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Do Criminal Politicians Deliver?: Evidence from India's Employment Guarantee and Hindu Holidays

A dissertation submitted in partial satisfaction of the requirements for the degree Doctor of Philosophy in Political Science

by

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ABSTRACT OF THE DISSERTATION

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and Hindu Holidays

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In India, politicians facing criminal charges are routinely elected at higher rates. In this dissertation, I investigate three primary questions to better understand criminal politicians' electoral success and performance in office: 1) Do criminal politicians deliver superior access to social welfare programs relative to clean politicians? 2) Do criminal politicians target benefits to co-partisans at higher rates than clean politicians? 3) Do voters reward criminal politicians for delivering more constituency service than clean politicians? On the one hand, powerful dons may be less responsive to voters' needs, banking on clout to keep voters in line. On the other hand, previous literature and my fieldwork suggest a more Machiavellian strategy, where criminal politicians use both violence and deep pockets to distribute resources to voters.

I present two key arguments to explain criminal politicians' distributive advantages. First, I contend that criminal politicians core assets of money, muscle and networks make them particularly suited to both deliver more state benefits and target co-partisans. Second, I identify a trade-off that candidates face between accruing enough capital to fund campaigns and remaining rooted in the constituency to provide personalized service to voters. I argue that criminals' muscle-power allows

them to sidestep this trade-off and optimize on both dimensions. Muscle enables criminals to establish lucrative protection rackets in their home constituencies. In effect, protection rackets turn muscle into money. To protect this money, criminals invest in networks for delivering resources to voters. Constituent service networks help criminal politicians maintain political power, which proves useful for protecting their illegal enterprises.

To measure criminal politicians' in-office performance, I focus on how India's state legislators influence the delivery of the world's largest public works program, India's National Rural Employment Guarantee Scheme (NREGS). Specifically, to determine if criminal politicians translate their assets of money, muscle and networks into superior social welfare delivery, I construct and combine three original datasets. First, to measure criminality, I scraped self-disclosed affidavits listing 87,000 candidates' criminal charges. The dataset details the criminal histories, wealth, and electoral results of all state legislative candidates in India between 2003 and 2017 (N = 87,000). To measure criminal politicians' benefit distribution, I combine the candidate dataset with original data on the geo-locations of over 20 million NREGS local public works projects. Finally, to determine if criminal politicians are more likely to target resources to co-partisans, I map the geo-tagged NREGS projects to over 400,000 polling stations. Methodologically, I use causal inference and machine learning techniques to analyze this data and strengthen the validity of my estimates.

Overall, I find that criminal politicians deliver more NREGS benefits in safe seats, though not necessarily in competitive constituencies. Second, I find suggestive evidence that criminal politicians target welfare benefits to co-partisans at higher rates relative to clean politicians. By remaining embedded in the constituency, I argue that criminals are better positioned to identify, and then meet, supporters needs. Finally, and perhaps unsurprisingly, I find criminals' core advantage derives from their capacity for violence. Both qualitative and quantitative evidence speak to criminal muscle as a necessary input for improved constituency service and benefit delivery. Empirically, I find that criminals with violent charges are associated with increased NREGS delivery. Whereas, non-violent criminals are not.

The dissertation of Galen Patrick Murray is approved.

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For my mother, Sharon.

Contents

List of Figures

List of Tables

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Chapter 1

Introduction

1.1 Setting the Stage

One muggy summer afternoon in 2017, Arun Yadav sat at the head of a long table outside his palatial estate. The state legislator from Sandesh constituency in Bihar, India was surrounded by aides and local politicians. Scores of rupees spilled from his pockets. A line of constituents sprawled across his lawn and onto the street, patiently waiting for an audience. With a whirl of action, one of Arun's aides emerged from inside the house carrying a large plastic sack stuffed with cash. The line of constituents slowly moved past Arun, who broke off a handful of notes for each attendee. Even children received a small cash transfer from the powerful politician. A few constituents paused for longer conversations with Arun, while his right hand man wrote down their names, requests and monetary grants in a notebook. This display was somewhat shocking as I had recently come from an interview that fingered Arun for "managing" the murder of six scheduled caste persons.[1] However, Arun's violent reputation is well known in Sandesh. When standing for election in 2015, Arun was under indictment for kidnapping and murder charges. Alarmingly, his violent actions likely surpass his extensive rap sheet.[2]

[1] Interview August 2017. Further details are omitted due to the sensitive subject nature. Throughout the dissertation, I take cautions to not identify interview subjects or locations. For the same reason I did not record interview subjects.

[2] In 2010, when Arun Yadav first ran for the stage legislature, his criminal charges included: 1 charge related to Kidnapping or abducting in order to murder (IPC Section-364), 1 charge related to Murder (IPC Section-302), 1 charge related to Voluntarily causing hurt by dangerous weapons or means (IPC Section-324), 1 charge related to Attempt to murder (IPC Section-307), and other lesser charges.

Despite Arun's violent pedigree, the Member of the Legislative Assembly (MLA) is extremely popular. His weekly janata darbars ("people's court") are well attended. His notebook is filled with names and payments for pricier constituent requests dating back months. Arun lavishes the constituency with proceeds obtained from his illegal sand mining operation. In brief, Arun is widely loved within his constituency. In particular, constituents revere the mafioso for his ability to deliver infrastructure projects where previous politicians have failed. For example, *prior* to being elected, Arun commissioned the retrofitting of a local bridge that improved connectivity for 25 villages (representing a sizable number of voters).[3] Originally built in 1962, the small bridge was constantly being washed out during the rainy season. Despite decades of requests, no politician had heeded these citizens' appeals before Arun did so. To paraphrase a rough translation from one interview "everyone knows the bridge was needed, but Arun was the first to act." In scores of interviews, Arun was lauded for his "social work," "living in the village," and demonstrating his locals ties.[4] In 2015, Arun again ran for office, beating out his local BJP rival, and the sitting MLA, Sanjay Tiger. Almost unfailingly, voters described Sanjay Tiger as an "honest man," who is focused on development. However, in the same breath, some accused him of focusing too heavily on roads for urban and peri-urban areas, spending too much time in Patna (the state capital) and not engaging in the same degree of face-to-face constituency service as Arun.[5] The race for the MLA seat was fierce, with Arun Yadav winning by just over 25,000 votes. Perhaps not coincidentally, Arun's margin of victory was similar in size to the population served by his bridge retrofit. In other words, Arun may have quite literally paved his own path to victory.

The differences between Arun Yadav and Sanjay Tiger speak to the heart of the political questions this dissertation addresses. Most notably, Arun Yadav and Sanjay Tiger employ contrasting governance strategies. Arun remains rooted in village life, an ear firmly to the ground, listening to

[3]Interview, August 2017.

[4]Of course Arun had his detractors as well. Though, interestingly, these sentiments did not always fall along party lines.

[5]Interviews, August 2017.

constituents problems and then demonstrating the capacity to solve them "then and there."[6] Conversely, some voters admonished Sanjay Tiger for spending too much time in the capital of Patna- even if there was a recognition that his efforts were in the service of engaging party leaders to spur development in Sandesh. In essence, Sanjay Tiger faced a conundrum between politicking in Patna for pork or fighting Arun on his own turf. Despite his reputation as an honest politician, or perhaps because of it, many ultimately labeled him as ineffectual and disconnected from Sandesh.

Criminal politicians[7] are hardly confined to struggling states such as Bihar. Rather, they are a mainstay of Indian politics (Vaishnav, 2017). To help understand criminals' political success, I construct a dataset that includes over 87,000 candidates and 10,000 state legislative elections. Between 2003 and 2017, I find that 18% of MLA candidates faced criminal charges, including 5% who faced violent charges ranging from assault to murder. Among winning candidates, these percentages nearly double. Overall, 32% of victorious MLAs have a criminal record, with nearly one-third facing violent charges.[8] In effect, violent criminality is predictive of electoral success. To put it starkly, 929 MLAs were charged with violent crimes in the 14 year period I study.

In this dissertation, I investigate three primary questions to better understand criminal politicians' electoral success and performance in office: 1) Do criminal politicians deliver superior access to social welfare programs relative to clean politicians? 2) Do criminal politicians target benefits to co-partisans at higher rates than clean politicians? 3) Do voters reward criminal politicians for delivering more constituency service than clean politicians? On the one hand, powerful dons may be less responsive to voters' needs, banking on clout to keep voters in line. On the other hand, previous literature and my fieldwork suggest a more Machiavellian strategy, where criminal politicians traffic in both fear and love (Hansen, 2005; Vaishnav, 2017). As Arun Yadav's rise

[6]Interview August 27th, 2017. "Then and there" is a direct quote in English from the interview subject. The interviewee emphasized Arun Yadav's ability to resolve disputes and distribute justice with great alacrity.

[7]Throughout this dissertation I use the terms criminal politicians, charged politicians, and accused politicians interchangeably. "Charged" politicians is technically more accurate since politicians convicted of crimes can not stand for election. I follow previous literature and use the canonical term of "criminal" politician interchangeably with charged for brevity and variety.

[8]Nine percent of winning MLAs face some type of violent charge. Violent charges include assault, sexual assault, armed robbery and charges related to homicide. I detail the coding of violent charges in Chapter 3.

to power illustrates, criminal politicians bring more than guns and money to the table. Equally important, I argue, is the ability to translate those resources into durable, political networks. In particular, I explore how criminal politicians' resources of money, muscle and political networks help them to serve voters and retain political power.

1.1.1 Focus on India's National Rural Employment Guarantee Scheme

Criminal politicians' nefarious deeds and political killings capture newspaper headlines and inspire Bollywood baddies.[9] However, criminal politicians' in-office behavior arguably matters more for voters' everyday lives and remains understudied. In India, politicians' mediate access to some of the largest social welfare programs in the world (Kruks-Wisner, 2018; Bussell, 2019). Understanding why politicians place barriers for some citizens, while easing access for others, is central to improving program outcomes for those who depend on public assistance.

Governments around the world deploy social assistance to protect and provide for vulnerable populations (Shahidi et al., 2019). Social welfare programs improve livelihoods, increase income, help purchase groceries and serve as a crucial safety net during economic downturns. These programs matter far more than their immediate benefits. In addition to economic relief, government assistance programs boost mental and physical well-being (O'Campo et al., 2015).

To measure criminal politicians' in-office performance, I focus on how MLAs influence the distribution of the world's largest public works program, India's National Rural Employment Guarantee Scheme (NREGS; World Bank, 2015).[10] All rural Indian households are eligible for 100 days of guaranteed employment under the program, which covers 65% of Indian citizens (approximately 11.5% of the world's population; Wong, 2020). By providing on-demand employment, NREGS helps protect against income shocks and smooths consumption. At its height, NREGS employed more than 48 million individuals in a single year to work on local infrastructure projects

[9] For example, the film *Dabaang* centers around a vigilante policeman and his encounters with a corrupt politician. Vaishnav (2012) notes a reciprocal relationship between the screen and politics, with many politicians styling themselves after Bollywood "hero-villains."

[10] NREGS employed a quarter billion people during it's first six years (Dey and Sen, 2017; Jenkins and Manor, 2017).

(Sukhtankar et al., 2016). Thus, NREGS employment simultaneously improves village assets. Given NREGS's scale, slight improvements in program performance can result in enormous benefits. For example, Banerjee et al. (2016) have demonstrated that an e-payment reform reduced program outlays by 19%, saving the government approximately 0.1% of India's GDP.

In 2005, the National Rural Employment Guarantee Act enshrined every Indian citizen's right to work. The rights based and demand-driven program was designed to put program access firmly in the hands of citizens, while limiting the control of bureaucrats and politicians. Despite the rights based framework, rationing, corruption, leakage and political interference remain (Sukhtankar et al., 2016; Gulzar and Pasquale, 2017; Niehaus and Sukhtankar, 2013). In Section 1.4 I further elaborate on NREGS's institutional structure and how politicians manage program access. First, I discuss the resources in criminal politicians' arsenals that may enable the manipulation of social welfare programs such as NREGS, and the delivery of constituency service that matters to voters.

1.2 The Argument

Criminal politicians routinely win elections in India (Aidt et al., 2011; Vaishnav, 2017). Why? I contend that, given a context where access to state benefits are heavily mediated by politicians and middlemen, politicians need to prove their capacity to solve constituents' problems and get work done prior to taking office. At the same time, politicians require deep pockets both to contest expensive elections and provide constituency service. In other words, money and constituency service are fundamental inputs for winning elections. I argue that politicians may find it difficult to optimize on both these dimensions at the same time. In order to provide routine, face-to-face constituency service politicians need to spend a large portion of their time in the constituency. Politicians rooted in the village, however, may find it difficult to accumulate enough wealth to fund expensive campaigns and win elections. Conversely, if politicians pursue wealth in India's major cities, they may lose connections with local political networks and under-provide constituency service. Put simply, politicians face a trade-off between wealth generation and personalized constituency service provision. This trade-off may be especially stark in rural constituencies with fewer opportunities for making money.

I argue that criminal politicians are particularly capable of solving this wealth-generation versus constituency-service conundrum. Criminals can employ violence to muscle in on whatever economic game happens to be in town. In essence, criminals set up protection rackets, squeezing wealth out of the constituency that is otherwise inaccessible to clean politicians. For example, during my fieldwork in Bihar, I found that criminal politicians' protection rackets often targeted state owned monopolies or government contracts. Once criminals take over the local, illegal economy they are well positioned to both continue accumulating wealth and provide constituency service. By maintaining political power, criminals can protect their lucrative illegal enterprises from the state and other brigands. Therefore, criminals face strong incentives to redistribute wealth to local communities- both to maintain the loyalty of the local populace and to continue winning elections.

Conversely, if clean candidates are locked out of the local economy, they may be forced elsewhere to find campaign funding. Wealthy, clean candidates may be more likely to accrue their fortunes in state capitals or abroad. This limits their ability to remain deeply embedded in the local community. For example, RK Sinha, one of Bihar's richest politicians, made his money by creating an international personal security service. Extremely wealthy politicians like Sinha, who lack the local leader bonafides, tend to end up in the Rajya Sabha (India's upper house of Parliament) (Vaishnav, 2012). Rajya Sabha members are selected by sitting MPs and do not need to face hotly contested elections. Instead, these politicians act as major party benefactors (Vaishnav 2017). Clean candidates that remain in the village, on the other hand, may lack the necessary funds to enhance their popularity through constituent service or even obtain a party ticket in the first place.

A core part of the dissertation explores the role that money, muscle and networks can play in facilitating criminal politicians delivery of state benefits and constituency service. I focus on how money, muscle and networks can theoretically aid criminal politicians in both delivering social welfare benefits and effectively target benefits to co-partisans. I outline the properties of each asset in greater depth below.

1.2.1 Money

> *"Small criminals with a few crores flowing would go on to become a sarpanch or gram pradhan or block pramukh. Beyond ten crores, he would run for MLA."*
>
> - Vikram Singh, former director general of UP police cited in Vaishnav (2017)

Money is useful for contesting increasingly expensive elections, greasing the wheels of bureaucracy and creating economic inter-dependencies between voters and politicians. Campaigns are costly, with wealthier politicians gaining a slight advantage at the polls (Dutta and Gupta, 2014). For these reasons, Indian political parties prefer self-financed candidates (Vaishnav 2017). In part, these costs stem from electoral expenditures on food, liquor, and cash for votes (Björkman, 2014; Björkman and Witsoe, 2019; Sukhtankar and Vaishnav, 2014). For example, Sanjay Singh an aspiring independent MLA candidate in Bihar, and large landholder, was well known for operating an open kitchen in the run up to the elections. Anyone who stopped by was welcome to share a meal, day or night (fieldwork interview, July 2017). To defray campaign costs, large liquid assets are almost an electoral prerequisite. To be clear, these funds are not necessarily in service of garden variety, quid-pro-quo clientelism. Instead, generous and grandiose displays of wealth signal a candidates' credibility and potential to deliver goods for voters in the future (Auerbach et al., 2020; Björkman, 2014). Once, in-office, criminal politicians' cash can help provide direct benefit transfers to voters or finance bribes necessary to unlock state resources. For example, sometimes bureaucrats demand up front payments from local politicians before allowing NREGS projects to break ground (Marcesse, 2018).

1.2.2 Muscle

> *"The more murders you commit the higher office you can aspire to. One murder equals Mukhiya. Five murders equals MLA. Seven murders and up equals MP"* - Paraphrased quotation from interview with village president in Bihar.

Criminal politicians' capacity for violence reinforces their foundation for constituency service and targeting supporters. For example, criminal politicians can physically intimidate bureaucrats to bring them to heel. In addition, muscle-power translates to effective contract enforcement and service delivery on the ground for supporters.

Arun Yadav, the aforementioned criminal MLA from Sandesh, Bihar, provides one window into this muscular brand of politics. Arun blasted his way into Bihari politics, establishing himself as a power-player by ruthlessly taking over the local sand syndicate. His notoriety garnered the attention of RJD king maker Lalu Yadav who handed him the MLA ticket for Sandesh. Arun's violent nature and quick temper is hardly a secret across the constituency. For example, one well-tread story claimed Arun ordered (or was otherwise directly involved) in the murder of six Scheduled Caste persons. While Arun did not pull the trigger, it was intimated that he "managed" the murders (fieldwork 2017). Subsequently, Arun leveraged his political and bureaucratic contacts to escape any criminal proceedings. In this case, his close connections with party-leader Lalu Prasad and the District Magistrate meant the police"managed" the situation and made it disappear.

