2.36	2.37	-0.01
	(1.19)	(1.15)	(0.08)	(1.15)	(1.21)	(0.13)
Runners-up Education	2.35	2.21	0.14*	2.44	2.26	0.18
	(1.25)	(1.20)	(0.08)	(1.23)	(1.19)	(0.14)
SC Constituency	0.11	0.13	-0.03	0.11	0.08	0.03
	(0.31)	(0.34)	(0.02)	(0.31)	(0.26)	(0.03)
ST Constituency	0.05	0.05	0.00	0.03	0.03	0.01
	(0.22)	(0.22)	(0.01)	(0.18)	(0.16)	(0.02)
Ruling Party in Previous Election	0.52	0.57	-0.05	0.47	0.50	-0.03
	(0.50)	(0.50)	(0.03)	(0.50)	(0.50)	(0.06)
Incumbent in Previous Election	0.36	0.44	-0.081**	0.36	0.38	-0.02
	(0.48)	(0.50)	(0.03)	(0.48)	(0.49)	(0.05)
No. of observations	503	438		159	159	

Notes: Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 1-C

Descriptive Statistics of Criminal Accusations

	RD Sample				Close Elections (difference in share <5%)			
Variables	All	Winners	Runners-up	Difference	All	Winners	Runners-up	Difference
Any case	50.00	53.50	46.50	7.00***	50.00	50.00	50.00	0.00
	(0.50)	(0.50)	(0.50)	(0.02)	(0.50)	(0.50)	(0.50)	(0.04)
Any serious charge	37.40	40.20	34.60	6.00**	37.40	39.60	38.50	-2.20
	(0.48)	(0.49)	(0.48)	(0.02)	(0.49)	(0.48)	(0.49)	(0.04)
Any financial charge	18.00	19.40	16.50	3.00*	16.70	17.90	17.30	-1.26
	(0.38)	(0.40)	(0.37)	(0.02)	(0.38)	(0.37)	(0.38)	(0.03)
Multiple charge (>1)	24.30	27.30	21.30	6.00***	24.50	26.40	25.50	-1.89
	(0.43)	(0.45)	(0.41)	(0.02)	(0.44)	(0.43)	(0.44)	(0.03)
Avg. number of serious charges	1.33	1.47	1.19	28.00*	1.61	1.66	1.64	-5.35
	(3.57)	(3.43)	(3.70)	(0.16)	(5.18)	(4.66)	(5.65)	(0.41)
Avg. number of financial charges	0.18	0.19	0.16	3.00*	0.17	0.18	0.17	-1.26
	(0.38)	(0.40)	(0.37)	(0.02)	(0.38)	(0.37)	(0.38)	(0.03)
No. of observations	1882	941	941		636	318	318	

Notes: Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 2

Effect of Electing Criminally Accused Politicians on Growth of Night Lights

Dependent Variable	**Growth of Night Lights**			
	(1)	(2)	(3)	(4)
Criminally Accused	-21.73**	-17.87**	-23.96*	-12.44**
	(8.83)	(7.37)	(13.28)	(5.50)
State and Year Fixed Effects	YES	YES	YES	YES
Bandwidth Size	6.35	8.43	3.17	12.70
No. of observations	1,581	1,930	783	2,547
Polynomial order of control function	Local Linear			
Bandwidth Type	IK (h)	CCT	h/2	2h

Notes: Standard errors are clustered at the constituency level and given in parentheses. The dependent variable is the residual from the regression of year growth of night lights on state and year dummies. Criminally accused is a dummy variable that is 1 if a criminally accused candidate wins against a non-accused candidate and 0 if criminally accused candidate loses against a non-accused candidate. The RD estimates in column (1)–(4) are on a local linear regression using a triangular kernel.

Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 3

Effect of Electing Criminally Accused Politicians by Accusation Type

Dependent Variable	**Growth of Night Lights**							
Type of Accusation	Financial Charge				Non-Financial Charge			
Polynomial order of control function:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	PANEL A							
Local Linear	-39.39***	-24.50**	-38.62**	-21.60**	-9.51	-9.22	-15.76	-3.01
	(14.50)	(10.87)	(18.84)	(9.74)	(8.18)	(7.82)	(14.14)	(4.97)
Bandwidth Size	7.72	12.61	3.86	15.44	8.97	9.47	4.48	17.94
No. of observations	611	844	306	958	1,332	1,390	724	1,976
	PANEL B							
Type of Accusation	Serious Charge				Non-Serious Charge			
Local Linear	-33.20***	-27.46***	-29.40*	-18.66**	8.46	5.12	5.97	-0.18
	(11.76)	(9.54)	(15.92)	(7.53)	(6.53)	(5.67)	(7.97)	(4.63)
Bandwidth Size	5.49	7.72	2.75	10.99	7.38	8.83	3.69	14.75
No. of observations	1,070	1,372	506	1,729	422	502	226	707
State and Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Bandwidth Type	IK (h)	CCT	h/2	2h	IK (h)	CCT	h/2	2h

Notes: Standard errors are clustered at the constituency level and given in parentheses. The dependent variable is the residual from the regression of state and year dummies. Results displayed in each panel-column come from a separate regression. In Panel A, columns (1)–(4) criminally accused is 1 for a winner who is accused of a financial crime and ran against a non-criminally accused loser; and 0 for a loser who is accused of a financial crime and ran against a non-criminally accused winner. In columns (5)–(8) criminally accused is 1 for a winner who is accused of a non-financial crime and ran against a non-criminally accused loser; and 0 for a loser who is accused of a non-financial crime and ran against a non-criminally accused winner. In Panel B, columns (1)–(4) criminally accused is 1 for a winner who is accused of a serious crime and ran against a non-criminally accused loser; and 0 for a loser who is accused of a serious crime and ran against a non-criminally accused winner. In columns (5)–(8) criminally accused is 1 for a winner who is accused of a non-serious crime and ran against a non-criminally accused loser; and 0 for a loser who is accused of a non-serious crime and ran against a non-criminally accused winner.

Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 4

Effect of Electing Criminally Accused Politicians by Multiple Cases

Dependent Variable	**Growth of Night Lights**							
Type of Accusation	Multiple Cases (>= 2)				Multiple Cases (>= 5)			
Polynomial order of control function:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Local Linear	-30.91***	-23.23***	-28.49**	-18.50**	-42.63**	-38.64**	-51.84*	-28.53**
	(10.36)	(8.56)	(12.61)	(7.23)	(20.16)	(19.21)	(26.04)	(14.28)
Bandwidth Size	6.96	9.97	3.48	13.93	7.83	8.80	3.92	15.66
No. of observations	815	1,054	413	1,245	238	253	122	351
Bandwidth Type	IK (h)	CCT	h/2	2h	IK (h)	CCT	h/2	2h

Notes: Standard errors are clustered at the constituency level and given in parentheses. The dependent variable is the residual from the regression of state and year dummies. Results displayed in each column come from a separate regression. In columns (1)–(4) criminally accused is 1 for a candidate who is accused of 2 or more criminal cases; and 0 otherwise. In columns (5)–(8), criminally accused is 1 for a candidate who is accused accused of 5 or more criminal cases.

Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 5

Effect of Electing Criminally Accused Politicians by Year in Power

Dependent Variable	**Growth of Night Lights**			
	$Log(Y_{ist+1}) - Log(Y_{ist})$	$Log(Y_{ist+2}) - Log(Y_{ist})$	$Log(Y_{ist+3}) - Log(Y_{ist})$	$Log(Y_{ist+4}) - Log(Y_{ist})$
	(1)	(2)	(3)	(4)
Criminally Accused	-3.51	-10.43	-57.67*	-101.05**
	(7.91)	(10.41)	(30.77)	(41.36)
State and Year Fixed Effects	YES	YES	YES	YES
Bandwidth Size	9.71	7.3	4.69	5.61
No. of observations	552	442	289	331
Polynomial order of control function	Local Linear			
Bandwidth Type	Imbens-Kalyanaraman			

Notes: Standard errors are clustered at the constituency level and given in parentheses. The dependent variable is the residual from the regression of state and year dummies. Criminally accused is a dummy variable that is 1 if a criminally accused candidate wins against a non-accused candidate and 0 if criminally accused candidate loses against a non-accused candidate. The RD estimates in column (1)–(4) are based on a local linear regression using a triangular kernel. Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 6

Heterogeneous Effect of Electing Criminally Accused Politicians by State Characteristics

Dependent Variable	**Growth of Night Lights**		
Sample	BIMAROU States	Least Developed States	High Corruption States
	(1)	(2)	(3)
	PANEL A		
Criminally Accused	-45.75**	-49.47**	-51.29**
	(18.16)	(20.20)	(23.19)
Bandwidth Size	5.37	5.06	6.55
No. of observations	563	535	485
	PANEL B		
Sample	Non-BIMAROU States	Relatively Developed States	Low Corruption States
Criminally Accused	-4.46	0.84	-7.84
	(3.99)	(1.04)	(4.87)
Bandwidth Size	9.44	9.67	7.28
No. of observations	1,226	855	1,152
State and Year Fixed Effects	YES	YES	YES
Polynomial order of control function	Local Linear		
Bandwidth Type	Imbens-Kalyanaraman		

Notes: Standard errors are clustered at the constituency level and given in parentheses. The dependent variable is the residual from the regression of state and year dummies. Results displayed in each column come year from a separate regression. In Panel A, column (1) the BIMAROU states include Bihar, Chattisgarh, Jharkhand, Orissa, Uttar Pradesh, and Uttarakhand; column (2) include Least Developed states as ranked by Ministry of Finance. They are Arunachal Pradesh, Assam, Bihar, Jharkhand, Odisha and Uttar Pradesh. In column (3) we use states as ranked by Transparency International India (TII) on index of corruption. High Corruption states include Tamil Nadu, Haryana, Jharkhand, Assam, and Bihar. In Panel B, column (2) the Relatively Developed states include Goa, Haryana, Kerala, Maharashtra, Punjab, Tamil Nadu, and Uttarakhand; and Low Corruption states include Kerala, Himachal Pradesh, Gujarat, Maharashtra, Punjab, West Bengal, Orissa, and Uttar Pradesh. Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 7

Effect of Electing Criminally Accused Politicians on Incomplete Road Projects

Dependent Variable	**Number of Incomplete Road Projects**			
Sample Type	All States	BIMAROU States	Least Developed States	High Corruption States
	(1)	(2)	(3)	(4)
			Panel A	
Criminally Accused	0.73*	1.54**	1.60**	2.42**
	(0.43)	(0.75)	(0.79)	(1.20)
Bandwidth Size	4.34	4.6	4.61	4.3
No. of observations	920	446	446	306
			Panel B	
Serious Charge	0.93*	1.78**	1.81**	2.63*
	(0.51)	(0.85)	(0.88)	(1.35)
Bandwidth Size	4.04	4.32	4.46	4.31
No. of observations	690	373	385	268
			Panel C	
Financial Charge	0.69	1.92*	1.93*	1.5
	(0.57)	(1.08)	(1.08)	(1.07)
Bandwidth Size	5.33	4.51	4.52	4.76
No. of observations	389	165	169	173
State and Year Fixed Effects	YES	YES	YES	YES
Polynomial order of control function	Local Linear			
Bandwidth Type	Imbens-Kalyanaraman			

