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Building connections

Political corruption and road construction in India

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Building Connections: Political Corruption and Road Construction in India

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Abstract

Politically-driven corruption is a pervasive challenge for development, but evidence of its welfare effects are scarce. Using data from a major rural road construction programme in India we document political influence in a setting where politicians have no official role in contracting decisions. Exploiting close elections to identify the causal effect of coming to power, we show that the share of contractors whose name matches that of the winning politician increases by 83% (from 4% to 7%) in the term after a close election compared to the term before. Regression discontinuity estimates at the road level show that political interference raises costs, lowers quality, and increases the likelihood that roads go missing.

Keywords: corruption; political connections; public procurement; kinship networks

JEL Codes: D72, D73, L14, O18

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1. INTRODUCTION

A growing literature documents the private returns to holding public office and the benefits from political connections in both the developed and developing world (e.g. Fisman et al. 2014; Cingano and Pinotti, 2013).[1] It is often not clear from existing work to what extent the observed gains by public officials or connected firms represent a welfare loss. We document political influence over the allocation of individual road contracts in India's major rural road development scheme, providing econometric evidence that politicians intervene in the allocation of contracts on behalf of members of their own network. By studying the performance of firms on contracts that are likely to have been allocated preferentially, our paper provides direct evidence on the welfare costs of political corruption. A distinguishing feature of our findings is that we study a programme in which politicians have no formal role.

Specifically, we use data on more than 88,000 rural roads built under the Pradhan Mantri Gram Sadak Yojana (PMGSY) programme to study how close-election victories shift spending. Using regression discontinuity (RD) estimates to identify the causal effect of coming to power we show in our preferred specification that the share of contractors whose name matches that of the winning politician increases from 4% to 7% (an 83% increase). The magnitude of these distortions are large relative to programme size. Applying our RD estimate to the full sample (i.e. extrapolating from a LATE) would imply that state-level parliamentarians (MLAs) intervened in the allocation of roughly 1,900 of the 4,127 road contracts let to connected contractors, approximately \$540M of the \$1.2B spent on such roads and approximately 4% of the total spent on the programme. These results are broadly representative of Indian polities. Our sample consists of 4,058 electoral terms from 2001 to 2013, covering 2,632 constituencies in 24 of the 28 states which existed in our sample period.

The allocation of contracts to those with political connections does not conclusively prove that politicians' motives are corrupt. In an environment of imperfect information, MLAs could, in theory, be better informed about, and better able to monitor, contractors in their own network, and might therefore improve programme performance through benevolent interference. RD estimation at the road level provides no evidence that is the case for PMGSY road construction.

[1] See Eggers and Hainmuller (2009) for members of the UK House of Commons and Truex (2014) for Chinese deputies.

Instead, we document direct negative welfare consequences for the people the programme is supposed to serve.

We find that roads allocated to politically connected contractors are significantly more likely never to be constructed. Census data at the village-level, collected after road construction was officially completed, reveal that a number of roads listed as having been completed in the PMGSY monitoring data, and for which payments were made, do not appear to exist. We define a road as "missing" if a village it was meant to reach subsequently lacked "all-weather road access" (PMGSY's stated objective). The preferential allocation of roads is estimated to increase the likelihood of a missing all-weather road by 86%. Assuming an extrapolation from a LATE were valid, this would imply that an additional 497 all-weather roads are missing as a result of corrupt political intervention and, that the 857,000 people these roads would have served, remain at least partially cut-off from the wider Indian economy. Political interference in PMGSY is also detrimental when road construction actually takes place. Further road-level RD estimations show that roads allocated to connected contractors are more expensive to construct. These results indicate that corruption in PMGSY imposes social costs while providing no offsetting benefits in terms of efficiency or quality. Importantly, because road locations were largely determined before the elections we study, the impact of the corruption examined here arises primarily from who is allocated a contract rather than where a road is built.

Our paper's first contribution is to provide micro-evidence on informal channels of political influence. A growing number of papers document how firms benefits from political connections, (Amore and Bennedsen, 2013; Do et al., 2015). Kwaja and Mian (2005) show that banks in Pakistan lend more to politically connected firms - in spite of higher default rates. Cingano and Pinotti (2013) show that connected firms in Italy benefit from a misallocation of public expenditures, which helps them to increase profits. Mironov and Zhuravskaya (2016) show that Russian firms who funnel money in the run-up to elections are significantly more likely to receive procurement contracts after the election. We add to these recent contributions, by showing a link between the election of Indian legislators and the allocation of PMGSY road contracts to connected contractors. Preferential allocation in the context of PMGSY is particularly striking, because state-level legislators do not have any formal role in the allocation of contracts. In fact, this programme's bidding rules were designed in ways that should have forestalled political influence at the bidding stage (NRRDA 2015). Our evidence on preferential allocation in such a programme helps us to understand the economic role of local politicians, in particular in India. Recent work shows how Indian state legislators have a sizable impact on local economic outcomes. Asher and Novosad (2017) show that employment is higher in constituencies whose MLAs are aligned with the state-level government. Prakash et al. (2015) find that the election of criminal MLAs leads to lower economic growth in their constituencies. Fisman et al. (2014) show that the assets of marginally elected MLAs grow more than those of runners-up, which confirms the idea that there are substantial private returns to holding office.[4] Existing work has shown that MLAs have influence over the assignment of bureaucrats (Iyer and Mani, 2012). In further analysis, our paper documents that the misallocation of roads is stronger when MLAs and local bureaucrats (District Collectors) share the same name, and weaker when local bureaucrats are up for promotion and subject to greater scrutiny. These results suggest that bureaucrats play an important role in facilitating political corruption, and they help to explain how politicians can exert influence even when they do not have any formal role. Hence, our paper provides micro-evidence that accounts for MLAs' disproportionate impact on the economies of their constituencies, and for the private benefits they derive from holding office.

Our second contribution is to demonstrate a new approach to quantifying politicians' influence over public procurement contracting. The core challenges we confront in doing so are that: (i) there is no information on actual connections between politicians and the contractors active in their constituency; and (ii), to the extent that politicians intervene in the allocation of roads on contractors' behalf, such improper interference would not be documented. We address the first problem by constructing a surname-based measure of proximity between candidates for state-level legislatures and contractors. This approach follows a number of papers that use Indian surnames as identifiers of caste or religion (e.g. Hoff and Pandey 2004, Field et al. 2008, Banerjee et al. 2014a). Dealing with the second issue – identifying improper intervention – requires isolating the variation in proximity to contractors that results from the MLA coming to power. We do so with a regression discontinuity approach that exploits the fact that in close elections, candidates who barely lost are likely to have similar characteristics to those who were barely elected. If MLAs are intervening in the assignment of contracts, one would expect a shift in the allocation towards contractors who share their name, and no equivalent shift for their

[4] Gulzar and Pasquale (2016) also confirm the importance of MLAs for local development outcomes. In blocks that are split between different MLAs, the implementation of India's rural employment guarantee is worse than in blocks that are entirely part of one MLAs constituency.

unsuccessful opponents. This approach to detect undue influence is what Banerjee et al. (2013) refer to as a "cross-checking" method for identifying corruption: the comparison between (i) an actually observed outcome, and (ii) a counterfactual measure which should be equivalent to the former in the absence of corruption.[5] In our setting, if politicians are not intervening in the allocation of road projects, they should be no 'closer' to contractors than their unsuccessful opponents.

Our third contribution is to shed light on the social costs of political connections. In principle, allocating contracts to connected firms could be beneficial – politicians could use private information to select higher quality firms, or they could use their social networks to discipline contractors. And while the common intuition is that political influence has deleterious effects, few papers document the social costs of political connections. For example, Mironov and Zhuravskaya (2016) show that politically connected firms have lower average productivity.[8] Our unusually rich data allows us to examine the performance of contractors in the exact contracts that are likely to be preferentially allocated. We introduce a particularly powerful measure of contractor underperformance: "missing roads". These are roads that are complete in the PMGSY records and for which payments have been made, but that do not appear in the (independently conducted) Population Census. This approach allows us to provide very clear evidence of the welfare costs of undue political influence: the roads that are built by connected contractors are more likely to go missing.

This finding speaks to an old debate in the corruption literature: the contrast between costly rent-seeking or "greasing the wheels". Theoretically, corruption is typically thought of as rent-seeking. Public officials use their control over the allocation of contracts or the provision of services to ask for bribes (e.g. Becker and Stigler, 1974; Krueger, 1974; Rose-Ackerman, 1975; Shleifer and Vishny, 1993). This behaviour is most likely to arise in contexts where enforcement

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[5] Other exponents of the "cross-checking" approach include Acemoglu et al. (2014), Golden and Picci (2005), Reinnika and Svensson (2004), Olken (2007), Fisman (2001), and Banerjee et al. (2014b). Several countries conduct regular audits of local government expenditure and make the results publicly available. Examples of research based on these data include: Ferraz and Finnan (2008 and 2011) and Melo et al. (2009) for Brazil; or Larreguy, Marshall and Snyder Jr (2014) for Mexico; and Bobonis et al. (2016) for Puerto Rico. In some settings, corruption can be observed directly, as in the driving license experiment conducted by Bertrand et al. (2007) or in the trucking survey of Olken and Barron (2009).

[8] Cingano and Pinotti (2013) measure the welfare costs of preferential contract allocation through simulation techniques. Fisman and Wang (2015) show that politically connected firms in China have higher worker death rates.

is weak and officials are poorly remunerated.[9] The so-called "greasing the wheels" hypothesis argues that such corruption can be optimal in a second-best world, by allowing agents to circumvent inefficient institutions and regulation (Huntington 1968, Lui 1985). In principle, both arguments could apply to the preferential assignment of PMGSY roads by Indian MLAs.[10] However, the evidence we present on missing roads clearly supports the rent-seeking hypothesis, which is consistent with most of the recent work that relies on the direct observation of corruption (e.g. Bertrand et al., 2007) or cross-checking approaches (Banerjee et al., 2013). The simple "missing infrastructure" measure we propose, can be used in a wide variety of contexts. It is a particularly cost-effective alternative to physical road audits as conducted by Olken (2007). Improvements in remote sensing techniques mean that confirming the existence of administratively completed projects will become a very economical way to detect and measure the diversion of public funds, even when Census data is not available.

Our fourth contribution is to shed new light on the electoral motives for corruption. A standard explanation for why politicians target patronage along in-group lines, which in India often means caste, is that it acts as a form of vote-buying (Banerjee et al., 2014a). Targeting patronage could be easier within ethnic or caste groups (Chandra 2004, Horowitz 1985). In the context of large-scale contracts like the ones we study, patronage could also be used to reward firms who help fund political campaigns. Mironov and Zhuravskaya (2016) document how Russian politicians allocate contracts to firms who funded their election. Sukhtankar (2012) shows that political candidates in India siphon funds from sugar mills in election years. However, in our analysis, we find no evidence that the preferential allocation of roads or cost inflation increase immediately before or after election dates. If vote-buying is going on for this programme it must be a long-run transaction. If anything, we observe that roads built by connected contractors are less expensive around election periods – which could be consistent with higher scrutiny in election times. In the context of Puerto Rican municipalities, Bobonis et al. (2016) show that financial audits are most effective in reducing corruption when they are conducted shortly before elections. A recent paper by Bohlken (2016) argues that road

[9] In the case of Indian MLAs, calculating efficiency wages (as suggested by Becker and Stigler, 1974) may be complicated by the fact that candidates frequently need to pay their parties significant sums for their place on the ticket. This could prompt them to engage in corrupt behaviour once elected (Jensenius, 2013).

[10] An intermediate argument is that initial corrupt allocations may not matter if there is scope for Coasian bargaining. Sukhtankar (2015) finds evidence in this direction for the allocation of the wireless spectrum in India.

completion in PMGSY is higher when the ruling party in the state is aligned with local MLAs in marginal constituencies, which suggests that voters hold the state government accountable for PMGSY performance. As a second test of electoral motives for corruption, we exploit India's 2008 re-drawing of electoral constituency boundaries to study the behaviour of MLAs in regions that have become "politically irrelevant" after the redistricting. We find no evidence of different behaviour in these regions. Thus, while our paper documents the preferential allocation of road contracts, we find no evidence linking corrupt behaviour to electoral incentives.

Our results are more consistent with either standard in-group favouritism, or a subtler mechanism by which caste or kinship networks facilitate corrupt exchange under the threat of punishment. Corruption is illegal and therefore requires either trust among collaborators, or a predictable ability to sanction defections, both of which are more likely to exist between members of the same family, ethnic group, or network (Lambsdorff 2002; Tonoyan, 2003). While we are unable to test it explicitly, this interpretation fits our findings and the context of PMGSY. The involvement of the central government in the programme guarantees a minimum level of monitoring. In line with the idea that contractors trade off rent-seeking and the cost of detection, we find no evidence that preferential allocation affects the performance markers that are most easily observed in the administrative data collected at the central level: over-runs and delays. We also find that preferentially allocated contracts are less expensive in the run-up to elections, a time when monitoring may be greater.

The remainder of the paper proceeds as follows. Section 2 provides context on PMGSY, the role of MLAs, and Indian surnames as identifiers of caste or religion. Section 3 describes the dataset used in the analysis. Section 4 outlines the empirical strategy. Section 5 presents the main results on re-allocation and robustness. Section 6 analyses the social costs of re-allocation. Section 7 provides evidence for the intermediary role of bureaucrats. Section 8 evaluates electoral motives for corruption. Section 9 concludes.

2. BACKGROUND

2.1 PMGSY

In the year 2000, an estimated 330,000 Indian villages or habitations – out of a total of 825,000 – were not connected to a road that provided all-weather access (PMGSY 2004). Their inhabitants were at least partially cut-off from economic opportunities and public services (such as health care and education). To address this lack of connectivity, the Indian government launched the Pradhan Mantri Gram Sadak Yojana (PMGSY) in December 2000. Its goal was to ensure all-weather access to all habitations with populations over 1,000 by the year 2003, and to those with more than 500 inhabitants by 2007. In hill states, desert and tribal areas, as well as districts with Naxalite insurgent activity, habitations with a population over 250 were targeted (PMGSY 2004). The proposed network of roads was determined ex-ante in 2001, and the implementation of PMGSY in subsequent decades has consisted of the gradual realisation of this "Core Network".

The programme has been described as "unprecedented in its scale and scope" (Aggarwal 2015), with roadwork for over 125,000 habitations completed and another 22,000 under construction as of November 2016.[17] A second phase of the scheme (PMGSY II), launched in 2013, targets all habitations with populations over 100. According to World Bank estimates, expenditures under PMGSY had reached 14.6 billion USD by the end of 2010, with a further 40 billion USD required for its completion by 2020 (World Bank, 2014).

Several studies have focused on the first-order research question that arises in relation to PMGSY: its impact on habitations and the lives of their inhabitants. Asher and Novosad (2016) analyse the employment effects of the programme in previously unconnected villages. They find that a new paved road raises participation in the wage labour market with a commensurate decrease in the share of workers employed in agriculture. This translates into higher household earnings and a rise in the share of households who live in houses with solid roof and walls. Aggarwal (2015) also finds a positive effect on employment and reduced price dispersion among villages. While these studies analyse *what* PMGSY has achieved, this paper looks at *how* it has been implemented.

Compared to other public works programmes, the implementation of PMGSY stands out because of its reliance on private contractors combined with relatively strong monitoring and quality assurance provisions, designed to limit the scope for undue corruption. All tenders have to follow a competitive bidding procedure, for which the rules were prescribed by the National

[17] OMMAS (Online Management, Monitoring and Accounting System), http://omms.nic.in/, accessed in November 2016.

Rural Roads Development Agency (NRRDA) and set out in the so-called Standard Bidding Document (SBD). The SBD consists of a two envelope tendering process administered at the circle level. Each bid consists of both technical and financial volumes. The technical bids are opened first. Contractors have to fulfil eligibility criteria, taking into account factors such as their current workload and experience. Only the financial bids of contractors whose technical bids are found to meet the requirements are evaluated, and subject to meeting the technical standards, the lowest bidder has to be selected. After the contract has been assigned, administrative data on the programme is gathered, while central and state-level inspectors can carry out quality inspections. In spite of these provisions, there remains clear scope for corruption, and the financial incentives are sizeable given the scale of the project.[18] A large number of newspaper reports document alleged corruption in PMGSY.[19] Corruption in PMGSY could take several forms, and the possible manipulation of road allocations is one of the challenges for impact evaluations of the programme (Asher and Novosad, 2016).[20] Our paper tests for a specific form of corruption: interventions by state-level parliamentarians (MLAs) in the allocation of road contracts (but not of the location of roads) within their constituencies.

An advantage of focussing on MLAs in this context is that under the programme guidelines, they should be in no way involved in the tendering process or the selection of contractors. In fact, they are granted practically no official role in the implementation of PMGSY whatsoever.[21] Funding for PMGSY comes primarily from the central government. The scheme is managed by local Programme Implementation Units (PIUs), which are under the control of State Rural Roads

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[18] Existing work reports that the price bid of only one firm was evaluated in 95% of a random sample of 190 road contracts issued between 2001 and 2006 in Uttar Pradesh; i.e. only one bid submitted or all other bids were disqualified based on technical requirements (Lewis-Faupel et al., 2016).

[19] Examples include articles in "The Hindu" on April 11 2012, "The Economic Times" on March 8 2013, "The Arunachal Times" on March 6 2013, the online news-platform "oneindia" on July 31 2006, and "Zee News" on 30 August 2014. For example, the "oneindia" article reports that the former Chief Minister of Sikkim accused the current administration of "widescale corruption" in the implementation of PMGSY and "alleged that the works were awarded to relatives of Chief Minister, Ministers and MLAs of the state".

[20] These authors find that the habitation population figures reported to PMGSY had been manipulated, particularly around the 1,000 and 500 population cut-offs used to target the programme.

[21] MLAs are mentioned in the PMGSY guidelines, but only in reference to the initial planning stage. Intermediate panchayats and District panchayats were responsible for drawing up a planned "Core Network" which encompasses all future roadwork to be carried out under PMGSY. These plans were to be circulated to MPs and MLAs, whose suggestions were to be incorporated. MLAs could therefore have influenced which habitations were targeted ex-ante through official channels. However, this role is irrelevant for the timing of the construction work and assignment of road contracts, on which MLAs have no formal influence. Moreover, these consultations took place prior to our sample period. The majority of MLAs in our sample were not in office at the time and therefore had no opportunity to review the planned network. Our results are unchanged when we drop MLAs who were in office prior to 2001 from the sample (see appendix table A16).

Development Agencies (SRRDA). These agencies are responsible for inviting tenders and awarding contracts. Given their lack of formal involvement, any systematic relationship between MLAs and the contractors working in their constituencies can therefore, in itself, be construed as evidence for an irregularity in the allocation of contracts.

2.2 The role of MLAs

MLAs, or Members of Legislative Assembly, are India's state-level parliamentarians. They are elected for 5-year terms in a first-past-the-post voting system in state-wide elections. In general, the state assembly elections in India's different states do not coincide. MLAs are typically nominated by the party, and each MLA represents a single constituency.

Is it plausible that these MLAs would seek to intervene on behalf of specific contractors? While their official function is to represent their constituents in state legislative assemblies, surveyed MLAs overwhelmingly report this to be a minor part of their work (Chopra 1996). State assemblies meet rarely and according to Jensenius (2013), individual legislators have little impact on political decisions: "much more important to the MLAs are all their unofficial tasks of delivering pork, blessing occasions, and helping people out with their individual problems". Qualitative accounts suggest that MLAs spend much of their time receiving requests from their constituents. Describing such meetings Chopra (1996) writes "constituents came to ask for favours that clearly contravened rules and laws". MLAs often respond to requests by passing them on to ministers or high-ranking officials, and they are also known to put pressure on bureaucrats by threatening them with reassignment (Iyer and Mani 2012, Bussell 2015).