Arun's deadly power makes his word law in Sandesh. In turn, Arun leverages his violent reputation to settle disputes and deal with a recalcitrant bureaucracy, substituting for an indifferent state. For example, two neighbors were involved in a heated dispute over property boundaries. Arun Yadav helped the parties- one Rajput and the other a member of Scheduled Caste from the village next door- reach a compromise. The scheduled caste villager claimed that his Rajput neighbor encroached on his land to such an extent that he could no longer access his front door. He demanded ten extra feet in the disputed area but Arun Yadav settled on three feet- noting that three feet is more than sufficient space for leaving one's house. With this simple statement, the dispute was resolved and both parties accepted Arun's decision. Despite scheduled castes making up a substantial portion of the RJD support base, in this instance Arun ruled in favor of the forward caste Rajputs. In essence, the compromise represented a political overture to the forward castes, and Rajputs specifically, with concerns towards the next election. Arun (a Yadav) reached across typical caste lines for electoral incentives in an attempt to move beyond his traditional support base. In addition to serving as the constituencies' de facto surveyor, Arun has a stranglehold on the bureaucracy. When meeting with Arun he emphasized how bureaucrats were at his beck and call, picking up a rose gold iPhone to demonstrate that his decree was just a phone call away, and assuring me that bureaucrats "never refused" his demands.[11] In short, Arun's authority is enforced at the barrel of a gun. Leveraging fear and intimidation allows criminal politicians to exploit extralegal measures to aid supporters and court new voters.

1.2.3 Networks

> "You must be my lifeline in the village" - Times of India Reporter regarding voters views on effective politicians.

Voters may rely on prior demonstrations of constituency and personal problem solving as a strong indicator of candidates' likely behavior in office. In order to build up local popularity candidates can invest in constituency service, which requires access to money and dense, local networks. Further, to reach a broad swath of voters candidates' networks should consist of contacts among local bureaucrats, leaders and politicians.

To put a finer point on the argument, criminality aids networks in three primary ways. First, criminality places an emphasis on trust and loyalty that may be more readily provided by family, kinship and locality ties rather than relying on caste (Michelutti, 2019). The illicit nature of illegal economies may therefore strengthen network bonds beyond concerns of caste based voting. For

[11] I give additional examples when describing Arun's power in Chapters 3 and 4.

example, Arun Yadav campaigned hard to have his relative installed as the Block Pramukh, a key local political position, in order to strengthen his grip over village politics in a core sand extraction area (sand mining representing the cornerstone of his illegal enterprise).[12] Arun went to great lengths to ensure victory, paying for a dummy candidate from the majority Rajput caste to split the Rajput vote and make sure his Yadav relative won the election. By installing his relative as the Block Pramukh, Arun was able to strengthen the direct bond between himself and a key lower level politician that he depends on both for votes and control over his sand mining operations. At the same time, as the president of the Panchayat Samiitti, a strong Pramukh controls a majority of block resources (fieldwork interviews July 2017).

Second, criminality fosters political networks by aligning the economic incentives of politicians, bureaucrats and voters. Criminal networks generate employment and kickbacks that bind voters, bureaucrats and local politicians to the criminal boss. Sand mining is by far the largest economic activity in Arun Yadav's constituency of Sandesh. As the major economic activity in the area it generates employment for voters and kickbacks to bureaucrats and politicians. A village president in Sandesh explained that his village favored Arun Yadav due to the economic benefits from sand trucking. In order to access the river, the sand syndicates' trucks would pay a toll to cross villagers' land. As compensation, households could make between Rs 140-200+ per truck load (Interview 2017). Police and the District Magistrate were well aware of this illegal sand extraction and corrupt counterparts in the business (Interview 2017).[13] In essence, these networks produce a self reinforcing equilibrium where locals are dependent on the criminal MLA for economic benefits and the MLA requires the votes, silence and compliance of villages to secure their illegal enterprise. In other words, the networks are bound by a reciprocal need for protection between patrons and clients.

Third, by dominating the local illegal economy, criminals can remain rooted in their com-

[12] The Pramukh is a block level politician responsible for development and oversees the Panchayat Samitti council.

[13] For similar anecdotes outside of Bihar consider an investigation of the Sand and Oil mafia in North India which revealed that "the highest single cost component in this business consists of the 'protection' fees. It is this extorted money that goes to feed 'the big mafia' coffers and directly and indirectly sustains local political machines."(Michelutti, 2019)

munity. In turn, this facilitates the development of politicians local bonafides via investments in face-to-face network building. Beyond the provision of patronage, criminal politicians are seen as "sons of the soil' (Vaishnav 2017). They speak the local language, address the local people, and perhaps most importantly, take the time to sit with voters and connect.[14] Put differently, to strengthen network ties, politicians must demonstrate more than munificence during campaigns. Importantly, developing deep-rooted and reciprocal networks requires personal investments over a prolonged period. Voters may discount politicians who parachute in to give handouts and attend rallies. As one reporter put it, voters rightly question, "how much money can [they] give me for life?" Whereas, "sharing meals together shows I'm close to you, I'm dear to you."

Ritualized, routine and repeated connection with local voters, builds a networks' connective tissue over time. In turn, criminal politicians can call on this personal vote to help protect them from authorities, win a party nomination and eventually an election (Times of India Reporter Interview in Patna, 2018). This resonates with recent revisions in the clientelism literature emphasizing politicians doling out benefits to build credibility and bolster their reputations (Hicken and Nathan, 2020). In essence, delivering services to constituents can buy candidates a seat at the electoral table and voters have come to expect it. Those who fail in this core area of service work hardly stand a chance come election time (Bussell (2019), fieldwork).[15] Here, criminal politicians who dominate local economies have an advantage in cultivating deep-rooted and durable networks. Control over the local illegal economy enables criminal politicians to repeatedly invest in reciprocal protection networks while also aligning voters economic incentives with their own.

[14] Arun Yadav is particularly proud of his local bonafides. He brags about speaking only Bhojpuri in legislative sessions despite hardly any of the other legislators being able to understand him. Needless to say this is more parochially performative than helpful in passing legislation.

[15] Interviewees consistently emphasized the importance of social work and village connectivity as core assets of successful politicians. They also highlighted, caste, party and money as important aspects of successful MLAs, but social work was a near universal requirement. This "social work," consists of face-to-face meetings, repeated village visits, empathizing with voters problems and then having the power to solve them.

1.3 Research Design and Chapter Overviews

To determine if criminal politicians translate their assets of money, muscle and networks into superior social welfare delivery, I construct and combine three original datasets.[16] To measure criminality, I scraped self-disclosed affidavits listing 87,000 candidates' criminal charges. Specifically, the dataset details the criminal histories, wealth, and electoral results of all state legislative candidates in India between 2003 and 2017 (N = 87,000). To measure criminal politicians' in-office performance, I combine the candidate dataset with original data on the geo-locations of over 20 million NREGS local public works projects.

1.3.1 Chapter 2: Do Criminals Deliver NREGS?

In Chapter 2, I leverage this data to compare the constituency-wide distribution of NREGS benefits between criminal and clean MLAs. Using project geo-coordinates I map all 20 million NREGS infrastructure works to state assembly constituencies. Each project contains information on the number of workdays generated, wages paid and total cost. In this first empirical chapter, I ask if criminal politicians improve or hinder the provision of India's National Rural Employment Guarantee Scheme overall? NREGS provides guaranteed jobs for local infrastructure improvement (e.g. roads and irrigation) and represents a huge portion of government spending.[17] Given its size, politicians are keen to exert control over NREGS distribution. Recent survey evidence suggests that voters think criminal politicians can "get things done" and are willing to vote for criminals if it means increased benefits (Vaishnav 2015).

However, simply comparing NREGS delivery between criminal and clean constituencies would likely result in a biased estimate of the effect of MLA criminality on NREGS outcomes. For example, if there is a greater supply of criminal candidates in areas with poor functioning bureaucracies,

[16] I provide greater detail on dataset construction and sources in Chapters 2 and 3. These chapters and their appendices explain the multiple sub-sources comprising each of the three primary datasets and the merging process.

[17] In some states NREGS funds are 20 times the size of state legislators personal development funds (Gulzaar and Pasquale 2017).

a straightforward comparison might underestimate the effect of criminal governance on NREGS provision. On the other hand, if criminals buy their way into safe seats- with high performing local bureaucracies- this would upwardly bias the effect of criminality. To overcome potential confounding and endogeneity concerns I employ a regression discontinuity design. This estimation strategy compares NREGS outcomes in constituencies with "knife-edge" races between criminal and clean candidates. In other words, the "assignment" of criminal politicians to a constituency can be considered "as-if-random," allowing for a precise causal estimate of the impact of criminality on NREGS delivery in legislative constituencies. Overall, I find that criminal politicians complete 34% fewer NREGS infrastructure projects during their terms. However, the estimates for wages, employment and project funds are imprecise and results remain inconclusive. In short, these findings point to differential distributional strategies based on politicians' criminality.

Second, I consider an alternative argument that perhaps criminal politicians deliver more corruption captured by village elites. In turn, village politicians could reward MLAs with political support and/or a share of the rents. To measure corruption I construct a novel, qualitatively informed measure of NREGS projects that facilitate graft. A common method to extract rents from NREGS projects is for local politicians and bureaucrats to overstate the amount of work or material expenditures and pocket the difference. Auditors and project engineers are required to sign off on the total amount of work completed for each project. However, some NREGS projects are more susceptible to this type of fraud. For example, some projects are more durable and visible (e.g. pucca roads) whereas some projects present problems for measuring the total amount of work conducted (e.g. extending a water catchement pond). This creates variation in engineers ability to verify the true amount of work completed on a project. I leverage this variation to test whether criminal constituencies authorize projects more amenable to corruption. However, at least for the RD sample where political competition is high, I find no evidence that constituencies governed by criminal politicians are more likely to engage in graft prone NREGS projects.

These negative and null results for both overall delivery and corruption beg the question of what else can explain criminal politicians' popularity?

1.3.2 Chapter 3: Do Criminals Target Co-partisans?

In Chapter 3, I investigate one potential alternative explanation. Namely, that criminal politicians are better at targeting NREGS benefits to co-partisans. Put differently, while in Chapter 2 I do not find evidence that criminal politicians increase the size of the NREGS pie, they may more efficiently distribute slices to supporters. Previous qualitative studies suggest that criminal politicians' comparative advantage rests on protecting and providing for their *own* communities (Vaishnav, 2017; Hansen, 2005; Berenschot, 2011b; Michelutti, 2019). If criminals do not deliver benefits in the aggregate, perhaps they prioritize their in-group. Since MLAs require only a bare plurality of votes to win, targeting supporters could provide one efficient path to victory.

In this empirical chapter, I build on the clientelism literature to investigate whether criminal or clean Members of the Legislative Assembly (MLAs) reward local voting strongholds with NREGS resources. In fact, a burgeoning literature on criminal politicians in India analyzes the welfare and in-office effects of constituencies governed by criminal politicians (Chemin (2012), Prakash et al. (2019), Asher and Novosad (2016)). This chapter moves beyond comparisons of criminals' aggregate welfare effects *across* constituencies to consider micro-targeting *within* constituencies. Specifically, I map the location of millions of NREGS projects to micro-pockets of political support, estimated from the results of over 120,000 polling stations. By matching NREGS projects to polling station returns, I precisely compare whether criminal or clean politicians are more efficient in rewarding their supporters. Not only is NREGS a vital anti-poverty program, but state legislators control several influential distribution levers. Thus, I provide a quantitative test that hews closely to the theoretical expectations of why criminal politicians are viewed as effective governors, in a setting where they can demonstrate this power.

To the best of my knowledge, my polling station results and locations data is the most comprehensive collection of polling station geo-data for India's legislative assemblies.[18] In total, I include

[18]Raphael Susewind maintains an incredible data repository on polling station geo-data in India. How-

six Indian states and nine state elections.[19]. The polling station dataset covers 494 million citizens (36% of India's population).[20]

Theoretically, given criminal politicians' close communal ties, they should be better situated to understand, and then meet, constituents needs. I find a positive association between criminality and NREGS provisions- both for benefit delivery to villages overall, and the targeting of core support villages. In competitive polling stations, I find that criminals are associated with delivering 25% more NREGS resources relative to clean MLAs. However, this increases to 33% for polling stations that strongly support the incumbent MLA, suggesting superior targeting on behalf of criminals.

In the second half of this chapter, I attempt to unpack the mechanism explaining the association between criminality and NREGS delivery. In particular, I test whether criminal politicians resources of money, muscle and networks can explain improved delivery. I find the most support for the muscle and networks resources. Criminals superior NREGS delivery is concentrated among violent politicians, suggesting muscle is an important characteristic. Second, I note that increased targeting is consistent with the networks hypothesis where criminal politicians are able to identify co-partisans, understand and then meet their needs. On the other hand, results are inconsistent with the money mechanism. Using politicians' reported assets, I compare NREGS benefit delivery across the range of MLAs' wealth. Overall, improved NREGS delivery is driven by poorer criminal politicians.

To be fair, these findings are based only on correlations. However, I do undertake several methodological safeguards to minimize specification bias. First, I map projects and polling stations

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ever, most of Dr. Susewind's polling station geo-data is for national parliamentary elections. For more information see Dr. Susewind's github repository `https://github.com/raphael-susewind/india-religion-politics`. To create the polling stations dataset, I scrape, extract and match results from original Form 20 data archived by the Election Commission of India. I detail this process in Chapter 3.

[19] I have comprehensive polling station location and results data for Assam, Himachal Pradesh, Kerala, Tamil Nadu, Uttar Pradesh, West Bengal

[20] The complete polling station dataset includes over 400,000 polling stations. However, I break this data into training and test samples to see if results hold for unseen data. Thus, the core analysis of this chapter is conducted on the 120,000 polling station sub-sample in the training dataset.

to census villages, which allows me to adjust for demographic and development differences at an extremely local level. Simple differences in NREGS demand should not explain why criminals target co-partisans at higher rates. Second, I use Kernel Regularized Least Squares to regularize estimates while allowing the model to flexibly fit the data, reducing the chances of misspecification likely to occur under OLS. Third, given the large dataset, I split my sample into training and test sets leaving one half of the data untouched. In essence this allows me to perform a self replication. I find that criminals continue to target co-partisans at higher rates when re-estimating the model on unseen holdout data. Nevertheless, I can still not definitively rule out that some unobserved confounder may be driving the result.

Still, this analysis is useful for addressing some of the theoretical and analytical shortcomings in the regression discontinuity design. First, RD designs only identify a local average treatment effect. In my case, that means I estimate the causal effect of criminality only in highly competitive constituencies featuring an electable criminal and electable clean candidate. Therefore, my findings from the regression discontinuity in Chapter 2 may not generalize to safe seats, where criminal politicians rule the roost. One way to reconcile the discrepancy between the negative RD estimates and the positive association between criminality and NREGS delivery in the targeting chapter, is that this positive association is driven by criminals in safe seats. Though of course, differences in the sample of constituencies between the two chapters could also lead to divergent results.

Second, extremely competitive elections (which the RD analysis requires) conflict with qualitative descriptions of how criminal politicians deliver resources. The canonical representation of criminal politicians describes a don, supplanting the state, and ruling their own fiefdom (Vaishnav, 2012, 2017). In fact, criminal politicians are regularly accused of murdering opposition candidates and party-workers (Vaishnav, 2017). In other words, the setting where criminal politicians are thought to provide for voters is entirely absent from the RD analysis, which is constrained to highly competitive elections. In this chapter, I complement the regression discontinuity analysis by providing insights for safe seats where we might expect, and I find, criminal politicians to be more effective.

1.3.3 Chapter 4: Do Criminals Deliver Constituency Service?

Do criminal politicians deliver benefits outside of government sanctioned welfare programs? And, if so, is this really what drives criminals electoral success? NREGS (while vastly important) is just one program among an array of potential resources under politicians' control. In Chapter 4, I ask if criminal politicians provide alternative forms of constituency service outside of formal government channels.

My final chapter provides an alternative explanation for criminal candidates' continued success. Based on 12 months of qualitative fieldwork (interviews with criminal and non-criminal politicians, local elites, voters and party-workers), I argue that criminal politicians cultivate superior communal bonafides by investing in *personalized constituency service*. This "social work," (as it was referred to by many interviewees) consists of face-to-face meetings, repeated village visits, empathizing with voters' problems and then having the power to solve them. I argue that criminal politicians are better positioned to invest in these forms of social work because they dominate the local, illegal and legal economy through coercive force. In turn, criminals can remain rooted in the community while still acquiring the necessary capital to credibly contest elections. One way politicians signal their communal credentials is by routinely showing up at weddings, often providing large cash gifts and enhancing the status of the wedding celebration (fieldwork and Rao (2001)). Here, cash acts not as an instrument for vote buying but as a continuing lubricant of pre-existing social ties (Bjorkman 2014). Conversely, voters may discount the generosity of helicopter drops from candidates who primarily show up during campaigns, reasoning these politicians will be unavailable after votes are tallied. Thus, I expect criminal politicians to be electorally rewarded when given ample opportunities to remind voters of their continued communal ties.

As a proxy test for this argument, I exploit variation in the demand for weddings based on the Hindu wedding calendar. The Hindu religious period of *Chaturmas* runs roughly from July to October. Very few Hindu weddings take place at this time as it is devoted to austerity, fasting and penance (Gupte, 1994). Since the timing of state elections is unrelated to the Hindu wedding calendar, campaigns running from July to October realize an exogenous decrease in the demand for candidates to pay dowry expenses and attend weddings. On the other hand, elections that take place during Hindu wedding season should provide more opportunities for candidates to demonstrate their community bonafides. With more wedding ceremonies, criminals can consistently remind voters of their strong community ties and deep pockets. I find that criminal politicians are less likely to win when elections coincide with Chaturmas relative to elections held during "wedding season."

1.4 Why NREGS and MLAs?

Before presenting my chapters, I provide additional motivation for selecting NREGS as my primary dependent variable and background information on how the program operates. NREGS provides a useful setting to test criminal politicians distributive strategies for several reasons. First, the program can be politicized and manipulated by Members of the Legislative Assembly. Despite ostensibly being a demand driven program, there is consistent evidence of NREGS rationing (Sukhtankar et al., 2016), with the degree of rationing varying across states (Imbert and Papp, 2011). NREGS has been found to be highly politicized with resources distributed via partisan channels (Dunning and Nilekani (2013), Dasgupta (2016), Das (2015)).

Second, NREGS provides granular and geotagged administrative project data, enabling a systematic comparison of targeting by state legislators within constituencies, across India. Leveraging the geo-locations of individual NREGS projects I map program benefits to polling stations measures of political support. In turn, this aligns NREGS benefits with the relevant political unit MLAs would exert effort to target (i.e. the polling station and attendant villages). Previous studies of criminal politicians could not examine differential targeting due to outcomes measured at the constituency level.

