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Crime, incentives and political effort: Evidence from India

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ABSTRACT

Political representatives with criminal backgrounds are considered a great problem in many countries. In India, public disclosure of the large share of politicians currently facing criminal charges has sparked a heated public debate and emerging literature assessing the causes and effects. We develop two hypotheses based on our theoretical considerations. Based on the coding of published affidavits and a comprehensive set of three proxies to measure effort in the 14th Lok Sabha over the 2004–2009 legislative period, we put these hypotheses to an empirical test. Members of the parliament (MPs) facing criminal accusations exhibit on average about 5% lower attendance rates and lower utilization rates in a local area development fund, but only insignificantly lower parliamentary activity. In line with our hypotheses, these differences decline in the development level of the constituency - a proxy for higher rent-seeking possibilities and monitoring intensity. We argue and demonstrate why these negative relations should constitute an upper bound estimate of the causal effect, and show that even under conservative assumptions the effect is unlikely to be caused by unaccounted selection-bias.

1. Introduction

Many democracies, particularly in developing countries, face problems with regard to the personal background of some politicians. The world's largest democracy, India, is only one example. In 2014, a year where we possess reliable data, roughly 34% of the recently elected members of the lower house of the Indian parliament faced pending criminal charges.¹ A significant share of the Members of Parliament (MPs) are accused of serious criminal acts ranging from rape to murder.² The Indian and international media have reported on this issue, and it is widely believed that it poses a threat to the Indian democracy in general and the constituencies represented by those individuals specifically. While these problems supposedly exist in a wide range of countries worldwide, the unavailability of data has impeded analysis of their consequences. We outline why India is a unique case that allows us to assess the economic and developmental consequences of parliamentarians with pending criminal charges holding public office.

While there is some anecdotal evidence that electoral constituencies represented by MPs with pending serious criminal charges

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¹ See: <http://timesofindia.indiatimes.com/news/Every-third-newly-elected-MP-has-criminal-background/articleshow/35306963.cms?> The members of the 16th Lok Sabha were elected during the 2014 national elections held between 7th April and 12th May 2014. The 16th Lok Sabha commenced on 4th June 2014. The situation has not changed significantly based on newer data. (accessed between March and November 2013).

² Roughly 21% of the members face serious charges such as murder, rioting, theft, kidnapping, rape, etc., see: <http://adrindia.org/research-and-reports/election-watch> (accessed between March and November 2013).

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remain underdeveloped and such members tend to underperform in terms of their effort in parliament, systematic empirical evidence remains scant. Can criminal charges explain the variations in parliamentarians' performance in parliament and the development of their electoral constituencies: The existing theoretical literature models the effect of electoral accountability on economic policy choices (Besley and Case, 1995; Dutta and Gupta, 2014), of compensation on policy outcomes (Besley, 2004), and of outside income opportunities on self-selection and behavior (Gagliarducci et al., 2011).

Political competition could mitigate the effect of a candidate with a criminal background if political parties can make credible commitments to the voters and are thereby able/likely to discipline their party members (e.g., Besley and Coate, 1997). In the Indian context, however, Keefer and Khemani (2004) argue that the biggest obstacle for development is that promises made by the political parties lack credibility. Under such circumstances, elections only serve the purpose of removing the incumbent in the constituency from power (or keeping the incumbent in power). This paper ties in with the emerging literature on electoral competition in the context of India (e.g., Keefer and Khemani, 2009). For instance, Aidt et al. (2011) investigate why parties field criminal candidates, and Dutta and Gupta (2014) analyze competition between candidates that include criminals.

In order to empirically test our hypotheses, we use details about the criminal records of the candidates available due to a 2003 Indian Supreme Court judgment that made it mandatory for every candidate contesting state and national elections to provide sworn affidavits detailing their background. These include details not only about their personal, educational, and financial particulars, but also detailed information on any criminal charges they had faced, the status of their criminal cases and any pending charges against them.³

We use criminal charges as a signal of whether a MP is a criminal type, and refer to those with pending charges as “criminal MPs” in the rest of the paper. To alleviate potential bias in our estimates caused by measurement errors due to individual false charges, we run all regressions both with a binary variable *Criminal(a)*, coded one for those with a least one charge, and *Criminal(b)*, which takes the value one only for those with more than one charge. We study the 14th Lok Sabha (2004–2009) instead of the 15th (2009–2014), or a combination of both, because electoral boundaries of constituencies changed between elections, making it impossible to match constituencies. Using the 14th Lok Sabha allows us to control for confounding factors such as past electoral performance or party strongholds which are crucial in determining MP effort.

We want to assess whether elected candidates with criminal records differ from their colleagues with regard to effort. Various measures have been used in the literature to gauge MP effort. Instead of picking just one factor, which might not capture differences between MPs comprehensively, we use three measures, each capturing a different facet of MP behavior. First, we use attendance rates (or absenteeism respectively) as, for example, in Besley and Larcinese (2011); Gagliarducci et al. (2010, 2011) and Mocan and Altindag (2013). Second, we make use of MPs' effort in parliament by considering information on the number of questions they asked and their participation in debates (cf. Mocan and Altindag, 2013; Arnold et al., 2014). While both of these measures capture effort, one might question their relevance for the electorate. On the other hand, we doubt to some degree that economic outcomes like consumption or economic activity at an aggregate level can be directly linked to MP activity, which makes it more problematic to draw a causal link to MP effort.

Thus, we draw on Keefer and Khemani (2009) and use the cumulative utilization rate of the MP Local Area Development Scheme (MPLADS). The fund is intended for the development of electoral constituencies and offers several advantages in making the effort that MPs undertake on behalf of their constituencies observable. Advantages are, for example, that the amounts available are identical across constituencies, implementation of projects requires substantial effort on behalf of the MP, projects are clearly identifiable with the MP's name, and that the considerable media coverage makes it likely that voters learn about the effort. Attracting public spending for one's electorate is a widespread phenomenon, that is also relevant in more developed political systems (Gehring and Schneider, 2018). More details are outlined below.

Our paper is most closely linked to two other papers that are also evaluating the impact of Criminal actors in Indian politics. Chemin (2012) suggests electing criminal MPs results in very large declines of up to 19% in consumption of the most vulnerable parts of society. The surprisingly large estimates might be driven by two potential problems with the regression discontinuity design. First, the study needs to rely on relatively few observations and a rather large bandwidth in the 178 constituencies in which a criminal faced a non-criminal candidate in 2004. Second, it matches political constituencies to administrative districts, but both overlap in non-trivial ways. Prakash et al. (2014) use data at the state assembly level over the 2004–2008 period and various development proxies based on nighttime light as their outcome. They also use a regression discontinuity design and find very large effects: declines in luminosity of around 22 percentage points. Our study is complementary to these existing studies, as it examines measures more closely linked to the MP and estimates the average treatment effect instead of the local average treatment effect measured in the RD designs.

The observation level in our study is the 543 constituencies, which each elect one MP in a first-past-the-post-system. We find that across specifications, *Criminal(a)* is related to around 5% higher absenteeism rates. Parliamentary activity, on the other hand, does not differ significantly between criminal and non-criminal MPs in our baseline specification. In line with our hypotheses, criminal MPs show both significantly lower attendance rates and less parliamentary activity in constituencies that are economically underdeveloped. The reason for the latter could lie in better monitoring of politicians' behavior in the more developed constituencies and/or the greater attractiveness of these constituencies for criminals in terms of rent-extraction possibilities. The coefficients become more negative when we use our *Criminal(b)* indicator instead. MPLADS fund utilization is lower for criminal MPs in general, but only significant for *Criminal(b)*.

The most difficult challenge for identification is that MPs are obviously not randomly assigned to constituencies. Omitted variable

³ The court also asked the Election Commission of India to make it mandatory to publicize the information about electoral candidates provided through these affidavits. Voters can now use this information to make better informed electoral choices.

bias arises if expected effort generally differs in those constituencies that have voted for a criminal MP based on unobserved factors we cannot fully control for. The direction of the bias is not *ex ante* trivial. It would be negative, if, for example, less developed electorates are more likely to elect a criminal and it would be harder to recommend an MPLADS project in such a constituency. A positive bias would occur, if less developed constituencies were more likely to elect a criminal and exhibit higher MPLADS utilization rates because it is easier to identify necessary projects. Given the common *a priori* assumption of a negative relationship between Criminal and effort, we are less concerned about a potential upward bias, because our estimated negative coefficient would then be an upper bound of the true effect.

As part of our strategy to identify the causal effect of having a criminal background on the outcome variables, we control for observable pre-determined constituency-specific and MP-specific factors. Second, fixed effects for major states ensure that the results are not driven by factors specific to certain Indian regions such as, for example, economic underdevelopment. Fixed effects for major parties make sure that the coefficients are not driven by unobserved factors specific to a party or related to being part of the government or opposition. Third, we show that the control and treatment groups are strongly balanced, and that our results remain unchanged when using alternative matching estimators. Overall, we argue and provide evidence that selection effects are more likely to shift our coefficients towards zero.

As our approach cannot explicitly exploit exogenous variation, we accept that omitted variable bias remains a concern. Still, it seems a relatively mild concern regarding the direction of causality. We use selection-on-observables to show why the negative coefficient of *Criminal(b)* on MPLADS utilization rates is more likely an upper bound estimate of the negative effect. With the methods developed in [Altonji et al. \(2005\)](#) and [Oster \(2017\)](#), we then demonstrate that on average selection bias (on unobserved factors) would have to be between two and sixteen times greater than selection on observed factors to fully explain the negative relationship between Criminal and attendance rates or MPLADS utilization. If the reader accepts this logic and the notion that the observable controls give an idea about the direction of the biasing effect of unobservables, then we provide robust evidence that, on average, criminal MPs indeed behave in a way that is detrimental to their constituency.

The paper is structured as follows: the next section summarizes the relevant literature. Subsequently, we describe our theoretical framework about the impact of a criminal background on political effort depending on the development of the electoral constituency. Section 4 describes our data, methods and estimation strategy. Section 5 presents the results and discussion and section 6 concludes and gives policy implications.

2. Literature

This paper ties in with the growing literature concerned with the political system of India (e.g., [Besley and Burgess, 2002](#)). In particular, the criminalization of politics has received significant interest. One strand of literature is focused on understanding why political parties field candidates with criminal backgrounds in elections in the first place and why voters elect these candidates. [Aidt et al. \(2011\)](#) argue and find that political parties in India field candidates with criminal backgrounds when faced with intense electoral competition. [Vaishnav \(2012\)](#) finds no evidence in favor of electoral competitiveness increasing the likelihood to field criminal candidates when examining 28 state elections.⁴

This does not mean that voters are unable to recognize criminality as a potential problem. [Dutta and Gupta \(2014\)](#) reveal that voters actually punish candidates with criminal charges that contest in elections. This suggests that the intensity of monitoring by voters might be decisive: if monitoring costs are too high, fewer constituents might be aware of candidates' characteristics. These findings are contradicted by [Banerjee et al. \(2009\)](#), whose field experiment shows that voters in rural India tend to vote on caste (ethnic) considerations even after being provided with information on the criminal background details of the contesting candidates. Positive preferences for certain characteristics that criminals possess need to be accounted for, as they can be enough to trump anti-corruption efforts and help criminal candidates to get elected.