2.3 Surnames as a measure of interpersonal proximity in India

To measure proximity between MLAs and contractors we construct a proxy based on politicians' and contractors' surnames.[22] Indian surnames can be an indicator of caste affiliation, religion, or geographic provenance. Naming conventions differ across India; it is common for Indians to have multiple surnames and the same name can appear in different positions within the list of names. This is also true of caste identifiers. Still, as a general rule, the last name will be caste or religion specific and follow the paternal line. This pattern is sufficiently strong for Indian

[22] Angelucci et al. (2010), and Mastrobuoni and Patacchini (2012) also uses name-based matching to study social networks.

surnames to have been used as identifiers of caste or religion in many empirical studies (Banerjee et al. 2014a, Hoff and Pandey 2004, Vissa 2011, Fisman et al. 2012, Field et al. 2008). One clear exception to this rule is Tamil Nadu, where surnames do not exist, and we will document that our method does not work for Tamil Nadu. Surnames are a significantly more accurate predictor of connections when comparing individuals from the same area or linguistic region. The contractors and politicians in our setting are highly likely to be from the state and will in many cases be from the same district or constituency.

Our paper treats a match between the names of a politician and a contractor as a rough overall measure of proximity, without seeking to establish whether the individuals are of the same religion, caste, or (potentially) family. All of these types of connections are likely to increase the probability that a contractor would approach an MLA when bidding for a contract, and that the MLA would be receptive.

Name-based matching is an imperfect measure of proximity. Contractors may have connections to politicians without sharing a name, or equally, share a name but have no connection. Surnames that are not caste-identifiers, former honorific titles for example, are likely to dilute the accuracy of the measure. Hence, the estimates in this paper can be viewed as a lower bound for MLAs' true effect on contract allocation.

3. DATA

The empirical strategy requires three kinds of data. Information on contractors and agreements is available in the administrative records of the PMGSY project, at the road level. Data on political candidates and elections are at the level of the assembly constituency. These two are linked using the population census, which allows for habitations to be matched to constituencies, as well as providing additional covariates used in the analysis.

3.1 PMGSY data

The administrative records of projects sanctioned under PMGSY are publicly available in the Online Management, Monitoring, and Accounting System (OMMAS). The dataset used for this paper contains the agreement details of 110,185 roads serving 188,394 habitations. This information includes: the date of contract signing, sanctioned cost, proposed length, proposed date of completion, name of the contracting company, and – crucially for this analysis – the name of the winning contractor. In addition to the agreement details, which precede road construction, the OMMAS also contains later data on the physical progress of work, data on completed roads, and reports from subsequent quality inspections. These are used in section 6 to evaluate the effect of political interference on the efficiency and quality of road construction.

3.2. Assembly election data

The Election Commission of India (ECI) publishes statistical reports on assembly elections that record each candidate's name, party, gender and vote share. Since 2003, candidates have moreover been required to submit sworn affidavits to the ECI with information on their assets, liabilities, educational attainment, and any pending criminal cases. Both the election reports and affidavits are publicly available from the ECI in pdf format. This paper draws on digitised versions of this information from four separate sources. Table A1 of the online appendix lists these sources – which cover different time periods and variables – and describes which variables from each source are used in the analysis (all these secondary sources are based on the ECI).[23]

Assembly elections operate on a plurality rule. While the median number of candidates per election is eight, typically only the top-2 candidates are competitive: the third placed candidates average 7% of the vote, the fourth placed candidates average 3%, the fifth 1.6% and the rest less than 1%. To estimate the RD we restrict attention to elections in which there are PMGSY contracts issued in the term before and after the election and focus on the winner and runner-up. This gives us a sample of 8,116 candidates in 4,058 elections from 2001 to 2013, covering 2,632 constituencies. In our preferred specification we estimate on the resulting sample of 8,116 candidate-terms. In a placebo test we show that the effects are not present for the contrast between the runner-up and third-placed candidate. Map 1 (of the online appendix) shows the constituencies included in the sample which cover 24 of the 28 states that existed during the

[23] The matching process is complicated by discrepancies in the spelling of constituency and candidate names. These occur not only across datasets but also across time within datasets. Using different secondary sources helps us to construct a consistent data set. In a small number of cases, multiple constituencies within the same state have the same name. We drop all of these constituencies from our sample, to prevent false matches between election datasets and to avoid the risk of assigning roads to the wrong constituency.

timeframe under analysis.[24] Map 2 shows the constituencies which had at least one close election, the sub-sample for our local linear RD estimation.

3.3. Matching roads and electoral terms using census data

The Population Census of India 2001 contains village-level data on demographic and socio-economic variables used as controls in the analysis. We use the Village Amenities part of the 2011 census, to identify the 'missing roads' evaluated in section 6.

The 2001 census is also the source for habitation-level data, which is collected by the PMGSY in order to determine the prioritisation of roads. This includes information on the size of the population (the project guidelines stipulate that habitations above certain population thresholds are to be prioritised), whether or not it was connected to a road in 2001, and if so, whether this road provided all-weather access. Moreover, it reports the MLA constituency in which it each habitation was situated in 2001.

Using this information, it is possible to match PMGSY roads (at the habitation-level) to the assembly election data described in the previous sub-section. However, changes in the delimitation of MLA constituencies – which took effect in mid-2008 – led to changes in boundaries, the abolition of some constituencies, and the creation of new ones. For roads built in electoral terms after the new delimitation we use the coordinates of habitations and match these to GIS data on constituency boundaries.

While the census data allows for spatial matching of roads and constituencies, it is also necessary to match them in time. Road contracts are allocated to electoral terms based on the date of the agreement, as recorded in the PMGSY data. In order to precisely assign road contracts, it is necessary to set an exact date that marks the end of one term and the beginning of the next. We define this as the date on which the results of an election are announced.[25]

3.4 Matching politicians and contractors using surnames

In the electoral terms that preceded and followed the elections in the sample, 88,020 road agreements were signed. For each political candidate, we assess whether they share a surname with the contractors who received projects in their constituency in the term after the election. For

[24] Goa, Meghalaya, Nagaland and Sikkim are not part of our sample.
[25] These dates were collected from the website www.electionsinindia.com (accessed in 2015).

every politician-contractor pair, we exclude all names except for each individual's final name and then look for matches among these surnames. The results are, however, robust to broader definitions of matches.[26] To account for different spellings of the same name, we implement a fuzzy matching algorithm optimised for Hindi names.

Matches are aggregated at the electoral term level as follows. The variable $match_{nijt}$ takes the value of 1 if the contractor for a road agreement *n*, signed in constituency *j* in term *t*, shares a name with candidate *i,* and 0 otherwise. This variable is determined for the *N* road agreements signed in the constituency during an electoral term. $share_{ijt}$ is defined as the share of contracts in term *t* allocated to contractors who share a candidate's name. $share_{ijt-1}$ provides the equivalent share for contracts in the term prior to the election in which a candidate took part.

$$share_{ijt} = \frac{\sum_{n=1}^{n} match_{nijt}}{N_{jt}} \qquad\qquad share_{ijt-1} = \frac{\sum_{n=1}^{n} match_{nijt-1}}{N_{jt-1}}$$

The dependent variable in the main regressions is the difference between these two:

$$\Delta share_{ijt} = share_{ijt} - share_{ijt-1},$$

which we calculate for all candidates $i \in \{winner, runner\text{-}up\}$.

A complication arises in elections where winning and losing candidates have the same surname. It is not possible to estimate the effect of winning an election in this situation, as candidates who lost will see their proximity to contractors evolve in parallel to that of the elected politicians. In the main regressions, we therefore exclude candidates from elections where this issue arises.

3.5 Descriptive statistics

Table 1 reports descriptive statistics for the sample of candidates used in the main regressions. For the average term in the sample, the number of road contracts signed is 28. The average value of $share_{ijt-1}$ – which can be construed as a baseline measure of the frequency of surname-matches – is 4%. There is however, significant geographic variation in the frequency of matches, ranging from a mean of 0% in Mizoram to a mean of 13% in Andhra Pradesh (Map 3 of the

[26] The results are robust to considering all matches among individuals' names (excluding their first name) or only matches based only on the last two names.

online appendix shows this variation at the constituency-level).[28] However, these means do not distinguish between winning and losing candidates – the variation exploited in the empirical strategy below.

[28] It is likely that these baseline frequencies lead to heterogeneous treatment effects. In states or constituencies, where the distribution of names is such that matches are relatively rare, a politician who is elected may not have many potential contractors of the same name to allocate roads to.

Table 1A: Descriptive Statistics (Candidate/Constituency)

Variable	Observations	Mean	Std. Dev.	Min	Max
Panel A: Roads allocated to contractors of the same name					
$Share_{t-1}$	8116	0.037	0.141	0.000	1.000
$Share_{t}$	8116	0.035	0.134	0.000	1.000
ΔShare	8116	-0.002	0.151	-1.000	1.000
Panel B: Candidate characteristics					
Vote share	8036	0.279	0.103	0.020	0.837
Margin	8116	0.000	0.102	-0.695	0.695
Incumbent	8116	0.282	0.450	0.000	1.000
Runner-up previous election	8116	0.147	0.354	0.000	1.000
Age	7357	49.164	10.207	23	87
Female candidate	8116	0.062	0.242	0.000	1.000
Candidate with criminal charges	4434	0.182	0.386	0.000	1.000
Total assets (1000000s of INR)	5010	106	4740	0.000	300000
Liabilities (1000000s of INR)	5286	1.841	17.800	0.000	644
University graduate	5286	0.596	0.491	0.000	1.000
Postgraduate degree	5286	0.192	0.394	0.000	1.000
Congress candidate	7239	0.295	0.456	0.000	1.000
BJP candidate	7239	0.203	0.403	0.000	1.000
Named Kumar	8116	0.058	0.234	0.000	1.000
Named Lal	8116	0.022	0.145	0.000	1.000
Named Patel	8116	0.009	0.094	0.000	1.000
Named Ram	8116	0.018	0.133	0.000	1.000
Named Reddy	8116	0.016	0.124	0.000	1.000
Named Singh	8116	0.112	0.316	0.000	1.000
Named Yadav	8116	0.014	0.117	0.000	1.000
Panel C: Constituency characteristics					
Reserved seat	8116	0.335	1.349	0.000	84.092
Road $count_{t}$	8116	27.691	30.822	1.000	479
Road $count_{t-1}$	8116	22.086	25.744	1.000	388
Mean road $length_{t}$	8116	5.833	3.999	0.350	42.654
Mean road $length_{t-1}$	8116	4.963	3.838	0.410	53.985
Mean population	7822	961.697	633.986	30.000	7230
Mean SC/ST population	7822	244.078	193.401	0.000	2283
Mean connectivity	7822	0.561	0.308	0.000	1.000

Note: The number of observations varies due to missing values. Reserved seat refers to constituency reserved for MLAs from scheduled castes or tribes. Road $count_{t}$ is computed at the term-level by counting the number of road contracts signed in a constituency within a term. Mean road length is the average length of roads (in km) built in a constituency and term. Mean population and mean SC/ST population are averages of 2001 census data for all of a constituency's villages. Mean connectivity is the share of a constituency's villages that had all-weather road access at the time of the 2001 census.

Table 1B: Descriptive Statistics (Roads built by same name contractors)

Variable	Observations	Mean	Std. Dev.	Min	Max
Panel A: Road construction					
Length of road	4,921	3.997	3.654	0.01	41
Cost (1000000s of INR)	4,921	133.664	149.625	3	2730.56
Days overrun	3,202	588.024	543.761	-1750	3932
Actual cost/sanctioned cost	3,867	0.954	0.257	0	9.349
Failed Inspection	1,513	0.365	0.482	0	1
Missing all-weather road	1,815	0.260	0.438	0	1
Missing any road	1,815	0.025	0.156	0	1
Panel B: Local Geography					
Altitude	4,319	670.8	827.1	7.7	4864.6
Ruggedness	4,319	0.277	0.695	0	8.506
Forest Cover	4,494	0.398	0.110	0	0.861
Bridge	4,921	0.008	0.091	0	1
Panel C: Village Demographics					
Total population	4,464	2416.8	3425.2	1	49192
Number of households	4,464	449.2	707.8	1	9851
Village area	4,482	722.7	1772.0	0	67737
Sex ratio	4,462	1.062	0.101	0.583	2.1
Population under 6	4,464	0.179	0.042	0	0.335
SC share	4,464	0.173	0.154	0	1
ST share	4,464	0.106	0.238	0	1
Panel D: Village Socioeconomic Characteristics					
Literacy	2,952	54.019	16.546	0	100
Employment	2,952	39.166	12.141	12.938	100
Male employment	2,952	67.500	36.700	22.716	100
Female employment	2,952	27.431	19.772	0	82.304
Drinking water	4,464	0.998	0.047	0	1
Power supply	4,461	0.767	0.423	0	1
Phone connections	4,482	10.576	79.597	0	1713
Approach path	3,837	0.605	0.489	0	1

Note: Table 1b provides descriptive statistics for the sample used in Tables 4, 5 and columns 4-6 of Table 6. Cost, delays, cost overruns, and quality inspections are based on the PMGSY data. Quality is a dummy variable equal to one if the road is "unsatisfactory" or "in need of improvement" in latest inspection. "Missing roads" are defined on the basis of the 2011 census and the PMGSY data. The dummy for the all-weather road missing variable takes the value of one if any village on the route of an officially completed road lacks all-weather road access according to the 2011 census. For the missing-any-road definition, we set the missing dummy equal to one if all villages on the completed PMGSY road had no road of any type (tarmac, gravel, or water bound macadam) according to the 2011 census.

4. EMPIRICAL STRATEGY

A natural control group for elected politicians are those who aspire to the same office. If being an MLA is associated with the power to intervene in the allocation of roads in one's constituency, one would expect the share of contractors with the same name as a winning candidate to be higher than the corresponding share for losing candidates.

As our main outcome of interest, we use the first difference of $share_{ijt}$. Taking the difference should remove unobservable, time-invariant characteristics of an individual candidate that may be correlated with the number of matches with contractors. In our context, this is primarily a way of controlling for specificities that individual names may have within certain constituencies. Some candidates' names will be more common than others. Some may be more prevalent among certain professions (e.g. contractors) for historical reasons. Under the assumption that winning and losing candidates had a common trend in their share of matches with contractors, a simple difference-in-differences (DiD) approach would be sufficient for identification. However, given that winners are likely to be systematically different from losing candidates in many respects, it is possible that they may face divergent trends in $share_{ijt}$ that are not determined by election outcomes. This suggests the use of a regression discontinuity (RD) design.[29]

In order to identify whether there is a causal relationship between the election of politicians and the allocation of road contracts in their constituencies, we exploit the fact that in close elections, the assignment of victory can be considered conditionally independent of subsequent contracting patterns. The underlying assumption is that candidates who won an election by a very small margin are comparable to those who narrowly lost (Lee, 2008). We evaluate whether this assumption holds in our sample by running balance checks on observable characteristics (see below). In order to determine how close elections were, we define the variable $margin_{ijt}$ for candidate *i* in constituency *j* and term *t*:

$$margin_{ijt} = \begin{cases} vote\ share(winner)_{jt} - vote\ share(runnerup)_{jt} & if\ winner_{ijt} = 1 \\ vote\ share(runnerup)_{jt} - vote\ share(winner)_{jt} & if\ winner_{ijt} = 0 \end{cases}$$

[29] We obtain similar results when estimating the RD using levels with $share_{ijt}$ as the dependent variable (Table A2). Taking the first difference of $share_{ijt}$ in our RD estimation contributes to precision and allows for a direct comparison of our RD results with the DiD results for the full sample (see discussion of LATE in section 5).

We estimate equation (1) in a non-parametric RD for a range of bandwidths μ, controlling for the assignment variable $margin_{ijt}$ and its interaction with $winner_{ijt}$ to allow for a different relationship between $\Delta share_{ijt}$ and $margin_{ijt}$ among winning and losing candidates:

$$\Delta share_{ijt} = \alpha + \beta winner_{ijt} + \delta margin_{ijt} + \rho winner * margin_{ijt} + \varepsilon_{ijt}$$
$$\forall\, i \text{ where } margin_{ijt} \in [-\mu, \mu] \text{ and } i \in \{winner, runner\text{-}up\} \qquad (1)$$

In order to improve the efficiency of the estimates, we introduce constituency-level controls, individual-level controls, state fixed-effects, and year fixed-effects in most specifications although these are not required for identification.[30] Because we have the top-two candidates in each election we cluster standard errors at the election level.[31] The results of non-parametric RD estimations can be sensitive to the choice of bandwidth, and there is trade-off between bias and efficiency inherent in this choice (Lee and Lemieux 2010). For our main results we apply two bandwidths: 6.2% (derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman 2011) and a more conservative bandwidth of 3%. In later sections, when we show results for other dependent variables with different optimal bandwidths, we continue to report the 3% bandwidth for consistency.[32]

While using an RD to identify the effects of electoral outcomes is standard, our setting is different from many applications in that we exploit within-constituency variation. We compare winning and losing candidates (and the contractors sharing their surnames), rather than the electoral constituencies which were narrowly won or lost by a particular type of candidate. Recent work using similar candidate-level RDs includes Do et al. (2015) and Fisman et al. (2014).

5. REALLOCATION RESULT

5.1 Continuity test

[30] Legislative assembly terms are not synchronised across Indian states. In each year in our sample window, there were elections in multiple states.
[31] The main results are robust to clustering standard errors at the state-year level to account for within-state-political-season correlations in the errors.
[32] Applying the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011) for each RD estimation in our paper, the mean optimal bandwidth is 3.8% and the median is 3.5%.

Our identification strategy is based on the premise that all confounding factors are smooth at the treatment threshold, so that any discontinuities capture the causal effect of a politician winning his/her MLA election. Table 2 presents the results of a continuity test for the optimal bandwidth of 6.2% and the 3% bandwidth. At the optimal bandwidth none of the MLA characteristics display a discontinuity when the vote margin exceeds zero. At 3%, only two variables are discontinuous at the 10% significance level.[33]

Table 2: Continuity test for MLA level local linear regression

Margin of victory:	6.2%			3%		
	Obs.	Winner	St. Error	Obs.	Winner	St. Error
Panel A: Candidate characteristics						
$Share_{t-1}$	4,396	-0.0072	(0.0084)	2,472	-0.0114	(0.3339)
Incumbent	4,396	-0.0366	(0.0296)	2,472	-0.0644	(0.1164)
Runner-up in prev. election	4,396	-0.0119	(0.0214)	2,472	0.0212	(0.4719)
Age	4,036	0.2874	(0.6014)	2,314	0.3868	(0.6441)
Female candidate	4,396	-0.0004	(0.0129)	2,472	-0.0086	(0.6140)
Cand. faces criminal charge	3,352	0.0075	(0.0264)	1,889	0.0562	(0.0370)
Assets (INR millions)	2,770	261.119	(718.93)	1,554	365.99	(0.7542)
Liabilities (INR millions)	3,049	0.1260	(0.5146)	1,747	-0.0494	(0.9168)
University degree	3,049	-0.0139	(0.0328)	1,747	-0.0262	(0.5587)
Post-grad. Degree	3,049	-0.0047	(0.0271)	1,747	-0.0150	(0.6873)
BJP candidate	3,940	0.0104	(0.0274)	2,465	-0.0468	(0.2556)
Congress candidate	3,940	-0.0194	(0.0312)	2,461	0.0265	(0.4634)
Panel B: Share of roads built by contractors of same name in term prior to election						
Share 5 yrs before election	2,502	0.0078	(0.0111)	1,416	0.0138	(0.3714)
Share 4 yrs before election	2,898	-0.0151	(0.0124)	1,624	-0.0060	(0.7304)
Share 3 yrs before election	2,634	-0.0096	(0.0116)	1,490	-0.0119	(0.4402)
Share 2 yrs before election	1,688	0.0037	(0.0133)	942	0.0319*	(0.0926)
Share 1 year before election	1,866	-0.0131	(0.0152)	1,070	-0.0203	(0.3278)
Panel C: Prevalence of most common names						
Named Kumar	4,396	0.0119	(0.0136)	2,472	0.0190*	(0.0878)
Named Lal	4,396	-0.0019	(0.0084)	2,472	-0.0049	(0.5208)
Named Patel	4,396	0.0026	(0.0061)	2,472	0.0060	(0.4477)
Named Ram	4,396	-0.0021	(0.0076)	2,472	0.0068	(0.3950)
Named Reddy	4,396	0.0074	(0.0054)	2,472	-0.0066	(0.4123)
Named Singh	4,396	0.0195	(0.0171)	2,472	0.0217	(0.3200)
Named Yadav	4,396	0.0052	(0.0076)	2,472	0.0067	(0.5178)

Note: Coefficients are estimated by regressing the row variables on winner, the vote margin, and the vote margin interacted with winner in OLS regressions Standard errors are clustered at the election level. The bandwidth of 6.2% is derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

5.2 Main results

[33] Our results are robust to controlling for the name Kumar, and the overall pattern of results in Panel B does not suggest differential pre-trends. A simple comparison of means for winners and losers within the 3% bandwidth shows that the two samples are balanced on all variables, including the name Kumar and the share two years before the election (appendix Table A3).