Finally, NREGS is a valuable program in and of itself, which makes it both politically and practically important. NREGA "serves more people than an other anti-poverty program in the world" (Manor 2014). At the programs peak in 2010, total government outlays reached over 250 billion rupees per year, or about 0.33% of India's GDP (Sukhtankar et al., 2016). To put it mildly, NREGS is a massive welfare program relative to both development initiatives around the world and other programs in India. Voters also care deeply about the programs benefits. Voters list employment and development near the top of their concerns in public opinion surveys (Lokniti 2014). Similarly, NREGS size makes it a crucial funding vehicle for both village and state level politicians. NREGS represents 80-95% of Gram Panchayat spending and thus is a key development and political tool for village presidents (Dey and Sen, 2017).[21] Constituency-wide spending far outpaces even MLAs' personal development funds (MLALADs) (Gulzar and Pasquale, 2017). At the same time, MLAs attempt to grab control of NREGS fundings both to cash in and maintain control over village politicinas (Jenkins and Manor, 2017).

The fiscal incentives for both voters and politicians are clear. Given its sheer size and political importance, NREGA serves as a crucial point of service for politicians to bolster their reputations as critical problem solvers and engage in partisan targeting. In short, NREGS serves as a backdrop to systematically test a "most-likely" scenario of criminal MLAs delivering state resources to their supporters.

1.4.1 NREGS Background and Political Interference

In 2005 the Indian parliament passed the National Rural Employment Guarantee Act (NREGA). The act guaranteed 100 days of manual labor employment, to all rural households, at a state mandated minimum wage. The scheme (NREGS) officially began in February 2006, initially rolling out to 200 of the most underserved districts in India, with the remaining rural districts covered by 2008 (Dey and Sen, 2017; Sukhtankar et al., 2016). NREGS employs laborers to build village-level assets, including irrigation canals, dirt roads, water catchments, ponds and even latrines.

After obtaining a job-card, any rural household is eligible for yearly allotment of 100 days of

[21] Village presidents are sometimes referred to as Mukhiyas or Pradhans, depending on the state.

employment. Ostensibly, villages hold "all-hands," gram-panchayat meetings to generate lists of preferred NREGS projects and laborers interested in employment under the scheme. In reality, political considerations and lack of fund disbursement from the central government induce supply-side constraints (Imbert and Papp, 2011; Jenkins and Manor, 2017; Gulzar and Pasquale, 2017; Sukhtankar et al., 2016).[22] Villages preferred projects are passed up the Indian bureaucratic chain for approval. Once a project is greenlit by the block, district and state bureaucracy, 60 percent of project funds are legally required to be paid to laborers. Material expenditures are capped at 40% of the allocated budget, in part to reduce rents from leaking out to contractors and local elites. While rural employment is the primary goal of NREGS, a second aim is the creation of village infrastructure. In this sense NREGS projects are best thought of as local public works that also enable individual targeting via project employment. In some circumstances, infrastructure benefits may accrue primarily to local elites since projects can be placed close to their households (**?**). Still, all else equal, improving local irrigation or roads is more likely to aid households in the village where the asset is located rather than villages farther away (Andrew Harris and Posner, 2019).

Despite being demand driven, rationing of project funds and employment leaves space for politicians to mediate NREGS access (Dutta et al., 2014; Dunning and Nilekani, 2013). Typically, rural citizens levy employment claims via local village politicians, though they may also demand access directly from MLAs (Bussell, 2019). Village level institutions remained largely neutered until the large influx of cash made available by NREGA. With 50-90% of NREGA funds funneled through local panchayat councils, NREGA greatly empowered village level politicians (Manor, 2013) Given this devolution of power, local village presidents became increasingly important to NREGA access and implementation because of the huge amount of funding now under their purview.[23]

[22]The central government guarantees all labor payments but only pays for 75% of material costs, with states contributing the remaining funds.

[23]Local village council presidents hold both formal and informal power over the distribution of NREGA benefits. Indeed, (Jeong et al., 2018) leverage close village elections to show that village presidents control over NREGS distribution results in greate wages and employment allocated to president's relatives.

Nevertheless, despite NREGS funding and implementation shifting to the village, MLAs can intervene to control program outcomes. First, MLAs sit on the block council, a middle tier of the Indian administration. Blocks bureaucrats have veto power over project approval, making the block the first hurdle in getting NREGS off the ground (Jenkins and Manor 2017). Informally, MLAs may pressure block development officers responsible for project approval to fast-track funding for their preferred projects (Marcesse, 2018). Village politicians may therefore require the aid of MLAs to get their NREGS project off the ground. Finally, MLAs serve as a key intermediary between citizens and the state facilitating access to development programs like NREGS (Kruks-Wisner (2012), Bussell (2019)). By focusing on NREGS and MLAs I am able to compare delivery of state resources for a program where MLAs control several influential levers, voters care deeply about outcomes and the consequences of controlling program funds can be instrumental for re-elections.

1.5 Contributions

In addition to original data collection and inferences, this dissertation contributes to literatures on criminal politicians, clientelism and distributive politics. My dissertation builds most directly from Vaishnav's (2012, 2017) seminal work on criminal politicians in India. Vaishnav and Hansen (2005) highlight money and muscle as core resources at criminal politicians' disposal.[24] Previous studies have theorized that criminal politicians are elected because they act as safety nets and redistribute resources to their supporters (Vaishnav 2012, Vaishnav 2017, Hansen 2005, Berenschot (2011b)). While bringing rich qualitative evidence to this questions, these studies fail to ascertain if criminal politicians are systematically better at connecting supporters with core state resources.[25]

[24] Vaishnav's argument draws inspiration from several other qualitative works including but not limited to Witsoe (2012), Berenschott (2011), and Michelutti (2014).

[25] In addition, I add to Vaishnav's scholarship in several key ways. Vaishnav's (2012, 2017) quantitative studies focus on candidate selection and parties' preferences for criminal politicians. In other words, Vaishnav emphasizes the role of criminal politicians' prior to election. Whereas, I investigate post-election outcomes and how money and muscle aid criminal politicians while governing in office. Second my argument stresses the importance of the *source* and *use* of candidate wealth beyond buying party tickets and financing expensive electoral campaigns (Vaishnav 2017). I argue, money and muscle serve as critical inputs into developing durable and deep-rooted political networks that are

Alternatively, studies that attempt to quantify the causal effect of criminality on welfare look only at constituency wide outcomes and typically consider broad measures of welfare or public goods (Chemin (2012), Prakash et al. (2019), Chapter 2). These RDD studies thus fail to consider whether criminal politicians are better at redistributing resources to their in-group, which the qualitative literature suggests is criminal politicians true comparative advantage. Secondly, these quantitative studies focus on broad measures of welfare or public road provision that MLAs may lack fine control over. In other words, their is a misalignment between the qualitatively generated theory of criminals connecting core supporters to resources and the quantitative tests to date.

To overcome these challenges, I develop a granular dataset that enables a direct comparison between how criminal and clean politicians reward their supporters with access to one of India's largest anti-poverty programs. In addition, I take seriously the theoretical predictions of criminal politicians advantage in money and muscle. I provide direct quantitative tests investigating how each mechanism may independently aid in delivering state resources to voters.

More broadly, my work is indebted and connected to the distributive politics' literature. Political scientists have continuously argued over the determinants of politicians' distributive strategies (Kitschelt and Wilkinson, 2007; Posner, 2005). Do politicians target co-ethnics or co-partisans? Do they condition benefits on political support, or distribute goods in a non-contingent manner to bolster their reputation (Bussell, 2019; Hicken and Nathan, 2020)? In Golden and Min's (2013) taxonomy my dissertation speaks most directly to studies on government responsiveness and patterns of political favoritism. Broadly, I contribute to this distributive politics literature illustrating how candidates' background characteristics and personal resources can influence clientelistic strategies. Not all candidates are created equal. In other words, the ability to engage in strategic clientelism may depend on politicians ex-ante characteristics and could explain variation in programmatic performance within the same institutional setting. While previous studies have em-

core to criminal politicians' electoral success. While discussion of how criminals provide constituency service is not absent from Vaishnav's work, how networks operate and quantitative tests of criminals' personal resources are novel to my dissertation.

phasized politicians' demographics such as gender (e.g. Chattopadhyay and Duflo, 2004; Brollo and Troiano, 2016) or caste (Dunning and Nilekani, 2013; Gulzar et al., 2020), I expand the pool of candidate characteristics to consider how personal resources- such as a politicians' source of wealth- can alter patterns of clientelistic distribution.

More narrowly within the distributive politics literature, I add to the growing analysis of NREGS. In particular I consider how clientelism and partisan targeting may influence program delivery. Several studies have found that politicians target NREGS to co-partisans (Dunning and Nilekani (2013), Dasgupta (2016), Das (2015)). However, even within the same ruling party and same institutional contexts MLAs bring different assets that alter their personal targeting calculus. For example, politicians must have the ability to identify supporters and influence the bureaucracy where their supporters are located (e.g. targeting depends on pre-existing networks). This expands on prior work that considers how partisan alignment of MLAs with the ruling party (Das and Maiorano (2019b), Dasgupta (2016)) or individuals with village presidents (Jeong et al. (2018), Das (2015)) increase access to NREGS benefits. Here, I focus on MLAs as a conduit that links both state level party strategies and village level decisions of allocation. At the same time, using original data on polling station results, I am able drill down analyzing how individual characteristics of MLAs influence targeting within constituencies.

My results add to emerging research that higher-level politicians are more likely to target local group benefits (Busell 2019), especially when co-partisans are geographically concentrated, minimizing spillover (Andrew Harris and Posner, 2019). In these environments targeting returns more bang for your buck, by maximizing partisans reached while minimizing leakage to opposition voters. Since NREGS project function as village public goods with limited spillover, it is more likely that MLAs would exert effort in targeting projects to core areas based on partisan alignments, where returns could be larger and partisanship more observable (polling stations release results). Whereas, (Busell 2019) finds MLAs and MPs do not condition individual constituency service on partisanship (e.g. helping constituents access job cards or gain employment via NREGA). In part, because individual partisanship is unobservable (Bussell 2019, Schneider (2019)) and it may be more effective to burnish a reputation as an effective problem solver. Similarly, targeting partisans is more likely when they are geographically concentrated so that politicians efforts does not spillover to opposition supporters (Andrew Harris and Posner, 2019).

These contributions are made possible by combining original geo-data on both NREGS outcomes and polling station results. Previous studies focused on NREGS distribution have been limited to survey measures of individual targeting (**?**Dasgupta, 2016), do not measure partisanship (Niehaus and Sukhtankar (2013), Jeong et al. (2018)) or aggregate to a higher level of government (e.g. constituency Das and Maiorano (2019); or block Gupta and Mukhopadhyay (2016), Gulzar and Pasquale (2017)). I improve upon the NREGS targeting literature by mapping the desired political benefit to a relevant level of political support for MLAs. Second, most NREGS studies focused on targeting are limited to a single state. However, heterogeneity in program outcomes is a fundamental fact of NREGA distribution (Imbert and Papp 2011, Sukhtankar et al. (2016)).[26]

In short, my dissertation represents the first study of how criminally accused politicians fare in relaying social welfare benefits to their constituents and helps clarify a puzzling and troubling trend in Indian politics. At the same time, I contribute to our understanding of clientelism, the delivery of anti-poverty programs, and why violent politicians win elections.

[26]Studies of NREGS partisan targeting have found limited evidence of targeting in Andhra Pradesh (Das and Maiorano (2019), Sheahan et al. (2016)) but consistent evidence of partisan preference in Rajasthan and West Bengal (Das, 2015).

Chapter 2

Do Criminal Politicians Deliver? Evidence from NREGS

2.1 Introduction

Despite high levels of inter-party competition, criminals routinely win elections across India. In fact, several scholars have demonstrated that criminal candidates are actually "rewarded" by voters for their checkered past (Aidt et al. 2011, Vaishnav 2012, Dutta and Gupta 2014).[1] Over the past decades, India has witnessed a rise in criminal politicians at both the national and state level. More than 40 percent of current Members of Parliament faced criminal charges during the 2019 elections, up from 24 percent in 2004 (ADR, 2019). The problem is particularly acute in several Indian states. In 2010, 58% of Bihar's Members of the Legislative Assembly (MLAs) faced criminal charges, with 34% of these charges considered "serious" (e.g. murder, kidnapping, extortion, theft-related, etc.). In short, this troubling trend cuts across party and state lines, continuing to corrode Indian politics (Vaishnav 2017).

Why do voters elect politicians with a criminal record? One common explanation cites criminals' ruthless ability to solve voters' everyday problems. Recent survey evidence suggests that voters think criminal politicians "can get things done" and are willing to vote for candidates facing criminal charges if it means increased benefits (Vaishnav, 2015).[2] Similarly, qualitative accounts

[1] Vaishnav (2012) finds that politicians facing criminal charges are elected at higher rates to the Lok Sabha (the lower house of India's national parliament) and state legislatures, relative to candidates with no criminal history.

[2] The literature on corrupt politicians raises a similar "trade-off" argument, claiming that voters may be willing to ignore self-dealing if politicians are capable of delivering public or private goods (Boas et al., 2019; Winters and

across India, claim voters view criminal politicians as effective strongmen capable of delivering state resources where others have failed (Witsoe, 2013; Martin and Michelutti, 2017; Berenschot, 2011a,b; Vaishnav, 2017).

When politicians and middlemen heavily mediate access to state benefits, candidates often need to prove their capacity to solve constituent problems and get work done before taking office (Kruks-Wisner, 2015; Berenschot, 2015). If criminals are uniquely suited to substitute for the state, then this may explain their sustained and increasing success. For example, Pappu Yadav, a mafioso MP from Bihar, raided health clinics demanding doctors lower fees for his constituents (Jha, 2014). Nearby, Anant Singh, affectionately referred to as "Chotte Sarkar" (or Little Government), runs a parallel state from his own deep pockets (Tewary, 2019).

Are criminal politicians merely misunderstood robin hoods? Do they systematically deliver more resources to their constituents? In this paper, I test whether criminal politicians help or hinder the delivery of India's National Rural Employment Guarantee Scheme (NREGS). NREGS is the world's largest workfare program, guaranteeing 100 days of paid labor to all rural Indian households. At its peak, the program employed upwards of 50 million people per year, generating over $US 6 billion in central government expenditures (Gulzar and Pasquale 2017). In addition to employment generation, NREGS aims to improve village infrastructure (e.g., roads and irrigation). To date, over 30 million local infrastructure projects have been completed under the scheme. Given NREGS size, politicians are keen to exert control over distribution (Maiorono 2014, Marcesse 2018). At the same time, citizens care deeply about employment and demand access to the program from politicians (Marcesse 2018). Thus, if criminal politicians connect constituents to state resources, NREGS provides a viable and valuable conduit. In short, if voters elect criminally accused candidates because they can "get things done," then we might expect discrepancies between charged and clean candidates in the implementation and execution of one of India's largest social welfare programs.

Weitz-Shapiro, 2013).

Using a regression discontinuity design, I estimate the causal impact of electing a criminally accused politician on the distribution of NREGS projects and program employment. I find that constituencies governed by criminal politicians complete 34% fewer local infrastructure projects. On average, this translates to nearly 475 fewer roads, irrigation improvements, or other works implemented during a criminal politicians' tenure. A back of the envelope calculation suggests that these missing projects would have resulted in an additional 29 million rupees in wage payments across criminal constituencies.[3] Similarly, I find adverse effects of criminality on employment and material expenditures. However, these estimates are more imprecisely measured and model dependent. Beyond the welfare loss, this finding illustrates how politicians' personal backgrounds can shape public service delivery and challenges prevailing explanations that criminal politicians are elected because they "get things done."

Second, I test an alternative explanation of criminal candidates' popularity. Namely, that criminal politicians facilitate corruption, enriching local elites in exchange for votes. I find no evidence that constituencies run by criminal politicians engage in significantly more NREGS corruption. To test this alternative channel, I develop a qualitatively informed measure of corruption based on interviews with contractors and bureaucrats engaged in NREGS malfeasance. The key insight is that specific projects are more amenable to corruption. However, constituencies run by criminal politicians are not more likely to undertake corruptible projects. While I estimate negative effects of criminality on other, potential indicators of fraud, such as excess expenditures on labor and materials, these results are imprecise and inconclusive. Overall, I find little evidence that criminal politicians facilitate more corruption than clean politicians.

To determine if criminal politicians deliver, I draw on several originally collected administrative datasets. In order to run for office, candidates for Members of the Legislative Assembly must

[3] The back of the envelope calculation is based on average project wage payments of 61,538 Rs x 475 projects = 29,230,895 Rs. Average project wages are calculated using the training sub-sample of projects from Chatper 3. In addition, I am not able to precisely estimate a causal effect of criminality on lost wages. Therefore it is possible that criminal MLAs do not reduce wages compared to clean politicians. However, as the back of the envelope calculation suggests, if criminals' completed projects at the same rate as clean politicians, wage payments in criminal constituencies would be much higher.

submit affidavits containing personal details. I leverage these disclosures to assemble the criminal histories, wealth, and identifying characteristics of all Members of the Legislative Assembly in India from 2003-2016. Using probabilistic record linkage, I match the affidavit data to electoral results. In turn, the linked datasets allow the identification of bare criminal winners and losers necessary to estimate the regression discontinuity. Finally, to measure criminal politicians' performance in delivering state resources, I combine the candidate dataset with original data on the geo-locations of over 20 million local public works projects from the National Rural Employment Guarantee Scheme.

Using NREGS data provides several advantages over previous work. First, it allows a closer alignment between measurement and theory. Given that NREGS is a program voters care about and politicians' control, this paper represents a direct test of whether criminals deliver superior access to state resources. Previous studies using a similar regression discontinuity design, report criminal politicians reduce overall economic development (Chemin 2012, Prakash et al. 2016). However, these investigations lack a direct link between what voters expect Members of the Legislative Assembly to influence and outcomes measured.