Notes: Standard errors are clustered at the constituency level and given in parentheses. The dependent variable is the residual from the regression of state and year dummies. Criminally Accused, Serious Charge, and Financial Charge is constructed as in Tables 2 and 3. The definitions of BIMAROU, Least Developed, and High Corruption states remains same as Table 6.
Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE 8

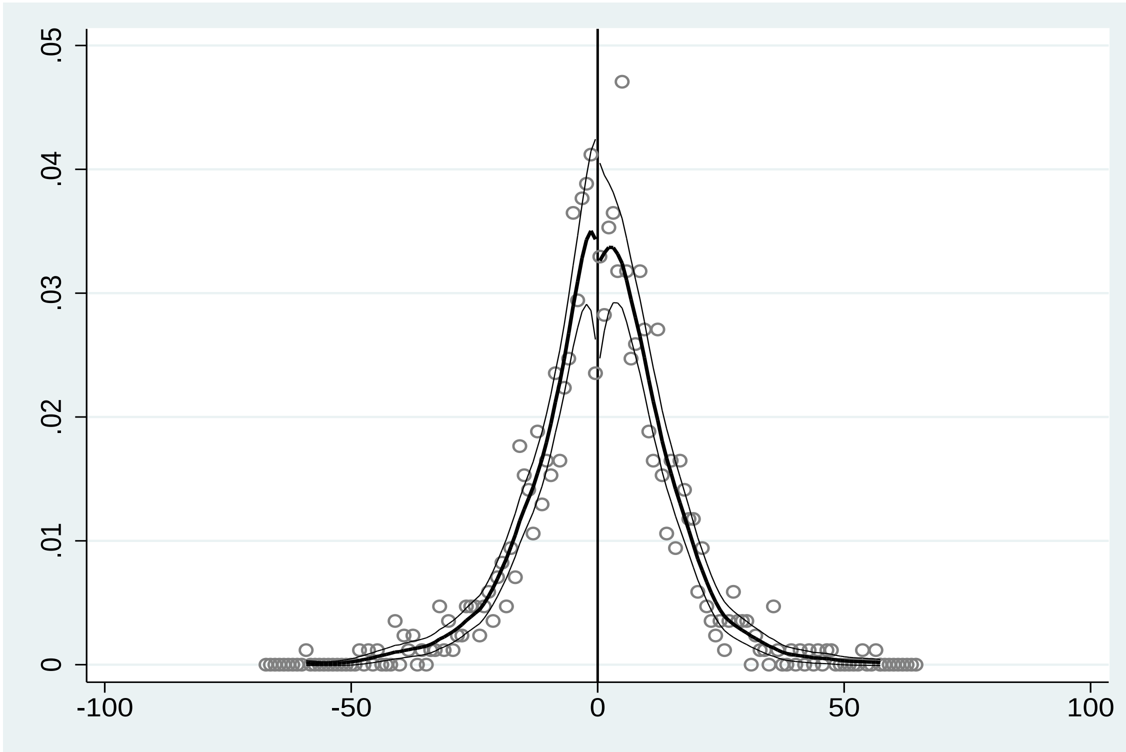
Effect of Electing Criminally Accused Politicians on Constituency GDP Growth

Dependent Variable	**Growth of Night Lights**		
	Baseline Estimate	Financial Charge	Serious Charge
	(1)	(2)	(3)
RD Estimate	-21.73**	-39.39***	-33.20***
Polynomial order of control function	Local Linear		
Bandwidth type	Imbens-Kalyanaraman		
Estimated Effect on GDP Growth Rate (in percentage points)			
Using global average (Henderson et al. 2014)	-6.5	-11.8	-10.0
Using India-specific average (Bickenback et al. 2014)	-2.3	-4.2	-3.6
Assuming 6% growth - what would growth look like?			
Upper Bound	5.61	5.29	5.40
Lower Bound	5.86	5.75	5.78