The results of local linear regression RD estimation are presented in Table 3 and Figure 1. For each bandwidth there are two columns in the table. The first corresponds to the basic RD in equation (1). The second adds state fixed effects, year fixed effects and additional controls. These include whether or not a constituency is reserved for candidates from scheduled castes (SC) or scheduled tribes (ST), characteristics of the PMGSY roads built in the constituency prior to the election, and candidate-level controls. The latter set of variables includes a candidate's vote share, their age, gender, and whether they were an incumbent or a former runner-up.

For both bandwidths, the effect of winning an election on the change in $share_{ijt}$ is consistently positive and significant.[34] In our preferred specification including fixed effects and the full set of controls the coefficient is around 0.024 at the 6.2% bandwidth and 0.032 at the 3% bandwidth. Relative to the baseline, pre-election level of matches, the latter estimate implies that the effect of a candidate coming to power is an 83% increase in the share of roads allocated to contractors who share their surname. [35]

Reassuringly, the results are consistent across a wide range of bandwidths. Figure 2 plots the coefficient on $winner_{ijt}$ for the main specification for different bandwidths. As the samples get smaller the estimates are less precise but the coefficient is relatively stable for all but very small bandwidths (less than 1%).

Relative to the total number of roads – most of which are allocated to contractors whose name does not match the MLA's – the absolute value of the coefficient implies a small effect. Yet as explained in section 3.3, these estimates can be considered a lower bound on MLAs' true intervention in PMGSY contract allocation. [36]

[34] In Appendix Table A4 we report results for a fully non-parametric RD, excluding the controls for the running variable and its interaction with the treatment. The results are similar in magnitude and significant for all bandwidths.
[35] Fafchamps and Labonne (2016) provide evidence from the Philippines that politicians punish family members of losing candidates. In theory, our results could be partly driven by reductions for contractors connected to losing candidates. Our identification strategy does not allow us to test this directly. However, we analyze the simple change in $share_{ijt}$ for losing candidates comparing the terms before and after the election. Excluding losing incumbents (for whom there is a significant reduction after they leave office), we find no significant decline in $share_{ijt}$ for losing candidates (see Table A5).
[36] Spurious matches in names will bias the coefficient towards zero. In Appendix table A6 we drop all elections where either candidate has a name that occurs with a frequency of more than 10% in their state. This reduces the mean within-state name frequency by around 50%, significantly reducing the likelihood of spurious matches. Compared to our main results the implied proportional increase in the share of matches rises from 83% to 108%.

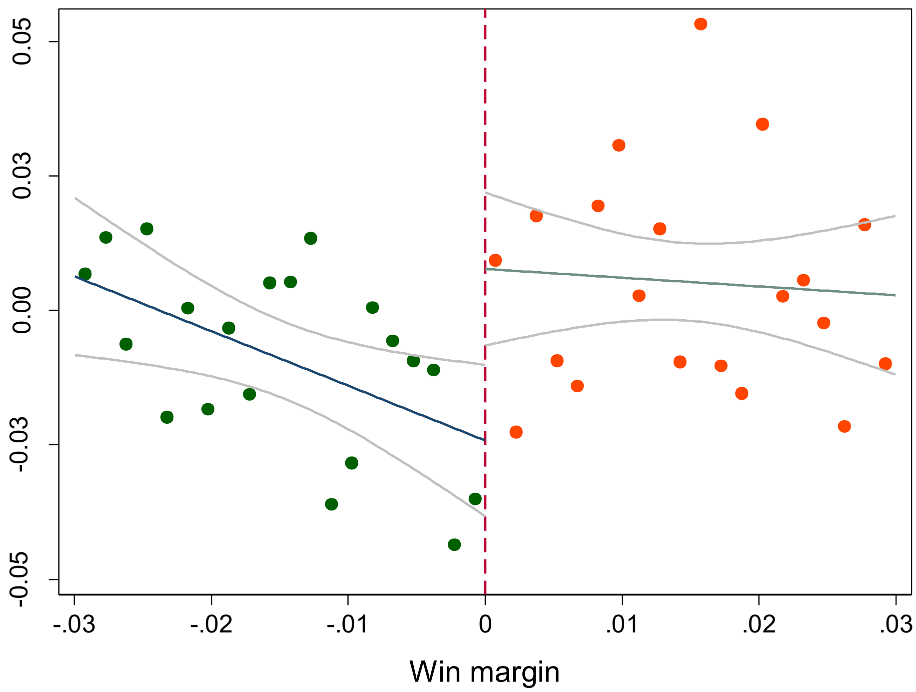
Table 3: Local linear regression RD

ΔShare of same name contractors$_t$	Whole Sample		Margin of Victory <6.2%		Margin of Victory <3%	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0094*	0.0099*	0.0252***	0.0242**	0.0319**	0.0323**
	(0.0052)	(0.0054)	(0.0092)	(0.0097)	(0.0128)	(0.0138)
Margin	-0.0001	0.0114	-0.3153*	-0.2697	-1.0165**	-1.0816**
	(0.0299)	(0.0337)	(0.1856)	(0.1839)	(0.5049)	(0.5481)
Margin*winner	-0.0059	-0.0353	0.0835	0.0577	0.8541	0.9464
	(0.0414)	(0.0445)	(0.2724)	(0.2671)	(0.6269)	(0.6683)
Incumbent		-0.0014		0.0034		-0.0053
		(0.0045)		(0.0065)		(0.0086)
Runner-up in previous election		0.0068		0.0074		0.0069
		(0.0055)		(0.0079)		(0.0110)
Female candidate		-0.0012		-0.0130		-0.0014
		(0.0066)		(0.0107)		(0.0117)
Age		0.0002		-0.0000		0.0004
		(0.0002)		(0.0003)		(0.0003)
AC controls		X		X		X
State fixed effects		X		X		X
Election year fixed effects		X		X		X
N	8,116	7,290	4,396	4,012	2,472	2,248
Control group mean share$_{t-1}$	0.0342	0.0344	0.0355	0.0362	0.0383	0.0390

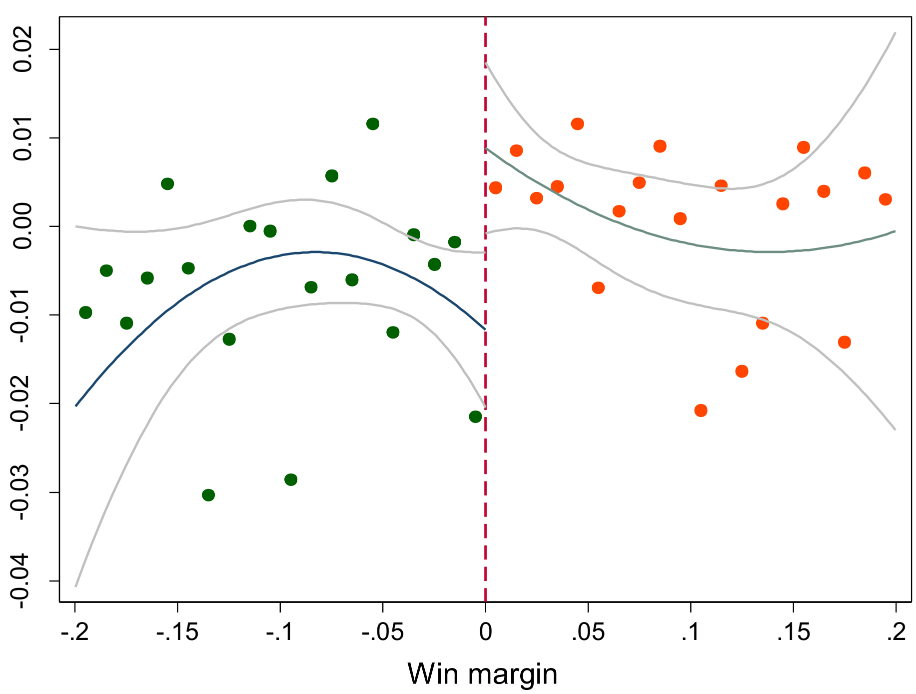
Note: Local linear regression estimates. Standard errors are clustered at the election-level. Variables are defined either in the text or in the note for table 1. AC controls include: Reserved seat, Road count$_{t-1}$, Mean population, Mean SC/ST population, Mean connectivity, Mean road length$_{t-1}$. The bandwidth of 6.2% is derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Graphical depiction of RD

Change in share of same name contractors – linear fit

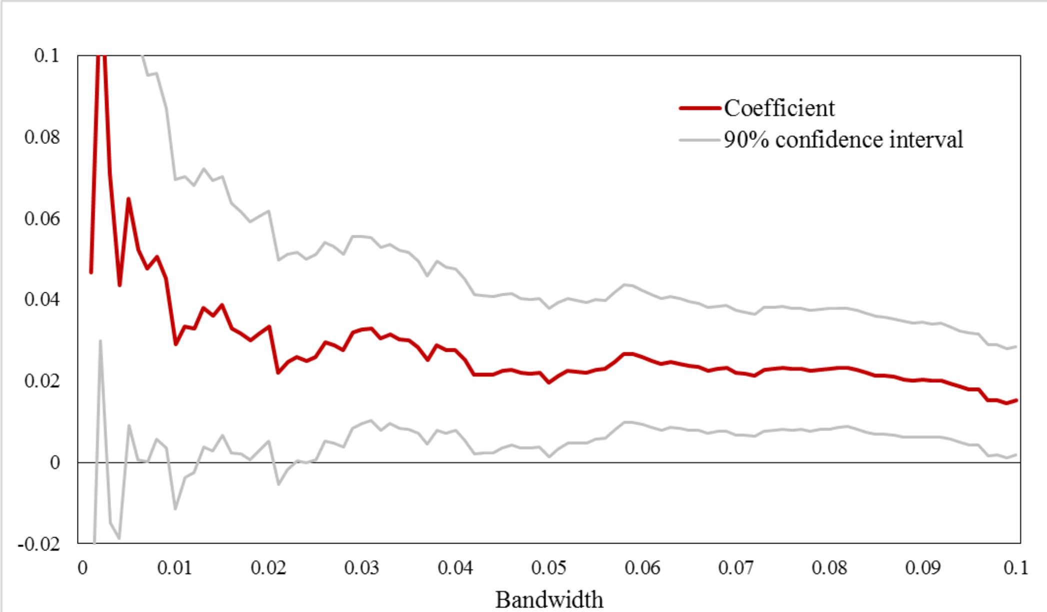


Change in share of same name contractors – quadratic fit



Note: Lines fitted separately on the samples left and right of the cut-off. Each dot represents the mean for a bin. The number of observations in a bin varies, based on the density of candidates at a given margin. 90% confidence intervals plotted in grey. The first panel shows a linear fit within the 3% bandwidth used in the RD estimation. The second shows a quadratic fit for a wider margin of 20%.

Figure 2: Main effect by bandwidth



Note: The chart plots the coefficient for winner in our main specification (equivalent to Table 3, columns 2, 4, 6, and 8) with the full set of candidate and constituency controls as well as state and year fixed effects.

If the results are interpreted as evidence of improper political involvement in the assignment of roads, it raises the question whether this improper involvement *only* occurs on behalf of individuals with the same surname. In this sense the sign and significance of the coefficient might be seen as more important than the magnitude. Secondly, given the scale of PMGSY, even a relatively small fraction can translate into what can be considered a sizeable number of affected roads and substantial financial expenditure. This is illustrated by the following, back-of-the-envelope calculation. In our dataset (including the first electoral term), 4,127 road projects were allocated to contractors sharing a name with the MLA. The total sanctioned cost of these projects was 56 billion INR, or around 1.2 billion USD.[40] Applying our preferred RD estimate (3% bandwidth) to the full sample, would imply that MLAs had intervened in the allocation of roughly 1,900 road contracts worth around 540 million USD.[41]

These estimates serve to illustrate the economic significance of even proportionately small misallocations in PMGSY contracts but they rely on an extrapolation from a LATE. To what

[40] Applying the average exchange rate over the period (December 2000 to December 2013): 1 INR=0.021 USD.
[41] The estimated impact in the RD with a full set of controls on a 3% bandwidth is an 83% increase. This implies that 45.3% of roads allocated to contractors with the same name as the politicians would otherwise have gone to another contractor.

extent can the results of our RD estimation be considered informative for the programme as a whole? The constituencies with close elections in our sample are characterised by a different political equilibrium. They are typically contested by more candidates and parties, with each earning a lower vote share.[42] Ex-post, the winner of a close election may be more concerned about re-election, may need to raise greater resources for the next campaign and may choose different strategies with regard to patronage and political corruption. One way to evaluate this concern is to compare our RD estimate to the equivalent DiD estimate for the full sample (column 2 of Table 3). The coefficient is lower, and the implied post-election increase in contractors of the same name falls to 28%. This estimate would indicate that 'only' around 910 road contracts were diverted to contractors who shared the MLAs surname. While we are less inclined to accept the DiD results as causal, one possible interpretation of this discrepancy is that electoral competition intensifies political corruption in this setting.

The coefficients in Table 3 are based on a sample that includes practically all Indian states, but there are reasons to expect significant heterogeneity in the results across regions. Our name-based approach will provide a more accurate measure of proximity in areas where there is a strong association between castes and surnames. This is more likely to be the case in northern states than southern ones and in constituencies not reserved for members of Scheduled Castes or Scheduled Tribes. Figure 3 shows that restricting the estimation to these areas results in a higher coefficient, although the sample is underpowered to test heterogenous effects. By contrast, when we focus only on Tamil Nadu, a state whose naming conventions imply that surnames will not provide an indicator of proximity, the coefficient is statistically insignificant and very close to zero. Heterogeneity could also result from differences in the underlying level of corruption across regions. Figure 3 shows that relative to the main sample, the effect is roughly 28% larger in the so-called BIMAROU states that are widely reputed to be more corrupt.[43] However, given

[42] The correlation between the number of candidates and the margin of victory is -0.22. The correlation between the winner's vote share and the margin of victory is 0.59. We find no heterogeneity in our main result based on these observable measures of competition (see Table A7).

[43] The definition of BIMAROU is loose. We use the broadest set which includes Bihar, Madhya Pradesh, Rajasthan, Orissa, and Uttar Pradesh, as well as new states created on their historical territory: Chhattisgarh, Jharkhand, and Uttarkhand.

that these are northern states with a relatively strong association between names and caste affiliation, we are unable to attribute this to systemic corruption.[44]

A second possible source of heterogeneity is the party affiliation of the MLA. Asher and Novosad (2017) find that MLAs aligned with the party in power at the state-level appear to have greater control over the bureaucracy.[45] In Appendix Table A8 we evaluate whether this applies to their involvement in PMGSY contract allocation, with inconclusive results. The differential effect for aligned MLAs is consistently positive but not statistically significant. Bohlken (2016) highlights a potential tension between political parties and their legislators. A party interested in winning a majority at the next election may seek to curb corruption (which could damage its image). While legislators are motivated by (their own) re-election, they also seek to maximise the private rents from elected office, bringing them into conflict with their party. Applying this theory, we test whether aligned MLAs engage in less corruption when their party's control of parliament is more precarious (as measured by its seat share). We find no statistically significant evidence of such a mitigating effect (see Table A11).[47]

If politicians were to selectively intervene in contract allocation based on the political significance of a road's destination, one might expect within-constituency heterogeneity. In Appendix Table A13 (columns 1 and 2) we repeat our analysis while splitting each constituency into (i) roads going to villages with Panchayat headquarters (which may be more politically important to politicians) and (ii) roads going to other villages. Columns 3 and 4 show the results of a similar test which differentiates villages based on whether over 50% of their population were members of Scheduled Castes (SC) or Scheduled Tribes (ST). Columns 5 and 6 repeat this

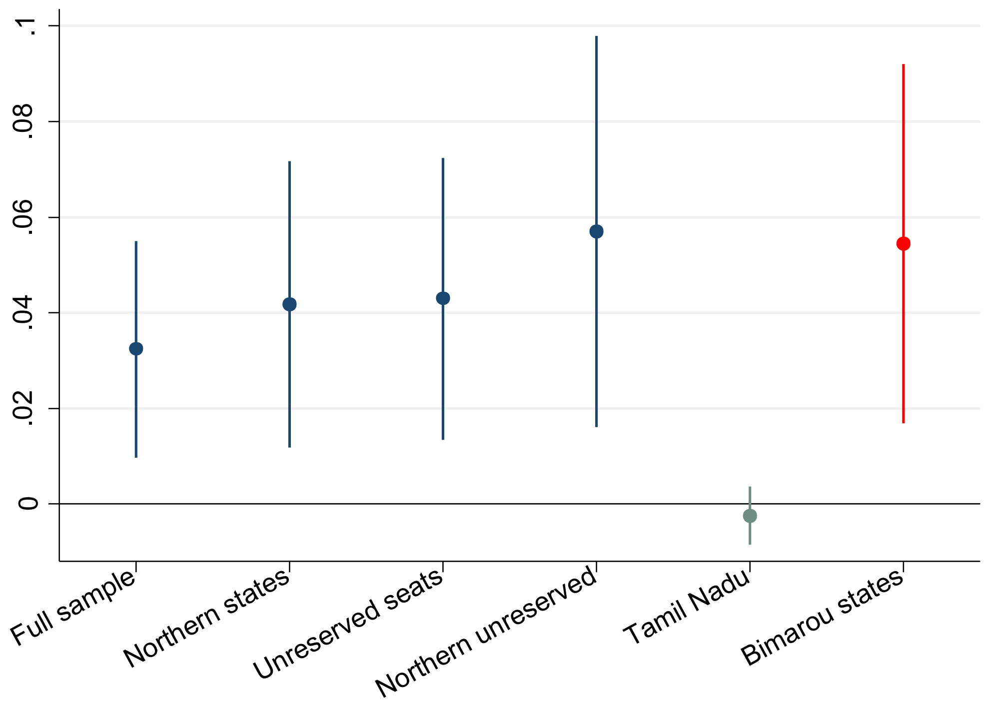
[44] The differential effect for BIMAROU states may also be driven by different levels of development. In Appendix Table A9 we evaluate whether our main result varies based on a constituency's level of literacy in 2001 or the density of the road network in 2001 (both variables are significantly lower in BIMAROU states). We find no evidence of heterogeneity in preferential allocation. Columns (5) to (8) evaluate measures of ethnic composition: the share of a constituency's population that are members of Scheduled Castes or Scheduled Tribes (more fine grained caste data is not available at the constituency level), and a form of Herfindahl index that measures how dispersed the SC and ST population is within the constituency. We again find no differential effect.

[45] The state's Chief Minister (generally the head of the ruling party) is responsible for assigning top-level bureaucrats to districts, and therefore has the most direct control over the bureaucracy. As the Chief Minister is not directly elected, we are unable to replicate our RD approach at this higher level. In Appendix Table A10 we therefore simply analyse how the share of contractors who share the Chief Minister's name changes after they come to power. We find no significant increase.

[47] A related argument suggests, that if MLAs were able to influence contract decisions outside their constituency, they could obtain private rents without incurring a political risk. In Appendix Table A12 we analyse whether elections affect the composition of contractors outside the constituency but within the same district. We find no significant effect.

test while restricting the sample to unreserved constituencies – where high SC and ST areas may be less politically salient. None of these tests provides evidence that the destination of the road affects the likelihood of political interference in contract allocation.

Figure 3: Heterogeneous effect



Note: Chart plots the coefficient for our main specification at the 3% bandwidth, for a range of samples: (1) the full sample; (2) northern states, which include Bihar, Chhattisgarh, Gujarat, Haryana, Himachal Pradesh, Jammu and Kashmir, Jharkhand, Madhya Pradesh, Punjab, Odisha, Rajasthan, Uttarakhand, Uttar Pradesh and West Bengal; (3) constituencies not reserved for Scheduled Caste or Scheduled Tribe candidates; (4) the intersection of (2) and (3); (5) the state of Tamil Nadu; and (6) BIMAROU states (see footnote 43).