Theoretically, if voters judge politicians on their ability to mediate access to state resources, then it is necessary to examine distributional differences of vital social services between criminal and clean politicians. In some respects, NREGS represents an ideal test of criminal politicians' ability to deliver state resources. The program is visible, manipulable, and provides a source of credit claiming for politicians (Muralidharan et al., 2016; Gulzar and Pasquale, 2017; Banerjee et al., 2016). NREGS also represents a large pot of cash that politicians can potentially control in their constituency. For example, in Andhra Pradesh, NREGS expenditures are up to 20 times greater than Members of the Legislative Assembly personal development funds (Gulzar and Pasquale 2017). What's more, voters consistently rank employment among their top concerns (CSDS, 2018).

Additionally, NREGS offers several empirical benefits. The program reports universal, micro-level administrative data on both payment and project completion, across India. In fact, it is one of the only programs where standardized outcomes can be precisely mapped to political constituencies across every Indian state. Studies of distributive politics in India often suffer from a mismatch between programs administered in bureaucratic districts and areas represented by politicians (Golden and Min, 2013). I use newly released, granular, geotagged data on NREGS projects to bypass the mismatch between administrative data and political boundaries. The geolocations allow me to map local public works projects to MLA constituencies, facilitating a direct comparison between criminal and clean politicians in the delivery of a core anti-poverty program.

To buttress my results, I subject the analysis to several stress tests. Criminal politicians consistently complete fewer NREGS projects across various specifications, functional forms and alternative bandwidths.[4] Secondly, I restrict the definition of criminality to serious charges. These charges are harder to fabricate and carry severe sentences, which should strengthen the alignment between charges and latent criminal ability (Vaishnav 2015). When examining serious charges, the deleterious effects of criminality tend to increase, or at least, remain consistent with the initial results. This robustness check increases confidence that criminal charges are measuring the desired traits of criminality. Finally, I conduct a series of placebo tests. As is common in the RD literature, I test placebo cutoffs far from the threshold of bare criminal winners and losers. I do not find a significant difference in the number of NREGS projects completed at these placebo cutoffs. In sum, I find little to suggest that criminal politicians are better at delivering a crucial government program for their constituents.

2.2 Criminal Politicians and Service Delivery in India

Several qualitative works have noted that criminal politicians possess several tools that make them uniquely suited (relative to non-criminal politicians) to deliver targeted benefits and win elections. Largely, these advantages can be grouped under Money, Muscle, and Networks. Put simply, money

[4] see Appendix 2.B for sensitivity Analysis.

helps charged candidates contest increasingly expensive campaigns and develop a block of loyal voters via direct transfers. Muscle is multifaceted in its applications. On the one hand, muscle imbues criminal politicians with both the ability and reputation of being able to "get things done." Criminal politicians can use this muscle power to intimidate bureaucrats into diverting benefit flows to their voting blocks or protect their favored constituents from extortion at the hands of the bureaucracy, police or other criminal cadres (Martin and Michelutti, 2017). On the other, muscle can be used to intimidate these same voters and suppress turnout (Witsoe, 2012; Vaishnav, 2017). Finally, money and muscle are hardly sufficient without networks of organized, loyal subordinates who can act as political brokers and vote mobilizers, projecting criminals' money and muscle-power across electoral constituencies (Berenschot 2012).

Similarly, the lack of programmatic service delivery may aid the election of accused candidates. Vaishnav (2017) points to charged politicians' ability to act as "community warriors" protecting parochial communities' interests, especially when local ethnic divisions are salient. When (legal) economic opportunities are limited, and the rule of law is unequally applied, criminal candidates may gain advantages in funding and local network building that serve as critical inputs to benefit delivery. In this setting, criminally charged candidates may be ideally suited to meet constituent needs via targeted service delivery.

On the other hand, recent empirical evidence finds that the election of criminally charged candidates leads to negative outcomes for the constituency. This paper is most similar to work by previous scholars that found criminal politicians reduce monthly per capita expenditure among scheduled castes, scheduled tribes or other backward classes (Chemin 2012)[5] and lower overall economic activity (Prakash et al. 2016). Criminally charged politicians may also be less interested

[5]Chemin finds that electing a criminal MLA or MP lowers expenditure of this marginalized groups by 19 percent. However, constituencies do not map perfectly into districts the level at which per capita expenditure is measured at in National Sample Surveys. Thus, it is possible that the results in Chemin 2012 are subject to an ecological fallacy. Secondly, outcomes on expenditure are measured in 2005, and criminal status is measured using election results from 2004 . Given that the National Sample Survey asks respondents to recall consumption 6 months prior for some measures, it could be that politicians have little impact early in their term or may not have taken office when expenditure was measured.

or capable of interfacing with the legislature and bureaucracy in order to procure state resources. For example, Members of Parliament facing serious criminal charges are less likely to attend legislative sessions relative to those not facing serious charges (Sircar, 2018b).[6]

How do we reconcile these adverse aggregate outcomes with voters' willingness to elect criminal politicians and survey evidence indicating that citizens favor criminally accused candidates if they deliver benefits (Vaishnav 2015)? One possibility may be that criminal politicians engage in more strategic clientelism, looking to solve local citizens' problems in order to claim credit and strengthen clientelistic relationships. Given that access to state resources can require the mediation of local politicians,[7] we might expect that criminally accused politicians focus their efforts on targeted programs like India's National Rural Employment Guarantee (NREGS), even if these same politicians harm overall welfare in their constituency. In fact, Gulzar and Pasquale (2017) finds improved NREGS provision when MLAs can claim credit for their efforts.

However, my findings suggest that criminal politicians do not deliver by improving benefit delivery, at least when it comes to NREGS. In this sense, my results are more consistent with the literature that finds deleterious effects of criminal politicians. I add to this literature by studying NREGS implementation, a program that politicians can manipulate, and voters expect them to deliver. In this way, I provide a concrete test of whether or not criminal politicians connect constituents to state resources, which the qualitative literature suggests undergirds criminal politicians' electoral success.

2.3 Research Design

Constituencies that elect criminally charged politicians may differ in observable and unobservable ways from constituencies that elect clean candidates. The criminal status of an MLA may, there-

[6]One MLA I interviewed speaks only Bhojpuri (the only language he knows) in state legislative sessions. Most other politicians do not speak this regional language. While legislative sessions are supposed to be carried out in Hindi, the use of the local language provided further evidence of the MLAs' community bonafides. However, to paraphrase one interviewee, "how can he get anything done in the legislature?" (Authors' interviews).

[7]For evidence see Kruks-Wisner (2015), Witsoe (2012), and Berenschot (2011)

fore, be endogenous to benefit delivery.[8] In this paper, I use a regression discontinuity design to determine the causal effect of electing a criminally charged candidate on NREGS benefit delivery, in state legislative assembly constituencies, between 2004 and 2016. This estimation strategy compares the delivery of India's National Rural Employment Guarantee Scheme (NREGS) benefits in close elections. That is, I compare constituencies where criminal candidates just barely won, to those where criminal candidates just barely lost, when facing a clean counterpart. As long as legislative assembly candidates are not capable of precisely controlling final vote tallies, the assignment of criminal politicians to a constituency can be considered "as-if-random," at the threshold where the winning candidate changes discontinuously from uncharged to charged. In turn, this allows for a causal estimate of the impact of criminal accusations on NREGS delivery.

To clarify, the regression discontinuity sample compares elections in which one of the top two candidates faced criminal charges while the other candidate was clean. Under this scenario, the margin of victory between criminal and clean candidates (i.e., the forcing variable) deterministically assigns treatment to a given assembly constituency. Treated assembly constituencies are those where the margin of a criminal candidate's victory is greater than 0. Control constituencies are those where a criminal candidate loses to a clean candidate (i.e., the criminal candidate's margin of victory is less than 0) (Prakash et al. 2016). Subsequently, I compare differences in the provision of NREGS benefits between constituencies assigned to treatment and control.

Formally, treated constituencies are determined by the assignment variable *Criminal Margin of Victory (CMV)*, which discontinuously changes from 0 to 1 as CMV crosses the 0 threshold. CMV subtracts a clean candidates' vote share from their criminally accused challenger. Following Prakash et al. 2016, in the baseline specification I estimate the causal effect of criminal accusations

[8]For example, variation in historical colonial institutions could influence the current rule of law, provision of public goods, and the salience of caste relations (Banerjee and Iyer, 2005; Iyer, 2010). The British outsourced colonial rule and tax collection to landed Zamindar classes in some regions and maintained direct control in others. Thus, criminals could flourish where weak institutions in the past led to current deficiencies in the rule of law. These same regions may suffer from a lack of institutional capacity to provide public goods and development resources, causing politicians to focus on the delivery of individualized benefits. Alternatively, criminal politicians could potentially self-select into constituencies with better benefit delivery, paying parties for the privilege of running in these districts.

using a local linear regression that estimates the discontinuity at the CMV threshold:

$$NREGS_{i,s,t} = \alpha_s + \beta_t + \tau Criminal_{i,s,t} + f(CMV_{i,s,t}) + e_{i,s,t}$$
$$\forall CMV_{i,s,t} \in (0 - h, 0 + h) \tag{2.1}$$

Where α_s is the state-level fixed effect and β_t is the election-year fixed effect. $\tau Criminal_{i,s,t}$ is the treatment indicator, $f(CMV_{i,s,t})$ is the forcing variable and $e_{i,s,t}$ represents the error term. h is the bandwidth for close elections around the cut point of 0. In most specifications NREGS outcomes are measured over a politicians' entire term in office, allowing for a short lag immediately after elections.

2.3.1 Population and Sampling Frame

To estimate this equation I combine data from four datasets across India. There are a total of 4,033 legislative assembly constituencies in India.[9] In 2003 the Supreme Court ruled that all parliamentary and legislative candidates would submit sworn affidavits detailing their assets, education and pending criminal chargers. Candidates need only submit charges that had been registered at least 6 months prior to election and where a judge has taken cognizance of the case. In other words accusations represent more than mere mud flinging but indicate that judicial proceedings are underway. Below, I discuss further attempts to address potential politically motivated charges. To compare accused and non-accused MLAs, I consider all state assembly elections between 2004 and 2016. The full dataset includes 4,654 state assembly constituencies and a total of 83,028 candidates competing across 10,222 elections.[10] The RD design compares constituencies where criminally accused candidates barely won to those where the accused candidate barely lost. Therefore, I only consider "mixed" races, where one of the top two candidates faced criminal charges and the other had a clean record. Restricting the analysis to mixed races reduces the sample to 3,149 elections (6,304

[9]There were 4,120 Members of the Legislative MLA constituencies created by the 1976 delimitation, this was reapportioned to 4,033 in the 2008 delimitation.

[10]The dataset begins prior to the 2008 delimitation and therefore includes constituencies both before and after the 2008 delimitation.

candidates).[11] Overall, the full RD sample considers 31% of the elections in the entire dataset.[12] Since the causal effect of "criminality" is identified when the "criminal" treatment discontinuously changes at the 0% threshold for a criminal candidates' margin of victory, I further restrict the sample to consider only "close" elections. Table 2.1 presents the number of mixed elections that fall within a given bandwidth of competition.[13]

Table 2.1: Mixed Election Observations for Varying Bandwidths

Bandwidth	**Election Obs.**
RD Sample	3149
Close +/- 10%	1754
Close +/- 5%	969
Close +/- 1 %	199

2.3.2 NREGS Background and Data

Employment guarantees have a long history in India.[14] The current incarnation of NREGS, enacted in 2005, guarantees rural households 100 days of paid labor per year. Overall, NREGS employs around "50 million households annually" and is the largest workfare program in the world (accounting for about 0.3-0.4 percent of India's GDP) (Mookherjee, 2014). While NREGS is a universal program, laborers are paid the state minimum wage, leading to self-targeting of poorer households. In addition to employment, a secondary goal of the scheme is the creation of village-level infrastructure assets. Projects include road construction, irrigation improvement, and other local public works (mostly concerning water management) (Sukhtankar 2016). While the central government finances NREGS, states are responsible for administration and delivery of funds to beneficiaries. Initial seed money is released from the center to states based on demand from the

[11] 347 elections are dropped because I was unable to match the affidavit for either the winner or runner-up, or the election was uncontested.

[12] However, this only equates to about 7.5 percent of the total candidates, considering MLA elections typically include more than two candidates.

[13] The total number of observations may vary depending on the outcome analyzed. For example averaging NREGS provision over the MLAs entire term versus year over year growth. In most specifications I use the CCT data-driven approach to select an optimal bandwidth (explained below).

[14] For example, the Employment Guarantee Scheme, an early predecessor to NREGS began in 1972 (Puri et al., 2016).

previous fiscal year. To release the next set of funds, the state must demonstrate demand in the form of requested workdays uploaded to the central governments' electronic reporting system (Banerjee et al. 2014). Within states, request for workdays and project funding flow-up the administrative hierarchy (Gram Panchayat $\rightarrow$ Block $\rightarrow$ District $\rightarrow$ State) and funds flow back down (State $\rightarrow$ District $\rightarrow$ Block $\rightarrow$ Gram Panchayat). Gram Panchayats (village-level governing bodies) are responsible for village-level implementation (Banerjee et al. 2014). Finally, funds are released into a bank account or local post office for last mile collection by beneficiaries. Fund leakage can occur at any part of this flow. Similarly, politicians may attempt to influence allocation decisions by pressuring bureaucrats at multiple points in the administrative chain.

Ten years after implementation, there remains substantial variation in NREGS quality and access (Sukhtankar 2016). Despite the universal guarantee, certain states are considered "star performers,"[15] while others lag behind (e.g. poorer states like Bihar, Uttar Pradesh and Jharkhand). Undoubtedly, some of the state-level variation in implementation results from differences in demand for NREGS employment. However, poorer states, are actually some of the worst implementers, failing to provide requested work (Dutta, 2012). Unmet demand tends to cluster among these poorer states, exactly where demand is highest. Partially, this reflects low bureaucratic and fiscal capacity. At the same time, numerous studies document high levels of leakages (Imbert and Papp 2011, Muralidharan et al. 2016, Niehaus and Sukhtankar 2013, Banerjee et al. 2014).[16] Mounting empirical evidence suggests NREGS is hardly programmatic in its application but instead serves political ends (Dasgupta 2106, Gulzar and Pasquale 2017).[17]

How do politicians interfere in this ostensibly demand driven, universal program operated by the bureaucracy? Politicians (particularly MLAs) often act as intermediaries solving everyday problems for their constituents (Kruks-Wisner 2015, Witsoe 2012 and 2013, Berenschot 2011).

[15] Andhra Pradesh, Chhattisgarh, Himachal Pradesh, Madhya Pradesh, Tamil Nadu, Rajasthan, and Uttarakhand (Imbert and Papp, 2015).

[16] Despite these problems studies have found large, aggregate benefits to the NREGS rollout (e.g. see Mookherjee 2014 and Sukhtankar 2016 for reviews).

[17] For constituency level variation in NREGS pay in Bihar see map in Appendix 2.E.

MLAs manipulate state resources via their control over bureaucrats' employment prospects. In India, politicians ability to transfer state employees to desirable or undesirable postings effectively undermines bureaucratic independence (Iyer and Mani 2012). In turn, this allows politicians to influence resource allocation and development outcomes (Wade, 1985). In fact, senior Indian Administrative Serivce bureaucrats who are placed in their home states are seen as more corrupt and subordinate to political masters (Xu et al., 2018). This nexus between bureaucrats and politicians can be mutually beneficial and opens doors for manipulation of programs like NREGS. For example, when bureaucrats fall under the jurisdiction of a single politician NREGS benefit delivery improves. Gulzar and Pasquale (2017) interpret this as evidence that when politicians can internalize the benefits to service delivery (i.e. credit claim) they pressure bureaucrats to improve the programs' performance. Similarly, constituencies aligned with the state ruling party (which controls the NREGS faucet) receive increased wages, workdays and project approvals (Dasgupta, 2016). More nefariously, politicians may also engage in rent-seeking in league with bureaucrats (Dreze, 2011). Recent technological reforms that enable fund transfers to bypass bureaucratic middlemen reduce NREGS corruption (Banerjee et al. 2014, Muralidharan et al. 2016).

In short, Members of the Legislative Assembly have both the wherewithal and incentives to manipulate NREGS distribution. Given that there is room for political interference in NREGS, I argue that politician type can alter the delivery of this universal program. If, as Vaishnav (2017) and others argue, criminal politicians are better situated to provide service delivery then their election should result in a net increase of NREGS benefits within their constituencies. However, if electing charged politicians reduces NREGS benefits this would be more in line with the findings of Chemin (2012) and Prakash et al. (2016) that politicians with criminal backgrounds harm constituency welfare.

NREGS Outcomes

The NREGS dataset includes observations on project implementation, work days and costs for all of India from 2006 to 2017, spanning the range of the elections dataset. Table 2.D.1 in appendix

2.D summarizes the state-election years included in my analysis.

I collected original data on the provision of NREGS jobs, payment and projects from `http://bhuvan.nrsc.gov.in/governance/mgnrega_phase2.php`. The NREGS data contains information on over 20,000,000 completed projects between 2006 and 2017. Using geo-coordinates of NREGS project locations I assign the projects to the nearest polling station, mapping them into either a bare criminal winner or bare criminal loser constituency. Specifically, I test the causal effect of electing a criminally accused candidate on the following outcomes:

- *Workdays*: The total number of NREGS work person days summed over every project in a constituency-year.
- *Pay*: Total unskilled labor expenditure summed over every project in a constituency-year.
- *Materials*: Total materials expenditure summed over every project in a constituency-year.
- *Projects Completed*: The sum total of NREGS projects completed during the MLA's constituency-term.

NREGS outcomes are summed over the MLA's constituency-term (generally 5 years). While constituencies are roughly similar in size, I test for imbalance in the number of votes cast to proxy for population and program demand between treatment and control constituencies.[18] The RD should balance on constituencies characteristics but I include these as controls in certain specifications.