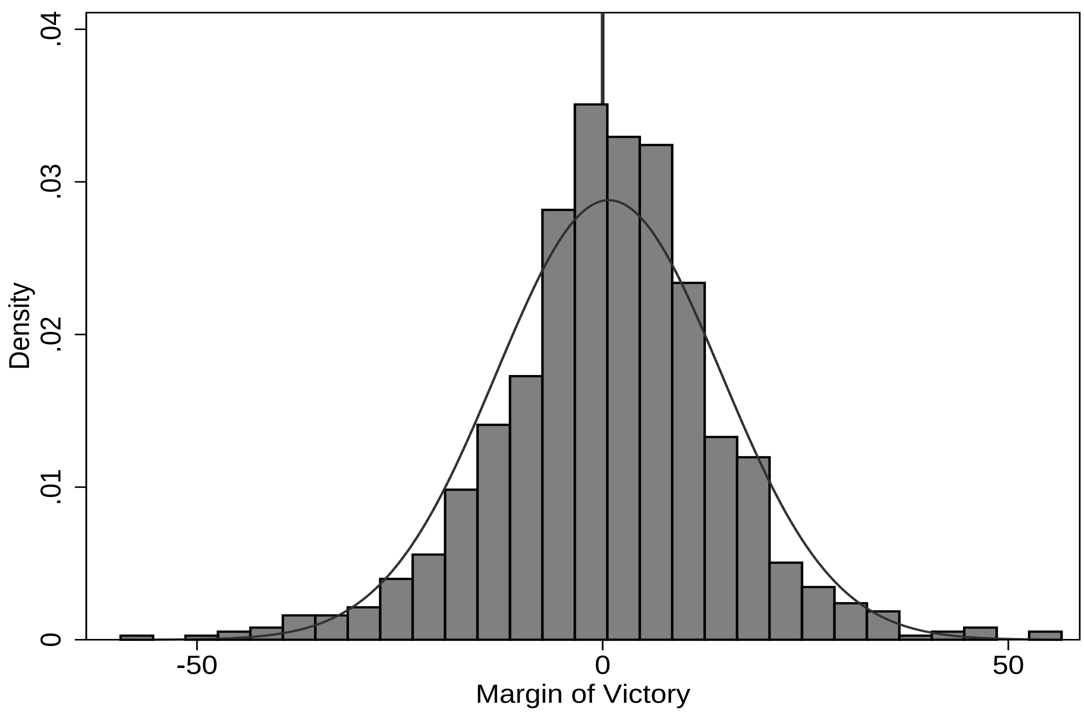
Notes: The definition of the main explanatory variable changes across the columns: criminally accused, financial criminal accusation, and serious criminal accusation. Column (1) reports the RD estimate for criminally accused from Table 2, Column (1). In Column (2), we report the RD estimate for any financial charge from Column (1) of Table 3 in Panel A, while we report the RD estimate for any serious charge from Column (1) of Table 3 in Panel B in Column (3) of this table. The upper-bound uses an elasticity of 0.3. The lower-bound uses an elasticity of 0.107.

Figure 1

Continuity of the Victory Margin between Criminally Accused and Non-Accused



(a) McCrary Test

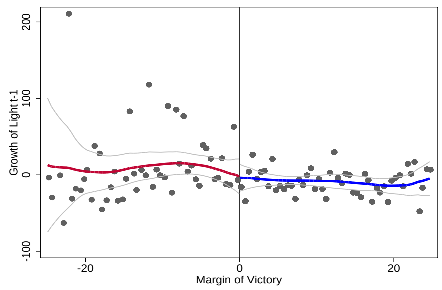


(b) Density of the Victory Margin

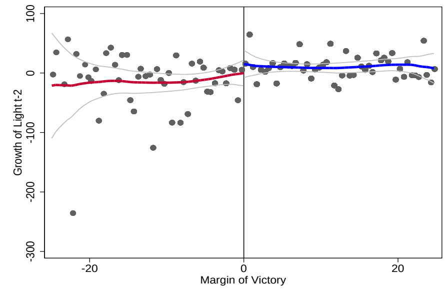
The forcing variable is the margin of victory of a criminally-accused candidate. Negative values are the difference in the vote shares of a criminally-accused runners-up and a non-accused winner. Positive values are the differences in the vote shares of a criminally-accused winner and a non-accused runners-up. The estimated size of discontinuity in margin of victory (log difference in height) is -0.061 (se = 0.2).

Figure 2

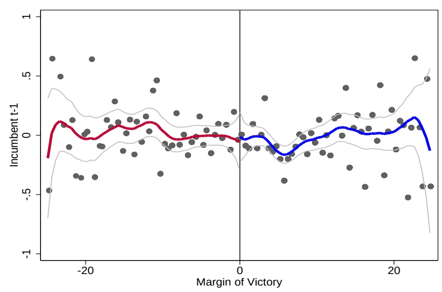
Pre-determined Characteristics: Continuity Checks