The results of this section lend support to qualitative accounts on favouritism in the allocation of PMGSY contracts. Only recently, BJP leader Munna Singh Chauhan accused the Uttarkhand State Government of such misallocations:

> "There is a huge scam in tender allotment in Pradhan Mantri Gram Sadak Yojana (PMGSY) in Bahuguna government. Of a total of 113 mega road construction projects, 75 contracts were awarded to chosen ones close to the echelons of power on a single bid basis. […] Coincidentally, one of the contractors awarded the project is also the brother-in-law of state rural development minister Pritam Singh," (Quoted in Zee News, 30 August 2013).

Our analysis suggests that episodes of suspected favouritism in particular states match a wider pattern of corruption that shows up in our sample covering the whole of India.

5.2 Validity of the RD approach

The RD design requires that no variables other than the dependent variable exhibit discontinuities at the cut-off. The test in Table 2 constitutes the first assessment of the continuity of observable characteristics across the win threshold. Figures A2 of the appendix provide graphical evidence for a number of candidate characteristics that there are no discontinuities across the cut-off.

Close elections can only be considered to provide quasi-random treatment assignment when the probability density function of candidates' vote shares is continuous (Lee 2008). This will not be the case if candidates are able to strategically manipulate their vote share.[49] The standard test for strategic manipulation of the running variable in a RD design was formulated by McCrary (2008). Applying the McCrary test to the assignment variable in this analysis ($margin_{ijt}$), would not make sense because the density is continuous by construction. For every winner with a positive $margin_{ijt}$, there is a runner-up with the equivalent negative value of $margin_{ijt}$. We therefore test for manipulation in the vote share based on an alternative variable: the margin of victory/defeat for the candidate in the constituency with the higher value of $share_{ijt-1}$. The McCrary test does not reject the continuity of this variable at the threshold. Figure A1 in the online appendix presents a graphical depiction of the test.

The online appendix provides further robustness checks, including results of a parametric RD regression estimated on the full sample (in Table A14), as well as the main results in levels instead of first differences (Table A2). The main result is robust to these alternative specifications. To evaluate whether our results can be interpreted as the causal impact of gaining public office, as opposed to the information revealed by performing well in an election, we conduct a placebo test comparing runners-up to third-placed candidates (Table A15). If shifts in allocation somehow reflect individuals' increased status following a strong electoral performance, rather than their official position, second-placed candidates might experience gains relative to

[49] Using data on close US house races, Caughey and Sekhon (2011) provide evidence of such strategic sorting. Eggers et al. (2015) examine over 40,000 close elections from a range of countries (including India) and find no other country that exhibits sorting.

third-placed candidates. We find no such effect; the coefficient on coming second is close to zero across all specifications.

6. SOCIAL COSTS OF MISALLOCATION

PMGSY roads have been shown to deliver significant benefits in targeted villages: improved labour market access, higher incomes, and better living conditions (Asher and Novosad 2016). In this section we evaluate whether political interference in the allocation of PMGSY contracts undermines these benefits, or whether it is in fact welfare-enhancing. Theoretical work has contended that corruption could be beneficial by 'greasing the wheels' and allowing agents to circumvent inefficient bureaucracy (Leff 1964). In a low information environment with a potential for adverse selection and moral hazard, political connections may be associated with better information ex-ante or greater sanctioning power ex-post. Improved outcomes under preferential allocation would constitute evidence in support of these theories. Given that the location of PMGSY roads is officially predetermined, politicians are unlikely to influence *where* a road is built, but their informal control over *who* is awarded a contract may alter the welfare impacts. To estimate the social costs (or benefits) of corruption we analyse projects at the road level, distinguishing between those allocated to connected and unconnected contractors. Our principal outcome of interest is whether roads listed as completed – and for which funds were disbursed – exist in practice. Clearly, the employment opportunities and resultant welfare gains associated with PMGSY are contingent on construction actually taking place. We also consider the impact of MLA's interventions on the cost, timeliness, and quality of road construction.

Identifying the causal impact of corruption on road-level outcomes poses several selection problems. [50] As before, we adopt an RD-approach that exploits close elections. We drop all roads from the sample that were not built either by a contractor who shares a name with the current MLA, or by a contractor who shares a name with the runner-up in the most recent election. Once

[50] One way to approach this question empirically would be to run regressions of road characteristics on a dummy variable that takes the value of one if the MLA and the contractor for road have the same name. However, this approach would fail to control for two important sources of unobserved variation. Firstly, contractors who have the same name as politicians may have systematically different characteristics from other contractors. Secondly, the locations where contractors of the same name as the MLA operate could be systematically different from other areas targeted by PMGSY.

this sample is restricted to close elections, the latter set of roads can be considered a more appropriate control group, since they were assigned to contractors who are connected to a politician who could have won. Once again we control for the margin of victory (the assignment variable) and its interaction with whether the candidate of the same name won. The equation for this non-parametric RD is given by:

$$Road\ Characteristic_{nsy} = \alpha + \beta * MLAsamename_{nsy} + \delta margin_{ijt} + \rho MLAsamename * margin_{ijt} + \gamma X_{nsy} + \theta_s + \vartheta_y + \varepsilon_{nsy},\ margin_{ijt} \in [-\mu, \mu] \quad (3)$$

While this RD-design is likely to be an improvement on a naïve OLS approach, it does not address one potential source of selection bias. To the extent that politicians only intervene on behalf of their network for a subset of roads, and this selective intervention is not random, the ex-ante characteristics of the roads in the treatment group may differ from those in the control group. For example, politicians might try to ensure that more difficult projects are allocated to contractors from their network whom they trust. Given that the available PMGSY data are predominantly determined ex-post – at the time of the contract or during construction – this possibility cannot be ruled out. We address this concern in two ways. Firstly, we check whether pre-determined characteristics of the roads (and the villages they serve) exhibit discontinuities at the cut-off in our running variable. The variables we consider are the length of the road, whether the project involved the construction of a bridge,[52] demographic, socioeconomic, and infrastructure indicators from the 2001 Demographic Census as well as a set of geographic variables likely to affect the difficulty of road construction: altitude, ruggedness, forest cover and distance from the nearest town. Appendix Table A17 does not indicate systematic differences between the locations of roads built by connected and unconnected contractors, based on their observable characteristics. Two of 30 variables exhibit a statistically significant discontinuity: forest cover and bridge construction.[53] We control for these variables in subsequent road level regressions. Secondly, we show that all road level results are robust to the inclusion of local geographic controls, and that our coefficient of interest remains stable.

[52] The length of the road and the requirement for a bridge should have been established in the planning stage as part of the 'Core Network'. As such they can be seen as a pre-determined characteristics rather than an outcome of the contracting process.

[53] The coefficient on bridge construction is negative, which is contrary to what one would expect if MLAs were allocating harder projects to members of their network.

Does political corruption result in more roads 'going missing'? This can be evaluated by comparing PMGSY's administrative records to data from the 2011 Demographic Census. When PMGSY lists a road as having been completed prior to the census, one would expect the villages on that road to have all-weather road access according to the census. We define roads that do not meet this criteria as 'missing all-weather roads'. By this measure, around 26% of roads listed as completed prior to the census are missing.[55] Estimating equation (3), we find that preferential allocation has a large, statistically significant impact on the likelihood of a road going missing (see Table 4 and Figure 4). This result is robust to the inclusion of additional observable characteristics of the location.[56] The coefficient for the 3% bandwidth implies that this probability increases by 86% when the contractor shares a name with the constituency's MLA. Applying this estimate to our whole sample in a back-of-the-envelope calculation, suggests that preferential allocation accounts for 497 additional missing all-weather roads that would have served around 860,000 people.[57]

As a robustness check, the third and fourth columns of Table 4 provide the equivalent results for an alternative, more conservative, definition of missing roads. Instead of applying PMGSY's stated objective (all-weather road access), we define roads as missing if none of the villages located on the planned road had either a black-topped road, a water bound macadam road, or a gravel road, according to 2011 census data. This definition yields a much smaller number of missing roads (2.6%) which is likely to be an underestimate.[58] While the coefficient in Table 4 is correspondingly smaller, the implied effect on the probability of a road not being constructed is significantly higher: 519%. Performing the same back-of-the-envelope calculation for this more conservative measure implies that preferential allocation accounted for 87 additional missing

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[55] There are two reasons why a road could appear as missing, both of which are indicative of corruption. Firstly, roads may be listed as completed without ever being built. Secondly, roads could be built with sub-standard materials leading to complete or partial deterioration by the time of the 2011 census.

[56] Performing the test proposed by Oster (2017) yields a value of δ – the proportionality of selection – of 3.612 when the maximum R^2 is set following Oster's proposed criterion ($Rmax = min\{1.3R^*, 1\}$). Oster suggests that values above 1 can be typically be considered indicative of robust treatment effects.

[57] 4,127 roads in our sample were built by connected contractors. Of these 26% are missing all-weather road access. Our estimates imply that the share of these missing roads due to preferential allocation is 46% (1-1/(1+0.86), or 497 roads. Multiplying this by the average number of inhabitants on a road, gives an estimate of 857,018 people affected.

[58] Given that some existing roads (gravel roads in particular) would not have met PMGSY's objective of all-weather access, villages that have such roads in the census may still never have received the PMGSY road they were supposed to. Moreover, it is possible for villages to be on more than one planned PMGSY road (if they are on through roads), so the presence of a road in that village need not indicate that all scheduled PMGSY roads were built.

roads with around 150,000 people affected.[59] In short, the finding that political intervention reduces the number of roads actually constructed is robust to widely different definitions of what constitutes a missing road.

Table 4: Road-level regression discontinuity – Missing Roads

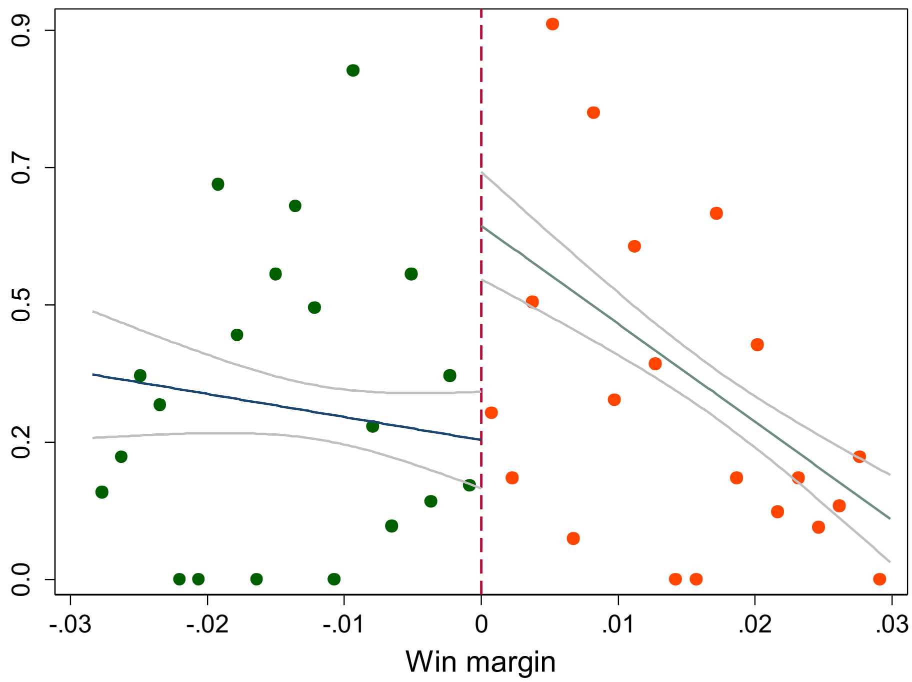
Dependent Variable:	Missing all-weather road		Missing road	
Margin of victory	<3%		<3%	
	(1)	(2)	(3)	(4)
MLAsamename	0.2664***	0.2273***	0.1359**	0.1349**
	(0.0951)	(0.0862)	(0.0645)	(0.0594)
Margin	-2.9287	-1.0865	-2.1717	-2.1586
	(4.2918)	(4.0746)	(2.0146)	(1.7037)
Margin*MLAsamename	-5.6897	-7.9904	-1.8635	-1.9458
	(5.4173)	(5.1932)	(2.8181)	(2.6496)
Bridge		-0.1255		-0.0027
		(0.0984)		(0.0363)
Altitude		-0.0001		0.0000
		(0.0000)		(0.0000)
Ruggedness		-0.0041		-0.0534***
		(0.0280)		(0.0176)
Forest cover		0.6860**		0.0858
		(0.2962)		(0.1157)
Power supply in 2001		-0.1938***		-0.0631*
		(0.0664)		(0.0336)
Road-level controls	X	X	X	X
State fixed effects	X	X	X	X
Agreement year fixed effects	X	X	X	X
N	581	581	581	581
Control group mean dep, var	0.2639	0.2639	0.0260	0.0260

Note: Standard errors clustered at the contractor level to account for intra-contractor correlation of the error term at the road level. All regressions include the following set of road-level controls: ln(length) (to account for non-linear relationship between cost and distance), whether the constituency is a reserved seat, the mean population of habitations on the road, the mean population share of Scheduled Castes and Scheduled Tribes of habitations on the road, and the mean connectivity of those habitations in 2001. To ensure comparability of coefficients, the sample for columns (1) and (3) is restricted to observations for which all additional controls are available. Appendix table A18 presents results for the respective optimal bandwidths of 2.5% and 4.4% derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

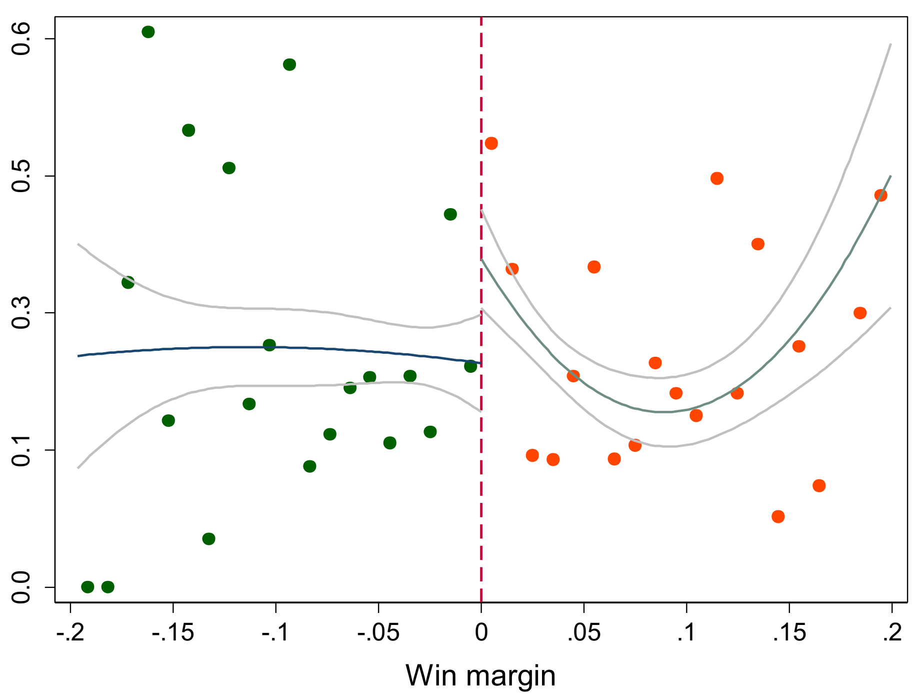
[59] 4,127 roads in our sample were built by connected contractors. Of these 2.6% are deemed to be missing. Our estimates imply that the share of these missing roads due to preferential allocation is 81%. This yields an estimate of 87 roads. The average road in our sample serves villages with a total of 1,726 inhabitants, giving an estimate of 149,508 people left unconnected.

Figure 4: Graphical depiction of RD

Share of missing all-weather roads – linear fit



Share of missing all-weather roads – quadratic fit



Note: Lines fitted separately on the samples left and right of the cut-off. Each dot represents the mean for a bin. The number of observations in a bin varies, based on the density of candidates at a given margin. 90% confidence intervals plotted in grey. The first panel shows a linear fit within the 3% bandwidth used in the RD estimation. The second shows a quadratic fit for a wider margin of 20%.

Assuming that construction does take place, its efficiency and quality may depend on whether contractors were selected for political reasons. Using PMGSY's administrative data, we therefore analyse four additional measures of quality using the same RD approach: (i) the initial cost of the project (per kilometre), (ii) the number of days between the completion date specified in the contract and the actual date of completion; (iii) the ratio between the actual cost of the project and the cost sanctioned in the agreement; and (iv) a dummy variable for whether a road was deemed "unsatisfactory" or "in need of improvement" in either the latest state quality inspection or the latest national quality inspection.[60]

If rent-seeking politicians are putting pressure on bureaucrats to reject the lowest bidder (or the most qualified bidder) in favour of their preferred contractor, we would expect to see a rise in costs (or a deterioration in quality). Table 5 [61] shows that roads built by contractors who share a name with an elected official are more expensive (per kilometre). The inclusion of additional geographic controls (column 2) does not significantly alter the coefficient.[62] For delays and cost discrepancies we find no significant difference between roads constructed by contractors whose name matches the MLA's and those whose name matches the runner-up. Our results on quality inspections are less conclusive. At the 3% bandwidth the coefficient is positive but not statistically significant. However, at the optimal bandwidth the effect is significant (see Appendix Table A18), which would imply that preferentially allocated contracts are more likely to result in sub-standard construction.

Even though the set of indicators on which we have data is necessarily limited, preferential allocation appears to reflect costly corruption with no mitigating improvements in the efficiency of road construction. Indeed, we find suggestive evidence that political intervention leads to

[60] The quality data available on the OMMAS has some shortcomings for the purpose of this analysis. Data is available on national and state quality inspections, and a single road may have multiple inspections in each category. However, only the grade assigned in the latest inspection is provided (for each category). The data therefore do not allow us to distinguish between roads that were satisfactory at the outset, and roads that initially did not pass inspection but were improved prior to subsequent inspections. Moreover, only a fraction of the roads in our sample appear in the quality data, and many of these only had one of the two inspection types (national or state). Pooling the two inspections is not ideal, but it provides the best available measure of initial road quality.

[61] The results in Table 5 are reported for the common bandwidth of 3%. Equivalent results for the 5% and 2.5% bandwidths and the optimal bandwidth derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011) are reported in Table A18. There are some outliers in the road level data for cost, delays and cost overruns (see Table 1B). All results are robust to dropping the top 1% and bottom 1%, or the top 5% and bottom 5% (see Table A19).

[62] In this case the δ resulting from Oster's (2017) test is 1.38, i.e. above the value of 1 indicative of robust treatment effects.

roads that are not only more expensive, but also more likely to be either of poor quality or never to have been built at all. In principle, these costs could be offset if political intervention allowed new firms to enter the market, leading to greater competition and better performance over time. There is no evidence in the data that preferential allocation disproportionately targets small firms or facilitates entry.[63] As such, it appears that the benefits of corruption are confined to the private rents for politicians (see Fisman et al. 2014), contractors and potentially, intermediary bureaucrats.

[63] For contractors with no prior PMGSY experience, the share of contracts where the MLA shared the contractors name is 4.53%. The equivalent share for experienced contractors (with between 10 and 40 prior road contracts) is 4.52%. There is thus no obvious evidence that preferential allocation favours small firms or promotes entry.