Previous scholars investigating NREGS outcomes relied on administrative data detailing wages and employment down to the village level. There are a few reasons to prefer the geotagged project data used in this paper. First, since it is linked to physical assets (including digital pictures and project location) the geotagged data are less likely to be subject to over-reporting. Several studies have found that NREGS administrative data overestimates wage and employment creation when

[18]For a full list of controls see Table 2.A.1 in Appendix 2.A. In chapter 3, I match projects to village census controls for a subset of states to better adjust for program demand.

compared to survey estimates of these outcomes (Imbert and Papp 2007, Niehaus and Sukhantankar 2012). Second, geotagging the projects requires local officials to assess, map and sign off on completed NREGS projects. Thus, geotagging acts as a partial post completion audit on project creation. However it does not completely alleviate the possibility that labor costs or employment are inflated for a given project but should greatly decrease the probability that the project is missing entirely.

In addition, the sheer size and granular details of the projects dataset allows me to conduct an India-wide analysis. This massive data helps on two fronts. First, it maximizes the number of close elections in my sample. Second, it addresses heterogeneity in NREGS outcomes across states. Several "star implementing states" are responsible for most of the positive program impacts across India (Sukhtankar 2016). Typically, NREGS studies are much smaller in scope: confined to a single state, a shorter time-period, or analyzed at very high levels of aggregation (e.g. districts).[19] For example, Niehaus and Sukhtankar's (2012) seminal study on NREGS corruption relies on results from 110,000 work-spell observations, spanning six months in Orissa and Andhra Pradesh. Ravi and Engler (2015) definitive work on NREGS impacts- while laudable for its fine-grained panel-data from 1,064 households- is confined to just 200 villages in Andhra Pradesh over two years. Results from studies analyzing a single state are difficult to generalize. This is doubly true when considering variation in program implementation quality (Sukhtankar 2016).[20]

There is one drawback to using the geotagged data. The creation of the geotagged NREGS project database is a brand new initiative and currently only includes completed projects. In other words, I do not observe ongoing projects that will only be added to the database during the second phase of geotagging and miss some completed projects still being added. Overall, I observe

[19]One notable exception is Gulzar and Pasquale (2017) which uses different administrative data to conduct an India-wide analysis of principal-agent problems between MLAs and block bureaucrats for NREGS delivery. However, this study only penetrates down to the block level, which is still too highly aggregated to precisely measure political targeting in NREGS. Blocks represent 35 villages, on average (Dunning and Nilekani 2013). To measure political targeting in chapter 3, I use precise geo-coordinates to map NREGS projects to villages and polling station results.

[20]NREGS studies tend to focus on Andhra Pradesh, one of the aforementioned "star-implementing" states, due to Andhra Pradesh's ready-made administrative data and better implementation.

roughly 83% of all geotagged projects (20 out of 24 million) and 63% of all completed NREGS projects (20 out of 32 million). There are a further 11 million ongoing projects for a total of 43.8 million projects created since the inception of NREGS. Thus, I capture roughly 46% of all projects (ongoing + completed) (MoRD, 2017). Effects should therefore be interpreted as conditional on completed NREGS projects.[21] However, since the assignment of a criminally charged politician is discontinuous at the threshold, criminal status should be orthogonal to reporting and geotagging of NREGS project creation. In other words, overestimation of project benefits or the type of missing projects should not be correlated with the criminal status of the MLA.

Still, I can not simply claim projects are completely missing at random. As noted above, there are two sources of missingness. Each source accounts for about half of the missing projects in the dataset. First, some completed projects still require geo-tagging. This bureaucratic hurdle should not be correlated with constituencies barely won or barely lost by criminals. Incomplete projects, on the other hand, present a thornier issue. For example, if criminals are better at timing their NREGS distribution to ramp up right before elections, then missingness would be positively correlated with criminality for *ongoing* election terms. I include 23 state elections where legislators had not finished their entire five year term by 2017- the last year I collected NREGS data. These later electoral terms will have more incomplete projects and more future projects yet to break ground. In other words, if criminal politicians back-load NREGS distribution during their electoral terms, then incomplete projects from later state elections are more likely to be missing from criminal constituencies. In turn, this may penalize criminal politicians for failing to deliver NREGS early in their tenures.

One analytical solution to this problem would be to drop unfinished electoral terms. However, since project data is aggregated to the constituency-level in this chapter, causal estimates are already noisy. Dropping state elections would make this problem worse. In other words, there is

[21]To alleviate this concern in future work, I plan to re-run the analysis with the complete village level data on wages and employment. This will help serve as a check on discrepancies between the project-level administrative data and village-level administrative data.

a bias-variance tradeoff. It is not obvious to me that minimizing an unknown bias by dropping elections is the best path forward in this case. Second, the bulk of the data are from electoral terms that finished prior to 2017. Forty-nine out of 72 state-election terms (68%) finished before 2017. With more than 75% of all projects completed in less than 400 days, incomplete projects are less problematic in these finalized electoral terms. The evidence is, therefore, weighted heavily towards MLAs who have finished their entire electoral term. Third, it is not necessarily the case that voters are myopic and only consider projects and employment generated right before elections. For these reasons, I retain all state elections in the analysis.

Defining Criminality

I use affidavit data I scrapped from the Association for Democratic Reform[22] to code criminality after matching affidavits to election outcomes by candidates names.[23] Politicians convicted of crimes are not allowed to hold office. However, politicians can contest elections while cases are pending trial. Some cases remain on the dockets for decades. Once in office, criminally charged politicians can use their new found power to postpone court dates. Candidates are only required to report charges where there is sufficient evidence for a judge to have deemed the case worthy of proceeding to trial (similar to an indictment in the U.S.) (Vaishnav 2012). This helps assuage, though not completely remove, concerns about politically motivated indictments. Additionally, to help alleviate concerns that criminal charges are politically motivated, I restrict some of my analysis to only "serious" charges. Briefly, serious charges are those that carry at least a 2 year prison sentence if convicted or are a non-bailable offense. Often these charges are associated with violence such as murder, attempted murder, rape, or committing grievous physical harm.[24]

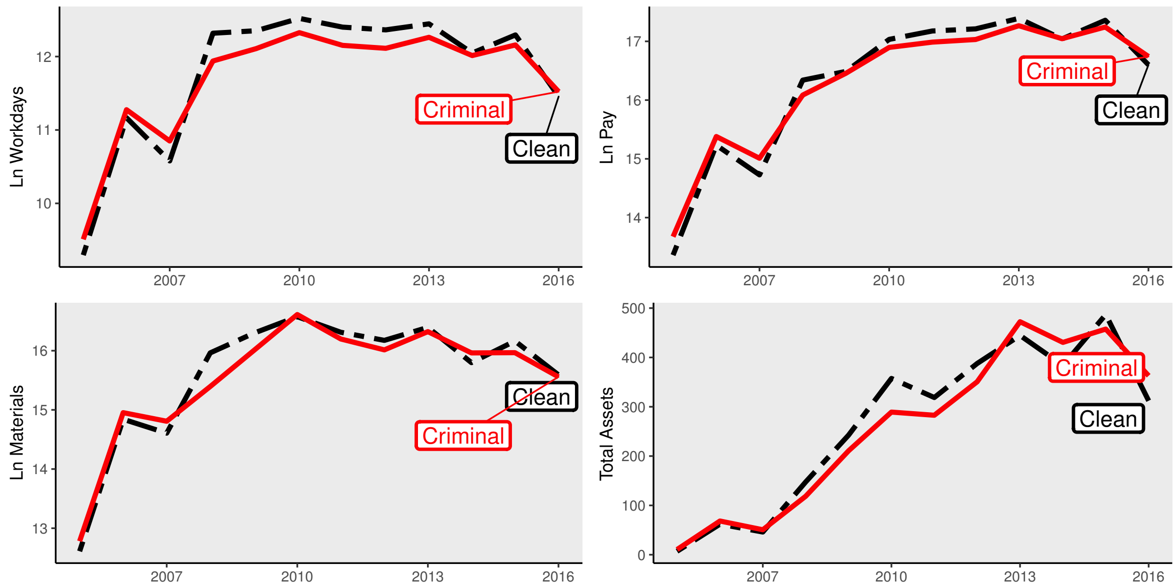
In the baseline specification, I include all criminal accusations. Subsequently I restrict some analyses to just serious charges, in part to alleviate concerns of conflating non-criminal and "crim-

[22] `http://myneta.info/`

[23] Candidates are further matched by state, constituency, election year and age.

[24] I code serious charges based on the crime committed as described by the associated Indian Penal Code (IPC) that accompanies each charge sheet. In follow up work I check the sensitivity results to alternate codings of serious charges.

Figure 2.1: NREGS Outcomes by Criminal Status of MLA



Average NREGS benefit delivery in Indian state legislative constituencies by type of MLA (i.e. Criminal vs. Clean). Data is unadjusted.

inal" politicians. I follow the coding rules set forth by the Association for Democratic Reform (ADR, 2014) which considers serious charges to be:

1. Whether the maximum punishment for the offense committed is of 5 years or more?
2. Whether the offense is nonbailable?
3. Offenses pertaining to the electoral violation (IPC 171E or bribery)
4. Offenses related to the loss to exchequer
5. Offenses the nature of which are related to assault, murder, kidnap, rape
6. Offenses that are mentioned in Representation of the People Act (Section 8)
7. Offenses under Prevention of Corruption Act
8. Offenses related to the Crimes against women.

Despite NREGS's ostensibly universal guarantee, the program is known for wide variation in performance and implementation (Sukhtankar 2016).[25] Figure 2.1 compares NREGS outcomes averaged over criminal and clean constituencies, for all of India. In general, there is no discernible gap between average NREGS delivery in criminal and clean constituencies. In other words, the raw, unadjusted data does not suggest that criminal politicians are noticeably better (or worse) at implementing NREGS. Across both employment (*Workdays* and *Pay* in top panel) and local infrastructure measures (*Material Expenditures* and *Projects Completed*, bottom panel), the trends in criminal and clean constituencies are highly similar. If anything, criminal politicians slightly underperform in NREGS delivery.

The lack of discernible difference between charged and clean politicians could be spurious. For example, clean politicians may typically win elections in monsoon affected areas with higher demand for seasonal NREGS employment. Or criminal politicians may flourish where state capacity is weak and NREGS implementation poor. To estimate the causal effect of electing a criminally charged candidate on NREGS outcomes, I turn now to the RD analysis.

2.4 RDD Validity

The RD literature suggests several strategies and diagnostic tests to validate the regression discontinuity design. First, I consider the possibility that criminally accused candidates are capable of sorting around the threshold. In other words, criminal candidates may be particularly suited to first noticing they are in a tight race and then propelling themselves to bare victories. This would invalidate the assumption of quasi-random treatment assignment around the threshold. In fact, it could be criminal candidates' superior access to money, muscle and networks that enable them to win close races. For example, criminal candidates tend to be wealthier (Vaishnav 2017) and may marshal these extra resources during the campaign to convince late deciders and push themselves to bare victories. Similarly, criminals may have stronger ties to local communities, which could

[25] For comparisons of state-level variation in NREGS outcomes between criminal and clean constituencies see Appendix 2.G.

provide an informational advantage on likely vote outcomes and a more precise control over the final vote total. Marshaling these resources could turn a criminal candidates' bare loss to a bare victory and would likely be correlated with NREGS outcomes.

However, as Eggers (2015) point out, the idea that well-resourced candidates (in their case incumbents) are able to marshal extra-human efforts to win close elections requires two crucial corollaries. First, it requires precise information about exactly how close the race is and what resources are necessary to push a candidate from a bare loss to a bare win. Second, this must only be true for candidates in "extremely close" but not "somewhat close" races.[26] This precise level of vote intention forecasting is unlikely to hold in Indian state legislator races, where campaigns are less well funded and organized than those operated by longer standing parties in richer democracies. Public polling is nascent and unlikely to provide precise enough information.[27] Even the well oiled BJP machine does not claim to have precise predictions of electoral outcomes (Jha, 2017). Instead, parties often rely on more nebulous "caste calculations" when selecting candidates (Chandra, 2007).

Second, it is plausible that criminal candidates influence vote counts either during or after voting. In fact, early criminal politicians were known for "booth capturing." Candidates would muscle in on polling stations, stuff ballot boxes and deter opposition voters (Witsoe, 2009; Vaishnav, 2017). However, the Electoral Commission of India (ECI) has gone to great lengths to crack down on booth capturing, often deploying para-military troops from out of state to ensure electoral integrity. Overall, Indian elections are seen as free and fair, especially when it comes to vote counts.[28] In short, it appears unlikely that criminal candidates can systematically sort themselves

[26]To see this, consider that a discontinuity in the density of the Criminal Vote Margin at the threshold would indicate sorting around the threshold for candidates who knew the race was very tight. However, if candidates who are somewhat close to the threshold engaged in extra efforts then there would likely be a second discontinuity in the density of Criminal Vote Margin. In short criminal candidates who would barely lose, for example, by less than 0.25% percent must engage in this sorting behavior while criminals who lose by slightly more than 0.25% do not.

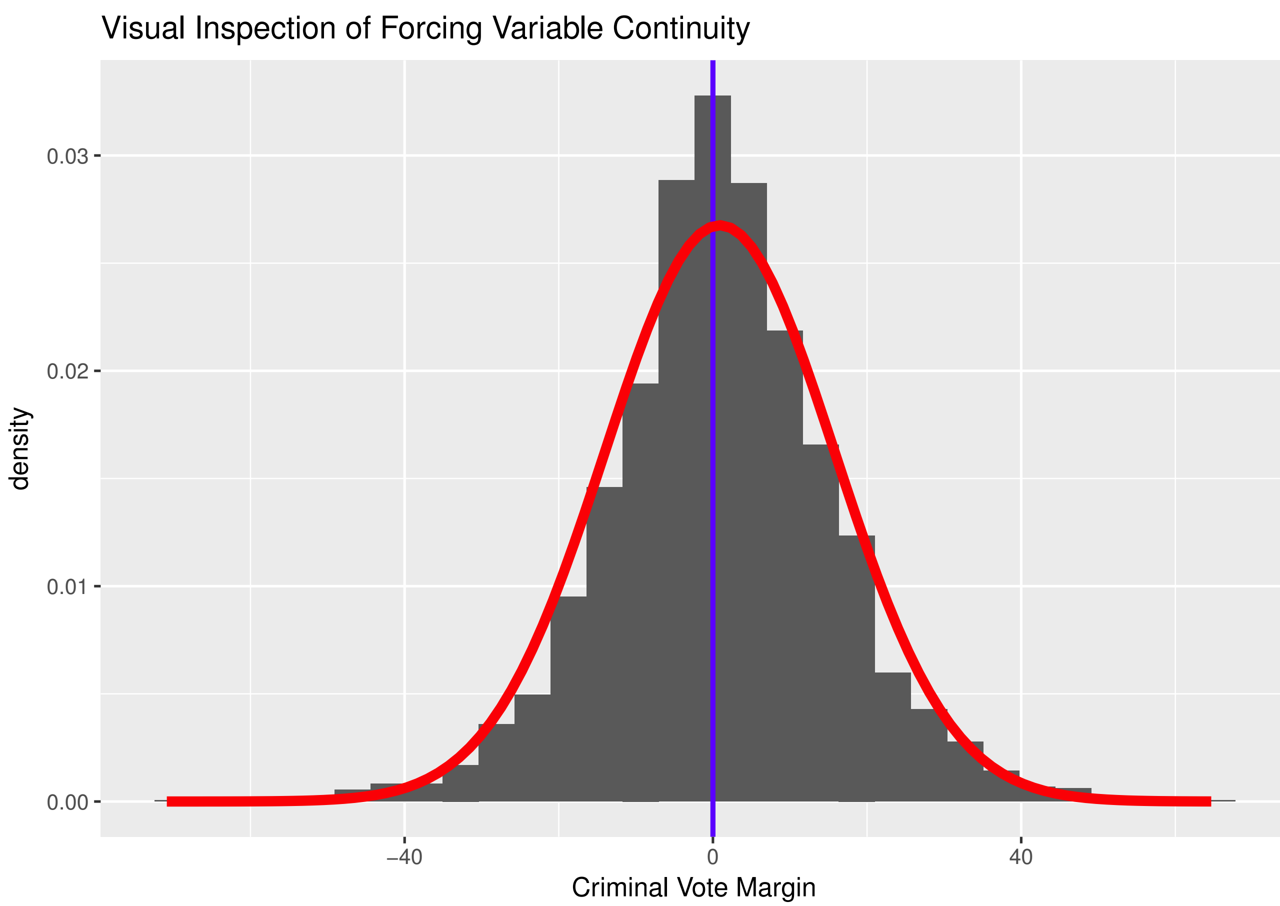
[27]Eggers et al. 2015 argue that candidates are unlikely to be able to predict close outcomes in U.S. house races, where polling is far more abundant

[28]For example, "Indian parliamentary election ranked above average in the worldwide 2016 Perceptions of Electoral Integrity index produced by the Electoral Integrity Project, due to its favorable ratings in election management, laws, electoral procedures, counting and result announcement" (Mahmood and Ganguli, 2017).

into the category of bare winners.

Beyond these theoretical considerations, I directly test for candidate sorting by inspecting the density of the forcing variable (*Criminal Vote Margin*, see figure 2.2). If criminal politicians are indeed sorting into the bare winning column this should create a noticeable discontinuity at the cut-point. In other words, there will be more criminal candidates just to the right of the 0 threshold than just to the left. Figure 2.2 provides a visual check by plotting the density of *Criminal Vote Margin*. It is not indicative of criminal candidate sorting at the threshold.