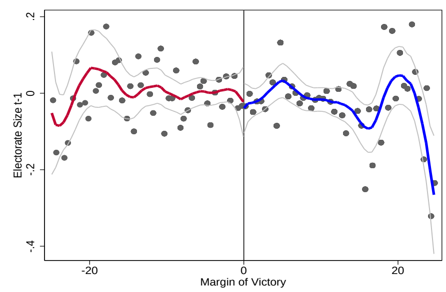
(a) Growth of Light t-1



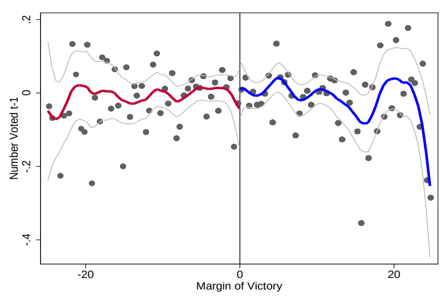
(b) Growth of Light t-2



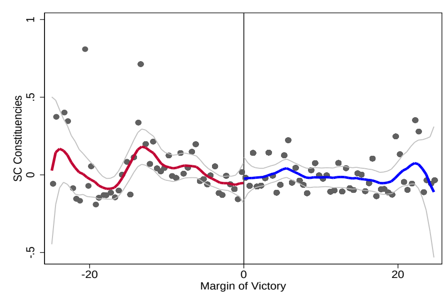
(c) Incumbent in t-1



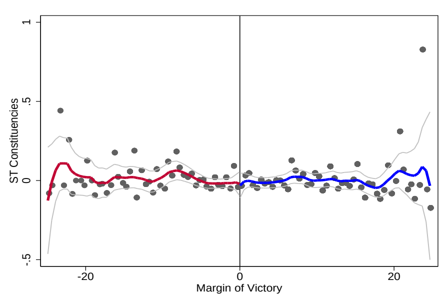
(d) Electorate Size t-1



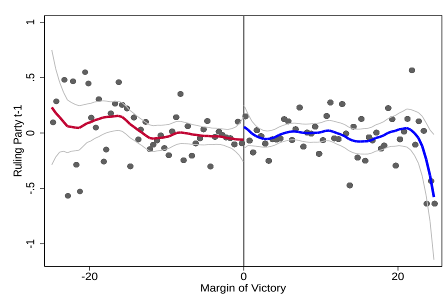
(e) Number Voted t-1



(f) SC Constituencies



(g) ST Constituencies

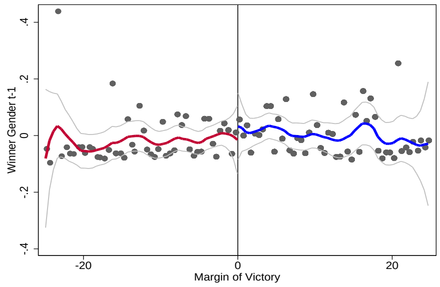


(h) Ruling Party t-1

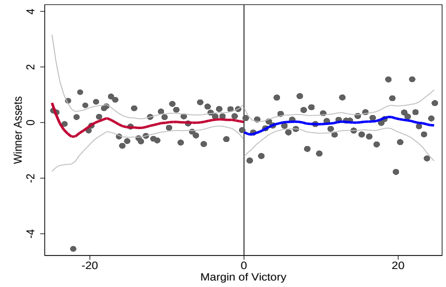
The forcing variable is the margin of victory of a criminally-accused candidate. Negative values are the difference in the vote shares of a criminally-accused runners-up and a non-accused winner. Positive values are the differences in the vote shares of a criminally-accused winner and a non-accused runners-up. Each variable on the y-axis is net of state and year fixed effects. The dots in the scatter plot depict the averages over each successive interval of 0.5% of margin of victory. The curves are local linear regressions fit separately for positive and negative margins of victory using a triangular kernel and an optimal bandwidth calculator as suggested in Imbens and Kalayanaraman (2012). The confidence intervals are the 95% confidence intervals plotted using standard errors that are clustered at the constituency level.

Figure 2

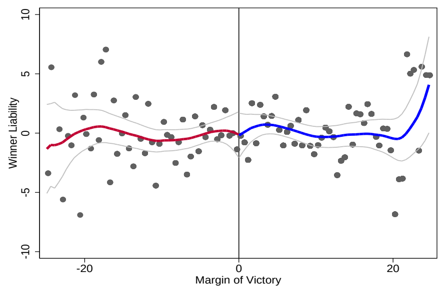
Pre-determined Characteristics: Continuity Checks (contd)