Table 5: Road-level regression discontinuity – efficiency and quality

Dependent Variable:	Ln(sanctioned cost/km)		Days overrun		Actual cost/sanctioned cost		Failed inspection	
Margin of victory:	<3%		<3%		<3%		<3%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
MLAsamename	0.1163**	0.1027**	-88.9	-78.1	-0.0518	-0.0547	0.0881	0.1018
	(0.0503)	(0.0476)	(91.6)	(95.1)	(0.0528)	(0.0520)	(0.0975)	(0.0973)
Margin	-7.6807***	-6.7278***	-472.1	-1,176.1	1.8696	2.0243	1.9865	1.7221
	(2.5758)	(2.4074)	(4,423.1)	(4,548.7)	(2.7133)	(2.6636)	(4.1406)	(4.2063)
Margin*MLAsamename	10.4191***	9.2084***	1,076.6	1,784.4	-2.1774	-2.3181	-7.0371	-6.7129
	(3.3424)	(3.1324)	(5,546.3)	(5,551.0)	(2.9985)	(2.9145)	(5.7334)	(5.7120)
Bridge		0.0545		-32.4632		-0.0181		-0.0453
		(0.3156)		(94.5708)		(0.0432)		(0.2325)
Altitude		-0.0000		-0.0575		0.0000		-0.0000
		(0.0000)		(0.0554)		(0.0000)		(0.0001)
Ruggedness		0.0146		-10.3473		0.0116		-0.0870**
		(0.0141)		(30.2280)		(0.0090)		(0.0410)
Forest cover		0.4163***		-398.4231		0.0714		0.0028
		(0.1424)		(320.2260)		(0.1103)		(0.3220)
Power supply in 2001		-0.0076		-22.0734		0.0158		-0.0539
		(0.0185)		(38.9772)		(0.0179)		(0.0548)
Road-level controls	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X
Agreement year fixed effects	X	X	X	X	X	X	X	X
N	1,470	1,470	970	970	1,161	1,161	468	468
Control group mean dep, var	3.325	3.325	538.5	538.5	0.974	0.974	0.373	0.373

Note: Standard errors clustered at the contractor level to account for intra-contractor correlation of the error term at the road level. All regressions include the following set of road-level controls: ln(length) (to account for non-linear relationship between cost and distance), whether the constituency is a reserved seat, the mean population of habitations on the road, the mean population share of Scheduled Castes and Scheduled Tribes of habitations on the road, and the mean connectivity of those habitations in 2001. To ensure comparability of coefficients, the samples for columns (1), (3), (5), and (7) are restricted to observations for which all additional controls are available. Appendix Table A18 reports results for the respective optimal bandwidths derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

7. MECHANISMS

Our findings are consistent with recent work showing that MLAs have a significant impact on economic outcomes in their constituency (Asher and Novosad 2017; Prakash et al. 2015), even though the parliamentarians in question have limited official power. In the literature this discrepancy between de jure responsibilities and de facto influence is often explained by politicians' informal control over the assignment of bureaucrats (Iyer and Mani, 2012). Bureaucrats have many ways to sway the allocation of contracts. The technical evaluation of bids allows for discretion. In conversations with bureaucrats involved managing PMGSY tenders, we were told that MLAs will often get technical requirements from bureaucrats and then share that information with favoured contractors who then have a better chance of winning the tender (Author interviews, April 12, 2016). In this section, we provide evidence for role of bureaucrats as intermediaries. In particular, if bureaucrats are part of the corruption chain, we expect that (i) "connected" bureaucrats facilitate corruption; and that (ii) bureaucrats who are under more scrutiny are less inclined to facilitate corruption.

PMGSY tendering is generally administered at the district level. The highest ranking official in a district is the District Collector (DC) – a member of the Indian Administrative Service (IAS).[64] It is likely that MLAs who have a connection to their district's collector would find it easier to influence the allocation of contracts. Applying our surname-matching approach we therefore test for heterogeneity in our main result based on whether or not the DC and politician have the same surname. We see such an alignment in 1.4% of the bureaucrat-election observations. As bureaucrats are reassigned frequently, we split each constituency-term into separate observations for each DC. Columns (1) to (4) of Table 6 show that the interaction between winning an election and sharing the DC's name is positive and significant at the optimal bandwidth. The RD estimates lose precision in the smaller sample for the 3% bandwidth, but the magnitude of the coefficients is comparable. These results suggest that MLAs may engage in more preferential allocation when the highest ranking bureaucrat is also a member of their

[64] IAS officers' executive record sheets provide data on the location and duration of their postings, allowing us to match DCs to districts, and by extension, to the constituencies in our sample. This data is available on the website of the Department of Personnel and Training: dopt.gov.in (accessed in December 2016).

network. However, as the assignment of DCs may not be exogenous we cannot prove that this result is causal.

Our second test exploits variation in the incentives which bureaucrats face over their career. IAS officers are screened for promotion in their 13th and 16th years of service. The prospect of higher positions and increased pay may cause bureaucrats to improve their performance during these periods. For example, Nath (2016) find's that DCs close to these promotion screening dates are faster at approving local public good projects. To assess whether such a dynamic is in play here, we split each constituency-term into separate observations for each DC who held office during the term. DC's are reassigned frequently with a median tenure of 17 months. We define a DC as being subject to promotion screening if their 13th or 16th year of service overlap with their tenure in a district.

The increase in connected contracts is, attenuated during such periods, as one would expect. Columns (5) to (8) of Table 6 show that under a DC in their 13th or 16th year of service, preferential allocation towards contractors who share the MLA's name is much lower than when the DC is not in those years and the point estimate is indistinguishable from zero.[67] This could reflect bureaucrats' reluctance to participate in risky corruption, or greater monitoring of improprieties on the part of their subordinates. Both channels suggest that political interference involves the cooperation of bureaucrats.

In Table 7 we apply the same sources of variation to test for heterogeneity in our road-level results. There is no evidence of a differential effect on the likelihood of a missing road or the cost of the road for contracts allocated to contractors who share a surname with both the MLA and the DC. However, when the DC is close to their promotion screening date, our results for missing roads and for cost inflation are weaker. This is consistent with the idea that bureaucrats up for promotion are less amenable to corrupt activities within their district.

[67] Appendix table A20 shows that this result is robust to controlling for officers' years of service and the interaction of years of service with winning the election. Columns (5) to (8) compare the effects in the promotion screening year to effects in prior and subsequent years. There is a weak negative effect in the year before promotion screening which may reflect anticipation of the screening process. The result is thus not due to some linear trend in DC behaviour; the attenuation in the effect of winning on the share of connected contractors appears to be specific to having a DC under the scrutiny that comes with evaluation for promotion.

The results of this section provide support for the hypothesis that local bureaucrats are a crucial part of the chain that links politicians to the preferential allocation of contracts.

Table 6: Bureaucrat characteristics and contract allocation

Dependent var.: ΔShare	<6.2%		<3%		<6.2%		<3%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner	0.0167*	0.0147	0.0249**	0.0275**	0.0227**	0.0204**	0.0304**	0.0345***
	(0.0097)	(0.0103)	(0.0125)	(0.0128)	(0.0096)	(0.0100)	(0.0125)	(0.0127)
Margin	-0.0793	-0.0017	-0.8301*	-0.9857*	-0.0694	0.0052	-0.7915*	-0.9514*
	(0.1933)	(0.2146)	(0.4574)	(0.5023)	(0.1930)	(0.2138)	(0.4593)	(0.5047)
Margin*winner	-0.4108	-0.4531	0.3327	0.3723	-0.4253	-0.4615	0.3087	0.3579
	(0.2970)	(0.3247)	(0.6445)	(0.6649)	(0.2963)	(0.3246)	(0.6416)	(0.6643)
DCsamename	-0.0512	-0.0293	-0.1108	-0.1037				
	(0.0569)	(0.0542)	(0.0902)	(0.0949)				
DCsamename*winner	0.1670**	0.1287*	0.1385	0.1404				
	(0.0701)	(0.0667)	(0.0890)	(0.0966)				
DCpromotionyear					0.0089	0.0113	0.0233**	0.0262**
					(0.0080)	(0.0090)	(0.0110)	(0.0120)
DCpromotionyear*winner					-0.0313**	-0.0318**	-0.0342*	-0.0441**
					(0.0136)	(0.0144)	(0.0180)	(0.0197)
AC controls		X		X		X		X
State fixed effects		X		X		X		X
Election year fixed effects		X		X		X		X
N	7,342	6,613	4,146	3,732	7,336	6,607	4,144	3,730

Note: Local linear regression estimates. The unit of observation is the constituency-term-DC so each term is split into a separate observation for every DC in office during the term. Standard errors are clustered at the district-level. DCsamename is a dummy that takes the value of 1 if the DC and MLA have the same surname. DCpromotionyear is a dummy that takes the value of 1 if the DC is in their 13^{th} or 16^{th} year of service during their tenure in the district. The bandwidth of 6.2% is derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1.

Table 7: Bureaucrat characteristics and road outcomes

Dependent Variable:	Missing all-weather road	Missing road	Ln(sanctioned cost/km)	Missing all-weather road	Missing road	Ln(sanctioned cost/km)
	(1)	(2)	(3)	(4)	(5)	(6)
MLAsamename	0.2895***	0.1504**	0.1126**	0.3885***	0.1676**	0.1479***
	(0.0978)	(0.0649)	(0.0467)	(0.1068)	(0.0779)	(0.0569)
Margin	-3.8176	-2.5438	-7.3982***	-7.4694	-1.8026	-7.8830***
	(4.1530)	(1.8993)	(2.4709)	(5.4338)	(2.6045)	(2.8675)
Margin*MLAsamename	-4.2841	-1.3217	10.0164***	-1.9472	-2.7491	10.9856***
	(5.2783)	(2.7645)	(3.2321)	(6.5345)	(3.3937)	(3.6501)
DC same name	0.1144	0.0480	-0.0692			
	(0.0805)	(0.0371)	(0.0463)			
DCsamename*winner	-0.0794	-0.0506	0.0148			
	(0.0961)	(0.0457)	(0.0587)			
DCpromotionyear				0.0262	0.0181	0.0261
				(0.0862)	(0.0253)	(0.0393)
DCpromotionyear*winner				-0.0911	-0.0990**	-0.1303**
				(0.1160)	(0.0472)	(0.0603)
Road-level controls	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
Agreement year fixed effects	X	X	X	X	X	X
N	581	581	1,471	459	459	1,057

Note: Local linear regression estimates. Standard errors clustered at the contractor level to account for intra-contractor correlation of the error term at the road level. All regressions include the following set of road-level controls: ln(length), whether the constituency is a reserved seat, the mean population of habitations on the road, the mean population share of Scheduled Castes and Scheduled Tribes of habitations on the road, and the mean connectivity of those habitations in 2001. DCsamename is a dummy that takes the value of 1 if the DC and MLA have the same surname. DCpromotionyear is a dummy that takes the value of 1 if the DC is in their 13th or 16th year of service during their tenure in the district. The bandwidth of 6.2% is derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

8. ELECTORAL INCENTIVES FOR CORRUPTION

We implicitly assume that kinship ties to politicians are relevant connections in the structure of local political corruption in India, and our results appear to validate this assumption. But why should patronage be targeted along caste or familial lines? The literature offers two main explanations: vote-buying and particularised trust. In this section we attempt to shed light on which is more applicable to corruption in PMGSY

If road contracts are awarded in exchange for political contributions or political support, one could expect the bias towards connected contractors to increase in election periods. To test for this we construct more disaggregated measures of proximity: the share of contractors with a candidate's name in the first 12 months after an election ($start\ of\ term_{ijt}$.), the equivalent share for the last 12 months before the subsequent election ($end\ of\ term_{ijt}$), and finally the share for the intermediate, mid-term, period. The first three columns of Table 8 shows the results of applying our main estimation approach to this disaggregated sample and interacting dummies for $start\ of\ term_{ijt}$ and $end\ of\ term_{ijt}$ with $winner_{ijt}$. The overall effect of winning the election is comparable to the term-level results, and we find no differential effects in election years.

Although the bias towards connected contractors does not increase in election periods, there could be different patterns for the within-term variation on the cost margin. Politicians might need to extract rents, buy support, or reward supporters were higher before or after elections. Including the interactions between $MLAsamename_{njt}$ and $start\ of\ term_{ijt}$ and $end\ of\ term_{ijt}$ in the road-level regressions, we again find no evidence of a political cycle in which election periods see increased corruption (columns 4-6 of Table 8). The observed negative effect for both the start and end of term is more consistent with increased scrutiny in the run-up to elections acting as a deterrent to corruption.

Table 8: Three tests for electioneering

Dependent variable: $\Delta Share_t$ (Ln(sanctioned cost/km) in cols 4-6)	Electoral cycles: allocation			Electoral cycles: cost			Effect in "politically irrelevant" areas
	Start of term heterogeneity	End of term heterogeneity	Start and end of term heterogeneity	Start of term heterogeneity	End of term heterogeneity	Start and end of term heterogeneity	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Winner (MLAsamename in cols 4-6)	0.0215**	0.0250**	0.0232**	0.1166**	0.1359***	0.1433***	0.0250*
	(0.0109)	(0.0108)	(0.0112)	(0.0518)	(0.0525)	(0.0540)	(0.0146)
Margin	-0.3625*	-0.3563*	-0.3608*	-7.0964***	-7.6324***	-7.0332***	-0.2272
	(0.2134)	(0.2136)	(0.2135)	(2.5415)	(2.6163)	(2.5659)	(0.5518)
Margin*winner (Margin*MLAsamename in cols 4-6)	-0.0144	-0.0239	-0.0160	9.8967***	10.3096***	9.7854***	-0.0918
	(0.2967)	(0.2968)	(0.2969)	(3.2878)	(3.3607)	(3.2915)	(0.7128)
Start of term	-0.0076		-0.0082	0.1270***		0.1420***	
	(0.0056)		(0.0058)	(0.0422)		(0.0444)	
Start of term*winner (Start of term*MLAsamename in cols 4-6)	0.0059		0.0043	-0.0746		-0.0973*	
	(0.0081)		(0.0081)	(0.0546)		(0.0552)	
End of term		0.0015	-0.0020		0.0521	0.0796	
		(0.0056)	(0.0057)		(0.0568)	(0.0581)	
End of term*winner (End of term*MLAsamename in cols 4-6)		-0.0063	-0.0045		-0.1303**	-0.1480***	
		(0.0081)	(0.0081)		(0.0551)	(0.0553)	
Politically irrelevant*post announcement*winner							-0.0273
							(0.0218)
Constituency controls	X	X	X	X	X	X	X
Candidate controls	X	X	X				X
Road-level controls				X	X	X	
State fixed effects	X	X	X	X	X	X	X
Agreement year fixed effects	X	X	X	X	X	X	X
N	5,625	5,625	5,625	1,470	1,470	1,470	2,707

Note: Terms in the sample for columns 1-3 are disaggregated by time: first year, last year, and remaining period. Terms in the sample for column 7 are disaggregated by time (pre- and post- the announcement of delimitation) and spatially (based on intra-constituency boundaries introduced by delimitation). The RD bandwidth for all columns is 3%. Appendix Tables A21, A22, and A23 reports the equivalent results for the respective optimal bandwidths derives from the optimal bandwidth rule of Imbens and Kalyanaraman (2011). Standard errors are clustered at the election level in columns 1-3 and 7 and at the contractor level in columns 4-6. Controls for columns 1-3 and 7 are the same as in Table 3. Controls for columns 4-6 are the same as in Tables 4 and 5. Regression in column 7 includes all lower-order interactions (not reported). All regressions include a constant. *** p<0.01, ** p<0.05, * p<0.1

Changes to the delimitation of parliamentary constituencies allow for an additional test of the vote-buying hypothesis. The changes proposed by the delimitation commission of 2002 were approved in February 2008. Subsequent assembly elections, starting with Karnataka in May 2008, were carried out under the new delimitation. After the reform had been announced and approved, the majority of MLAs elected under the old delimitation continued to hold office for several years until the next election. In constituencies where the boundaries were redrawn, this meant that only some areas would remain part of the constituency at the next election, while others would be of no consequence to the MLA's chances of re-election. We identify such areas with a dummy variable $politically\ irrelevant_{ijt}$ and also disaggregate temporally, splitting the applicable electoral term into the period before the announcement, and the period between February 2008 and the next election (the variable $post\ announcement_{ijt}$ denotes the latter). Given that the boundaries were defined by an independent commission following objective pre-set guidelines, the reform could provide plausibly exogenous variation in the incentive for vote-buying.[68] The final column of Table 8 presents the results of our main specification for the disaggregated sample and interaction terms. The coefficient of interest is the triple interaction term:

$$winner_{ijt} * politically\ irrelevant_{ijt} * post\ announcement_{ijt}$$

A negative and significant coefficient would suggest that political corruption is weaker in areas where politicians have no incentive to buy votes. The coefficient on the triple interaction is negative at the 3% bandwidth, positive at the optimal bandwidth (see Appendix Table A23), and statistically insignificant throughout. Hence, we find little evidence of strategic vote-buying. This result is also consistent with recent work by Chhibber and Jensenius (2016), who use the same delimitation experiment and find that MLAs from "ethnic" or "well-organized" parties tend to target existing loyalists rather than the electorally valuable voters who joined their constituencies post-delimitation.

In the absence of clear evidence for vote buying, it is possible that corruption arises within kinship networks because these provide the "particularised trust" needed to engage in risky collusive behaviour (Tonoyan, 2003). While we are unable to test this explanation explicitly, it fits the context of PMGSY in that the involvement of the central government provides a

[68] According to the Electoral Commission of India's Guidelines and Methodology for Delimitation, "the delimitation of the constituencies in a district shall be done starting from North to North-West and then proceeding in a zig-zag manner to end at the Southern side." Constituencies were to have equal populations, as far as possible, with maximum deviations of 10% from the State average, based on the 2001 Census.

minimum level of monitoring. Given this supervision, it is notable that we only find evidence of corruption in certain aspects of the programme. Our results suggest that preferential allocation adversely affects every performance marker except the two that are most easily observed in the central administrative data: over-runs and delays. This is in line with the idea that contractors face a trade-off between potential rents and the cost of detection. Similarly, we find that preferentially allocated contracts are less expensive in the period before elections, when scrutiny may be higher and detection is more damaging to politicians. The overall pattern of our results is therefore consistent with a setting in which politicians, bureaucrats, and contractors are constrained by monitoring and operate on the least risky margins and with the least risky collaborators – the members of their family or caste network.

The introduction of e-procurement for PMGSY also allows us to evaluate the effect of an intervention that increased the monitoring capacity of higher level officials. Lewis-Faupel et al. (2016) find that e-procurement improved the quality of PMGSY road construction. They identify the entry of higher quality contractors from outside regions as a key mechanism behind the quality improvement. However, we find no evidence that e-procurement has helped to prevent the specific form of corruption we document in this paper.[69] This null result is consistent with corrupt practices being relatively sophisticated in the context of PMGSY, even before e-tendering.

A distinguishing feature of PMGSY is that the programme has been subject to a central monitoring system from its inception. However, by explicitly prohibiting political involvement, it foregoes a mechanism that could provide local accountability. If voters held their MLAs responsible for the services delivered under PMGSY, the latter would have an incentive to limit corruption. By contrast, a scheme in which local politicians have no formal role but over which they still retain influence through informal channels, can be seen as an ideal vehicle for rent-seeking. Our results provide mixed evidence on electoral accountability. While cost overruns are weaker around elections, the preferential allocation does not show any electoral cycle. One interpretation of these findings is that PMGSY limited political control in the allocation of contracts, but that this came at the expense of reduced political accountability. While we cannot test this trade-off directly in our context, we believe it is an interesting avenue for future research.

[69] These results are reported in the Appendix (Table A24).

9. CONCLUSION

This paper provides direct empirical evidence that local politicians in India abuse their power to benefit members of their own network. We exploit the variation in political leadership due to the electoral cycle, to identify systematic distortions in the allocation of contracts for a major rural road construction programme (PMGSY). By matching contractors' and political candidates' surnames, we generate a measure of proximity which evolves as the pool of contractors changes. A regression discontinuity design based on close elections, suggests that the causal impact of a politician coming to power is an 83% increase in the share of roads allocated to contractors who share their surname.

This result withstands a series of alternative specifications and robustness checks. Further regression discontinuity estimates at the road level, indicate that corruption is welfare-reducing in this context. Political interference is associated with higher costs and a greater likelihood that roads go missing. While constituencies with close elections are clearly in a different political equilibrium (close elections are contested by more candidates, for example), a difference-in-differences estimate for the whole sample also shows a positive and statistically significant increase in the share of connected contacts, albeit a smaller one than in the RD specification. This finding suggests that the LATE we estimate in the RD is externally valid.

A distinguishing feature of our analysis, is that we identify the effect of political connections to state-level legislators who have no official involvement in the road construction programme. Our results therefore not only indicate preferential treatment of the politically connected, they also provide indirect evidence that local politicians' power over purportedly neutral bureaucrats is sufficient to coerce them into corruption. From a policy perspective, these findings indicate that more could be done to insulate the officials implementing government programmes at the local level, including those involved in PMGSY. The result that misallocation is lower and contract performance is better for bureaucrats who face promotion incentives, points in this direction.