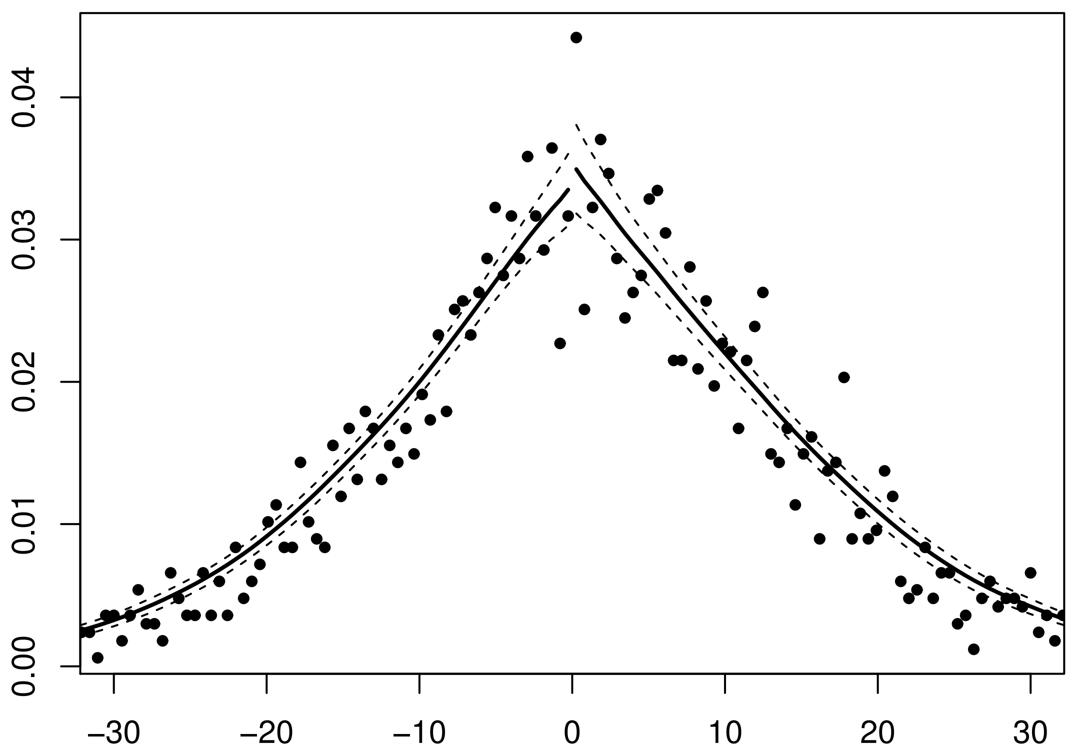
Figure 2.2: Check for Sorting of Bare Criminal Winners



CVM subtracts clean candidates vote share from criminal candidates vote share for a given constituency-election. Negative values indicate the percentage that criminally accused candidates lost by to a clean winner. Positive values indicate the percentage that criminally accused candidates won by against a clean loser.

More formally, I conduct a McCrary test for sorting at the threshold (see figure 2.3). The test is inconsistent with the hypothesis that criminal candidates are more likely to win in close elections (p-value = 0.58).[29]

Figure 2.3: McCrary Test for Sorting



The estimated log difference in heights at the threshold is 0.042 (s.e. 0.075) which equates to a p-value of 0.58 and is not consistent with sorting around the threshold.

[29]Eggers et al. 2015 and Prakash et al. 2016 also fail to find evidence of MLA sorting in India.

2.4.1 Balance Tests and Controls

A second implication of the regression discontinuity design is that if treatment assignment is quasi-random at the threshold, then treatment and control groups should be balanced on observable and unobservable characteristics. Treated constituencies that elect a bare criminal winner should look similar to control constituencies that barely elect clean candidates. Similarly, winning criminals should look like winning clean candidates around the threshold, with the only discrepancy being their criminal status.[30] Overall, treatment and control units seem relatively well balanced across both constituency and candidate characteristics (see Figures 2.4 & 2.5 and Tables 2.2 & 2.3). However, bare criminal winners are less likely to be a member of a national party.[31] I control for this imbalance (along with the other covariates) in my models. Finally, candidates are not imbalanced on National Party membership when the analysis is restricted to just serious charges (see Appendix Figure 2.C.1 and Table 2.C.1).[32]

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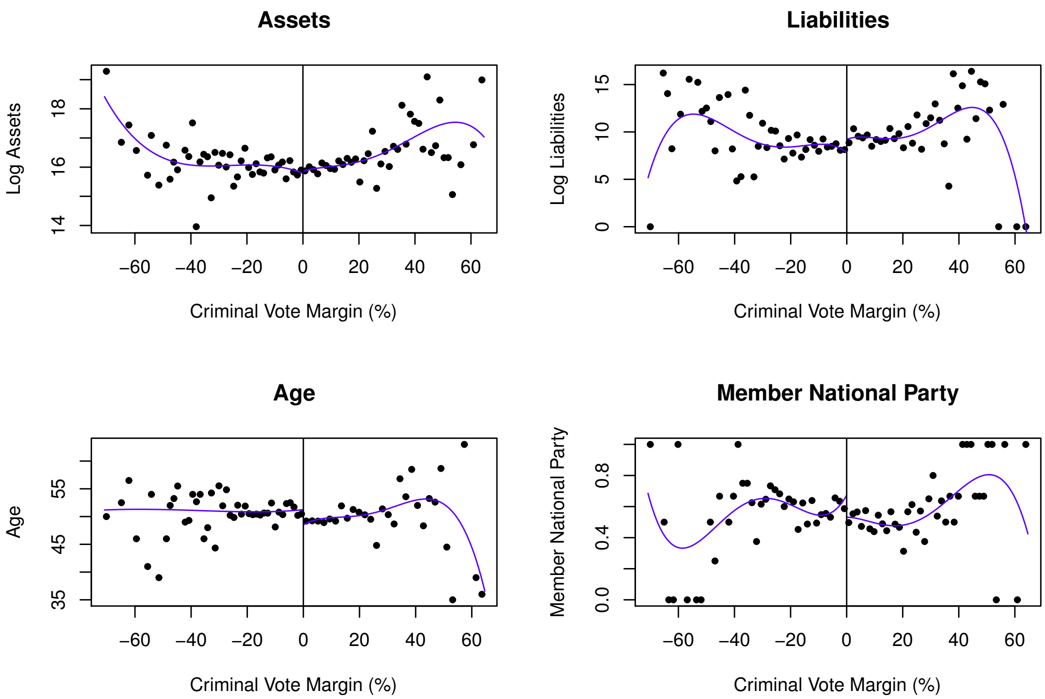
[30]The regression discontinuity design should balance treatment and control constituencies, but does not guarantee that bare criminal winners are similar on average to clean candidates who just beat out criminals. While acknowledging this limitation, I note that several other papers employ similar designs (e.g. for RDs comparing candidates' gender in the U.S., Brazil and India see Ferreira and Gyourko (2014); Brollo and Troiano (2016); Brown (2017), respectively; for RD comparisons of candidates' criminality in India see Chemin (2012); Prakash et al. (2019); Nanda and Pareek (2016)) and that this does not mean that treatment and control units will be unbalanced on other covariates.

[31]While this could arise due to chance, I will use the remaining variables listed in Appendix 2.A Table 2.A.1 to adjudicate if there is indeed evidence of imbalance between treated and control candidates.

[32]If criminal politicians are less likely to be members of national party this could be problematic if this means they are also less likely to be members of the INC or aligned with the ruling party (both of which are associated with the provision of NREGS). I test these possibilities in my forthcoming section on heterogeneous treatment effects.

Figure 2.4: Balance of Candidate Characteristics



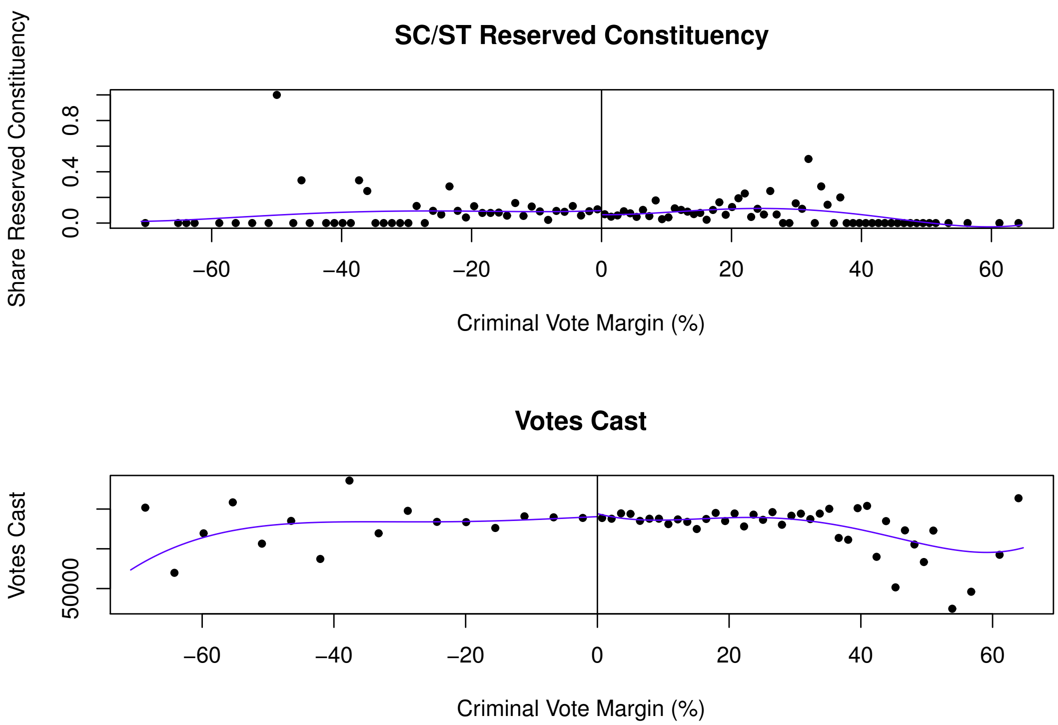
Balance tests for pre-treatment MLA candidate characteristics. Assets and liabilities refer to candidates' self reported wealth on candidate affidavits. Criminal Vote Margin subtracts clean candidates vote share from criminal candidates vote share for a given constituency-election. Positive values indicate the winning candidate faced criminal accusations. Negative values indicate the winning candidate was unaccused at the time of election. The discontinuity is estimated using a global, fourth-order polynomial on either side of the cutpoint. Bandwidths are estimated using a mean squared error optimal bandwidth selector (Calonico et al., 2015).

Table 2.2: Balance across Candidate Characteristics

	Log Assets	Log Liabilites	Age	Member Nat. Party
Accused	0.08	1.37	-1.49	-0.12*
	(0.16)	(0.7)	(1.0)	(0.05)
Obs.	3047	3052	3049	3052
BW est.	10.32	10.56	9.8	10.16

Assets and liabilities refer to candidates' self reported wealth on candidate affidavits.
Standard errors are in parentheses, * $p < 0.05$.
Estimates are from a local polynomial RD treatment effect points estimator. Bandwidths are calculated using a mean squared error optimal bandwidth selector (Calonico et al. 2015).

Figure 2.5: Balance of Constituency Characteristics



Balance tests for pre-treatment MLA constituency characteristics. Criminal Vote Margin subtracts clean candidates vote share from criminal candidates vote share for a given constituency-election. Positive values indicate the winning candidate faced criminal accusations. Negative values indicate the winning candidate was unaccused at the time of election. The discontinuity is estimated using a global, fourth order polynomials on either side of the cutpoint. The bandwidth is estimate using a mean squared error optimal bandwidth selector (Calonico et al. 2015).

Table 2.3: Balance across Constituency Characteristics

	Reserved Const.	Votes cast
Accused	-0.046	1386
	(0.026)	(4103)
Obs.	3052	3052
BW est.	11.343	9.466

Standard errors are in parentheses, * p < 0.05.
Estimates are from a local polynomial RD treatment effect points estimator. Bandwidths are calculated using a mean squared error optimal bandwidth selector (Calonico et al. 2015).

2.5 Results

2.5.1 Main Results Without Controls

The figure below provides the main RD graphs in the baseline specification, without controls or fixed effects, for all four outcomes of interest (*Workdays, Pay, Materials, Projects Completed*). The outcomes are logged transformed, with sample means grouped in evenly spaced bins.[33] The forcing variable, *Criminal Vote Margin* subtracts the vote share of the unaccused candidate from the accused candidate. Thus the treatment status of the winning MLA changes discontinuously from unaccused to accused at the 0 threshold.[34] The vertical distance between the blue regression lines at this threshold estimates the causal effect of criminal accusations on the provision of NREGS benefits in an MLA constituency. For Figure 2.6, the blue regression lines are estimated separately for treatment and control units (accused and unaccused) using a global, fourth order polynomial.

The global parametric strategy borrows strength from data points far from the discontinuity threshold to estimate the true regression function at the cutpoint. In essence, to estimate the treatment effect, I ask what regression model best fits the data on either side of the cutpoint (Jacob, 2012). The global polynomial leverages all available data and is therefore useful to reduce the variance of estimates in a noisy setting with fewer observations bunched up against the cutpoint. I employ this variance-reducing parametric strategy when visualizing the regression discontinuity. However, when reporting treatment effects in the tables below, I adopt a more conservative approach and estimate the regression discontinuity using local-linear regression. Recent advances in the regression discontinuity literature suggest that global polynomials are sensitive to polynomial order and result in overconfident estimates (Gelman and Imbens, 2019). In either case, it is useful to fit both global parametric and local non-parametric models to determine how sensitive results are to model specification.

[33] The number of bins is determined separately for treatment and control candidates by a data-driven approach introduced in Calonico, Cattaneo, Titiunik 2015. Specifically, I use the “mimicking variance evenly-spaced method [with] spacings estimators.” to select the number of bins

[34] This model includes both serious and non-serious accusations.

There seems to be some visual evidence of discontinuity. In fact, criminally accused candidates show a reduction in the number of workdays, total pay, material expenditure, and completion of NREGS projects at the threshold. However, these negative discontinuities seem to be small relative to the general variability in NREGS provision as estimated by the wide dispersion of binned sample means in the scatterplot. Given the lack of a strong visual discontinuity relative to the large overall variation in NREGS benefits across the sample, further investigation is required. In table 2.4, I explore the results more formally using local polynomial regressions to estimate the causal effect of criminal accusations. Below, I investigate the sensitivity of these initial results to alternative specifications, bandwidth size and selectors, and the inclusion of covariates.

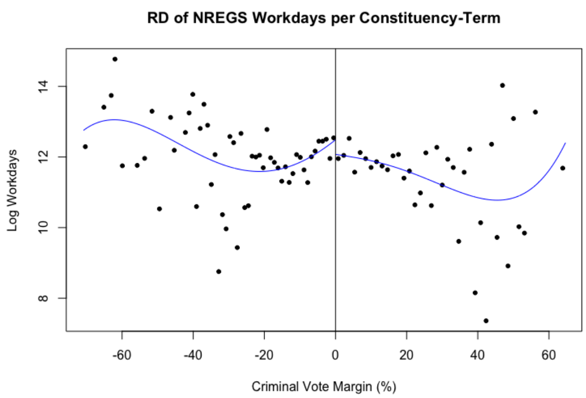
Table 2.4: Main RD Estimates of Criminal Accusations on NREGS Outcomes

	Log Workdays	**Log Pay**	**Log Materials**	**Log Projects Completed**
Conventional	-0.36	-0.25	-0.74	-0.42^{*}
	(0.25)	(0.27)	(0.44)	(0.21)
Bias-Corrected	-0.39	-0.27	-0.86	-0.48^{*}
	(0.25)	(0.27)	(0.44)	(0.21)
Robust	-0.39	-0.27	-0.86	-0.48^{*}
	(0.29)	(0.31)	(0.51)	(0.24)
Num. obs.	2679	2678	2670	2679
Eff. Num. obs. Left	874	917	854	831
Eff. Num. obs. Right	930	966	912	868
Eff. Num. obs. Left BC	1144	1165	1157	1133
Eff. Num. obs. Right BC	1227	1250	1249	1220
BW (h)	12.95	13.76	12.58	11.86
BW Bias Corr. (b)	22.30	23.77	23.67	21.70
Order (p)	1	1	1	1
Order Bias Corr. (q)	2	2	2	2
Model	Non-parametric Local Polynomials			

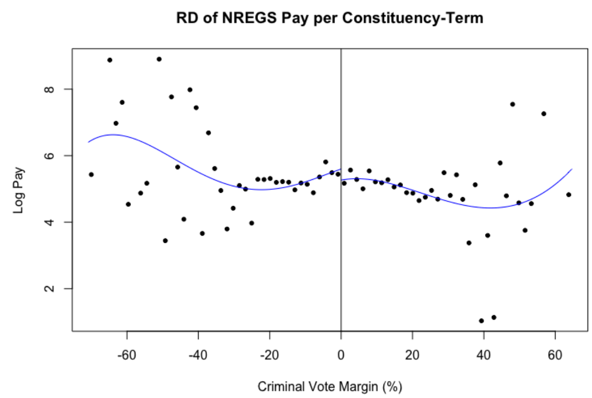
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective Number is the number of observations included inside the bandwidth (with BC indicating effective number of observations for bias corrected estimates). Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

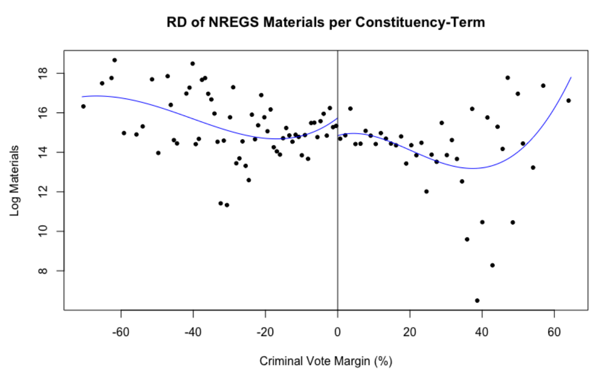
Figure 2.6: Main RD Plots - Baseline Specification



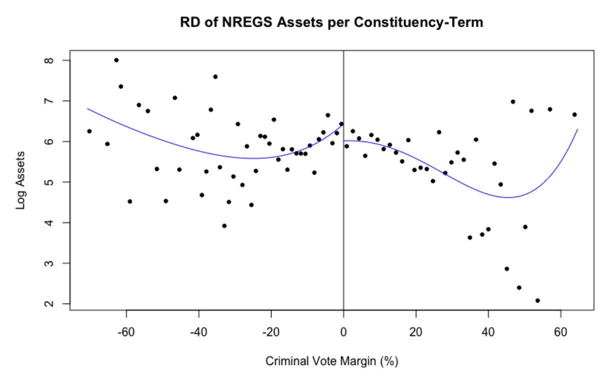
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Projects Completed

Criminal Vote Margin subtracts clean candidates vote share from criminal candidates vote share for a given constituency-election. Negative values indicate the percentage that criminally accused candidates lost by to a clean winner. Positive values indicate the percentage that criminally accused candidates won by against a clean loser. The model estimates the effect of criminality on NREGS delivery at the threshold (0%), where the criminal status of the local politician changes discontinuously from un-accused to criminally accused. The discontinuity is estimated using fourth order, global polynomial regression on either side of the cutpoint. All outcomes are transformed by ln(outcome +1).