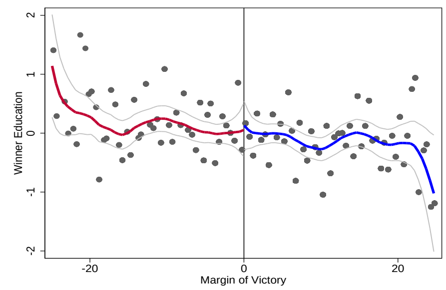
(i) Winners Gender t-1



(j) Winners Assets



(k) Winners Liabilities

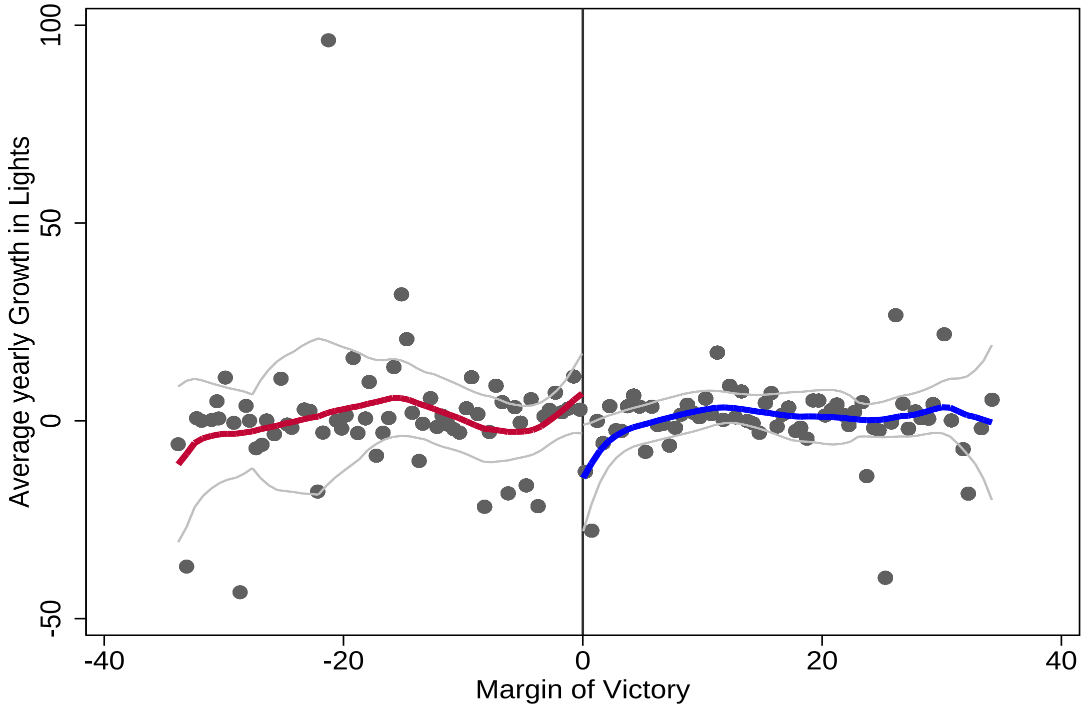


(l) Winners Education

The forcing variable is the margin of victory of a criminally-accused candidate. Negative values are the difference in the vote shares of a criminally-accused runners-up and a non-accused winner. Positive values are the differences in the vote shares of a criminally-accused winner and a non-accused runners-up. Each variable on the y-axis is net of state and year fixed effects. The dots in the scatter plot depict the averages over each successive interval of 0.5% of margin of victory. The curves are local linear regressions fit separately for positive and negative margins of victory using a triangular kernel and an optimal bandwidth calculator as suggested in Imbens and Kalayanaraman (2012). The confidence intervals are the 95% confidence intervals plotted using standard errors that are clustered at the constituency level.

Figure 3

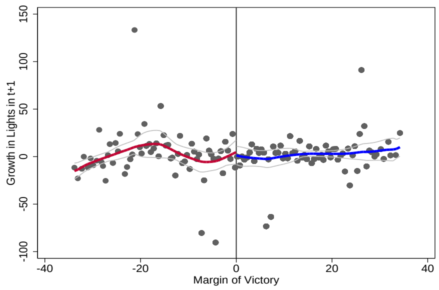
Effect of Electing Criminally Accused Politicians on Growth of Night Lights



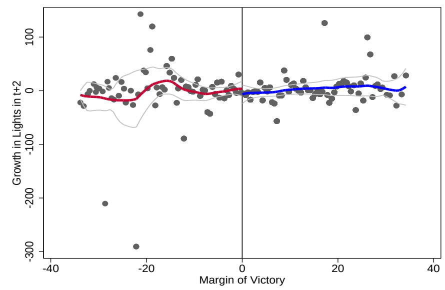
The forcing variable is the margin of victory of a criminally-accused candidate. Negative values are the difference in the vote shares of a criminally-accused runners-up and a non-accused winner. Positive values are the differences in the vote shares of a criminally-accused winner and a non-accused runners-up. The variable on the y-axis is the growth of night lights net of state and year fixed effects. The dots in the scatter plot depict the average of growth of night lights over each successive interval of 0.5% of margin of victory. The curves are local linear regressions fit separately for positive and negative margins of victory using a triangular kernel and an optimal bandwidth calculator as suggested in Imbens and Kalayanaraman (2012). The confidence intervals are the 95% confidence intervals plotted using standard errors that are clustered at the constituency level.

Figure 4

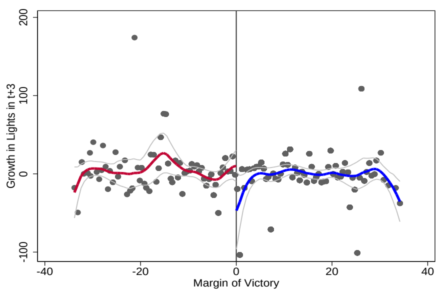
Effect of Electing Criminally Accused Politicians by Year in Power



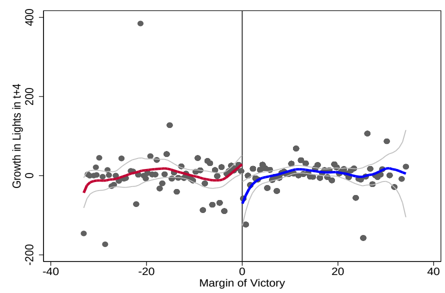
(a) Growth in Lights in t+1



(b) Growth in Lights in t+2



(c) Growth in Lights in t+3



(d) Growth in Lights in t+4

The forcing variable is the margin of victory of a criminally-accused candidate. Negative values are the difference in the vote shares of a criminally-accused runners-up and a non-accused winner. Positive values are the differences in the vote shares of a criminally-accused winner and a non-accused runners-up. Each variable on the y-axis is net of state and year fixed effects. The dots in the scatter plot depict the averages over each successive interval of 0.5% of margin of victory. The curves are local linear regressions fit separately for positive and negative margins of victory using a triangular kernel and an optimal bandwidth calculator as suggested in Imbens and Kalayanaraman (2012). The confidence intervals are the 95% confidence intervals plotted using standard errors that are clustered at the constituency level.

TABLE A-1

State Name and Year of Elections

State Name	Number of Constituencies	Election Years*
Arunachal Pradesh	60	1999, **2004**, 2009
Assam	126	2001, **2006**, 2011
Bihar	243	2000, **2005**, 2010
Goa	40	2002, **2007**, 2012
Gujarat	182	2002, **2007**, 2012
Haryana	90	2000, **2005**, 2009
Himachal Pradesh	68	2003, **2007**, 2012
Jharkhand	81	**2005**, 2009
Kerala	140	2001, **2006**, 2011
Maharashtra	288	1999, **2004**, 2009
Manipur	60	2002, **2007**, 2012
Meghalaya	60	2003, **2008**, 2013
Nagaland	60	2003, **2008**, 2013
Odisha	147	2000, **2004**, 2009
Punjab	117	2002, **2007**, 2012
Tamil Nadu	234	2001, **2006**, 2011
Tripura	60	2003, **2008**, 2013
Uttar Pradesh	403	2002, **2007**, 2012
Uttarakhand	70	2002, **2007**, 2012
West Bengal	294	2001, **2006**, 2011
Total	2823	

Notes: Bold years are the first election in each state in which candidates were required to file affidavits detailing criminal and financial background.