A second extant strand of literature focuses on the consequences of electing candidates with criminal backgrounds. In their state-level analysis covering a period of over 20 years, [Kapur and Vaishnav \(2011\)](#) show the ominous nexus between the candidates contesting elections (especially the ones with a criminal background) and the construction sector. Often, candidates contesting in elections stash their illegal money and assets with builders in real estate in return for them to fund the candidates' election campaigns. They argue that, as a result of this *quid pro quo* deal, growth declines, as measured by a reduction in the consumption of cement and other indispensable raw materials.

The studies that come closest to ours are [Chemin \(2012\)](#) and [Prakash et al. \(2014\)](#). [Chemin \(2012\)](#) examines the relationship between parliamentarians in India with criminal backgrounds and consumption levels in their respective constituencies. He finds that poverty levels tend to be higher and consumption lower in constituencies which are represented by MPs with a criminal background. [Prakash et al. \(2014\)](#) examine the economic consequences of Members of State Legislative Assembly (MLAs) having a criminal background. Using satellite nighttime light data across 20 major states in India, they find that electoral constituencies represented by MLAs with a criminal background have a 22% lower level of economic development as measured by plausible identification strategy. In our data, however, the RDD design does not seem appropriate, as there are significantly more criminals barely winning an election to the right of the threshold. This bunching is problematic, but plausible since one reason for fielding a criminal is that he can rig close

⁴ However, he finds that the personal wealth of criminal candidates is correlated with the criminal status of the candidates, suggesting that they could have accumulated wealth over their years of criminal activities. A similar correlation is found by [Paul and Vivekananda \(2004\)](#), who review the information provided by the candidates contesting in the 2004 national elections in India.

elections.

In our view, the main drawback of prior research is the lack of explicit transmission mechanisms through which these (extremely large) effects are realized. By considering how criminal backgrounds explain differences in parliamentarians' efforts in parliament and/or the varying efforts towards developing their constituencies, we provide direct evidence on such mechanisms. Potential reasons why the negative welfare effects measured using consumption data and nighttime light data are so large could be that (i) the local average treatment effect identified by the RDDs is much larger than the average effect, (ii) the bandwidths were not small enough to eliminate existing biases or (iii) identifying assumptions might have been violated. It is plausible that criminal politicians generate negative welfare effects since they have lower incentives to invest in (political) effort. In contrast to those prior approaches, we study more direct measures of effort that can clearly be influenced by politicians over the course of one electoral term.

We also refer to the empirical and theoretical literature on factors determining the performance of legislators. For instance, [Svaleryd and Vlachos \(2009\)](#) and [Strömberg \(2001, 2004\)](#) study the effect of political competition on economic outcomes. [Fisman et al. \(2014\)](#), [Ferraz and Finan \(2011\)](#), and [Snyder and Strömberg \(2010\)](#) examine political competition, media coverage and rent seeking behavior of incumbent politicians.

3. Theory and hypotheses

When we think about criminals in politics, it is important to distinguish between the following three key dimensions of criminal politicians:

1. The dimension “candidate – party”: why do parties field criminal candidates?
2. The dimension “candidate – opponents – voters”: why would it be rational for a noncriminal and a criminal candidate to enter and compete in the same election? How do these candidates' optimal electoral strategies differ?
3. The dimension “candidate/incumbent – office”: do non-criminal and criminal incumbents have different incentives to run for office? What drives candidates' effort for their constituency, once they are elected into office?

The order in which the dimensions are listed above can be considered chronological. All three dimensions are part of a complex equilibrium process that yields both criminal and non-criminal candidates – the observed outcome. However, to the best of our knowledge, there exists no model that incorporates all three dimensions. The questions of interest for us are: how do criminal and non-criminal candidates' optimal electoral strategies differ? and what are the implications for candidates' political effort? In the following, we discuss these questions. Our argumentation provides a theoretical frame for our empirical work but, as the main focus of the paper is empirical, we do not mathematically formalize all of our arguments.

[Aidt et al. \(2011\)](#) model why parties choose to field a criminal candidate in the first place. In their model, parties bear reputational costs when fielding criminal candidates. Aidt et al. show that political parties in India field candidates with criminal backgrounds when faced with intense electoral competition. One reason brought forward to explain this is that these candidates possess certain electoral advantages such as money and muscle power, which they can use to influence the electoral outcome in poorer electorates and under conditions of low voter literacy levels. In the words of Aidt et al.: “criminal candidates could enjoy access to an organized network of persons who can target swing or opposition voters with threats or the actual use of violence, thereby intimidating some of them not to show up at the polls.” In fact, there is evidence of clientelist vote-trading in India (see, e.g., [Anderson et al., 2015](#)). As a consequence, parties are more willing to bear the reputational costs of criminal candidates in close elections.

However, the model of [Aidt et al. \(2011\)](#) says nothing about candidates' incentives to exert effort. A reasonable way to incorporate effort is a Persson and Tabellini-style model (cf. [Persson and Tabellini, 2000](#)), in which voters care about fear (i.e., voter intimidation, vote buying, etc.) and policy. Both fear and policy require effort. Criminal politicians have lower marginal costs for spreading fear and are usually assumed to have relatively higher marginal costs of influencing policy. To make our arguments more precise, suppose the cost of spreading fear for a candidate $i = c, nc$ ($c =$ criminal, $nc =$ non-criminal) in a constituency with development level d is $f_i(e)$ and the cost of policy is $p_i(e)$. The ratio of marginal costs for candidate i is then f'_i/p'_i and the assumption is that this ratio is lower for criminal candidates: $f'_c/p'_c < f'_{nc}/p'_{nc}$. As suggested by Aidt et al., poor and illiterate voters are more vulnerable to intimidation and vote buying. On the other hand, parties bear reputational costs when fielding criminal candidates (that may vary by party). Given this setup, the following equilibrium is possible: a nc candidate, from a party with high costs of fielding criminals, makes a “high policy effort” offer to voters, and his criminal opponent, from a party with low costs of fielding a criminal, makes a “high fear” offer. This implies that the criminal candidate, if elected, will exert less political effort than the non-criminal candidate. Hence, our first hypothesis is:

Hypothesis 1. *Criminal incumbents exert less political effort than non-criminal incumbents.*

Further, it is plausible that the ratio of marginal costs increases in the level of development of the constituency d , and increases faster for criminal than non-criminal candidates.⁵ On the one hand, it is more expensive to spread fear among wealthier people. On the other hand, voters are better informed (higher literacy rates, more political interest). Hence, MP's political effort and activities are better monitored. This means that high policy effort is more rewarded and high fear is more penalized. There is convincing empirical evidence that a significant part of the Indian population is generally opposed to political criminality ([Banerjee et al., 2014](#)). While other reasons

⁵ Formally: $\frac{df_i/p_i}{dd} > 0$ and $\frac{\partial^2 f_i/p_i}{(\partial d)^2} > \frac{\partial^2 f_{nc}/p_{nc}}{(\partial d)^2}$.

Table 1
Descriptive statistics.

	Count	Mean	SD	Min	Max	Mean -Normal	Mean - Criminal
<i>Effort measure</i>							
Attendance rate	394	0.71	0.17	0.06	0.96	0.725	0.670
Parliamentary activity	394	0.82	0.80	0.00	4.38	0.810	0.847
MPLADS utilization	439	105.65	20	60.50	260.00	106.016	104.451
<i>Criminal Record</i>							
Criminal(a)	439	0.24	0.43	0	1		
Criminal(b)	439	0.07	0.25	0	1		
<i>Electoral Competitiveness</i>							
Party stronghold (3time winner)	439	0.21	0.41	0	1	0.224	0.154
Winning margin (2004)	439	0.12	0.10	0.00	0.61	0.127	0.110
PC is reserved for minority SC or ST	439	6.55	0.33	4.02	7.35	6.550	6.535
No. of voters	439	0.24	0.43	0	1	0.239	0.231
<i>Monitoring</i>							
Economic development (log sum of night light intensity)	439	9.78	1.03	6.36	11.58	9.796	9.706
Literacy rate	439	55.69	12.05	25.86	85.43	56.051	54.545
Voter turnout (2004)	439	0.60	0.12	0.33	0.92	0.605	0.570
<i>Candidate characteristics</i>							
Candidate age (at election)	439	52.17	10.60	26	77	52.731	50.375
Education of MP	439	1.61	0.74	0	2	1.642	1.500
Experience in parliament	439	0.72	1.09	0	3	0.773	0.548
Gender	439	0.92	0.27	0	1	0.916	0.942
Log of net assets	439	16.13	1.18	1.61	20.33	16.147	16.093

Notes: Descriptive statistics were calculated for the maximum regression sample size. MPLADS utilization can be on average higher than 100% after 2004 since not all funds were used up initially and consequently partly transferred in the next period. We later control for unused funds and omit extreme values.

contribute to the elections of criminals, [Dutta and Gupta \(2014\)](#) find that, all else equal, voters penalize candidates with criminal charges. Consequently, in equilibrium, criminal candidates will make a better policy offer to voters in constituencies with higher development level. This leads to our second hypothesis:

Hypothesis 2. *The difference in political effort between criminal and non-criminal incumbents decreases in the development level of the constituency.*

Another argument that supports [Hypothesis 2](#) refers to the third dimension outlined above and is based on rent-seeking considerations. The literature often assumes that politicians are governed by rent-seeking behavior. There is evidence that rent-seeking of politicians in power is by no means limited to developing countries. For instance, see [Kauder and Potrafke \(2015\)](#) for a documented case of rent extraction on the part of elected members of parliament in the German state of Bavaria. If more developed constituencies offer more or larger rent-seeking opportunities for criminal MPs, this increases their utility from re-election, and can also narrow the effort gap between criminal and non-criminal types, as criminals strive to maintain access to this resource.⁶

4. Data and empirical strategy

We use various data sources to construct a constituency-level data set for the 14th Lok Sabha legislative period. This section describes our proxies for the effort level chosen by the incumbent MP, our measure for whether an MP is of the criminal type, as well as the proxies for electoral competitiveness, monitoring intensity and candidate characteristics ([Table 1](#) provides descriptive statistics). We use two different measures to gauge MPs' parliamentary performance, and one indicator to assess constituency development (proposed by [Keefer and Khemani, 2009](#)). All three have the advantage that they can be directly attributed to actual MP effort.

4.1. Dependent Variables

To avoid selectively picking outcome measures that support our hypothesis, we rely on measures that were employed and established in the literature.

4.1.1. Attendance rates and parliamentary activity

The first measure of MP effort, *attendance rates* in parliament, has several advantages. First, it is easily quantified and clearly interpretable. Second, it has been widely used in the literature, for example [Gagliarducci et al. \(2010, 2011\)](#) and [Besley and Larcinese](#)

⁶ An earlier working paper version of this paper provides a formal probabilistic voting model, which builds on the model in [Besley and Burgess \(2002\)](#) with retrospective voters in the spirit of [Ferejohn \(1986\)](#) (cf. [Gehring et al., 2015](#)). This model shows that rent-seeking behavior and a lower impact of fear on the re-election probability can imply [Hypothesis 2](#). However, the model neither incorporates the dimension "party - candidate" nor pre-electoral competition.