A striking feature of PMGSY is that the bidding rules were designed explicitly to limit political influence. However, such anti-corruption measures could face a trade-off between political accountability and political control. If voters believe the limitations work, then a logical conclusion would be that their MLAs are not responsible for poor programme performance. While we find some evidence that cost inflation is weaker around election times, our preferential allocation results remain very stable throughout the electoral cycle. These findings are consistent with the idea that the limited control of politicians over PMGSY could have come at the expense of reduced accountability to voters.

While this paper is primarily about the measurement and mechanisms of corruption, its findings have significance beyond the potential number of misallocated roads or the amount of misdirected funds. If corrupt arrangements were made based on random matching between individuals, the empirical strategy would have revealed nothing. Our results provide further evidence for the role of networks in enabling corruption and point towards theories in which kinship networks facilitate corruption through trust or the ability to impose social sanctions. The irony is, that the setting for the analysis – PMGSY – is conceptually a profoundly inclusive programme, facilitating the integration of over 100 million people into the Indian economy (Aggarwal 2015). This paper suggests that allowing them to compete equally for jobs, permits, licenses, or government procurement contracts, may require building more than roads.

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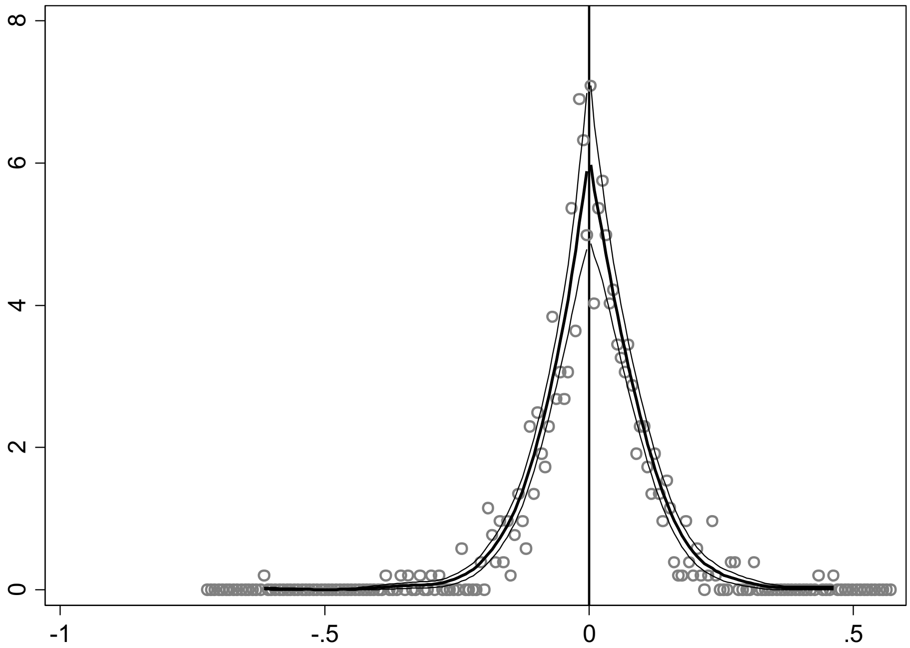
FIGURES

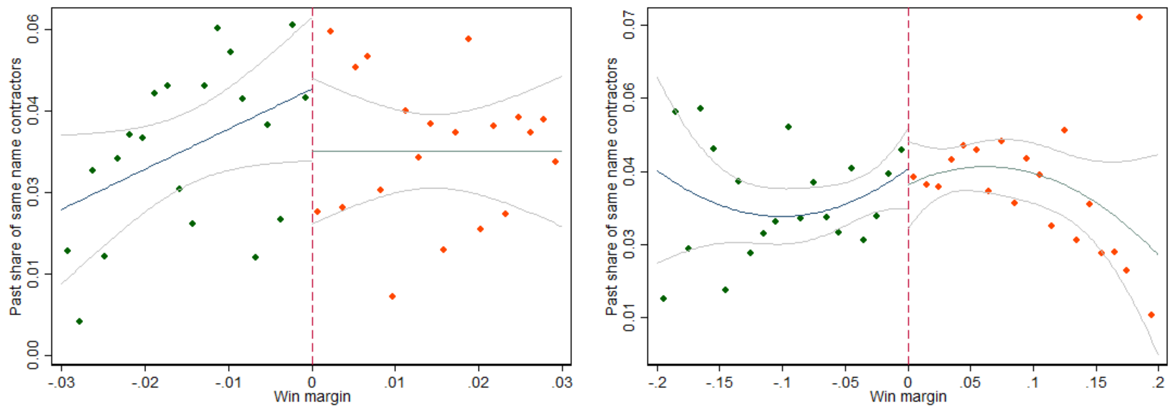
Figure A1: graphical depiction of the McCrary test



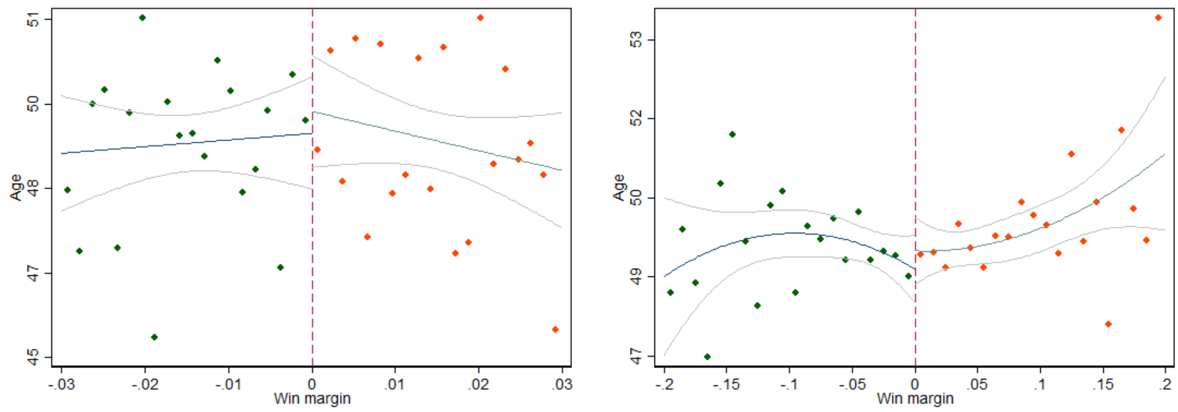
Note: This figure plots the McCrary test. The running variable in this analysis is continuously distributed by construction. The test is performed on an alternative version of the margin variable: the margin of victory for the candidate with the higher level of $share_{ijt-1}$.

Figure A2: continuity of observable variables at cut-off

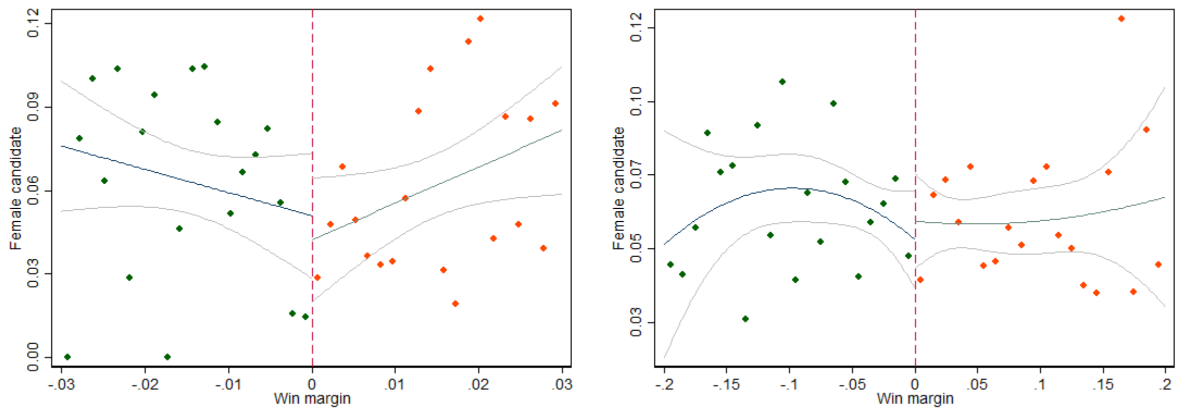
Past share of same name contractors



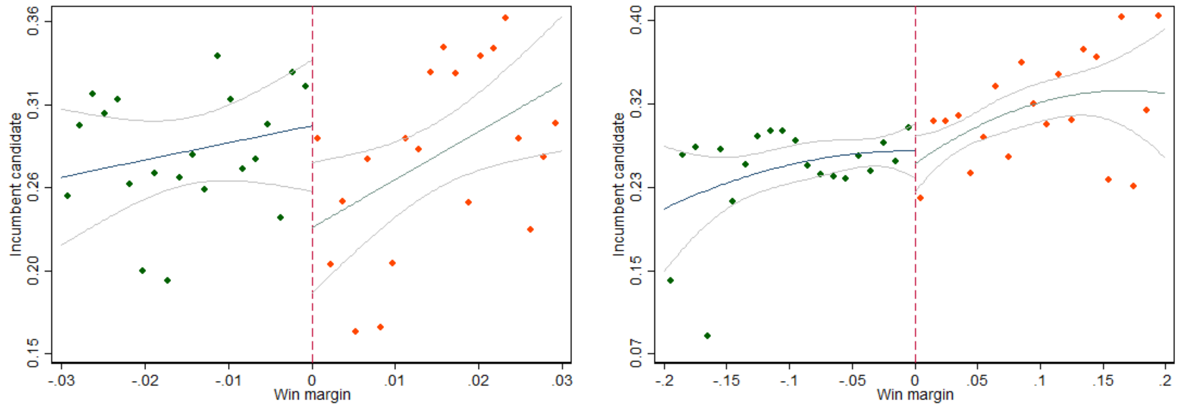
Age



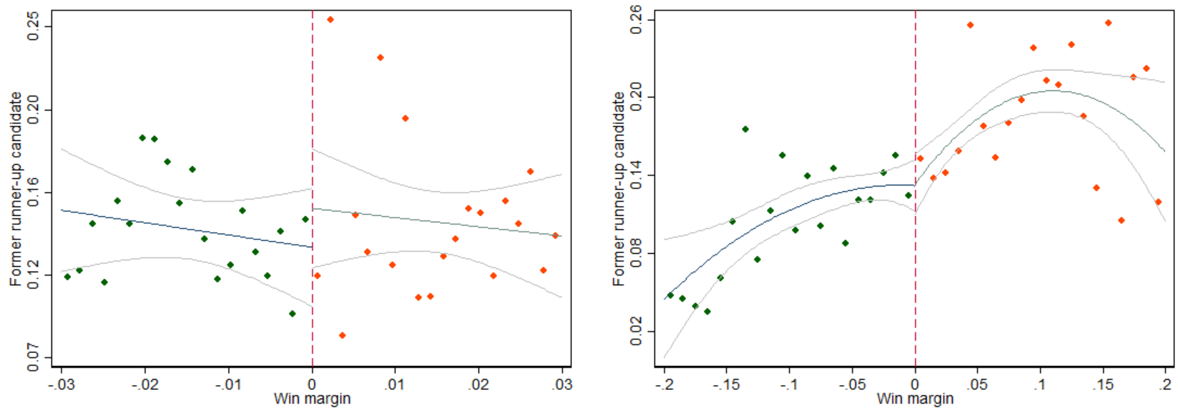
Gender



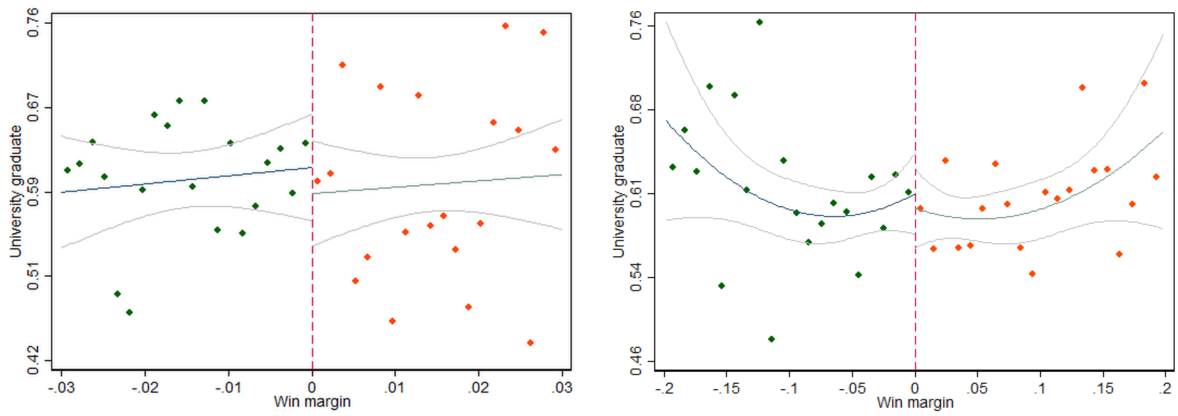
Incumbency



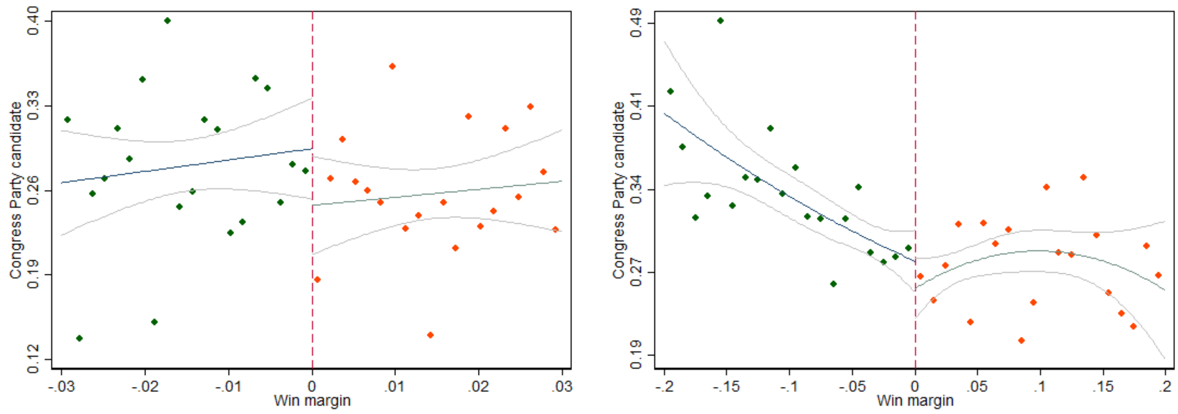
Former runner-up status



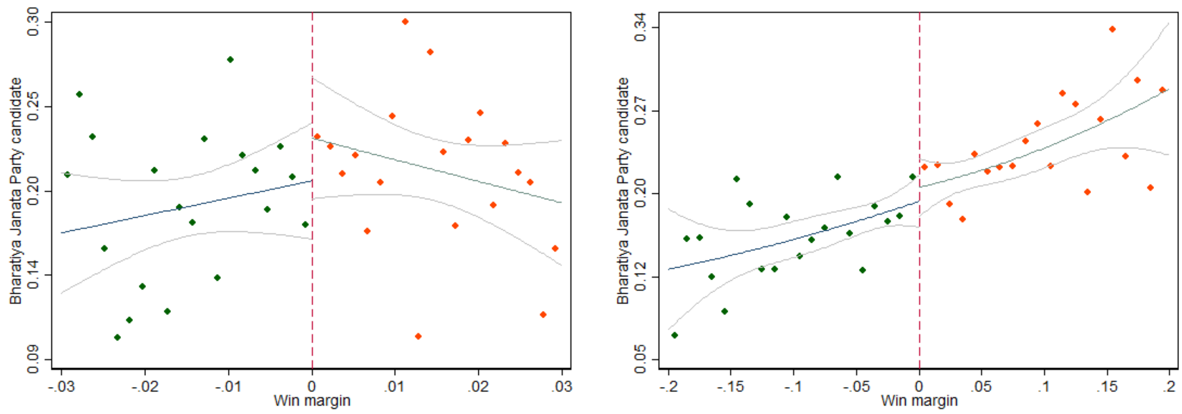
University graduate



INC candidate



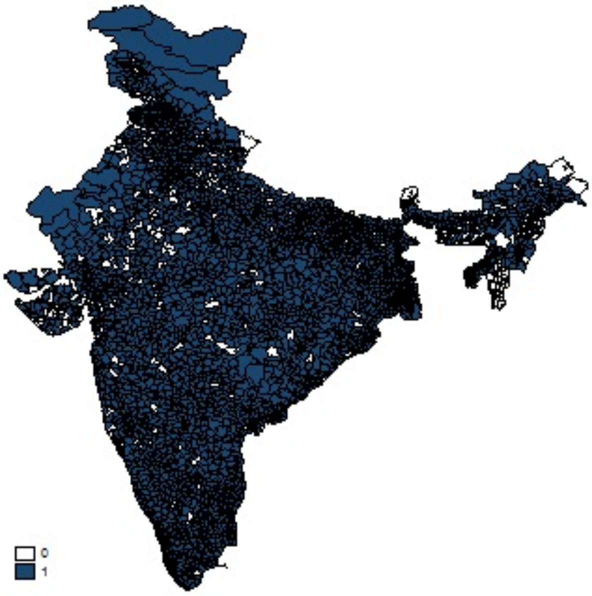
BJP candidate



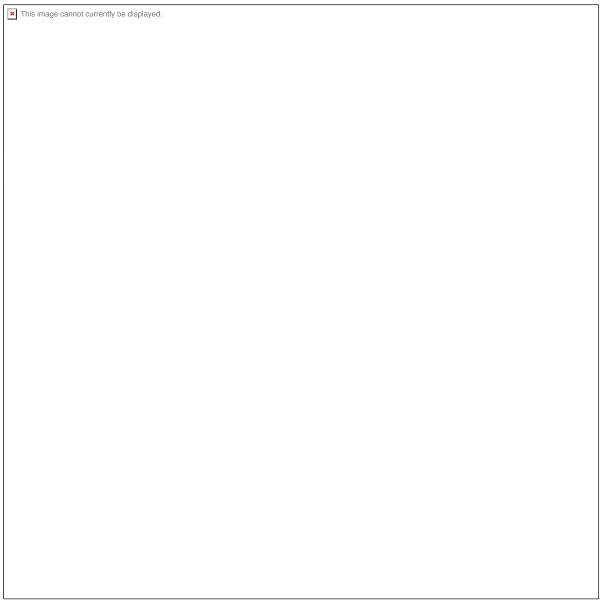
Note: Lines fitted separately on the samples left and right of the cut-off. Each dot represents the mean for a bin. The number of observations in a bin varies, based on the density of candidates at a given margin. 90% confidence intervals plotted in grey. The first panel shows a linear fit within the 3% bandwidth used in the RD estimation. The second shows a quadratic fit for a wider margin of 20%.

MAPS

Map 1: Constituencies in Sample

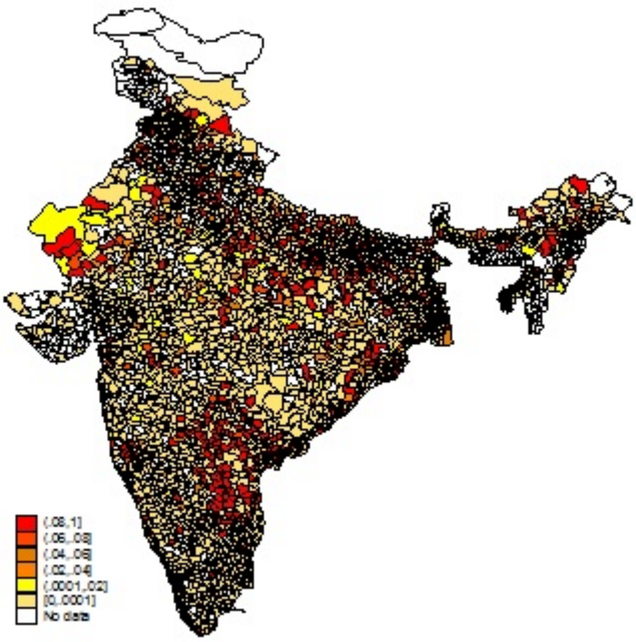


Map 2: Constituencies with close elections



Note: The constituencies shown on maps are based on the pre-2008 delimitation. Post delimitation data is assigned to pre-delimitation boundaries. In Map 1 all constituencies with consecutive electoral terms with PMGSY road construction are shaded blue. In Map 2 constituencies with at least one election with a margin of victory lower than 6.2% (derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011)) are shaded red.