TABLE A-2

Sensitivity Analysis of RD Specification by Polynomial order of Control Function

Dependent Variable	**Growth of Night Lights**			
Type of Accusation	Criminally Accused			
Polynomial order of control function:	(1)	(2)	(3)	(4)
Linear	-21.73**	-17.87**	-23.96*	-12.44**
	(8.83)	(7.37)	(13.28)	(5.50)
Bandwidth Size	6.35	8.43	3.17	12.70
No. of observations	1,581	1,930	783	2,547
Quadratic	-27.03**	-27.34**	-23.97	-22.48**
	(13.46)	(11.81)	(18.14)	(9.26)
Bandwidth Size	6.35	13.27	3.17	12.70
No. of observations	1,581	1,930	783	2,547
Cubic	-20.69	-25.45*	-17.76	-28.53**
	(17.00)	(15.06)	(22.00)	(12.65)
Bandwidth Size	6.35	13.27	3.17	12.70
No. of observations	1,581	1,930	783	2,547
Quartic	-22.69	-20.21	-10.65	-26.33*
	(19.87)	(17.98)	(24.03)	(15.26)
Bandwidth Size	6.35	13.27	3.17	12.70
No. of observations	1,581	1,930	783	2,547
State and Year Fixed Effects	YES	YES	YES	YES
Bandwidth type	IK (h)	CCT	h/2	2h

Notes: Standard errors are clustered at the constituency level and given in parentheses. Results displayed in each panel-column come from a separate regression that also controls for state and year fixed effects. Criminally accused is a dummy variable that is 1 if a criminally accused candidate wins against a non-accused candidate and 0 if criminally accused candidate loses against a non-accused candidate.

Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE A-3

Effect of Electing Criminally Accused Politicians by Alternate Dependent Variables

Dependent Variable	**Log(Night Lights)**	**Prop of Lit Villages**	**Avg Growth over the Election Term**
	(1)	(2)	(3)
Criminally Accused	-0.91**	-0.110	-23.30**
	(0.37)	(0.08)	(9.35)
State and Year Fixed Effects	YES	YES	YES
Bandwidth Size	3.16	2.16	5.71
No. of observations	783	500	371
Polynomial order of control function		Local Linear	

Notes: Standard errors are clustered at the constituency level and given in parentheses. Results displayed in each column come year from a seperate regression. The dependent variable is the residual from the regression of state and year dummies. Log(Night Lights) is the intensity of night lights in levels; Proportion of Lit Villages is the proportion of villages with detectable levels of light output in a given year; and Average Growth over the Election Term is the growth of night lights averaged over the election term of the candidate.

Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE A-4

Controlling for Covariates

Dependent Variable	**Growth of Night Lights**	
	(1)	(2)
Criminally Accused	-21.73**	-21.29**
	(8.83)	(8.49)
State and Year Fixed Effects	YES	YES
Covariates	NO	YES
Bandwidth Size	6.35	6.35
No. of observations	1,581	1,581
Polynomial order of control function	Local Linear	
Bandwidth Type	Imbens-Kalyanaraman	

Notes: Standard errors are clustered at the constituency level and given in parentheses. In Columns (1)–(2), we add additional controls. In Column (2) we add constituency and candidate's characteristics. They are growth of night lights in t-1, growth of night lights in t-2, electorate size, numbers voted, total turnout, ruling party, SC constituency, ST constituency, gender, education, asset, and liabilities.
Asterisks denote significance levels (*=.10, **=.05, ***=.01)

TABLE A-5

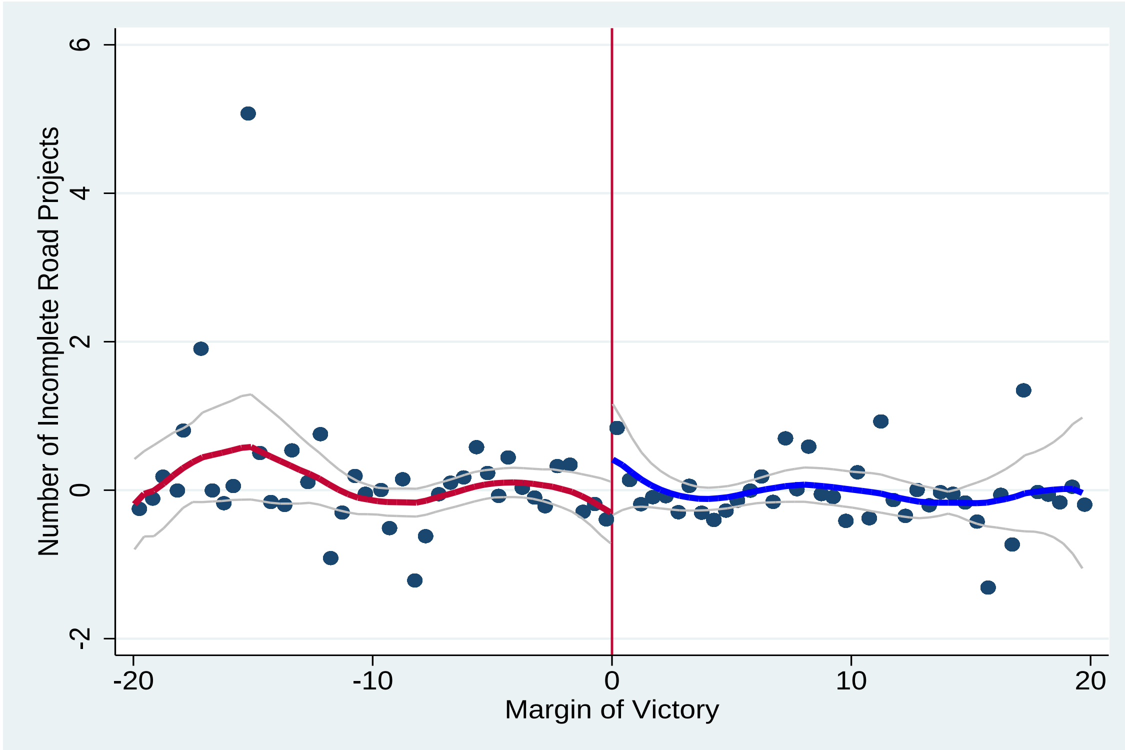
Does Top Coding Matter?