(2011). Mocan and Altindag (2013) and Fisman et al. (2014) use it as their main measure of effort in studies on MPs in the European Parliament. To avoid confusion, note that some papers use the absenteeism rate instead, which is of course simply the inverse of our measure. Our variable *attendance rate* is scaled between zero and one. The lowest rate is 6% for former prime minister Atal Bihari Vajpayee from Uttar Pradesh, who has no criminal background, but was already 76 years old at the date of election. The highest rates are 96% for two MPs from Bihar and Manipur, both without any criminal charges against them. The simple correlation between *Criminal(a)* and *attendance rate* is -0.14 .

Attendance rate is a useful measure, but does not necessarily correlate with a MP's work attitude and intensity once they are actually present. Therefore, we complement our analysis of MP effort by including a second measure of MP *parliamentary activity* within the parliamentary sessions in the 14th Lok Sabha period. The literature has suggested using speeches, oral contributions and private initiatives (cf. Arnold et al., 2014) or the number of questions asked (Mocan and Altindag, 2013). We combine two categories, the number of questions asked and the number of debates in which MPs have participated, into one indicator named *parliamentary activity*.⁷

An overall indicator is better suited to capture the total effort exerted by a MP that is observed by the voters. Still, it is worth mentioning that parliamentary activity (speeches and debate participation) is typically concerned with policy-based legislation that has little electoral value. Attendance rates, in contrast, might have a higher electoral value since they can be better observed by the voters and clearly measured. The electoral value of parliamentary activity is not clear: it could be even negative if informed voters consider the performance of a politician as weak. From a theoretical point of view, this means that politicians who intend to make a "high policy effort" offer to voters are supposed to show high attendance rates, whereas this is not necessarily true for parliamentary activity. It is thus less clear whether the incentives differ sufficiently in that regard to cause differences between criminal and other MPs.

We normalize each indicator by dividing it by its standard deviation to achieve comparability, and then take the simple average. This aggregate indicator ranges between 0, for nine MPs who have neither asked any questions nor participated in any debate, to 5.03 for C.K. Chandrappan from Trichur constituency in the state of Tamil Nadu who asked 415 questions and participated in 113 debates. There is no obvious correlation between *parliamentary activity* and criminal type, the simple correlation with *Criminal(a)* is a mere 0.003. The data for both *attendance rates* and *parliamentary activity* exerted by MPs is taken from the Association for Democratic Reforms (ADR), an independent body that has been researching Indian elections since 1999.

4.1.2. MPLADS utilization rate

Our third dependent variable is intended to capture MPs' efforts in developing their respective electoral constituencies.⁸ We follow Keefe and Khemani (2009) who use utilization of Member of Parliament Local Area Development Scheme (MPLADS) funds meant for development of MPs' constituencies. Introduced in 1993, each MP can receive about 20 million Indian rupees (about 160,000 \$US) annually to spend on developmental activities or on local public works recommended by the MP of that constituency. Any unspent money under the MPLADS fund accumulates and is carried forward to the next fiscal year until an MP leaves office or is replaced by a new MP.

The utilization of MPLADS funds is a particularly well-suited proxy for the actual effort exerted by an MP to develop her constituency for several reasons. First, it is noteworthy that the amount allocated annually to each MP is independent of an MP's constituency and its economic resources; and hence provides the same initial conditions to all MPs. Utilizing these funds to develop the constituency is purely the responsibility of the respective MPs, who must identify and initiate the public works. MPs themselves need to personally exert considerable effort to conduct these developmental works. They must work in tandem with various government bureaucrats at the national and state level to first identify viable projects, obtain permissions and sanctions for the work, and monitor the work once the project is undertaken.

There is ample anecdotal evidence about the required effort on behalf of the local MPs. Mohanty (2004) documents various bureaucratic hurdles faced by the MPs right from the stage of project identification to implementation stage. It requires constant deliberations and consultations with the district collectors, ministries at the state, and central government level. MPs need to invest time to overcome problems, with respect to delays in sanctioning, releasing funds from the district collectors, preparing estimates, and acquiring land.

Second, MPLADS funds and activities are periodically audited. The MPLADS accounts of all MPs are maintained by the respective district authorities and various project implementing agencies of the government. The project implementing agencies furnish (i) a works completion report which include the details on the progress of the project; (ii) the funds utilization report which contains the financial accounts of the work; (iii) a utilization certificate; and (iv) transfer the unspent amount to the district authority within one month of completion. All documents obtained by the Ministry of Statistics and Program Implementation (MOSPI hereafter) can be audited every year. The auditing covers the administration of funds, expenditure incurred with the implementing agencies, and other financial aspects related to the projects. Funds diverted for prohibited and inadmissible projects are flagged by the auditors. The Parliamentary committee on MPLADS, the MOSPI, and the state government meet at least once a year to review the audit accounts, reports, and certificates and discuss objections and overall implementation of the scheme.

⁷ We do not use the proposition of private member bills. In the Indian parliamentary system, the speaker, in consultation with the leader of the house (i.e. the Prime Minister), allots two and half hours on every Friday in each parliamentary session to discuss private bills proposed by the MPs. So far, 14 private members bills have been passed in Indian parliament, all before 1970. During the 14th Lok Sabha period a total of 300 private members bills were moved by various MPs, of which a mere 4% were actually discussed (see Kumar, 2010).

⁸ Note that electoral constituencies in India do not overlap with the boundaries of districts in the states. There is no easily applicable procedure to aggregate districts up to constituencies.

Third, the MPLADS permits MPs to take clear credit for the public work projects undertaken as a result of this scheme. This provides incentives for MPs to make use of this scheme as part of their re-election strategy. Keefer and Khemani (2009) describe that from the early 2000s on, voter awareness of the MPLADS reached a level high enough to make our assumption of a significant share of informed voters that are aware of their MP's effort as demonstrated by their use of the scheme credible. MPLADS utilization is hence a measure of MP effort that, unlike consumption (Chemin, 2012) and nighttime light intensity (Prakash et al., 2014), can directly be traced back to the MP's actions.⁹ Information costs are much lower than for the other two dependent variables. This leads us to expect a smaller or no further interaction effect with the monitoring variables.

We follow Keefer and Khemani (2009) and use the cumulative utilization rate, which is the actual spending incurred by an MP in her constituency as a percentage of the total amount released under the MPLADS each year during the 14th Lok Sabha period. Unfortunately, the data from the annual reports on the MPLADS on actual spending are not publicly available for the year 2005. Thus, our cumulative utilization rate includes the data from 2006 to 2008.¹⁰

As the distribution of the dependent variables deviates from a normal distribution to some degree (see Online-Appendix Fig. 1), we replicate our baseline models with the propensity score matching estimator that requires fewer distributional assumptions. We also re-estimate our models without potential outliers in the robustness section.

4.1. Variable of interest

Our key independent variable is the criminal background of MPs. With the Indian Supreme Court's 2003 order, all candidates contesting state or national elections in India are required to submit a sworn affidavit detailing their criminal background to the Election Commission of India. These are available to voters on the Election Commission's website.¹¹ They provide information about the number and types of criminal accusations against a candidate. If candidates or MPs are convicted of a crime, they are no longer allowed to run for office, and thus not contained in our sample. We make use of this information to create a binary variable *Criminal(a)* which takes the value of 1 if an MP has any accusation against them and 0 otherwise.

Our aim is to measure an MP's true type CR^* , but we observe only the information in the affidavits, which represent a noisy signal $CR = CR^* + u$. Criminal charges provided in the affidavits constitute a good but imperfect proxy. First, some of the cases registered against the candidates could be politically motivated. Still, Vaishnav (2012) argues that information disclosure about criminal charges is obligatory only if the judge deems the charge worthy of a criminal proceeding after a thorough investigation by the local police.¹² Second, candidates may underreport their criminal charges. However, the potential political costs of under-reporting are high as discovery creates the risk of opposition parties using their omissions as the fuel for a smear campaign or being prosecuted and disqualified from being a member of parliament. Thus, there is likely measurement error in CR, but there is no reason to expect it to be systematic, and it, hence, attenuates the estimated coefficient towards zero.

We can partly circumvent these problems by coding *Criminal(b)*, which only takes the value of 1 if an MP has more than one charge against him. This alleviates concerns about mistaking innocent MPs for criminals insofar as it is less likely that all charges are unfounded. In addition, for some of the accused MPs, their reported illegal activities might have been a one-off mistake. *Criminal(b)* is more likely to capture "real" criminal types. The approach is simple and objective compared to subjectively rating different types of crimes (See Online-Appendix Table 1 for frequencies and details).¹³

MPs with criminal accusations against them are not a phenomenon bounded to few states or certain parties. Table 2 shows that all parliamentary parties are comprised of some criminal members. The share is highest for Rashtriya Janata Dal, a party most prominent in the state of Bihar, with 10 out of 21, or 47.6%, of members being accused of criminal activity. Of the other major parties, the shares range

⁹ For more details on MPLADS, see: <http://mplads.nic.in/welcome.html> (accessed between March and November 2013). A detailed description of the advantages of MPLADS as a proxy for effort can be found in Keefer and Khemani (2009).

¹⁰ See: <http://mplads.nic.in/Annualreportmenu.html> (accessed between March and November 2013). Note that the actual spending incurred by an MP includes any unspent amount which is inherited from her predecessor. We will demonstrate later that the different initial inheritances do no bias our results. While there are some reports about corruption in MPLADS spending, there is no evidence of systematic mismanagement. Keefer and Khemani (2009) provide a more detailed explanation why the MPLADS is a particularly good measure of effort as well as additional background information.