Map 3: Variation in the baseline share of same name contractors



Note: The constituencies shown on maps are based on the pre-2008 delimitation. Post delimitation data is assigned to pre-delimitation boundaries. Map 3 plots $share_{ijt}$ for all constituencies in the sample. Darker shades indicate a higher value of $share_{ijt}$.

TABLES

Table A1: Sources of election data

Source	Years covered	No. of elections	No. of candidates	Candidate-level variables used in the sample
ECI digitised data	2005-2014	7,328	80,323	name, vote share, gender, party
Bhavani (2012)	1977-2012	31,422	300,087	name, vote share, gender, party
Empowering India	1951-2015	19,715	196,935	assets, education, age
National Election Watch	2004-2015	8,944	73,200	assets, liabilities, education, criminal charges

Note: ECI digitised data refers to a subset of the ECI data that are available online at eci.nic.in. Bhavani (2012) is a dataset kindly made public by Rikhil R. Bhavnani. Empowering India and National Election Watch are NGOs. Their data is accessible at www.empoweringindia.com and myneta.info respectively. Where a variable is listed twice in the fifth column, this is due to incomplete time series or missing values that are filled in by drawing on multiple datasets.

Table A2: Local linear regression RD - levels

Dep var.: share of same name contractors$_t$	Whole Sample		Margin of Victory <6.2%		Margin of Victory <3%	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0085***	0.0115**	0.0121**	0.0157**	0.0160**	0.0191*
	(0.0032)	(0.0046)	(0.0054)	(0.0078)	(0.0071)	(0.0103)
Margin	0.0119	-0.0168	-0.0891	-0.1289	-0.4285	-0.4816
	(0.0212)	(0.0247)	(0.1120)	(0.1432)	(0.3063)	(0.3594)
Margin*winner	-0.0364	0.0176	0.0632	0.1596	0.3747	0.4364
	(0.0349)	(0.0418)	(0.1802)	(0.2340)	(0.4924)	(0.5649)
Incumbent		0.0082**		0.0140**		0.0111
		(0.0039)		(0.0056)		(0.0072)
Runner-up in previous election		0.0023		0.0037		
		(0.0048)		(0.0067)		(0.0089)
Female candidate		-0.0179***		-0.0155**		-0.0073
		(0.0049)		(0.0072)		(0.0100)
Age		-0.0000		-0.0001		0.0001
		(0.0002)		(0.0002)		(0.0003)
AC controls		X		X		X
State fixed effects		X		X		X
Election year fixed effects		X		X		X
N	15,208	7,290	8,202	4,012	4,538	2,248

Note: Local linear regression estimates. Standard errors are clustered at the election-level. Variables are defined either in the text or in the note for table 1. AC controls include: Reserved seat, Road count$_{t-1}$, Mean population, Mean SC/ST population, Mean connectivity, Mean road length$_{t-1}$. The bandwidth of 6.2% is derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

Table A3: Balance test at 3% bandwidth

	Obs	Mean winner	Mean runner-up	P value
	(1)	(2)	(3)	(4)
Panel A: Candidate characteristics				
$Share_{t-1}$	2472	0.038	0.038	0.913
Incumbent	2472	0.274	0.278	0.8221
Runner-up in prev. election	2472	0.144	0.140	0.7734
Age	2314	48.853	48.806	0.9114
Female candidate	2472	0.061	0.063	0.8679
Cand. faces criminal charge	1889	0.220	0.220	0.990
Assets (INR millions)	1554	386.0	220.0	0.7005
Liabilities (INR millions)	1747	1.218	1.415	0.54
University degree	1747	0.598	0.602	0.8377
Post-grad. degree	1747	0.207	0.196	0.5706
BJP candidate	2465	0.257	0.281	0.192
Congress candidate	2461	0.208	0.185	0.1587
Panel B: Share of roads built by contractors of same name in term prior to election				
Share 5 yrs before election	1416	0.033	0.027	0.4586
Share 4 yrs before election	1624	0.033	0.041	0.3478
Share 3 yrs before election	1490	0.032	0.037	0.5861
Share 2 yrs before election	942	0.031	0.035	0.709
Share 1 year before election	1070	0.036	0.049	0.2334
Panel C: Prevalence of most common names				
Named Kumar	2472	0.070	0.061	0.3701
Named Lal	2472	0.028	0.027	0.9015
Named Patel	2472	0.011	0.011	0.8466
Named Ram	2472	0.018	0.020	0.6588
Named Reddy	2472	0.013	0.008	0.237
Named Singh	2472	0.134	0.120	0.2771
Named Yadav	2472	0.019	0.019	0.883

Note: Variables are defined as in Table 1. Columns 2 and 3 show the respective means for winners and runners-up at the 3% bandwidth. *** p<0.01, ** p<0.05, * p<0.1

Table A4: Fully non-parametric RD

ΔShare of same name contractors$_t$	Whole Sample		Margin of Victory <6.2%		Margin of Victory <3%	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0090**	0.0090**	0.0100**	0.0108**	0.0146**	0.0145**
	(0.0035)	(0.0037)	(0.0049)	(0.0051)	(0.0065)	(0.0069)
Incumbent		-0.0015		0.0028		-0.0056
		(0.0045)		(0.0065)		(0.0085)
Runner-up in previous election		0.0068		0.0067		0.0027
		(0.0055)		(0.0078)		(0.0110)
Female candidate		-0.0011		-0.0131		-0.0012
		(0.0065)		(0.0107)		(0.0117)
Age		0.0002		-0.0000		0.0004
		(0.0002)		(0.0003)		(0.0003)
AC controls		X		X		X
State fixed effects		X		X		X
Election year fixed effects		X		X		X
N	8,116	7,290	4,396	4,012	2,472	2,248

Note: Local linear regression estimates. Standard errors are clustered at the election-level. Variables are defined either in the text or in the note for table 1. AC controls include: Reserved seat, Road count$_{t-1}$, Mean population, Mean SC/ST population, Mean connectivity, Mean road length$_{t-1}$. The bandwidth of 6.2% is derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

Table A5: Change in $Share_t$ for losing candidates

Dep var.: share of same name contractors$_t$	Whole Sample	Margin of Victory <6.2%	Margin of Victory <3%
	(1)	(2)	(3)
Post election	-0.0035	-0.0060	-0.0073
	(0.0027)	(0.0037)	(0.0053)
Margin	-0.0211	0.0620	0.2091
	(0.0240)	(0.1363)	(0.3318)
Candidate controls	X	X	X
AC controls	X	X	X
State fixed effects	X	X	X
Election year fixed effects	X	X	X
N	6,291	3,446	1,918

Note: OLS estimates. This sample is restricted to candidates who finished second in their elections. Post election is a dummy that takes the value of 0 for the term prior to the election and the value of 1 for the term after the election. The list of candidate and constituency controls is the same as in Table 3. The bandwidth of 6.2% is derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

Table A6: Allocation results dropping candidates with common names

ΔShare of same name contractors$_t$	Margin of victory <6.2%		Margin of victory <3%	
	(1)	(2)	(3)	(4)
Winner	0.0150**	0.0149*	0.0248**	0.0256**
	(0.0076)	(0.0082)	(0.0104)	(0.0112)
Margin	-0.1510	-0.1105	-0.4478	-0.5700
	(0.1588)	(0.1587)	(0.4312)	(0.4599)
Margin*winner	-0.0116	-0.0889	-0.2812	-0.1567
	(0.2293)	(0.2324)	(0.5492)	(0.5864)
Candidate controls		X		X
AC controls		X		X
State fixed effects		X		X
Election year fixed effects		X		X
N	3,874	3,545	2,172	1,977
Mean share$_{t-1}$	0.0227	0.0229	0.0239	0.0237

Note: Local linear regression estimates. Standard errors are clustered at the election-level. The sample for these regressions includes only elections where both candidates' names occurs with a frequency of less than 10% within their state. Candidate and AC controls are the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table A7: Heterogeneity by electoral characteristics

ΔShare of same name contractors$_t$	3% (1)	3% (2)	3% (3)	3% (4)
Winner	0.0389**	0.0359*	0.0539**	0.0522**
	(0.0177)	(0.0188)	(0.0240)	(0.0254)
Margin	-0.5264	-0.5759	-0.9584*	-1.0158*
	(0.4514)	(0.5014)	(0.4897)	(0.5412)
Margin*winner	0.3074	0.4048	0.8575	0.9204
	(0.6035)	(0.6458)	(0.6250)	(0.6723)
Num. of candidates	0.0005	0.0001		
	(0.0008)	(0.0011)		
Num. of candidates*winner	-0.0016	-0.0013		
	(0.0013)	(0.0013)		
Winner's vote share			0.0446	0.0555
			(0.0488)	(0.0585)
Winner's vote share*winner			-0.0918	-0.0826
			(0.0714)	(0.0744)
Candidate controls		X		X
AC controls		X		X
State fixed effects		X		X
Election year fixed effects		X		X
Observations	2,234	2,017	2,472	2,248
R-squared	0.0031	0.0200	0.0046	0.0192

Note: Local linear regression estimates. Standard errors are clustered at the election-level. Variables are defined either in the text or in the note for table 1. Candidate and AC controls are the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table A8: Local linear regression RD – Alignment with Chief Minister

ΔShare of same name contractors$_t$	Whole Sample		Margin of Victory <6.2%		Margin of Victory <3%	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0054	0.0065	0.0191*	0.0192*	0.0249*	0.0280*
	(0.0057)	(0.0060)	(0.0099)	(0.0103)	(0.0136)	(0.0146)
Margin	0.0046	0.0165	-0.3143*	-0.2717	-0.9934*	-1.0686*
	(0.0305)	(0.0342)	(0.1879)	(0.1853)	(0.5097)	(0.5514)
Margin*winner	-0.0119	-0.0439	0.0589	0.0106	0.8658	0.9184
	(0.0420)	(0.0451)	(0.2718)	(0.2679)	(0.6324)	(0.6737)
Aligned	-0.0053	-0.0053	-0.0007	0.0012	-0.0104	-0.0076
	(0.0061)	(0.0067)	(0.0077)	(0.0083)	(0.0100)	(0.0111)
Aligned*winner	0.0103	0.0099	0.0158	0.0150	0.0165	0.0127
	(0.0077)	(0.0084)	(0.0105)	(0.0109)	(0.0134)	(0.0144)
Candidate controls		X		X		X
AC controls		X		X		X
State fixed effects		X		X		X
Election year fixed effects		X		X		X
N	8,108	7,290	4,392	4,012	2,469	2,248

Note: Local linear regression estimates. Standard errors are clustered at the election-level. Variables are defined either in the text or in the note for table 1. AC controls include: Reserved seat, Road count$_{t-1}$, Mean population, Mean SC/ST population, Mean connectivity, Mean road length$_{t-1}$. Individual controls include incumbency, former runner-up status, gender and age. The bandwidth of 6.2% is derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). *** p<0.01, ** p<0.05, * p<0.1

Table A9: Heterogeneity by constituency characteristics

ΔShare of same name contractors$_t$	3%		3%		3%		3%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner	0.061*	0.067*	0.042**	0.041**	0.035**	0.029*	0.038***	0.041***
	(0.032)	(0.035)	(0.018)	(0.020)	(0.017)	(0.018)	(0.014)	(0.015)
Margin	-0.959*	-1.006*	-1.026**	-1.083**	-1.019**	-1.059*	-0.977*	-1.037*
	(0.505)	(0.549)	(0.519)	(0.548)	(0.519)	(0.552)	(0.510)	(0.556)
Margin*winner	0.717	0.787	0.773	0.947	0.763	0.905	0.732	0.804
	(0.626)	(0.667)	(0.635)	(0.669)	(0.637)	(0.676)	(0.628)	(0.670)
Literacy	0.051	0.049						
	(0.040)	(0.048)						
Literacy* winner	-0.058	-0.070						
	(0.057)	(0.061)						
Road connectivity			-0.003	-0.027				
			(0.014)	(0.027)				
Road connectivity *winner			-0.013	-0.017				
			(0.020)	(0.021)				
SC/ST share					-0.015	-0.031		
					(0.018)	(0.036)		
SC/ST share*winner					0.001	0.011		
					(0.026)	(0.029)		
SC/ST herfindahl							0.045	0.044
							(0.030)	(0.037)
SC/ST herfindahl*winner							-0.048	-0.068
							(0.042)	(0.046)
Candidate and AC controls		X		X		X		X
State and election year fixed effects		X		X		X		X
N	2,440	2,216	2,406	2,248	2,406	2,248	2,440	2,216

Note: Local linear regression estimates. Standard errors are clustered at the election-level. Variables are defined either in the text or in the note for table 1. Candidate and AC controls are the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table A10: Change in $Share_t$ for Chief Ministers

	Share of same name $contractors_t$		
	(1)	(2)	(3)
CM in office	0.0021	0.0015	0.0074
	(0.0112)	(0.0088)	(0.0092)
State fixed effects		X	X
Election year fixed effects			X
N	174	174	174

Note: OLS estimates. CM in office is a dummy that takes the value of 0 for the term prior to the election and the value of 1 for the period in the term after the election when the CM is in office. $Share_t$ is computed for Chief Ministers in the same way as for MLAs but at the state level. *** p<0.01, ** p<0.05, * p<0.1

Table A11: Alignment and party seat share

ΔShare of same name $contractors_t$	3%		3%	
	(1)	(2)	(3)	(4)
Winner	0.0306**	0.0306*	0.0269*	0.0296*
	(0.0153)	(0.0164)	(0.0158)	(0.0171)
Margin	-1.0131**	-1.0758*	-0.9752*	-1.0554*
	(0.5058)	(0.5497)	(0.5151)	(0.5565)
Margin*winner	0.8613	0.9424	0.8702	0.9165
	(0.6342)	(0.6733)	(0.6388)	(0.6786)
Party seat share	-0.0101	-0.0071	-0.0038	0.0029
	(0.0173)	(0.0203)	(0.0256)	(0.0332)
Party seat share*winner	0.0043	0.0056	-0.0119	-0.0091
	(0.0264)	(0.0273)	(0.0356)	(0.0381)
Aligned			-0.0387	-0.0196
			(0.0379)	(0.0439)
Aligned*winner			0.0686	0.0547
			(0.0523)	(0.0557)
Party seat share*aligned			0.0571	0.0214
			(0.0680)	(0.0840)
Party seat share*aligned*winner			-0.0924	-0.0748
			(0.0961)	(0.1002)
Candidate controls		X		X
AC controls		X		X
State fixed effects		X		X
Election year fixed effects		X		X
Observations	2,472	2,248	2,469	2,248

Note: Local linear regression estimates. Standard errors are clustered at the election-level. Party seat share is the number of seats the candidate's party won in the election divided by the number of seats in the state. Variables are defined either in the text or in the note for table 1. Candidate and AC controls are the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table A12: Allocation in same district but outside of constituency

ΔShare of same name contractors$_t$	Margin of victory <6.2%		Margin of victory <3%	
	(1)	(2)	(3)	(4)
Winner	0.0066	0.0038	0.0085	0.0081
	(0.0074)	(0.0077)	(0.0107)	(0.0113)
Margin	0.0110	0.1127	0.1077	0.0657
	(0.1499)	(0.1501)	(0.4199)	(0.4504)
Margin*winner	-0.1218	-0.2343	-0.4145	-0.4420
	(0.2141)	(0.2180)	(0.5463)	(0.5803)
Candidate controls		X		X
AC controls		X		X
State fixed effects		X		X
Election year fixed effects		X		X
Observations	3,189	2,911	3,189	2,911

Note: Local linear regression estimates. Standard errors are clustered at the election-level. For these regressions, share$_t$ is defined as the share of projects allocated to contractors with the same name as a candidate in their district, but outside their constituency. Candidate and AC controls are the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table A13: Within-constituency heterogeneity

ΔShare of same name contractors$_t$	All constituencies Margin of Victory <3% (1)	(2)	All constituencies Margin of Victory <3% (3)	(4)	Unreserved constituencies Margin of Victory <3% (5)	(6)
Winner	0.0336**	0.0327**	0.0330***	0.0344***	0.0451***	0.0465***
	(0.0133)	(0.0143)	(0.0124)	(0.0130)	(0.0165)	(0.0173)
Margin	-0.587	-0.659	-0.969*	-1.063**	-1.678**	-1.826**
	(0.491)	(0.536)	(0.495)	(0.530)	(0.661)	(0.714)
Margin*winner	-0.116	0.0177	0.576	0.679	1.035	1.190
	(0.625)	(0.666)	(0.632)	(0.660)	(0.842)	(0.890)
Panchayat HQ villages	0.00380	0.00503				
	(0.00780)	(0.00888)				
Panchayat HQ villages*winner	-0.00648	-0.00626				
	(0.0120)	(0.0129)				
SC/ST majority villages			0.000904	0.00232	-0.00183	-0.00256
			(0.00894)	(0.0104)	(0.0134)	(0.0152)
SC/ST maj. villages*winner			-0.00004	0.00346	0.0170	0.0214
			(0.0126)	(0.0134)	(0.0186)	(0.0197)
Candidate controls		X		X		X
AC controls						
State fixed effects		X		X		X
Election year fixed effects		X		X		X
N	3,252	3,001	3,304	3,006	2,350	2,143

Note: Local linear regression estimates. Standard errors are clustered at the election-level. Constituencies are split: villages with panchayat HQs vs all other villages in columns (1) and (2); villages that are majority SC and ST vs all other villages in columns (3) to (6). Columns (5) and (6) restrict the sample to constituencies not reserved for SC or ST politicians. *** p<0.01, ** p<0.05, * p<0.1

Table A14: Parametric regression discontinuity estimated on full sample

$\Delta Share_t$	Linear		Quadratic Polynomials		Cubic Polynomials	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0094*	0.0094*	0.0138**	0.0138**	0.0190**	0.0193**
	(0.0052)	(0.0055)	(0.0062)	(0.0067)	(0.0075)	(0.0082)
Margin	-0.0001	0.0271	-0.0226	-0.0058	-0.1004	-0.1562
	(0.0299)	(0.0345)	(0.0609)	(0.0719)	(0.1209)	(0.1506)
Margin*Winner	-0.0059	-0.0504	-0.0740	-0.0998	-0.1247	-0.0253
	(0.0414)	(0.0456)	(0.0870)	(0.0994)	(0.1604)	(0.1890)
Margin^2			-0.0764	-0.1130	-0.6165	-1.2326
			(0.1540)	(0.1921)	(0.6727)	(0.9282)
Margin*Winner ^2			0.3834	0.3849	1.8161*	2.0317
			(0.2689)	(0.2869)	(1.0541)	(1.2413)
Margin ^3					-0.8224	-1.8595
					(0.8418)	(1.3418)
Margin*Winner ^3					-0.5365	1.0619
					(1.0901)	(1.5242)
Constituency-level controls		X		X		X
Candidate-level controls		X		X		X
State fixed effects		X		X		X
Election year fixed effects		X		X		X
N	8,116	7,068	8,116	7,068	8,116	7,068

Note: Estimated by OLS. Standard errors are clustered at the election-level. Variables are defined either in the text or in the note for table 1. The candidate and constituency level controls in columns 2, 4, 6 and 8 are the same as in column (2) of Table 3. These controls and state and election year fixed effects are not reported. All regressions include a constant. *** p<0.01, ** p<0.05, * p<0.1

Table A15: 2^{nd} vs 3^{rd} Placebo Tests

ΔShare of same name contractors$_t$	All 2^{nd} vs 3^{rd} with margin < 3%		2^{nd} vs 3^{rd} where margin between 1^{st} and 2^{nd} <3%		2^{nd} vs 3^{rd} where both margins <3%	
	(1)	(2)	(3)	(4)	(5)	(6)
2^{nd} place	-0.0282	-0.0132	0.0046	0.0038	0.0094	0.0463
	(0.0176)	(0.0190)	(0.0122)	(0.0138)	(0.0318)	(0.0368)
Margin (2^{nd} vs 3^{rd})	1.0832	0.1563	-0.0257	0.0625	0.5122	-1.4668
	(0.8269)	(0.8767)	(0.0394)	(0.0573)	(1.6560)	(2.0495)
Margin*2^{nd} place	-1.2061	-0.2548	0.0023	-0.1978**	-1.4159	0.3412
	(1.0470)	(1.1582)	(0.0505)	(0.0839)	(1.9071)	(2.3849)
Candidate controls		X		X		X
AC controls		X		X		X
State fixed effects		X		X		X
Election year fixed effects		X		X		X
N	8,116	7,290	4,396	4,012	2,472	2,248