Table 2.4 displays point estimates and standard errors for the four logged NREGS outcomes. Whereas Figure 2.6 employed global polynomials, here the discontinuity is estimated using local linear regressions with a triangular kernel in a window around the threshold. Local linear regression is a more conservative approach as it limits estimation to points in a very narrow neighborhood immediately around the cutpoint. To select the neighborhood bandwidth I adopt a data-driven approach recommended in Cattaneo et al. (2018).[35] This helps to minimize researcher degrees of freedom in selecting a "favorable" bandwidth.

Consistent with the main RD graphs above, all point estimates are negative, indicating criminally accused MLAs reduce NREGS delivery. For example, accused politicians are estimated to reduce the number of workdays provided during their term by 30% relative to unaccused politicians (column 1 conventional estimate). Similarly, criminally accused MLAs reduce expenditure on labor and materials by 22% and 55%, respectively. However, these effects are imprecisely estimated. In fact, the data are consistent with a causal impact of criminality on *Workdays* ranging from a 57% reduction to a 13% increase. The estimated percentage change from the 95% confidence intervals for *Pay* and *Materials* range from -54% to 32% and from -80% to 13%. As detailed below, the most consistent and precise estimates demonstrate a reduction in the total number of completed projects during accused MLAs terms. Under this specification, accused politicians cause a 34% reduction in the number of NREGS projects completed. The average constituency in the RD sample completes 1407 projects per MLA-term. A reduction of 34% would mean approximately 475 fewer local public works completed during an accused politicians time in office.

In addition to the conventional RD estimates, I include bias-corrected and robust-biased correct estimates and confidence intervals recommended by Cattaneo et al. (2015). Conventional estimates do not account for the bias introduced by the fact that local polynomials are an approximation of the true regression function within the neighborhood of the threshold (Cattaneo et al. 2018). Bias corrected estimates attempt to estimate and remove this bias, but fail to incorporate the

[35]The bandwidth is selected using the data-driven CCT approach that is mean squared error optimal.

variability from estimating this bias into their confidence intervals, resulting in confidence intervals that are too small. The robust bias-corrected methods account for this variability and include larger confidence intervals with better coverage properties (Cattaneo et.al. 2018). It is encouraging however that the point estimates do not change dramatically despite the bias correction and alternative bandwidth selection. Moreover, the *Projects Completed* outcome retains conventional levels of statistical significance throughout, even though standard errors increase in size under the robust correction for confidence intervals.

At the very least, accused politicians complete fewer NREGS projects during their term. This is worrying given that the construction of local public works is a primary goal and justification for the massive investment in NREGS. NREGS projects such as improved irrigation, roads or the construction of school walls, also provide a public benefit that can last well beyond the short term project investment and employment. The lack of a clear visual discontinuity (at least relative to the overall variation in NREGS outcomes) combined with the imprecisely estimated effects of criminal accusations suggests the need for reducing sampling variance of the estimates. There are two ways to improve the precision of RD estimates. First, using a global, parametric approach to estimate the discontinuity by including all observations (even those far from the threshold). However, this results in a bias-variance tradeoff as observations far from the threshold may have undue impact on the estimated treatment effect. Second the inclusion of covariates that are predictive of outcomes. Including controls can reduce variance while not biasing the RD design. Estimated treatment effects should not change after the inclusion of these covariates. This follows from the fact that assignment to treatment is independent of observable and unobservable covariates so including additional candidate characteristics in the local linear regression should only reduce the sampling variability of the estimate but not alter the estimate itself (Lee and Lemieux, 2010).

In the remainder of this section, I explore how discontinuity estimates change under alternative specifications and after including adjustors. In the appendices I include additional sensitivity analyses.

2.5.2 Main Results With Controls

In the following specifications I include baseline controls and fixed effects for state and election-year. There is well documented variation in NREGS provision by state and time period. Some states have delivered a high level of NREGS benefits (e.g. Andhra Pradesh, MP, Rajasthan, and Chhatisgarh), while others remain chronic underperformers (Bihar, Jharkhand, Orissa and Uttar Pradesh) (Imbert and Papp 2011). Secondly, Modi's BJP led government has focused on technological solutions to curb leakage, with the program generally improving over time (Banerjee et al. 2014).

Table 2.5 makes this comparison explicit by successively adding controls and fixed effects for State and Election-Year to each outcome. The first column for each outcome is the baseline (same model as in table 2.4). The second column adds in controls (for now, just the number of votes cast per constituency to proxy for NREGS demand). The third column includes controls and fixed effects for state and election-year. Following Lee and Lemieux (2010) the outcomes in the fixed effects models are residuals from a linear regression of the log of NREGS Benefit on state and election year. The residuals are then used in the RD model to estimate the treatment effect of criminally charged MLAs on NREGS provision. While controlling for the number of votes cast did not noticeably reduce the variance of the estimates, including fixed effects for state and year did improve precision. After including fixed effects, constituencies that elect a criminally charged MLA witness a 59% reduction in materials expenditure, on average (significant at the 95% level). At the same time accused MLAs cause a 40% reduction in the number of completed projects. Overall, the point estimates remain consistently negative and quantitatively similar after the inclusion of covariates.

The main RD graphs (figure 2.7) for the residuals also show a reduction in sampling variance consistent with State and Election-Year being informative predictions of the delivery of NREGS benefits. They also seem to indicate a greater visual discontinuity. For the rest of the paper I continue with specifications that include controls and fixed effects while including results without

Table 2.5: RD Estimates of Criminal Accusations on NREGS Outcomes- Including Covariates

	Log Workdays			**Log Pay**			**Log Materials**			**Log Projects Completed**		
Conventional	-0.36	-0.36	-0.41	-0.25	-0.25	-0.33	-0.74	-0.74	-0.84^{*}	-0.42^{*}	-0.43^{*}	-0.48^{**}
	(0.25)	(0.25)	(0.21)	(0.27)	(0.26)	(0.24)	(0.44)	(0.44)	(0.35)	(0.21)	(0.21)	(0.16)
Bias-Corrected	-0.39	-0.39	-0.39	-0.27	-0.29	-0.31	-0.86	-0.86	-0.89^{*}	-0.48^{*}	-0.50^{*}	-0.51^{**}
	(0.25)	(0.25)	(0.21)	(0.27)	(0.26)	(0.24)	(0.44)	(0.44)	(0.35)	(0.21)	(0.21)	(0.16)
Robust	-0.39	-0.39	-0.39	-0.27	-0.29	-0.31	-0.86	-0.86	-0.89^{*}	-0.48^{*}	-0.50^{*}	-0.51^{**}
	(0.29)	(0.29)	(0.25)	(0.31)	(0.31)	(0.29)	(0.51)	(0.51)	(0.41)	(0.24)	(0.23)	(0.19)
Num. obs.	2679	2679	2679	2678	2678	2678	2670	2670	2670	2679	2679	2679
Eff. N obs. Left	874	876	815	917	926	786	854	848	757	831	831	755
Eff. N obs. Right	930	934	843	966	979	808	912	900	772	868	869	766
Eff. N obs. LBC.	1144	114	1063	116	1177	1034	1157	1147.00	1011	1133	1179	1017
Eff. N obs. RBC.	1227	1230	1138	1250	1272	1101	1249	1237	1073	1220	1273	1072
BW (h)	12.95	13	11.44	13.76	14.08	10.86	12.58	12.45	10.28	11.86	11.87	10.06
BW BC (b)	22.30	22.63	18.48	23.77	25.08	17.58	23.67	23.05	16.83	21.70	25.21	16.78
Order (p)	1	1	1	1	1	1	1	1	1	1	1	1
Order BC (q)	2	2	2	2	2	2	2	2	2	2	2	2
Controls	NO	YES	YES	NO	YES	YES	NO	YES	YES	NO	YES	YES
FE	NO	NO	YES	NO	NO	YES	NO	NO	YES	NO	NO	YES

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

For the models including fixed effects, outcomes are the residuals after controlling for state and election year. BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW BC. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order BC. provide the polynomial order for the regression on either side of the threshold.

covariates in the appendix.

2.5.3 Sensitivity Analysis

Varying Bandwidth Selections

I test the sensitivity of my results using a variety of models and bandwidth specifications. To recover the treatment effect I compare the average outcomes from "close" elections on either side of the cutoff. Regression discontinuity results are sensitive to which elections are considered "close" (i.e. to bandwidth size). Narrow bandwidths can be noisy since they include fewer observations. Wider bandwidths stabilize estimates, but may bias results by including elections further from the cut-point.[36] Figure 2.8 plots the local average treatment effects (LATE) for the NREGS outcomes at various bandwidth sizes. The estimates appear stable across a wide variety of bandwidth choices. The reduction in the completion of NREGS projects (8(d)) remains significant across bandwidth choices too.

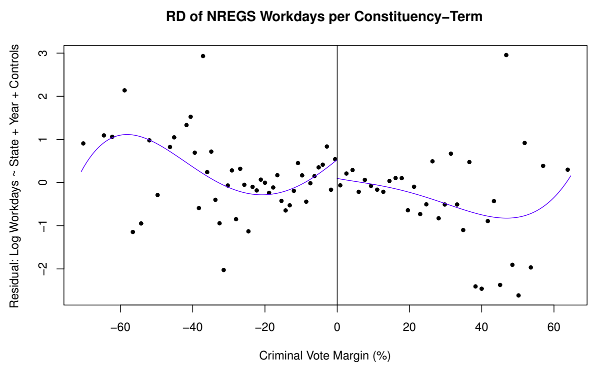
Secondly, the RD literature recommends several different bandwidth selection methods. Table 2.B.1 in appendix 2.B re-estimates the fixed effect models with different bandwidth selectors. Columns 1 (CCT) and 2 (CCT 2014) are the original specification using the data-driven bandwidth selector that optimizes MSE. Column 3 uses cross-validation to estimate the optimal bandwidth size for the baseline specification. In addition, I test the sensitivity of results to bandwidths selected by the Imbens and Kalyanaraman (2012) algorithm (Column 4).

Varying Functional Forms

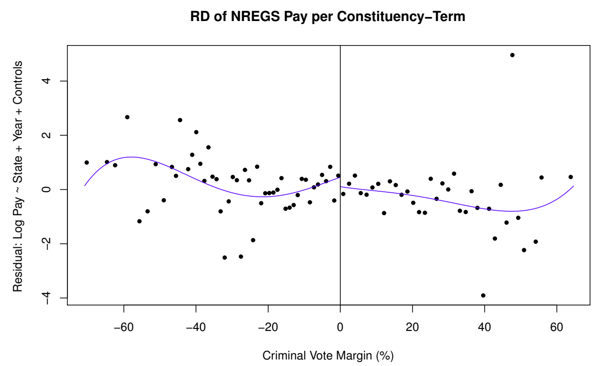
Next, I estimate treatment effects for a variety of functional forms. Gelman and Imbens (2019) recommend the use of local linear or quadratic polynomials instead of controlling higher order polynomials. Their results indicate that higher order polynomials can given large weight to observations far from the cut-point, are highly sensitive to the degree of the polynomial and produce

[36] That is, there is a bias/variance tradeoff to bandwidth selection. In short, researchers want to include enough observations in bins to reduce noise but not so many that you are nor longer comparing observations at the threshold where treatment is randomized.

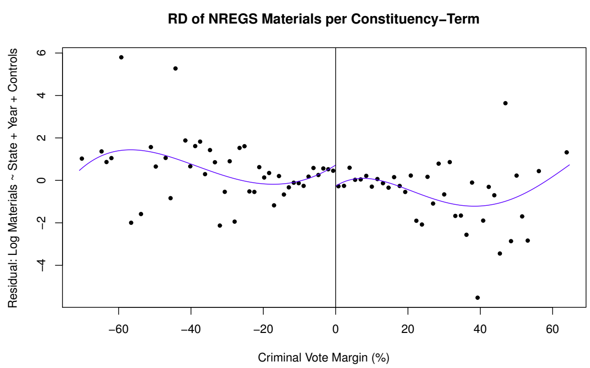
Figure 2.7: Main RD Plots - Controls and Fixed Effects



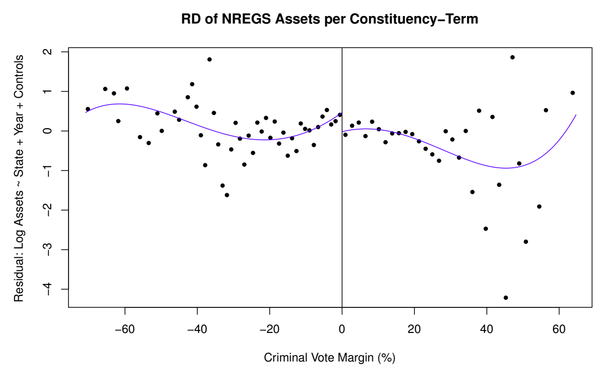
(a) Log Workdays



(b) Log Pay



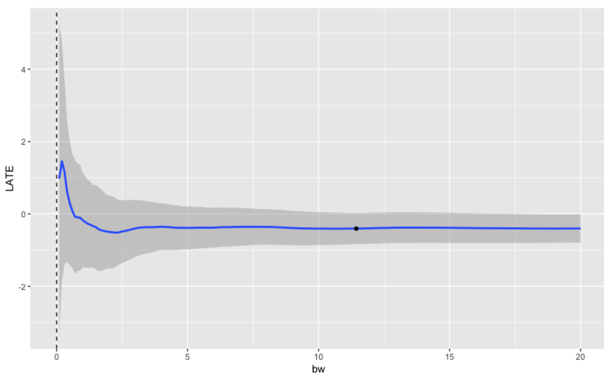
(c) Log Materials



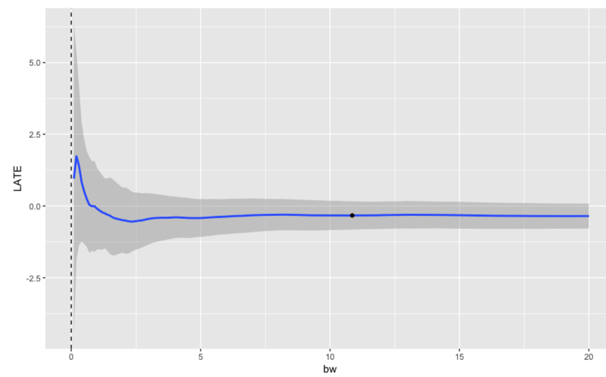
(d) Log Projects Completed

Criminal Vote Margin subtracts clean candidates vote share from criminal candidates vote share for a given constituency-election. Negative values indicate the percentage that criminally accused candidates lost by to a clean winner. Positive values indicate the percentage that criminally accused candidates won by against a clean loser. The model estimates the effect of criminality on NREGS delivery at the threshold (0%), where the criminal status of the local politician changes discontinuously from un-accused to criminally accused. The discontinuity is estimated using fourth order, global polynomial regression on either side of the cutpoint.

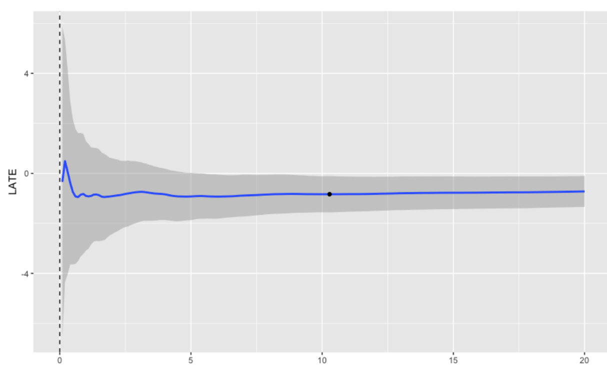
Figure 2.8: Sensitivity Analysis - LATE for Varying Bandwidths



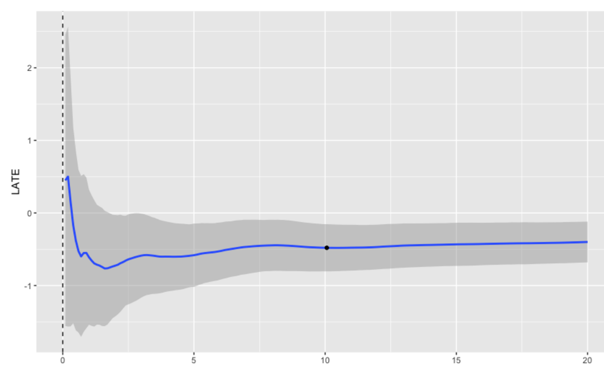
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Projects Completed

Note that RD estimates are non-parametric linear polynomials from the RDDTools package using the data-driven bandwidth selector from the RDRobust package. This leads to slightly different standard errors than those calculated under the RDrobust package (e.g. see Tables above). However the point estimates remain the same.

confidence intervals that are too small. To this end, I report results for a variety of local polynomials running from 1st-6th degree for each NREGS outcome (see tables in Appendix 2.B.2). Encouragingly, the *Projects Completed* outcome remains statistically significant across all polynomial choices though the estimate varies.

Placebo Tests

Finally, I conduct a number of placebo tests, including checking for discontinuities at other values of the forcing variable (Criminal Vote Margin). There should not be a discontinuity when comparing constituency outcomes in narrow windows at different values of CVM (see figure 2.9). The estimates for the placebo cutpoints are not entirely stable. For example, for the *Projects Completed* outcome there is a significant effect around a cutpoint of -9. Ideally, the placebo plots should look more like that of *Materials*, with insignificant effects everywhere except at the threshold of 0.