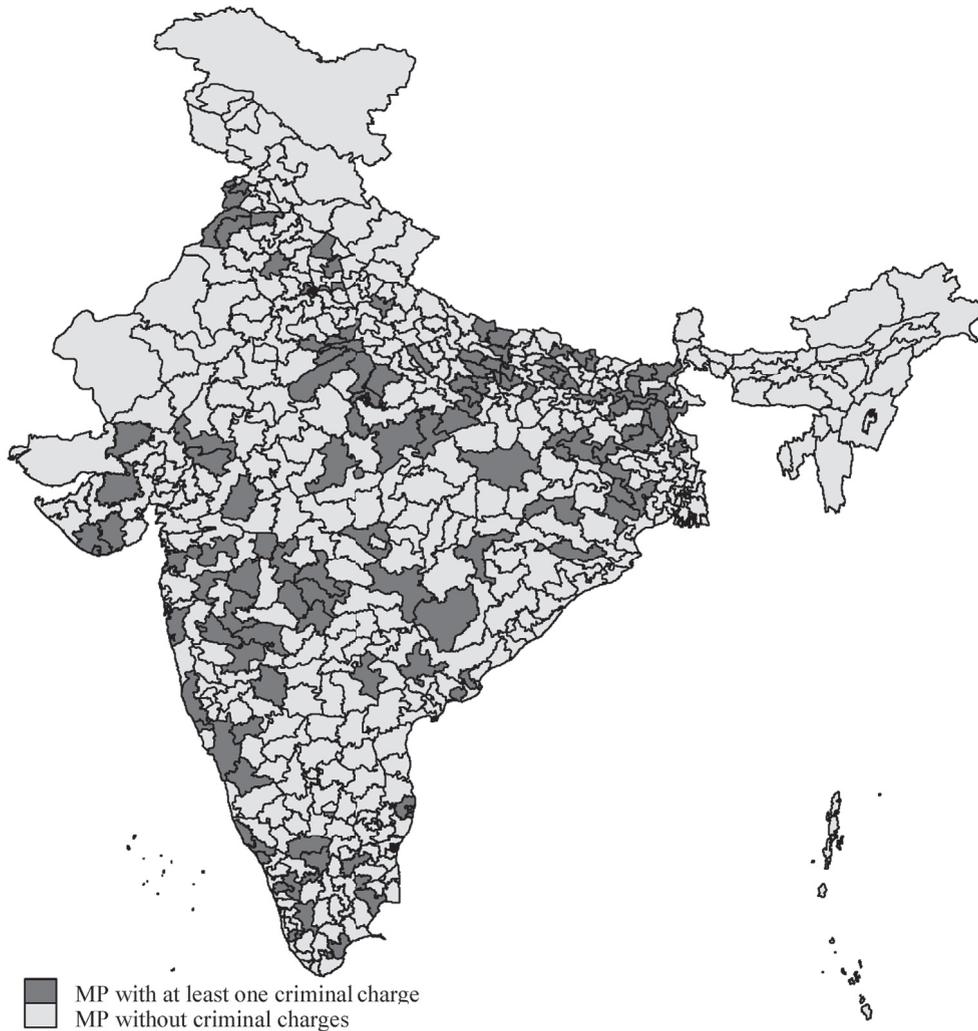
¹¹ See: http://eci.nic.in/eci_main1/LinktoAffidavits.aspx and http://eci.nic.in/archive/GE2004/States/index_fs.html (accessed between March and November 2013). In some cases it was necessary to manually adjust the spelling of names in the different data sources. This was done by comparing the names with the information available and adjusting the names accordingly. See Online-Appendix Fig. 2 for an example of such an affidavit and detailed information about the data collection process and replication.

¹² While anecdotal, speaking to people who are familiar with the issue or involved in politics gives the clear indication that most charges are indeed justified. The main reason why so many charges are still pending is the fact that the Indian judiciary system is notoriously overburdened and that it takes years until a specific case is finally dealt with in courts. In some sense, only the slow processing time of the Indian courts allows us to observe these supposedly criminal actors in their parliamentary role.

¹³ Some crimes could be thought of as more directly related to indicating that an MP will act to the detriment of their home constituency. Crimes related to corruption like accepting bribes might be particularly problematic in a political context. This is hard to distinguish, however. Murder, for example, could be related to pure greed or passion, but also to achieve political goals. Within our sample most crimes are capital crimes. A second difference to *Criminal(a)* could be that *Criminal(b)* captures differences in the abilities of "criminal" MPs, who must not necessarily form a homogenous group, to intimidate and bribe voters. If the latter group can acquire more voters that way, it is plausible that they would also engage in relatively less effort.

Table 2
Criminals by party affiliation.

	Normal		Criminal	
Bharatiya Janata Party	82	[78.8%]	22	[21.2%]
Communist Party of India (Marxist)	33	[82.5%]	7	[17.5%]
Indian National Congress	103	[84.4%]	19	[15.6%]
Rashtriya Janata Dal	11	[52.4%]	10	[47.6%]
Other	106	[69.7%]	46	[30.3%]
Total	335	[76.3%]	104	[23.7%]



Notes: The dark color indicates constituencies that in 2004 elected a politician with facing criminal charges in the published affidavit. State boundaries are not displayed to reduce complexity. Nearly all states have some MPs with criminal backgrounds, a detailed list of the distribution by state is provided in Appendix Table 2.

Fig. 1. Geographical distribution of candidates with criminal charges across constituencies for the 14th Lok Sabha (2004 national election).

from 15.6% for Indian National Congress to 21% for the Bharatiya Janata Party. The geographic distribution is equally dispersed, as can be seen in Fig. 1. Most states have at least one and usually more MPs facing criminal accusations. The highest shares in the major states are to be found in Kerala with 36.8%, Bihar with 38.7% and Maharashtra with 46.2%. Assam is the only large state without any such MP, and in general the far east of India seems to be mostly free of MPs accused of criminal activity (see Online-Appendix Table 2 for details).

4.2. Control variables

Our control variables fall into the three categories candidate characteristics, *monitoring intensity*, and *electoral competitiveness*.

4.2.1. Candidate Characteristics

The affidavits include details about candidates' total assets and liabilities, educational qualifications, age, gender, and experience in parliament. For age, we use MP age at the time of election in 2004. The gender variable is dummy coded and assumes a value of 1 if a MP is male, and 0 if they are female. For education of the candidate, we create an ordinal three category system which assigns a value of 0 if education is not given or indicated as "other" or "literate", 1 if the educational achievement is between the 10th to 12th grade passed, and 2 for all graduate, post graduate or other graduate attainments. For MPs' experience we use a simple count of number of times the MP has been elected in the three elections immediately preceding the 2004 election. It is also a proxy for incumbency advantages or disadvantages, but provides a more nuanced measure of experience which could foster effectiveness in parliamentary work. We calculate net assets as the difference between assets and liabilities, and take the logarithm after adding the minimum net assets plus the value one to all observations to avoid the creation of missing values.

4.2.2. Monitoring Intensity

In regard to constituency features related to monitoring intensity, we first control for voter turnout as a proxy for the extent to which voters within a constituency are interested in and informed about politics. A similar argument holds for literacy rates: Aidt et al. (2011) suggest that illiterate voters might be less put off by criminality and easier prey for vote buying and intimidation tactics.

Furthermore, we use nighttime lights as a proxy for the initial economic development of the constituencies. Henderson et al. (2011) demonstrate that such a measure correlates with official GDP growth numbers, and Chaturvedi et al. (2011) use nighttime light for a study on income distribution within India. We follow the standard approach and compute the log sum of lights using average visible, stable light on cloud free nights, collected by the F16 satellite for the year 2004.¹⁴ In more developed constituencies, voters are more likely to be informed about their MP's performance since they have, on average, better access to media.

4.2.3. Electoral Competitiveness

We control for the closeness of electoral races in constituencies with the winning margin in terms of the difference in the vote share received by a MP and the immediate runner-up in 2004. Moreover, we code a constituency as a party stronghold if the political party of the winning candidate has won elections in that constituency for the last three successive elections in 1996, 1998 and 1999. We also employ a dummy variable for constituencies reserved for candidates from Scheduled Castes (SCs) and Scheduled Tribes (STs).¹⁵ The number of voters proxies for constituency size, which Aidt et al. (2011) relate to the likelihood that criminals can intimidate a significant share of voters.

The resulting maximum sample size for our estimations varies between 395 and 439. This variation is caused by three reasons. First, MPs changing during a term period due to various causes: a MP may be promoted to a ministerial or other superior position at the state level, they might make a planned resignation within the period, they could be expelled from office, or they might die. We carefully check each of these cases with information from the election commission of India and exclude all changes.¹⁶ Data on attendance rate and parliamentary activity are only available in aggregate form over the legislative period, hence comparing MPs with two years in the Lok Sabha to those with four years is misleading. Online-Appendix Table 3 shows that a dummy variable coded one in cases where there was a change is not significantly related to either the *Criminal(a)* dummy or the MPLADS utilization rate. Hence, dropping out of the sample is not systematic and therefore does not pose a problem for our estimations. The second reason is that for six constituencies the affidavits could not be accessed either due to poor scanning quality or malfunctioning links that could not be repaired. Third, sample size is constrained by our first two dependent variables, which are only available for 395 out of the 435 constituencies left in the sample.

4.3. Empirical strategy

Essentially, our identification strategy is based on three components. The main component is a simple linear regression model with

¹⁴ For more on this measure, see: http://ngdc.noaa.gov/eog/gcv4_readme.txt. The original description states that "the cleaned up (file) contains the lights from cities, towns, and other sites with persistent lighting, including gas flares. Ephemeral events, such as fires have been discarded and background noise was identified and replaced with values of zero. Data values range from 1 to 63. Areas with zero cloud-free observations are represented by the value 255". Online-Appendix Fig. 3 graphically depicts the geographic variation of economic development in India.

¹⁵ In these constituencies, only members of the respective castes and tribes can be elected into office. All data were collected and coded from publicly available sources, mostly the Election Commission of India. Data on partywise competition since 1977 come from <https://eci.gov.in> (accessed between September and December 2014).

¹⁶ http://eci.nic.in/archive/GE2004/States/index_fs.htm (accessed between September and December 2014).

various fixed effects. As a robustness check, we also investigate matching and Heckman selection-models. In the following, we explain all three components in more detail.

4.3.1. Regression framework

We distinguish between the analysis of the dependent variables related to parliamentary work, attendance rates and parliamentary activity, and the one relating to MPLADS utilization. Both measure slightly different dimensions of MP effort. The former two relate directly to input and effort, whereas the latter also relates to output and MP effectiveness in promoting the development of his constituency. We refer to the dependent variables as Ef_i , the effort of the MP in constituency i . We aim to measure the Treatment effect on the Treated (TOT), where treatment consists of the MP being of criminal type ($Criminal(Cr) \in \{0,1\}$). Clearly, unconditional observed comparisons could be affected by selection bias.¹⁷

Absent clearly exogenous variation, our attempt to avoid selection bias is to carefully select an extensive set of observable control variables and rely on the conditional independence assumption. We employ different methods relying on selection-on-observables, and also consider the stability of coefficients and conduct boundedness exercises. Our main estimating equation is

$$Ef_i = b_0 + CR_i b_1 + X_i' b_2 + S_s + P_p + \varepsilon_i$$

where Ef_i indicates effort in one of the three dimensions in constituency i , $Criminal(CR_i)$ is our dummy for whether the MP has a criminal background. X_i is the matrix of control variables in the three categories (electoral competitiveness, monitoring intensity and candidate characteristics as specified above), and S_s and P_p are dummies for states and parties respectively. We follow Keefer and Khemani (2009) and use dummies for all major states. As outlined above, criminal MPs are found all across India, but some of the larger states obviously exhibit a higher percentage than others. With the fixed effects, we make sure our results are not caused by unobservable, time-invariant factors that are specific to, for example, Maharashtra, which has the highest share of MPs with criminal charges. With regard to parties, we choose to employ party dummies as additional controls for all parties that are comprised of twenty or more MPs. As mentioned above, the distribution of MPs by party and the respective share of *Criminal* is provided in Table 2.

In line with Hypotheses 1, we expect a negative coefficient for *Criminal*. If this a priori assumption is true, we would be less concerned about a possible upward bias in the coefficient. A problematic downward bias is possible if it would, for instance, be harder to find and develop projects in constituencies that are more likely to have a criminal MP.¹⁸ We deem it more probable that electoral conditions, which make the election of a criminal more likely, also set an incentive to exert less effort. The reason is that there is less accountability towards voters. Overall, this could cause a downward bias in our estimates. Notice that both our regression and matching estimates are able to control for observable factors like literacy rates and income, which relate exactly to the likelihood of monitoring MPs. Comparing specifications with and without controls provides an idea of how much our estimates might remain biased due to unobservable factors. An upward bias estimate would occur if it were easier to think of and recommend a project in a less developed constituency, where the election of a criminal MP is also more likely. In this case, our estimates would be biased against finding a negative effect of *Criminal*, and constitute a lower bound estimate of the true effect.