Note: the regressions in this table are equivalent to those in Table 3, except that these compare 2^{nd} placed candidates to 3^{rd} placed candidates. In columns (1) and (2) the sample is restricted to candidates whose difference in vote shares was smaller than 3% (as in the main specification). Columns (3) and (4) compare 2^{nd} and 3^{rd} placed candidates in the elections that make up the sample for the main specification. The sample for columns (5) and (6) is the intersection of the previous two, including candidates from elections where both the gap between 1^{st} and 2^{nd} and between 2^{nd} and 3^{rd} was smaller than 3%. Standard errors are clustered at the election level. All regressions include a constant. *** p<0.01, ** p<0.05, * p<0.1

Table A16: Sample of politicians not in power during planning stage

ΔShare of same name contractors$_t$	6.2%		3%	
	(1)	(2)	(3)	(4)
Winner	0.0217**	0.0209**	0.0306**	0.0319**
	(0.0099)	(0.0101)	(0.0130)	(0.0138)
Margin	-0.1765	-0.1324	-0.7526	-0.8056
	(0.2116)	(0.2072)	(0.5205)	(0.5678)
Margin*winner	-0.0941	-0.1553	0.4664	0.4533
	(0.3263)	(0.3198)	(0.7475)	(0.8018)
Candidate controls		X		X
AC controls		X		X
State fixed effects		X		X
Election year fixed effects		X		X
N	3,189	2,911	3,189	2,911

Note: Local linear regression estimates. Standard errors are clustered at the election-level. This sample is equivalent to that for Table 3, except that it excludes candidates in terms that started in 2001 or before, as well as candidates in later terms who had been in office in 2001. Candidate and AC controls are the same as in Table 3. *** p<0.01, ** p<0.05, * p<0.1

Table A17: Continuity test for road level local linear regression at 3% bandwidth

	Observations	Winner	Standard error
Panel A: Local Geography			
Altitude	1,470	10.3736	(0.8787)
Ruggedness	1,470	0.1615	(0.1995)
Forest cover	1,470	0.0280**	(0.0125)
Distance to nearest town	1,470	-1.9768	(0.2988)
Panel B: Village Demographics			
Total population	1,470	303.22	(0.3392)
Number of households	1,470	49.9927	(0.3728)
Village area	1,470	188.54	(0.4854)
Sex ratio	1,468	-0.0000	(0.9992)
Population under 6	1,470	-0.0016	(0.7036)
SC share	1,470	0.0223	(0.1999)
ST share	1,470	0.0268	(0.2403)
Panel C: Village Socioeconomic Characteristics			
Literacy	1,071	-0.0168	(0.5513)
Employment	1,071	0.0084	(0.5837)
Male employment	1,071	0.0154	(0.1211)
Female employment	1,071	0.0105	(0.6901)
Irrigated area under cultivation	1,387	-0.0260	(0.5689)
Unirrigated area under cultivation	1,404	0.0495	(0.2063)
Drinking water	1,470	0.0015	(0.5637)
Power supply	1,470	-0.0368	(0.4735)
Phone connections	1,470	-7.5092	(0.2526)
Primary school	1,470	0.1500	(0.4988)
Medical facility	1,470	0.0702	(0.2655)
Health centre	1,470	0.0236	(0.1879)
Post office	1,470	0.0419	(0.4602)
Bank	1,470	0.0064	(0.8450)
Road connectivity in 2001	1,470	0.0005	(0.9933)
Mud road	1,375	0.0774	(0.1425)
Foot path	1,281	-0.0700	(0.1848)
Panel D: Pre-determined Road Characteristics			
Length of road	1,470	0.1346	(0.2357)
Bridge	1,470	-0.0219*	(0.0654)

Note: Coefficients are estimated by regressing the row variables on winner, the vote margin, and the vote margin interacted with winner in OLS regressions, including state and year fixed effects. Standard errors are clustered at the contractor level. Where a road passes through multiple villages, the road level variable is an average of all villages on that road. *** p<0.01, ** p<0.05, * p<0.1

Table A18a: Road-level RD sample at multiple bandwidths – missing roads

Panel A

Dependent variable:	Missing all-weather road			Missing any road		
Margin of victory:	<5%	Opt. BW <2.48%	<2.5%	<5%	Opt. BW <4.4%	<2.5%
	(1)	(2)	(3)	(4)	(5)	(6)
MLAsamename	0.0849	0.3003***	0.2855***	0.0822*	0.0913*	0.1580**
	(0.0748)	(0.0991)	(0.0960)	(0.0418)	(0.0471)	(0.0721)
Margin	3.2279	-7.1166	-5.4180	0.1886	-1.0443	-3.7428*
	(1.9719)	(4.7207)	(4.7713)	(0.9401)	(1.0967)	(2.2274)
Margin*MLAsamenam	-5.5556*	-4.0300	-5.6604	-2.6142*	-1.0541	-0.6751
	(3.1955)	(7.3736)	(7.3804)	(1.5557)	(1.7138)	(4.2556)
Road-level controls	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X
Agreement year FE	X	X	X	X	X	X
N	829	456	464	829	757	464

Note: Standard errors clustered at the contractor level to account for intra-contractor correlation of the error term at the road level. Road-level controls are equivalent to the full set of controls used in columns 2 and 4 of Table 4. The optimal bandwidths of 2.5% (for missing all-weather roads), and 4.4% (for missing any roads), are derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). All regressions include a constant. *** p<0.01, ** p<0.05, * p<0.1

Table A18b: Road-level regression discontinuity - estimates for quality

Panel B

Dependent variable:	Ln(sanctioned cost/km)			Days overrun			Ratio: actual cost to sanctioned cost			Failed inspection		
Margin of victory:	<5%	<2.7%	<2.5%	<5%	<3%	<2.5%	<5%	<3.5%	<2.5%	<5%	<4.3%	<2.5%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
MLAsamename	0.069*	0.117**	0.116**	-108.7	-78.1	-134.1	-0.055	-0.057	-0.040	0.150*	0.162*	0.137
	(0.037)	(0.048)	(0.050)	(70.6)	(95.1)	(110.6)	(0.035)	(0.044)	(0.055)	(0.078)	(0.083)	(0.109)
Margin	-1.578	-7.331***	-7.641***	127.2	-1,176.1	883.3	1.180	2.733	2.216	-1.995	-1.894	-1.522
	(1.029)	(2.532)	(2.772)	(1,913.5)	(4,548.6)	(4,693.4)	(1.226)	(2.043)	(3.204)	(2.285)	(3.018)	(5.645)
Margin*MLAsamename	1.930	8.533**	8.972**	1,663.0	1,784.4	3,458.7	-0.591	-3.488	-3.577	-3.663	-4.272	-4.204
	(1.456)	(3.479)	(3.713)	(2,759.9)	(5,551.0)	(6,163.0)	(1.539)	(2.433)	(4.063)	(3.109)	(3.985)	(7.081)
Road level controls	X	X	X	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X	X	X	X
Agreement year FE	X	X	X	X	X	X	X	X	X	X	X	X
N	2,124	1,340	1,234	1,386	970	794	1,671	1,334	960	719	660	407

Note: Standard errors clustered at the contractor level to account for intra-contractor correlation of the error term at the road level. All regressions include the full set of road level controls from cols 2, 4, 6, and 8 in Table 5. The optimal bandwidths of 2.7% (for cost/km), 3% (for days overrun), 3.5% (for cost overruns), and 4.3% (for failed inspections) are derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011). All regressions include a constant. *** p<0.01, ** p<0.05, * p<0.1

Table A19: Robustness to outliers in road level data

Dependent Variable:	Ln(sanctioned cost/km)			Days Overrun			Actual cost/sanctioned cost		
	Margin of Victory <3%			Margin of Victory <3%			Margin of Victory <3%		
Margin of victory:	Full sample	Dropping bottom 1% and top 1%	Dropping bottom 5% and top 5%	Full sample	Dropping bottom 1% and top 1%	Dropping bottom 5% and top 5%	Full sample	Dropping bottom 1% and top 1%	Dropping bottom 5% and top 5%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
MLAsamename	0.116**	0.113**	0.089*	-88.9	-44.3	-26.2	-0.052	-0.002	0.008
	(0.050)	(0.050)	(0.050)	(91.6)	(73.0)	(67.2)	(0.053)	(0.025)	(0.017)
Margin	-7.681***	-7.582***	-5.278**	-472.1	377.6	-300.0	1.870	-1.025	-2.133**
	(2.576)	(2.574)	(2.464)	(4,423.1)	(3,696.2)	(3,337.5)	(2.713)	(1.184)	(0.858)
Margin*MLAsamename	10.419***	10.590***	7.819**	1,076.6	-2,359.6	-320.3	-2.177	0.992	3.104***
	(3.342)	(3.354)	(3.236)	(5,546.3)	(4,799.7)	(4,164.2)	(2.998)	(1.433)	(1.045)
Road-level controls	X	X	X	X	X	X	X	X	X
State fixed effects	X	X	X	X	X	X	X	X	X
Year fixed effects	X	X	X	X	X	X	X	X	X
N	1,470	1,449	1,327	970	956	908	1,161	1,143	1,056

Note: Standard errors clustered at the contractor level to account for intra-contractor correlation of the error term at the road level. All regressions include the following set of road-level controls: ln(length) (to account for non-linear relationship between cost and distance), whether the constituency is a reserved seat, the mean population of habitations on the road, the mean population share of Scheduled Castes and Scheduled Tribes of habitations on the road, and the mean connectivity of those habitations in 2001. *** p<0.01, ** p<0.05, * p<0.1

Table A20: Robustness checks for promotion screening results

ΔShare of same name contractors$_t$	Margin of victory <6.2%		Margin of victory <3%		Margin of victory <6.2%		Margin of victory <3%	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Winner	0.0342**	0.0332*	0.0404**	0.0403**	0.0254***	0.0225**	0.0254***	0.0225**
	(0.0165)	(0.0182)	(0.0177)	(0.0192)	(0.0096)	(0.0101)	(0.0096)	(0.0101)
DC promotion year	0.0053	0.0066	0.0207*	0.0258**	0.0125	0.0161*	0.0125	0.0161*
	(0.0080)	(0.0091)	(0.0114)	(0.0126)	(0.0088)	(0.0097)	(0.0088)	(0.0097)
DC promotion year*winner	-0.0274*	-0.0273*	-0.0308	-0.0420**	-0.0342**	-0.0342**	-0.0342**	-0.0342**
	(0.0140)	(0.0148)	(0.0187)	(0.0205)	(0.0144)	(0.0153)	(0.0144)	(0.0153)
Years of service	0.0011	0.0014	0.0008	0.0001				
	(0.0009)	(0.0011)	(0.0012)	(0.0014)				
Years of service *winner	-0.0012	-0.0013	-0.0010	-0.0006				
	(0.0015)	(0.0017)	(0.0018)	(0.0020)				
Promotion year – 2					0.0031	0.0013	0.0031	0.0013
					(0.0076)	(0.0084)	(0.0076)	(0.0084)
Promotion year – 2*winner					0.0011	0.0029	0.0011	0.0029
					(0.0129)	(0.0140)	(0.0129)	(0.0140)
Promotion year – 1					0.0153	0.0207*	0.0153	0.0207*
					(0.0097)	(0.0110)	(0.0097)	(0.0110)
Promotion year – 1*winner					-0.0277*	-0.0252	-0.0277*	-0.0252
					(0.0146)	(0.0159)	(0.0146)	(0.0159)
Promotion year + 1					0.0023	0.0059	0.0023	0.0059
					(0.0103)	(0.0118)	(0.0103)	(0.0118)
Promotion year + 1*winner					-0.0021	-0.0018	-0.0021	-0.0018
					(0.0143)	(0.0159)	(0.0143)	(0.0159)
Promotion year + 2					-0.0056	-0.0105	-0.0056	-0.0105
					(0.0145)	(0.0169)	(0.0145)	(0.0169)
Promotion year + 2*winner					0.0327	0.0264	0.0327	0.0264
					(0.0247)	(0.0266)	(0.0247)	(0.0266)
Candidate and AC Controls		X		X		X		X
State and election year FE								
N	7,336	6,607	4,144	3,730	7,358	6,629	7,358	6,629

Note: Local linear regression estimates. Standard errors are clustered at the district-level. All regressions control for margin and its interaction with winner. Candidate and AC controls and fixed effects as in Table 6. *** p<0.01, ** p<0.05, * p<0.1

Table A21: RD test for electoral cycles in preferential allocation

$\Delta Share_t$	Start of term heterogeneity		End of term heterogeneity		Start and end of term heterogeneity	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0267**	0.0215**	0.0301***	0.0250**	0.0277***	0.0232**
	(0.0106)	(0.0109)	(0.0103)	(0.0108)	(0.0107)	(0.0112)
Margin	-0.3575*	-0.3625*	-0.3563*	-0.3563*	-0.3588*	-0.3608*
	(0.1957)	(0.2134)	(0.1961)	(0.2136)	(0.1959)	(0.2135)
Margin*winner	-0.2042	-0.0144	-0.2033	-0.0239	-0.2002	-0.0160
	(0.3020)	(0.2967)	(0.3020)	(0.2968)	(0.3019)	(0.2969)
Start of term	-0.0063	-0.0076			-0.0073	-0.0082
	(0.0052)	(0.0056)			(0.0053)	(0.0058)
Start of term* winner	0.0068	0.0059			0.0057	0.0043
	(0.0077)	(0.0081)			(0.0077)	(0.0081)
End of term			0.0003	0.0015	-0.0028	-0.0020
			(0.0052)	(0.0056)	(0.0053)	(0.0057)
End of term* winner			-0.0054	-0.0063	-0.0029	-0.0045
			(0.0076)	(0.0081)	(0.0075)	(0.0081)
Constituency Controls		X		X		X
Candidate Controls		X		X		X
State fixed effects		X		X		X
Agreement year fixed effects		X		X		X
N	6,346	5,625	6,346	5,625	6,346	5,625

Note: Standard errors clustered at the election level. All estimates conducted on 5% bandwidth. All regressions include a constant. For this analysis there are potentially three observations per electoral term: the value of $Share_t$ for the first 12 months after an election, the value of $Share_t$ for the last 12 months before the next election, and the value of $Share_t$ over the remaining term. For constituencies where no roads were built in one of these periods, the number of observations will be less than three. *** p<0.01, ** p<0.05, * p<0.1

Table A22: RD test for electoral cycles in cost

Ln(sanctioned cost/km)	Start of term heterogeneity	End of term heterogeneity	Start and end of term heterogeneity
	(1)	(2)	(3)
MLAsamename	0.1414***	0.1522***	0.1652***
	(0.0530)	(0.0535)	(0.0553)
Margin	-7.9813***	-8.1813***	-7.7047***
	(2.6897)	(2.7732)	(2.7220)
Margin*MLAsamename	9.1694**	9.1362**	8.7095**
	(3.6056)	(3.6984)	(3.6404)
Start of term	0.1365***		0.1475***
	(0.0418)		(0.0445)
Start of term* MLAsamename	-0.0993*		-0.1193**
	(0.0556)		(0.0562)
End of term		0.0300	0.0572
		(0.0597)	(0.0611)
End of term* MLAsamename		-0.1249**	-0.1455**
		(0.0592)	(0.0595)
Road level controls	X	X	X
State fixed effects		X	
Agreement year FE		X	
N	1,340	1,340	1,340

Note: Standard errors clustered at the contractor level. All estimates conducted on optimal bandwidth of 2.7% derived from the optimal bandwidth choice rule of Imbens and Kalyanaraman (2011) . The set of road-level controls is the same as in Table 4 and Table 5. All regressions include a constant. *** p<0.01, ** p<0.05, * p<0.1

Table A23: RD test for heterogeneity based on "political relevance"

Δ Share$_t$	Full sample with interactions		Margin of victory <6.2%		Margin of victory <3%	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0114**	0.0138**	0.0300***	0.0279**	0.0211	0.0250*
	(0.0057)	(0.0061)	(0.0101)	(0.0109)	(0.0138)	(0.0146)
Margin	0.0139	0.0124	-0.1183	-0.0961	-0.1856	-0.2272
	(0.0282)	(0.0305)	(0.1729)	(0.1882)	(0.5357)	(0.5518)
Margin*winner	-0.0138	-0.0187	-0.3593	-0.2924	-0.0755	-0.0918
	(0.0445)	(0.0485)	(0.2589)	(0.2692)	(0.7141)	(0.7128)
Politically irrelevant	0.0028	0.0078	-0.0060	0.0034	-0.0082	0.0043
	(0.0052)	(0.0067)	(0.0079)	(0.0102)	(0.0116)	(0.0148)
Post announcement	0.0047	0.0062	0.0045	0.0077	-0.0041	0.0009
	(0.0070)	(0.0085)	(0.0097)	(0.0114)	(0.0147)	(0.0171)
Politically irrelevant*winner	-0.0069	-0.0103	-0.0109	-0.0151	0.0114	-0.0017
	(0.0082)	(0.0088)	(0.0119)	(0.0129)	(0.0162)	(0.0174)
Post announcement* winner	-0.0151	-0.0141	-0.0165	-0.0131	0.0071	0.0085
	(0.0103)	(0.0108)	(0.0145)	(0.0150)	(0.0202)	(0.0209)
Politically irrelevant*post announcement	-0.0032	-0.0062	0.0103	0.0016	0.0295	0.0205
	(0.0099)	(0.0114)	(0.0143)	(0.0165)	(0.0211)	(0.0241)
Politically irrelevant*post announcement* winner	0.0063	0.0060	0.0110	0.0144	-0.0412	-0.0273
	(0.0147)	(0.0156)	(0.0206)	(0.0218)	(0.0281)	(0.0295)
Constituency and candidate level controls		X		X		X
State fixed effects		X		X		X
Agreement year fixed effects		X		X		X
N	9,774	8,462	5,234	4,658	3,012	2,707

Note: Observations at the MLA level. Standard errors are clustered at the election-level. The term-level sample is disaggregated, allowing for multiple observations per term. 'Politically irrelevant' denotes areas that did not remain part of the same constituency after delimitation. Post announcement denotes the time period between the announcement of the delimitation reform and the first election under the new delimitation. *** p<0.01, ** p<0.05, * p<0.1.

Table A24: E-Procurement

ΔShare of same name contractors$_t$	Whole Sample		Margin of Victory <6.2%		Margin of Victory <3%	
	(1)	(2)	(3)	(4)	(5)	(6)
Winner	0.0092	0.0113*	0.0278***	0.0273**	0.0407***	0.0428**
	(0.0060)	(0.0067)	(0.0107)	(0.0117)	(0.0153)	(0.0173)
Margin	-0.0274	-0.0073	-0.4330**	-0.5064**	-1.1253*	-1.4149**
	(0.0266)	(0.0318)	(0.2043)	(0.2121)	(0.5810)	(0.6507)
Margin*winner	0.0420	-0.0235	0.1885	0.3052	0.8833	1.2506
	(0.0407)	(0.0461)	(0.2684)	(0.2833)	(0.7418)	(0.8296)
E-procurement	-0.0052	-0.0073	-0.0015	-0.0114	-0.0033	-0.0049
	(0.0039)	(0.0059)	(0.0056)	(0.0090)	(0.0078)	(0.0121)
E-procurement*winner	0.0033	0.0038	0.0032	0.0070	-0.0053	-0.0030
	(0.0058)	(0.0064)	(0.0086)	(0.0093)	(0.0114)	(0.0127)
AC controls		X		X		X
Candidate controls		X		X		X
State fixed effects		X		X		X
Election year fixed effects		X		X		X
N	9,990	8,612	5,324	4,665	3,012	2,625

Note: Local linear regression estimates. Standard errors are clustered at the election-level. Variables are defined either in the text or in the note for table 1. AC controls include: Reserved seat, Road count$_{t-1}$, Mean population, Mean SC/ST population, Mean connectivity, Mean road length$_{t-1}$. Candidate controls: age, gender, incumbency, former-runner-up status*** p<0.01, ** p<0.05, * p<0.1

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