Dependent Variable	**Growth of Night Lights**			
	(1)	(2)	(3)	(4)
Panel A: Dropping observations where constituency-year pixel = 63				
Criminally Accused	-21.75**	-17.59**	-23.79*	-12.34**
	(8.82)	(7.27)	(13.27)	(5.50)
Bandwidth Size	6.36	8.61	3.18	12.73
No. of observations	1,567	1,928	779	2,522
Panel B: Dropping any constituency with pixel intensity = 63				
Criminally Accused	-21.77**	-17.81**	-23.83*	-12.39**
	(8.83)	(7.33)	(13.26)	(5.52)
Bandwidth Size	6.35	8.5	3.18	12.7
No. of observations	1,561	1,910	779	2,511
State and Year Fixed Effects	YES	YES	YES	YES
Polynomial order of control function	Local Linear			
Bandwidth Type	IK (h)	CCT	h/2	2h

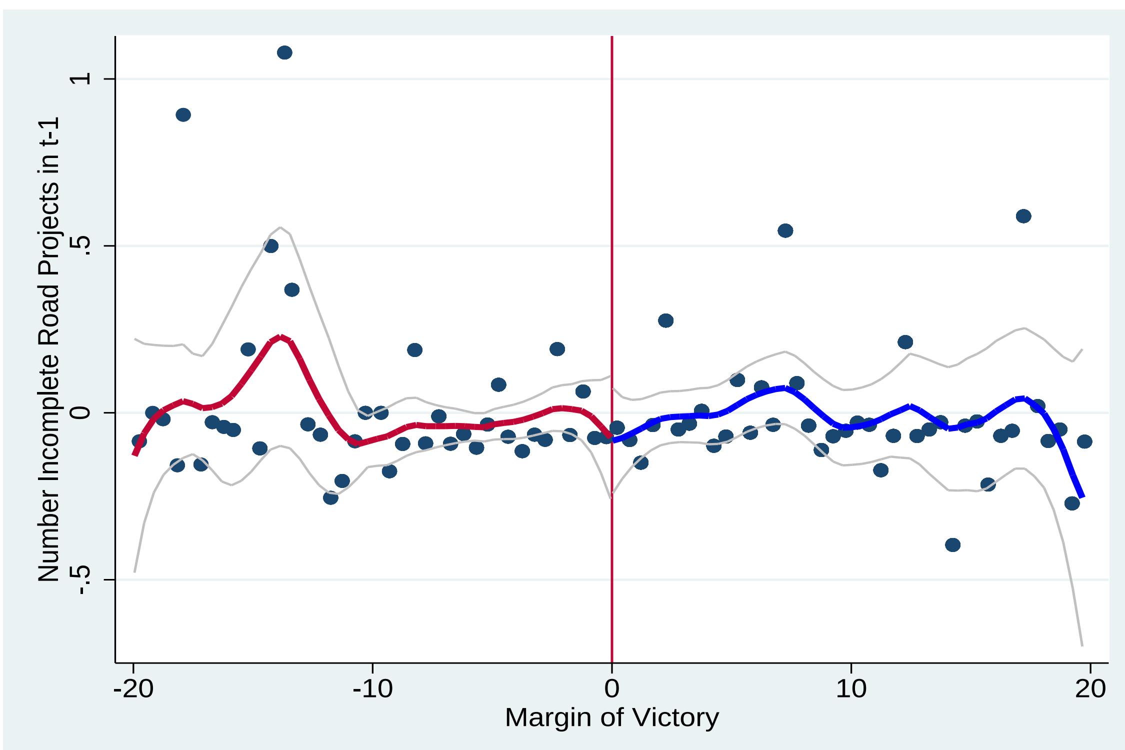
Notes: Standard errors are clustered at the constituency level and given in parentheses. Asterisks denote significance levels (*=.10, **=.05, ***=.01)

Figure A-5

Effect of Electing Criminally Accused Politicians on Number of Incomplete Road Projects



(a) RD Effect



(b) Balance Test

The forcing variable is the margin of victory of a criminally-accused candidate. Negative values are the difference in the vote shares of a criminally-accused runners-up and a non-accused winner. Positive values are the differences in the vote shares of a criminally-accused winner and a non-accused runners-up. The variable on the y-axis is number of incomplete road projects net of state and year fixed effects. The dots in the scatter plot depict the average number of incomplete road projects over each successive interval of 0.5% of margin of victory. The curves are local linear regressions fit separately for positive and negative margins of victory using a triangular kernel and an optimal bandwidth calculator as suggested in Imbens and Kalyanaraman (2012). The confidence intervals are the 95% confidence intervals plotted using standard errors that are clustered at the constituency level.

Figure A-6
Share of Criminally Accused Candidates in India