We use cluster-robust standard errors that allow arbitrary within-cluster correlation. With regard to parliamentary work, correlations between individuals' efforts are arguably most likely within parties. Parties are the natural unit of comparison within a parliament; MPs are, for example, seated beside other MPs from their own party. Thus, we choose the party level as the clustering unit for the first two variables. For the MPLADS fund, on the other hand, outcomes within states are more likely to be correlated and we cluster on the state level. The implementation probability and effectiveness depend on the individual state that processes and executes the projects. Some states might, for example, implement the proposals more quickly than others; and differences in ex ante success probability can affect the likelihood of applying for a project.¹⁹

4.3.2. Matching

Matching criminal to non-criminal MPs seems to be an intuitive alternative to a regression framework to examine our hypotheses.

¹⁷ Biases could potentially exist if having a criminal type MP and political effort or outcomes are simultaneously determining equilibrium outcomes. We are not explicitly modeling this - the underlying dynamics are explained in detail in Aidt et al. (2011) and Dutta and Gupta (2014). Our aim is to assess how likely it is that these potentially disturbing factors affect our estimations and in which direction. We will also show how large this selection-on-unobservables would have to be to account for our estimated coefficients.

¹⁸ Notice that the MP's effort in the parliament should not be affected by the time spent on preparing for the court cases. The Indian judiciary system allows those accused of a crime to appoint a lawyer (either a public prosecutor or a private lawyer) to defend her case in the court of law. This effectively means the accused need only be available in the court of law on certain important matters such as when being directly questioned or on the day the verdict is pronounced.

¹⁹ The number of clusters, 42 parties for parliamentary work and 33 states for the MPLADS fund, should be sufficiently high not to suffer from "few cluster" inference problems. Our main results are virtually unchanged when clustering on either state or party. Recently, MacKinnon and Webb (2017) also suggested that inference might be affected by wildly different cluster sizes. We programmed a cluster wild bootstrap procedure based on the suggestions in their appendix and their derivations in Cameron et al. (2011). To generate the bootstrap dependent variables, we used the "Rademacher" 2-point distribution as well as the "Webb" 6-point distribution. The results with 10,000 repetitions mostly confirm the findings with more standard procedures. Attendance rates remains significant with *Criminal(a)* (Rademacher p-value = 0.066/Webb p-value = 0.063) and *Criminal(b)* (Rademacher p-value = 0.009/Webb p-value = 0.010), and parliamentary activity remains insignificant. The only difference is for MPLADS utilization rates, where *Criminal(b)* becomes marginally insignificant (Rademacher p-value = 0.139/Webb p-value = 0.125).

Angrist and Pischke (2008) argue that OLS regressions are a natural starting point for empirical studies. Propensity score matching has advantages but requires many somewhat arbitrary choices, which can greatly affect results. In cases where both estimators are consistent, OLS is more efficient. Using matching as a robustness check has two advantages. First, it allows us to compare our regression estimates to those from matching the MPs with a criminal background (treatment group) to those without (control group). This is interesting as the weights differ between the two estimators. OLS assigns the highest weights to the observations with the largest conditional variance of the treatment status, whereas matching assigns the highest weight to those observations that are most likely to be treated. Second, we assess the reliability of our estimates by using matching diagnostics to examine how well the treatment and control groups are matched.

We use nearest-neighbor (NN) matching with the Mahalanobis distance-metric and robust standard errors (Abadie and Imbens, 2016). As NN-matching estimators were shown to be inconsistent when matching more than two continuous covariates, we use the consistent bias-corrected estimator as outlined in Abadie and Imbens (2006, 2011), which uses a linear function of all covariates as a correction term. We show results for the average treatment effect on the treated when matching to the two and three nearest neighbors. The underlying assumption of this approach is that matching on observables has a similar effect to matching on unobservables.

4.3.3. Heckmann models

We also investigate potential selection issues by using so-called endogenous binary-variable models (treatment effect models). This is similar to Heckman selection-models: The selection problem is approached by explicitly modeling selection instead of proposing a supposedly exogenous instrument. Treatment effect regression differs from sample selection models as the dummy treatment variable is directly entered in the regression equation and the outcome variable is observed for both the treated and the untreated subjects. The advantage of this potential outcome model is that it provides information about the effects of non-linear selection bias, which rests on specifying the correct functional form. Details are reported in the [Online-Appendix](#).

5. Results

5.1. Baseline

[Online-Appendix Table 4](#) depicts the baseline results for the regressions with attendance rates, parliamentary activity and MPLADS utilization as dependent variables. All regressions contain state dummies and dummies for the major national parties. Dummy coefficients are not displayed to improve clarity and readability. Attendance rates ranges from 0.06 to 0.96, parliamentary activity from 0 to 4.38 and MPLADS utilization from 60 to 260.

First, let us briefly consider *attendance rates*. The omitted reference category for the major party dummies are other or non-national party MPs. Positive relationships with attendance rates compared to this reference category can be seen for the “Indian National Congress” and the “Samajwadi Party”, both significant at the 1%-level. For *parliamentary activity* the positive effect of “Indian National Congress” disappears and we observe a negative relationship with being a member of the “Communist Party of India”. It is positively related with the “Rashtriya Janata Dal” and the “Samajwadi Party”. The only significant party for *MPLADS utilization* is a negative relationship to “Indian National Congress”. The remaining correlations can be seen in the table and are not discussed here; the most significant relationships with control variables occur with attendance rates.

5.2. Attendance rates and parliamentary activity

[Table 3](#) displays the relationship between our main variable of interest and attendance rates. The coefficient of *Criminal(a)* is -0.046 , which is significant at the 10%-level in column 1, when controlling for state and party dummies only. The coefficient becomes slightly more negative in columns 2 and 3 when we add controls for electoral competitiveness and monitoring intensity, and is significant at the 5%-level. This indicates that omitting the two categories leads to a slightly upwardly biased coefficient. The coefficient changes marginally to -0.043 when including candidate characteristics and remains significant. Using our alternative and more stringent measure *Criminal(b)*, which should alleviate measurement error problems in identifying criminal types, confirms and strengthens the existing results. The coefficient for *Criminal(b)* increases in absolute size from about -0.05 to about 0.13 , relating to 13 percentage points lower attendance rates for those with a criminal background (column 9–12). This effect is significant at the 1% level.

These two results would be in line both with measurement error in identifying criminal types and a scenario where the severity of criminal background relates to bigger differences between criminals and non-criminals. A classification of crimes is in our opinion highly arbitrary and the consequences of committing different types of crimes is theoretically unclear. Instead, we used the number of crimes and its square term instead of the dummy and find that there is no u-shaped non-linear relationship that would suggest more crimes generally translate into exerting less and less effort. Thus, the data suggest that the more robust effect for *Criminal(b)* is due to the fact that it identifies criminal types more precisely. In conclusion, we find a generally negative and significant relationship between criminality and attendance rates as predicted by our [Hypothesis 1](#).

The results look different when it comes to parliamentary activity. In fact, we find no systematic relationship between our measures (*Criminal(a)* and *(b)*) and parliamentary activity. Therefore, we will not discuss the regression results in detail - the interested reader is referred to the respective tables in the [Online-Appendix](#). As discussed in section 4.1, we expected to find a lower difference in parliamentary activity between criminals and non-criminals because parliamentary activity has little electoral value in contrast to attendance. An additional intuitive explanation could be that activity has additional unobservable private benefits to both types of MPs, for example, the utility derived from the attention gained when speaking in front of the parliament.

Table 3
Main results for attendance rates and parliamentary activity.

Dependent variable: Attendance rate	(1)	(2)	(3)	(4)
Criminal(a)	-0.046*	[0.025]	-0.049**	[0.021]
Criminal(a)			-0.050**	[0.020]
-0.044*				[0.023]
Dependent variable: Parliamentary activity	(5)	(6)	(7)	(8)
Criminal(a)	0.002	[0.066]	-0.006	[0.065]
Number of MPs	394	394	394	394
State Dummies	Yes	Yes	Yes	Yes
Party Dummies	No	Yes	Yes	Yes
Electoral Competitiveness	No	Yes	Yes	Yes
Monitoring Intensity	No	No	Yes	Yes
Candidate Characteristics	No	No	No	Yes
Dependent variable: Attendance rate	(9)	(10)	(11)	(12)
Criminal(b)	-0.126**	[0.051]	-0.128***	[0.046]
Criminal(b)			-0.126***	[0.045]
-0.105**				[0.045]
Dependent variable: Parliamentary activity	(13)	(14)	(15)	(16)
Criminal(b)	-0.180	[0.160]	-0.181	[0.152]
Number of MPs	394	394	394	394
State Dummies	Yes	Yes	Yes	Yes
Party Dummies	No	Yes	Yes	Yes
Electoral Competitiveness	No	Yes	Yes	Yes
Monitoring Intensity	No	No	Yes	Yes
Candidate Characteristics	No	No	No	Yes

Notes: Dependent variable as specified above over the full legislative period 2004–2009. Standard errors are clustered at the party level. *Criminal(a)* is defined as those having at least one criminal charge against them, *Criminal(b)* as those having more than one criminal charge against them. All regressions include the control variables as specified in Table 1 as indicated in the respective column. *** (**, *) indicates significance at the 1 (5, 10) percent level respectively.

This is in line with anecdotal evidence: MPs with no criminal background, like Jayanth Sinha or Manoj Sinha have maintained almost full attendance in Lok Sabha, despite heading the Aviation and Railway ministries respectively. Other MPs from opposition parties with no criminal background such as Tathagata Satpathy, Dinesh Trivedi, Gajendra Singh Shekhawat are known for their exemplary attendance record in the Parliament. PRS Legislative Research group, a think-tank which provides tracking of the performance of the MPs in parliament, suggest that none of the top 20 performers in parliament in terms of attendance have a criminal background.

We further want to test whether the effect of *Criminal* is moderated by monitoring (*economic development* and *literacy rate*), as suggested by our theoretical framework, and competitiveness (party stronghold and winning margin). We hypothesized that a high degree of information and hence monitoring of MP activity has a moderating effect, as a criminal MP who would normally exert less effort might not do so when the negative impact on his chances for re-election is sufficiently high. Competitiveness on the other hand should not have a significant moderating effect, as it affects criminals and non-criminals alike. *Economic development* proxies for both access to media and information about candidate performance, and for the average voter's interest in MP effort. A MP that shirks is more

Table 4
Interaction effects.

Dependent variable: Attendance rate								
Criminal(a)	-0.056***	[0.019]	-0.053**	[0.023]	-0.611***	[0.160]	-0.113	[0.119]
Interaction with:								
Party Stronghold	0.070	[0.066]						
Margin (2004)			0.089	[0.203]				
Economic Development					0.058***	[0.016]		
Literacy rate							0.001	[0.002]
Number of MPs	394		394		394		394	
Dependent variable: Parliamentary activity								
Criminal(a)	0.019	[0.084]	0.036	[0.106]	-1.900***	[0.559]	-0.068	[0.471]
Interaction with:								
Party Stronghold	-0.147	[0.112]						
Margin (2004)			-0.408	[0.718]				
Economic Development					0.195***	[0.059]		
Literacy rate							0.001	[0.008]
Number of MPs	392		392		392		392	

Notes: Dependent variable as specified above over the full legislative period 2004–2009. All regressions include all control variables as specified in Table 1, including dummies for major states and parties. *Criminal(a)* is defined as those having at least one criminal charge against them. Standard errors [in brackets] are clustered at the party level. *** (**, *) indicates significance at the 1 (5, 10) percent level respectively.

likely to experience negative consequences in more developed constituencies with better informed voters who are more interested in their MP's performance. To test these hypotheses, we interact *Criminal(a)* with *party stronghold*, *winning margin*, *economic development* and the *literacy rate*.

Table 4 shows the coefficients for *Criminal(a)* and the respective interaction term. All other variables are included but not displayed in the table. The results for both *attendance rates* and *parliamentary activity* show no significant interaction with party stronghold and margin (year 2004), as expected, but also none for *literacy rate*. However, the interaction effects between *economic development* and *Criminal(a)* are positive and significant at the 1%-level for both dependent variables. The most likely explanation is that more developed constituencies monitor their MPs more closely, which results in a higher share of informed voters who are able to punish shirking. An alternative explanation within our theoretical frame, which is supported by anecdotal evidence, is that part of a MP's utility can come from rent extraction (cf. Fisman et al., 2014; Kapur and Vaishnav, 2011). If more developed constituencies offer better rent-seeking opportunities for criminal MPs, this increases their utility from re-election, and can also narrow the effort gap between criminal and non-criminal types as criminal types strive to maintain access to this resource.

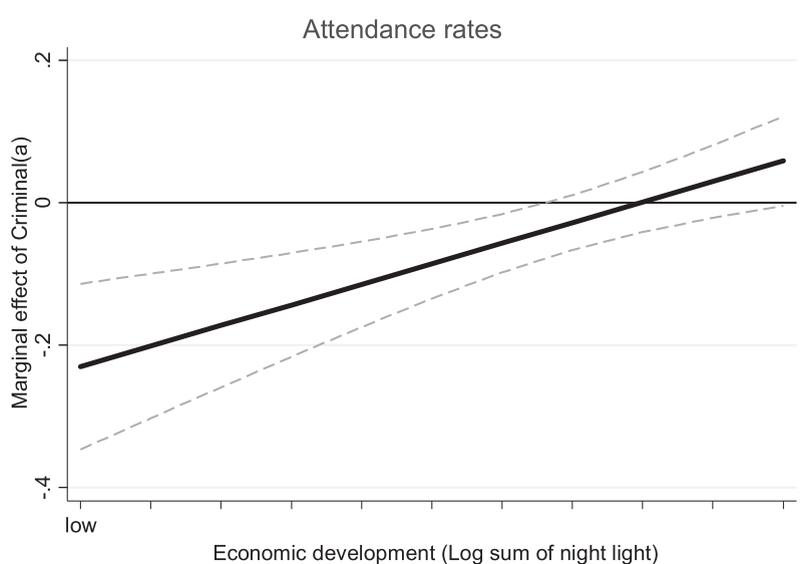
Figs. 2 and 3 illustrate the moderating effects. The y-axis displays the effect of *Criminal(a)* on effort with its 95% confidence-interval conditional on *economic development*, which is plotted on the x-axis. The marginal effect of *Criminal(a)* is negative and significant for low levels of *economic development* for *parliamentary activity*, respectively for low and median levels for *attendance rates*. These are constituencies where intense monitoring of MPs effort and access to such information is limited, with on average less resources and wealth. For highly developed constituencies there is no significant difference between MPs with and without criminal charges -a result that supports Hypothesis 2.

The insignificance of the interaction term of *Criminal(a)* with *literacy rates*, which also proxies for monitoring, suggests that rent-seeking rather than monitoring explains the varying effort levels between criminals and non-criminals. Further tests show that the interaction with economic development remains significant, even when controlling for *literacy rates*, while the interaction with *literacy rates* remains insignificant even when omitting *economic development*. Thus, the more plentiful rent-seeking opportunities that more developed constituencies possess counter-intuitively contribute to narrow the effort gap to non-criminals: criminals work relatively harder when there is a chance for a larger reward.

5.3. Member of Parliament Local Area Development Scheme (MPLADS) utilization rate

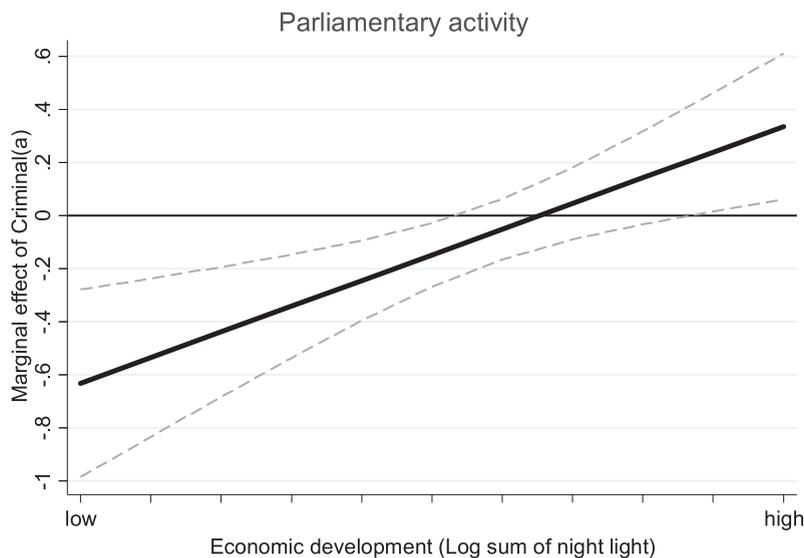
Now we turn to MPLADS utilization, which as Keefer and Khemani (2009) argue offers several advantages as a measure of MP effort. The baseline model specification is identical to the one for the first two indicators, except that standard errors are now clustered at the state level to allow for arbitrary correlation within states. As implementation of the project depends on the state bureaucracies, correlation within states is most likely. Our results are, however, unaffected by alternatively clustering on parties. Columns 1–4 in Table 5.1 show the results for *Criminal(a)* and columns 5–8 for *Criminal(b)*. Columns 1 and 5 only use party dummies, columns 2 and 6 add the electoral competitiveness controls and party dummies, columns 3 and 7 the monitoring intensity controls, and columns 4 and 8 the candidate characteristics.

For our variables of interest, the coefficient on *Criminal(a)* is negative, but remains insignificant in columns 1–4. The coefficients



Notes: Marginal effect of s *Criminal(a)* MP Dummy for different levels of economic development. Dotted lines represent the 95% confidence intervals.

Fig. 2. Marginal Effect of *Criminal(a)* on *attendance rates* conditional on economic development.



Notes: Marginal effect of a *Criminal(a)* MP Dummy for different levels of economic development. Dotted lines represent the 95% confidence intervals.

Fig. 3. Marginal Effect of *Criminal(a)* on parliamentary activity conditional on economic development.

Table 5.1

Main results for Member of Parliament Local Area Development Scheme (MPLADS) utilization.

	(1)	(2)	(3)	(4)
Criminal(a)	-3.014	-3.302	-3.273	-3.419
	[3.519]	[3.563]	[3.601]	[3.733]
	(5)	(6)	(7)	(8)
Criminal(b)	-5.080**	-7.436**	-7.571**	-7.723**
	[2.677]	[3.005]	[3.106]	[3.415]
Number of MPs	439	439	439	439
State dummies	Yes	Yes	Yes	Yes
Party dummies	No	Yes	Yes	Yes
Competition controls	No	Yes	Yes	Yes
Monitoring controls	No	No	Yes	Yes
Candidate characteristics	No	No	No	Yes

barely change when adding the controls, becoming slightly more negative in column 4 compared to column 1. Thus, we cannot reject the hypothesis that candidates with criminal charges against them generally perform equally well in terms of making use of the development fund scheme. As mentioned above, one concern about these results, however, could be whether the existence of any charge correctly identifies criminal MPs. We again use our *Criminal(b)* measure to alleviate these concerns. Columns 5–8 show the results when using the alternative measure. *Criminal(b)* is related to lower utilization rates in all specifications, significant at the 5%-level. This holds when adding the controls: the coefficient becomes slightly more negative from -5.080 in column 1 to -7.723 in column 4. This coefficient translates to about 7.5 percentage points lower cumulative utilization rates over the legislative period.²⁰

The obvious question is whether this relationship has a causal interpretation, or if the coefficient is biased upwards or downwards. In our model with two types of fixed effects, identification relies mostly on within-state, within-party variation. Hence, the results should not be driven by the geographic or political distribution of criminal MPs. Nonetheless, the coefficient might be biased if there are unobserved variables that vary within states or parties and that are related to characteristics that affect MPLADS spending. It might be that MP's with criminal accusations are much more likely to use the MPLADS funds for purposes other than the intended development goals. This would mean that the actual detrimental effect of *Criminal(b)* would be larger than the estimated 7.5 percentage points.

²⁰ We do not test for interaction effects with economic development, as this variable is clearly endogenous in a more problematic way than for the two other outcomes. The development level might increase or reduce the utilization rate for reasons unrelated to the criminal status of the MP. It could be easier to acquire more projects in a poorer constituency, or it might be easier to conduct a project in a more developed constituency where the bureaucracy that is required functions better. There is a negative and highly significant interaction effect with the presence of other criminal candidates.

While we do not want to rule out this possibility, note that MPLADS works are subject to inspection by the state government authorities and by the Auditor General of India on a random basis. There are inspection registers including details of the inspections of works carried out by the project implementing agencies, which can be used to intervene in problematic projects. For instance, Prakash (2013)'s comprehensive survey documents a wide range of cases where the Controller and Auditor General of India and government sponsored individual surveys found some MPs who were found misusing MPLADS funds. Conditional on state fixed effects, different degrees of corruptibility of the state authorities would thus not explain the observed relationships.

The results are in line with anecdotal evidence gathered from various news reports and expert bodies in India. For instance, the Daksh-ADR Survey finds that the top performing MPs in terms of utilization of MPLADS funds have always been MPs with high educational qualifications and with no criminal track record. Some well-known MPs like Shashi Tharoor or Manoj Sinha published their record on performance in parliament along with an annual progress report providing a detailed summary of how MPLADS funds have been spent in their constituencies (Daksh-ADR Survey 2014).

5.4. Robustness checks

5.4.1. Results on MPLADS

While our results provide important evidence of the negative consequences of criminals in politics, it is clear that the point estimate we observe is not an exact measure of the extent of these negative consequences. One source of a potential bias is that constituency-specific characteristics like differences in the level of economic development make it easier (or harder) to utilize available MPLADS funds. While we will argue that a downward bias in the coefficient is more likely, it is obviously possible to come up with arguments for a bias in both directions. At the same time, a sizeable share of the electorates in India attribute positive qualities like assertiveness to criminal candidates, which can only be refuted with measures of their actual performance. If one accepts the prior that criminals in politics are more likely to be harmful to democracy and their constituents, the negative coefficient we observe is in line with this. As long as there is no clear argument why the estimate should be upward biased, we interpret the negative direction of the relationship as support for initiatives to reduce the extent to which such candidates enter the political sphere in the world's largest democracy.

5.4.2. Extent and direction of omitted variable bias

The second-best strategy, in the absence of other convincing instruments, to make sure that the negative direction of the relationship can be meaningfully interpreted is to assess the extent and direction of omitted variable bias. A crucial insight in this regard is that almost all cases of omitted influencing factors that come to mind are conceptually captured by the control variables. For example, it is possible that countries with a lower literacy rate are at the same time more likely to elect criminals and it might be more difficult or costly to implement development projects, which would cause an upward bias. Ideally, this would be captured by our variable literacy rate. Still, it is obviously not clear whether these variables manage to adequately and precisely capture the effect of these factors. We can compare the results with and without control variables to get a useful indication of the direction of a potential bias. The idea is similar to Altonji et al. (2005): we use selection on observables to assess the effects of selection-on-unobservables. When adding controls X_i , i.e., controlling for selection on constituency and candidate observables, the coefficients in Table 5.1 becomes markedly more negative compared to columns 1 and 4 when adding more controls. This shows that if omitted variables bias caused by selection-on-unobservables works in the same direction as selection on observables, the negative coefficient will be an upper bound estimate of the true causal effect. Our Online-Appendix elaborates on this in more detail.

5.4.3. Controlling for past utilization rates

Another way to assess omitted variables is to add the cumulative utilization rate in the previous period to the equation. As

Table 5.2

Robustness tests (based on column 4).

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Criminal(b)	Coef.	−7.793*	−7.658**	−7.479**	−9.664**	−7.679**	−7.643**	−5.642*	−8.541**
Additional controls for	SE	[4.050]	[3.554]	[3.355]	[3.482]	[3.529]	[3.506]	[3.015]	[3.676]
Development fund utilization (1999–2004)	Yes						Yes		
Leftover funds from predecessor			Yes				Yes		
Political fragmentation 1991–1999				Yes			Yes		
Caste and religious fragmentation					Yes		Yes		
Electoral volatility						Yes	Yes		
Sensitivity analysis									
Omit 10 constituencies with highest utilization rates								Yes	
Only Criminal(b) vs. Non-Criminals									Yes

Notes: Robustness checks. Dependent variable is the cumulative utilization rate over the 2006–2009 period. *Criminal(a)* is defined as those having at least one criminal charge against them, *Criminal(b)* as those having more than one criminal charge against them. All regressions include the control variables as specified in Table 1 as indicated in the respective column. Standard errors [in brackets] are clustered at the state level. *** (**, *) indicates significance at the 1 (5, 10) percent level respectively.

constituency delimitation did not change between 1999 and 2004, this lagged dependent variable should capture time-invariant omitted factors, i.e., work similar to a constituency-fixed-effect. The coefficient of *Criminal(b)* in column 1 in the second part of Table 5.2 remains virtually unchanged, giving no indication of such a bias. As another possibility, we consider whether leftover funds from predecessors bias the coefficient. The terms for using the MPLADS funds state that unused resources can be carried over to the next year(s). Higher leftovers increase the overall amount of available money and could bias the measured degree of utilization upwards in cases where there are systematic differences across constituencies. While theoretically plausible, this either does not play a large role in reality or does not vary systematically between candidates with and without criminal charges, as the coefficient again remains nearly unchanged in size and significant at the 5%-level (column 2).

5.4.4. Are the differences caused by political/social fragmentation and electoral volatility?

In addition, we follow the robustness checks in Keefer and Khemani (2009) and test whether political and social fragmentation, as well as electoral volatility in the constituency influence the coefficient (columns 3–5). The original data source for the first two measures is Banerjee and Somanathan (2007). They argue that political fragmentation may reflect greater electoral competitiveness, which can affect a MP's effort as we argued in Section 3. The measure is defined as $1 - \frac{\sum_{p=1}^N \mu_{p,i}^2}{\sum_{p=1}^N \mu_{p,i}}$, where $\mu_{p,i}$ is the vote share of the p^{th} political party contesting the election in the given constituency i , which is then averaged over the 1991, 1996, 1998 and 1999 elections. Social fragmentation might be relevant if it reduces the provision and changes the composition of local public goods within constituencies. We use a measure of caste and religious fragmentation, based on the census of 1991. Keefer and Khemani (2009) also argue that electoral volatility can be related to MP behavior, as in constituencies where voters are prone to greater shocks and a more insecure environment, the expected returns to MP effort should be lower. As in their study, we use a measure from Nooruddin and Chhibber (2008) that defines

volatility as $\frac{1}{2} \sum_{p=1}^N \left| \mu_{p,i,t} - \mu_{p,i,t-1} \right|$, i.e., the sum of the changes in vote shares of N political parties. This is again averaged over the four previous elections. All three measures might lead to omitted variable bias if they are related both to MPLADS spending and to the likelihood of having a criminal MP. However, this does not seem to be the case. The coefficient of *Criminal(b)* remains negative, nearly unchanged in size, and significant in all specifications (column 2–6). Even when we control jointly for all three variables from Keefer and Khemani (2009), the cumulative utilization rate in the previous period and leftover funds from predecessors, the coefficient remains stable and significant at the five percent level. *Criminal(b)* is related to about 7.6 percentage points lower utilization rates.

5.4.5. Are the results driven by outliers?

It can be seen in the Violinplots for all dependent variables in Online-Appendix Fig. 1 that the distribution of the utilization rate exhibits some potential outliers in its right tail. To make sure these do not distort our results, column 7 of Table 5.2 drops the ten constituencies with the highest utilization rates that constitute this tail. The coefficient becomes somewhat smaller in absolute size but remains significant. Finally, we compare *Criminal(b)* only to the MPs without any charge at all, i.e., those who are most likely not of the criminal type. As we would expect, this leads to a larger negative coefficient which also remains significant at the 5%-level. Thus, we conclude that there is a negative relationship between criminal background and development fund utilization, which is significant for those MPs with at least two criminal charges, and unlikely to be explained by selection or omitted variable bias.²¹

5.5. Further robustness checks: matching and Heckman selection-models

As discussed in Section 4.4, we combine our regression framework with alternative and additional econometric models for all dependent variables and discuss whether the estimated coefficients capture the causal effect of criminal type. First, we employ propensity score matching techniques as an alternative estimator to examine whether this affects our results. Second, we analyze whether the results are driven by extreme values or outliers. The results in Table 6 mostly confirm our result above, however, they suggest a stronger negative relationship between criminal MPs and effort. Notice that we do not claim that the exact size of our point estimate is causally estimated, which is unlikely in our setting. What we want to support with the evidence presented in this section is how likely it is that the actual consequences of criminal MPs on development outcomes are negative, which is an important finding for the over one billion citizens in India.

5.5.1. Matching

As explained above, we use nearest-neighbor (NN) matching with the Mahalanobis distance-metric, using robust standard errors (Abadie and Imbens, 2016), a consistent bias-corrected estimator as outlined in Abadie and Imbens (2006, 2011). In a nutshell, the results from using matching estimators confirm the regression results both in direction and significance. Column 1 in Table 6 shows that the negative relationship for both *Criminal(a)* and *Criminal(b)* with *attendance rates* becomes stronger but similar in size to the regression results and is significant at least at the five percent level in all specifications. Column 2 confirms that *parliamentary activity* is not generally affected by criminal background. Column 3 for *MPLADS utilization* points in the same direction: the estimated coefficients become more negative. With matching, the negative coefficient of *Criminal(a)* becomes significant at conventional levels when matching to the two nearest neighbors. *Criminal(b)* remains significant, now at the 1%-level, with a more negative coefficient that is again more

²¹ As expected, there are no significant interaction effects with monitoring intensity. Online-Appendix Fig. 5 shows that there is a positive, but insignificant relation with economic development.

Table 6
Robustness checks for all dependent variables.

Dependent variable	Attendance rate		Parliamentary activity		MPLADS utilization	
Baseline results	Coef.	SE	Coef.	SE	Coef.	SE
<i>Criminal(a)</i>	-0.043*	[0.023]	-0.006	[0.075]	-3.419	[3.733]
<i>Criminal(b)</i>	-0.104**	[0.045]	-0.150	[0.153]	-7.723**	[3.415]
Matching estimator (nearest neighbor matching)						
<i>Criminal(a)</i> & NN (2)	-0.059***	[0.022]	-0.085	[0.136]	-6.575**	[3.168]
<i>Criminal(a)</i> & NN (3)	-0.055**	[0.021]	0.025	[0.129]	-4.494	[2.827]
<i>Criminal(a)</i> & NN (3)& mixed	-0.094***	[0.028]	-0.015	[0.171]	-9.595**	[4.024]
<i>Criminal(b)</i> & NN (2)	-0.144***	[0.043]	-0.434*	[0.232]	-16.475**	[6.950]
<i>Criminal(b)</i> & NN (3)	-0.131***	[0.044]	-0.648***	[0.235]	-18.165***	[4.909]
<i>Criminal(b)</i> & NN (3)& mixed	-0.175***	[0.055]	-0.211	[0.234]	-8.721*	[5.156]
Treatment effect estimator						
<i>Criminal(a)</i>	-0.184***	[0.070]	-0.048	[0.109]	-12.047***	[3.617]
Lambda	0.09		0.12		4.28	
Rho	0.57		0.16		0.22	
Prob > Chi ²	0.074		0.118		0.004	
Regressions	w/o 2% largest values of dependent variables					
<i>Criminal(a)</i>	-0.046*	[0.024]	0.005	[0.072]	-2.155	[2.792]
<i>Criminal(b)</i>	-0.101**	[0.045]	-0.034	[0.086]	-5.699*	[2.907]
w/o 1% largest positive and negative residuals						
<i>Criminal(a)</i>	-0.042*	[0.022]	-0.034	[0.086]	-3.472	[2.811]
<i>Criminal(b)</i>	-0.113**	[0.042]	-0.137	[0.136]	-6.666**	[3.138]
Using selection-on-observables to assess the bias from unobservables						
Controls (Restricted/Full)	Selection ratio SR = $\beta_R/(\beta_U - \beta_R)$					
<i>Criminal(a)</i>	Identified β -set	SR	Identified β -set	SR	Identified β -set	SR
None (U_1)/Full controls (R)	[-0.030, -0.050]	3.3	[-0.060, -0.010]	1.5	[-3.419, -5.570]	11.4
Fixed effects (U_2)/Full controls (R)	[-0.030, -0.043]	3.7	[-0.070, 0.000]	6.4	[-3.419, -5.470]	17.2
<i>Criminal(b)</i>						
None (U_1)/Full controls (R)	[-0.090, -0.104]	2.1	[-0.150, -10.690]	1.6	[-7.723, -10.690]	20.1
Fixed effects (U_2)/Full controls (R)	[-0.100, -0.104]	2.5	[-0.150, -10.550]	7.8	[-7.723, -10.550]	44.6

Notes: Matching was conducted on all variables that acted as controls in the prior regressions, including party and state dummies, and the number of criminal candidates running for election. The appendix shows balance statistics. Mixed restricts the estimations to constituencies where parties fielded both criminal and non-criminal candidates for election. The treatment effect regressions are estimated using maximum likelihood. In the first row, selection is based on all constituency characteristics from the baseline model. For the regression in the bottom part, we first calculated the baseline regression. Then we calculated the observations with the largest residuals and omitted them from the regressions. The selection ratio SR is further explained and derived in Altonji et al. (2005). The ratio indicates how much larger selection-on-unobservables would have to be to move the (negative) coefficient to zero. The identified set is explained in Oster (2017). It contains the range of possible β -estimates under the assumption of proportional selection on un- and observables, and a maximum R-squared comparable to the standards fulfilled by randomized studies. If the set does not include 0, we cannot rule out selection-bias, but its effect is under the assumptions not sufficient to be problematic for a causal interpretation.

negative than *Criminal(a)*. A third specification restricts the constituencies to those cases where parties selected both criminal and non-criminal candidates to run for the election, with the issue of endogenous candidate selection in mind. The results are not much affected; even in this restrictive specification we see a significant negative relationship for two of our three measures.

5.5.2. Selection-models

We also investigate potential selection issues by using so-called endogenous binary-variable models (treatment effect models), which is similar to Heckman selection-models (see Section 4.4 and Online-Appendix for details). We do not claim that our approach resolves all potential selection/omitted variables bias concerns. Contrary to IV, which has to assume no correlation between instrument and error term, we explicitly make an assumption about the outlined correlation structure. The selection equation contains all variables in x_i , plus the variable "other criminals" which counts the number of additional criminal candidates in the constituency in 2004. As reported above, other criminals plausibly provide exogenous variation about the selection of a criminal candidate. It did not pass the specification tests in (linear probability) IV regressions but works well in this (non-linear) selection framework. We run the regressions for all three dependent variables. In the results in Table 6 and Online-Appendix Table 7, Lambda is the inverse mills-ratio or non-selection hazard, and the parameter Rho indicates the correlation between the error terms ϵ_i and u_i .

We test the model assumptions with a likelihood ratio test that compares an independent probit and regression model with the treatment effect model, a test of $\rho = 0$ that is Chi-square distributed. The coefficient for other criminals is positive and significant in the selection equation as predicted and the test statistic rejects the null for attendance rates and MPLADS utilization, indicating that both models are valid. The results in Table 6 further support our earlier impression that not controlling for selection effects biases the OLS

coefficient upwards rather than downwards. The negative relationship between criminal background and attendance rates becomes more negative and significant at the 1%-level. The same holds for the relation with *MPLADS utilization*: the coefficient of *Criminal(a)*, which was negative but insignificant in the baseline model, becomes larger in size and significant at the 1%-level.

The next rows in Table 6 omit potential extreme values or outliers in the earlier regression specifications in Tables 3 and 4. First, we omit the observations that exhibit the largest values in the respective dependent variables, as the violinplots indicated some potential outliers. Second, we calculate the residuals of the full regression and omit the observations with the one-percent largest positive and negative residuals. The results for all dependent variables and both *Criminal(a)* and *Criminal(b)* are unaffected, indicating that the results are not driven by outliers or few observations.

5.6. The potential impact of selection-on-unobservables

Finally, we demonstrate how likely it is, if all our prior robustness tests suggesting an upward bias failed, that our results are explained by selection-on-unobservables. While our attempts so far suggest that selection, if any, biases against the negative coefficient we measure, we cannot rule out that there are unobservable factors that lead to a problematic bias in the direction of our effect. Thus, we use techniques developed in Altonji et al. (2005) to demonstrate how much larger on average selection bias on unobserved factors would have to be compared to selection on observed factors to fully explain our results.

The strategy is to use selection-on-observables to assess the severity and direction of potential selection bias based on unobservables. For instance, a critical reader might object that the true effect of criminal might have been positive, with a large negative OVB leading to the negative coefficient we observe. We are thus interested in how likely it is that such variables would bias our coefficients enough to become problematic. To get an idea of this, we compare two kinds of regressions: first, one without controls (U1 = unrestricted) to one with our full set of controls (R = Restricted); and second, one with a limited set of controls for fixed effects (U2) to one with full controls (R). We then calculated a “Selection ratio” (SR), which is the necessary ratio of selection-on-unobservables to observables to fully explain our coefficients as $\hat{\beta}_R / (\hat{\beta}_U - \hat{\beta}_R)$. The denominator, i.e., the difference between the β -coefficients indicates the degree to which our estimate is affected by selection-on-observables. A small difference indicates weaker selection effects. $\hat{\beta}_R$ in the nominator enters positively in the ratio, as we need stronger selection-on-unobservables to explain a larger coefficient. Altonji et al. (2005) provide the underlying assumptions and Bellows and Miguel (2008) a formal derivation.

The crucial insight in this regard is that while our empirical proxies do not perfectly capture the theoretical parameters, they are comprehensive and should be a useful guide to assess selection-on-unobservables. Altonji et al. (2005) posit that “there are strong reasons to expect the relationship between the unobservables and (...) generally any potentially endogenous treatment to be weaker than the relationship between the observables and dependent.” The bottom part of Table 6 shows the respective ratios for $\frac{\hat{\beta}_R}{\hat{\beta}_{U1} - \hat{\beta}_R}$ and

$\frac{\hat{\beta}_R}{\hat{\beta}_{U2} - \hat{\beta}_R}$, for our two limited sets (U_1 and U_2).

The results strongly confirm the negative relationship between criminal background and *attendance rates*: selection-on-unobservables would have to be at least 3.3–3.7 [2.1–3.7] times as strong as observables to fully explain the negative coefficient of *Criminal(a)* [*Criminal(b)*]. To explain the negative relationship between *Criminal(b)* and *MPLADS utilization rates*, selection-on-unobservables would have to be between 11 and 45 times as high as on observables. We can also use this exercise to assess the direction of a potential bias and again observe that our coefficient for *MPLADS utilization rates* becomes more negative when controlling for observables. This indicates it is more likely that controlling for unobservables would also lead to more negative instead of more positive coefficients.

Oster (2017) further formalizes and extends these ideas. More specifically, she argues that the extent to which robustness to selection-on-observables confirms our confidence in coefficient stability depends on the degree to which those observables explain variance in the dependent variable. Intuitively, this can be easily understood. We could add variables to our regression which are not correlated with either the dependent or our variable of interest. Adding them would not affect our coefficient estimate, and this could be problematic. If additional observable controls explain considerable variation, but do not affect our coefficient by much, we can assume that unobservables are not likely to do so either. In essence, this approach is very transparent and useful.

While experiments are the gold standard due to controlled randomization, the credibility of IV and RDD identification rests solely on the identification assumptions. These might or might not be credible. The reader has to believe in the exogeneity assumption, and in many cases evaluations some years after publication reveal severe problems. This approach, on the other hand, does not claim to solve endogeneity but allows clear and easily understandable numbers that indicate whether bias is problematic for a causal interpretation in each specific case.²² When applying the suggested assumptions our identified coefficient sets do not include zero for both *attendance rates* and *MPLADS utilization* in any specification. Thus, while we cannot make a causal claim for parliamentary effort, the test suggests that selection-bias does not seem to be problematic for a causal interpretation of the other two variables.

²² We also need an assumption about the maximum R-squared that can be systematically explained and is not due to pure noise. Oster (2017) suggests that one should apply the same standard to observational studies that are fulfilled by randomized studies which used control variables and were published in five selected top journals. She calculates that the appropriate R_{max} is 2.2 times the R^2 in the specification with all observable controls. With regard to δ , we use the most conservative suggested relation of $\delta = 1$. The formula for the identified set boundary is then $\beta^* = \hat{\beta} - \delta \times \frac{(\hat{\beta} - \hat{\beta}) \times (R_{max} - \hat{R})}{(R - \hat{R})}$.

6. Concluding remarks

This paper examines whether the fact that a member of parliament has a criminal background influences his effort in parliament and in developing his constituency. To be able to understand the implications of criminality on MP behavior, we developed hypotheses based on theoretical considerations that illustrate the incentives faced by elected MPs with regard to the effort they exert. The hypotheses derived are then put to an empirical test using data from the 14th Indian 2004 Lok Sabha election, and the subsequent 2004–2009 legislative period. While criminals in politics are a general issue, in India criminal MPs are a widespread phenomenon and widely regarded as a danger to the functioning of the world's largest democracy. This analysis was made possible by a judgment of the Indian Supreme Court in 2003 which asked every candidate to provide sworn affidavits that had to include details not only about their personal, educational, and financial particulars, but also about their criminal background. We restrict our analysis to this legislative period because constituency boundaries were changed in the 2009 election. Thus, it is no longer possible to control for important constituency characteristics like the winning margins in previous elections.

We augment the existing literature on the initial decision of whether to field a criminal candidate in the first place (Aidt et al., 2011), on the connection of MP criminality with proxies for MP effort, final consumption and luminosity (Chemin, 2012; Prakash et al., 2014). We provide a comprehensive direct assessment of effort by using three measures - each capturing a slightly different facet of MP behavior. We use *attendance rates* (or absenteeism respectively), *parliamentary activity* based on the number of questions they asked and their participation in debates, and the utilization rate of the Member of Parliament Local Area Development Scheme (MPLADS). We use two proxies for criminal MPs, *Criminal(a)* for all MPs with at least one crime, and *Criminal(b)* only for those with a least two charges.

Our empirical results support our hypotheses, but also provide further interesting details. Criminal MPs are related to higher absenteeism rates. But there is no obvious correlation between *parliamentary activity* and criminal background. We suggest that *attendance rates* might have a higher electoral value as they are easier to observe. In Section 3 we discussed that wealthier constituencies are more attractive for rent extraction and related to better monitoring, and criminal MPs in rich constituencies might work relatively more because they put more emphasis on their re-election prospects. This is exactly what the data shows: the difference in effort between criminals and other MPs is particularly pronounced in poor constituencies and decreases in richer areas. Criminal background has a statistically significant negative relation with both *attendance rates* and *parliamentary activity* in less and medium developed constituencies.

With regard to making use of an important local development fund for their constituency, criminal MPs are also related to lower utilization rates. The coefficient is not significant for *Criminal(a)* while being statistically significant for *Criminal(b)*. This suggests that not all MPs with criminal charges necessarily form a homogenous group. There are some individuals, who are only accused of one crime, or were falsely accused, and others who repeatedly broke the law. For the latter it is much more likely that they continue to engage in criminal activities and can, for example, use bribes or voter intimidation to secure their re-election. Throughout the paper, we try to rule out concerns about the causal interpretation using various different specifications and robustness tests. All coefficients are, however, remarkably stable across a wide range of specifications.

Credibility and trust in representatives is of crucial importance for the integrity of India's democracy, and more generally for young democracies in developing and emerging countries. Transparency increases the reporting of corruption events and corrupt officials (see Anderson et al., 2015), and the provision of background information to voters should help them make informed choices. Still, a large number of criminal candidates continues to be elected to the Indian parliament. Step by step, evidence continues to culminate around the detrimental consequences of criminals holding public office, and we hope that our paper adds to that growing evidence supporting enhanced transparency and helping voters to make informed decisions.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.ejpoleco.2018.12.005>.

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