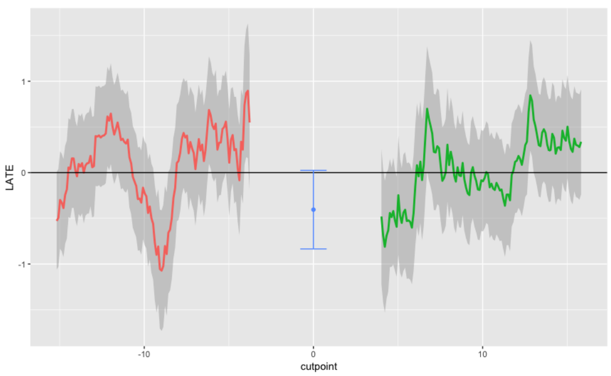
2.5.4 Serious Criminals Analysis

I now turn to the results for the subset of MLAs accused of serious crimes. To reiterate, I expect effects to be stronger when analyzing serious charges. Including all charges potentially conflates "criminal" politicians with those falsely accused by political rivals or who incur charges in the course of political activism (Jaffrelot and Verniers, 2014). In turn, this increase in measurement error may muddy the effect of criminal charges on NREGS provision. Moreover, serious charges correspond more directly to underlying criminal traits, such as the propensity for violence.[37] If these latent criminal traits help candidates' win elections, perhaps despite an inability to perform in office, then we might expect stronger, negative effects when examining politicians facing only serious charges.

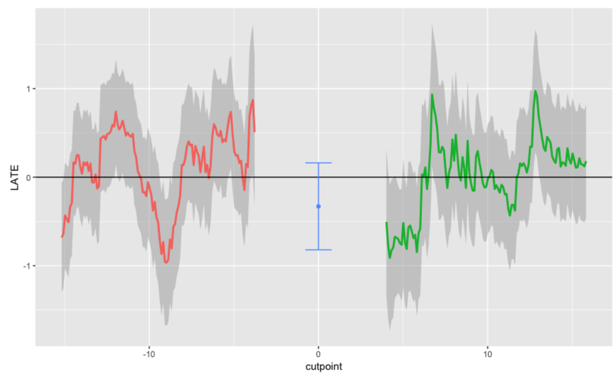
Specifications for the regression models analyzing serious charges remain the same as above (i.e. for all charges). Treatment effects compare constituency results from close races where a candidate facing a serious charge ran against a candidate who did not face a serious charge (i.e. the

[37] In future work I specifically inspect only violence related charges.

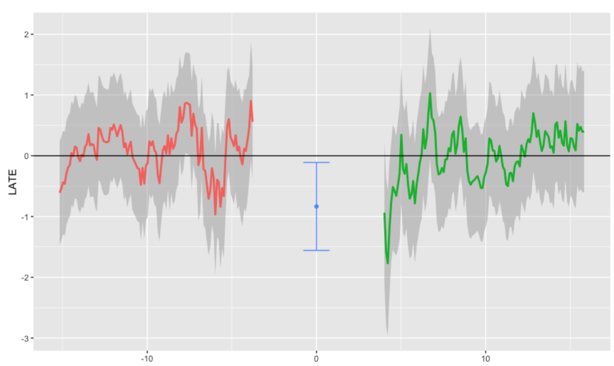
Figure 2.9: Placebo Tests - LATE for Varying Cutpoints- Baseline with Fixed Effects and RDRobust data driven BWS



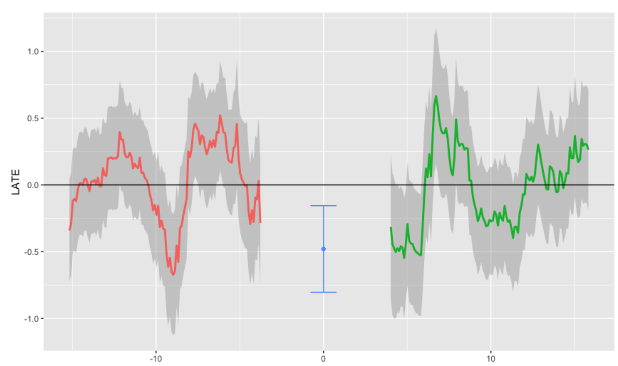
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Projects Completed

Note that RD estimates are non-parametric linear polynomials from the RDDTools package using the data-driven bandwidth selector from the RDRobust package. This leads to slightly different standard errors than those calculated under the RDrobust package (e.g. Tables above). However the point estimates remain the same.

candidate either faced no charge or faced a non-serious chrage). Notably, when restricted to serious charges the point estimates increase in magnitude while remaining negative (albeit the coefficients for *Materials* and *Projects Completed* remain roughly identical in the fixed effects specification, see Table 2.6).[38] *Workdays* and *Materials* also achieve conventional levels of statistical significance despite a 17% reduction in the number of observations when focusing on serious charges. These results are consistent with measurement error in coding criminality when including all types of charges. This strengthens the case that the affidavit charges are indeed picking up latent characteristics differentiating types of politicians in office and that criminal accusations negatively effect NREGS provision. MLAs accused of serious crimes reduce workdays, material expenditure and the number of completed projects over their term. For the models including controls, electing a criminally accused candidates results in an estimated 37% reduction in projects completed (with a 95% confidence interval ranging from -55% to -11% under the conventional specification). This evidence suggests that criminally accused politicians are not necessarily better equipped to "get things done in office."

2.6 Corruption

One alternative explanation for criminal politicians' continued success could be their comparative advantage in corruption. Several studies document large leakages in NREGS, especially early in the programs' implementation (Imbert and Papp 2011, Niehaus and Sukhtankar 2013, Banerjee et al. 2016). Criminal politicians may invest in patronage based networks by allowing corruption to flourish, enriching middlemen in exchange for votes. For example, in Andhra Pradesh MLAs appoint loyal subordinates as Field Assistants responsible for managing NREGS employment and village works. In return, Field Assistants carry out electioneering and information gathering for their patrons (Maiorano 2014). Similarly, government party supporters and politically active citizens are more likely to receive work in West Bengal (Das 2015). If criminal politicians cultivate

[38] For sensitivity analysis when examining serious charges see Appendix 2.C.3 and 2.C.4.

Table 2.6: RD Estimates for Serious Charges

	Log Workdays	**Log Pay**	**Log Materials**	**Log Projects Completed**
Conventional	-0.49^{*}	-0.38	-0.82^{*}	-0.46^{**}
	(0.22)	(0.25)	(0.40)	(0.17)
Bias-Corrected	-0.50^{*}	-0.39	-0.87^{*}	-0.51^{**}
	(0.22)	(0.25)	(0.40)	(0.17)
Robust	-0.50	-0.39	-0.87	-0.51^{*}
	(0.26)	(0.30)	(0.49)	(0.20)
Num. obs.	2216	2214	2212	2216
Eff. N obs. Left	678	636	642	596
Eff. N obs. Right	702	645	655	600
Eff. N obs. Left BC	876	831	832	809
Eff. N obs. Right BC	967	902	906	870
BW	11.33	10.13	10.39	9.16
BW Bias Corr.	18.87	16.79	16.93	15.73
Order	1	1	1	1
Order Bias Corr.	2	2	2	2
Controls	YES	YES	YES	YES
FE	YES	YES	YES	YES

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

For the models including fixed effects, outcomes are the residuals after controlling for state and election year. BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

superior networks and influence over the bureaucracy, they may be more adept at delivering corrupt rents. I construct several measures of corruption but do not find that criminal politicians systematically engage in more malfeasance.

Typically, investigations of NREGS corruption focus on over-reporting by officials, such as excess wages, workers or material expenditures (Niehaus and Sukhtankar 2013, Gulzar and Pasquale 2018).[39] To test for corruption, I construct two measures of over-reporting, based on excess wage payments and material overages.

First, I investigate corruption by comparing wages paid per workdays between criminal and clean constituencies. Excess wage payment would be consistent with corruption, though not definitive on its own.[40] I estimate the discontinuity in wages per workdays for criminal and clean constituencies in Table 2.7 column 1. These models are net state and election-term fixed effects since States set minimum NREGS wage rates. Any difference between criminal and clean constituencies should, therefore, not simply result from different wage rates across states or time. I do not find significant differential wage payments between criminal and clean constituencies (Table 2.7 Column 1). Pay per workday (column 1) is imprecisely estimated and consistent with both large positive and negative effects of criminality, though the data is not sufficient to differentiate the effect from zero.

Second, I check if criminal constituencies exhibit higher material expenditures on NREGS projects. Ostensibly, material expenditures are capped at 40% of project costs. Excess material expenditures could indicate rents that are shared between politicians and contractors responsible for providing the materials. For instance, Maiorono (2014) notes that MLAs push for higher material expenditures to reward crony contractors supplying NREGS projects. Under the regression

[39] Niehaus and Sukhtankar (2013) compare administrative NREGS expenditures to self-reported wages from surveys of NREGS labor and find substantial evidence of over-reporting of days worked in Orissa and Andhra Pradesh. Gulzar and Pasquale (2018) measure over-reporting by analyzing administrative data for discrepancies between wages paid under NREGS and deposits to laborers accounts.

[40] Ideally I would be able to observe the true number of days worked by actual laborers on NREGS projects, with excess wages indicative of corruption. However, since I only observe the reported number of workdays, I instead check for an overabundance of wage payments for a given number of workdays.

discontinuity design, criminality should be independent of project type or size near the threshold. In other words, differences in material expenditures around the threshold should not simply result from different project demands across constituencies. Column 2 in Table 2.7 shows that, if anything, criminal constituencies spend less on NREGS materials. The coefficient on criminality suggests a 40% reduction in material expenditures, though the data is consistent with criminal constituencies spending 2% to 63% less on NREGS materials. Voters could conceivably reward criminal politicians for limiting contractor corruption in NREGS materials. However, given that this is an indirect measure of material embezzlement, a lack of corroborating evidence in other measures of corruption and the model dependence of this result (i.e., not significant under the bias-correction estimate) I do not put much weight on this result as indicative of criminals curbing corruption. In short, I find suggestive evidence that criminal constituencies complete less materially intensive projects.

At best, administrative measures of over-reporting provide only indirect observations of corruption. Simply following the NREGS paper trail for fund disbursal says nothing about whose pockets ultimately get lined. As an alternative, I construct a qualitatively informed measure of corruption based on interviews in Bihar, India. Auditors and contractors involved in NREGS corruption identified certain types of NREGS projects as more amenable to corruption.[41] For example, contractors noted that "soilworks" e.g. (pond creation, water preservation) were preferred to roads or brick canal building. Soilworks provide two main advantages over other projects. First, it is easier to hide the amount of corruption in soil based projects compared to more visually verifiable projects like roads. Or, as it was relayed to me, once you put the shovel into the ground the first foot of soil looks exactly like the 10th. Thus, it is easier to exaggerate the amount of work completed on pond deepening and other soil based projects. Second, soil structures are more susceptible to heavy rains making post-completion audits difficult.[42] I leverage this information to create a ty-

[41]Contractors want to control both the placement and type of the project. By controlling the project location contractors ensure that "their guys" i.e. loyal workers and pliable politicians are involved in the scheme in areas where they have connections and clout.

[42]These sentiments were echoed by bureaucrats involved in project monitoring in Jharkhand, a neighboring state.

pology of NREGS projects by their susceptibility to corruption. As a first cut, I divide NREGS projects based on their broad category type into soil and non-soil related projects. I use the broad category type as these labels are standardized across all states.[43]

While I do find consistent evidence that criminal politicians' constituencies complete fewer "corruptible" projects (column 3), this seems to be an artifact of criminal constituencies completing fewer NREGS projects overall. When assessing the proportion of corruptible projects (column 4), I find a fairly precisely estimated zero. At most, the data is consistent with criminal constituencies completing 4% more corruptible projects (95% confidence interval of -0.04 to 0.04). In sum, I do not find any evidence that criminal politicians engage in excess corruption though more precise measures of corruption are necessary to rule out this pathway.

The bureaucrat also mentioned that the timing of projects could indicate corruption as a large uptick in projects during the agricultural season when demand for NREGS work is low is a red flag for auditors.

[43] In future work, I plan a more fine grained analysis by using individual project names, which requires transliteration and training a classifier for the millions of projects.

Table 2.7: RD Estimates for NREGS Corruption

	Pay per Workday	Log Material Expend. per Project	Log Corrupt Projects	Proportion Corrupt Projects
Conventional	-95.64	-0.50^{*}	-0.52^{***}	0.00
	(122.38)	(0.24)	(0.16)	(0.02)
Bias-Corrected	-94.62	-0.55^{*}	-0.54^{***}	0.00
	(122.38)	(0.24)	(0.16)	(0.02)
Robust	-94.62	-0.55	-0.54^{**}	0.00
	(141.72)	(0.28)	(0.18)	(0.02)
Num. obs.	2639.00	2669.00	2678.00	2675.00
BW (h)	6.49	10.30	11.44	10.98
BW Bias Corr. (b)	13.88	17.13	18.82	18.30
Order (p)	1.00	1.00	1.00	1.00
Order Bias Corr. (q)	2.00	2.00	2.00	2.00
Controls	YES	YES	YES	YES
FE	YES	YES	YES	YES

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

For the models including fixed effects, outcomes are the residuals after controlling for state and election year. BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

2.7 Conclusion

I find that criminally accused MLAs cause a reduction in the number of local NREGS projects completed during their time in office. This result is consistent across a broad range of model specifications and bandwidth selections. However, the lack of a clear visual discontinuity in the main RD graph and imprecise estimates for other outcomes, tempers these findings. When considering only serious charges, constituencies with accused politicians witness a reduction in employment and material expenditure in addition to completing fewer projects. The creation of local public infrastructure is one of the primary goals of NREGS and an increased emphasis under the BJP government. Thus, accused MLAs reduction in project completion demonstrates the importance of considering how politicians' backgrounds may translate to their (under)performance in office.

Overall, the results for *Workdays*, *Pay*, *Materials*, remain imprecisely estimated and inconclusive. While point estimates are consistently negative for these outcomes, standard errors are often large. I only reach conventional levels of statistical significance in some models (e.g. when comparing serious criminals to clean politicians). While fewer completed projects may result from an accused MLAs general underperfomance in NREGS provision (i.e., reduction in employment and expenditure) the imprecise estimates for other outcomes fail to rule out alternative interpretations. For example, it could be that I am looking for the keys under the lamp-post. While NREGS provides a clean, standardized measure of politician performance, there are myriad other programs and problems that criminal politicians could solve.

In short, NREGS is only one development program among a bundle of benefits distributed by the Indian government. I am therefore cautious in drawing broad conclusions regarding criminal politicians' distributive strategy based on a solitary (though important) government program (Kramon and Posner, 2013). Recently, NREGS has increased transparency and accountability initiatives (Banerjee et al. 2017). With this heightened scrutiny, perhaps criminal politicians focus their efforts elsewhere? For example, MLAs have access to local area development funds (MLALADs).

MLALADs can be used at state legislators' discretion to fund whatever development projects they see fit. However, the amount of NREGS funds flowing through a constituency can be 20 times greater than the amount available under MLALADs (Gulzar and Pasquale 2017). In addition, NREGS can be more precisely targeted to a larger number of constituents than large public works constructed using MLALADs. Therefore, it seems less likely that MLAs would ignore politicizing NREGS entirely.

Why then are criminal politicians routinely elected in India? I find little evidence that they facilitate rent extraction or benefit delivery. Still, I can not rule out that charged politicians are more effective at targeting NREGS delivery to their core supporters or that they provide other services outside of NREGS (e.g., protection, adjudication or direct cash transfers). Criminal politicians are often thought of as constituent problem-solvers substituting for a dysfunctional state (Vaishnav 2017). Voters may care more intensely about MLAs providing personalized constituency service (Bussell 2019). In turn, MLAs may more easily claim credit from personal service acts relative to manipulating programs that pass through multiple bureaucratic and government tiers. For example, during fieldwork, I witnessed criminal politicians ply voters with personal cash transfers to pay for school fees, weddings or economic shocks. Similarly, criminals' may derive their comparative advantage from mediating disputes, resolving issues with the police, and enforcing contracts. In effect, criminals may serve as judge, jury and executioner, enforcing their decisions at the barrel of a gun. All in all, I can not rule out that criminal politicians substitute other, unmeasured benefits, for poor NREGS performance. However, in Chapters 3 and 4 I do explore two alternative explanations for criminal politicians' success.

First, in Chapter 3, I investigate if criminals locally rooted networks allow them to more easily target NREGS to co-partisans. MLAs do not need to win every vote in their constituency. By efficiently targeting NREGS, criminals can cultivate a stable of core supporters needed to win a bare plurality. I find some evidence that criminals are associated with superior NREGS targeting. Second, I consider whether criminal politicians provide extra services to voters that fall outside of government programs. In Chapter 4, I provide some suggestive evidence that voters reward criminal politicians who leverage their local roots to deliver personalized constituency service.

Appendix

2.A Controls

Table 2.A.1: Variables for Balance Checks

Dataset	Constituency Characteristics
Elections	Lagged DV
	Number of Registered Voters
	Votes Cast
	Alignment with Party in Power
	Reservation Status of Constituency (Scheduled Caste/Tribe)
Dataset	**Candidate Characteristics**
Affidavits	Wealth (self reported Assets)
	Liabilities (self reported)
	Education
	Age
	Member of National Party
	Member of Congress Party
	Caste
	Incumbent

2.B Sensitivity Analysis for Models Including All Charges

Given that regression discontinuity estimates are sensitive to bandwidth choice and model specification, I include multiple alternative modeling choices in the following appendices. I consistently estimate negative effects of criminality on completed projects.

2.B.1 Varying Bandwidth Selectors

Table 2.B.1: Varying Bandwidth Selectors - All Charges, Including Covariates

	Workdays				**Pay**			
	CCT	CCT 2014	IK	CV	CCT	CCT 2014	IK	CV
Conventional	-0.36	-0.36	-0.31	-0.25	-0.25	-0.23	-0.15	-0.23
	(0.25)	(0.26)	(0.29)	(0.18)	(0.27)	(0.29)	(0.35)	(0.19)
Bias-Corrected	-0.39	-0.37	-0.43	-0.43^{*}	-0.27	-0.23	-0.22	-0.28
	(0.25)	(0.26)	(0.29)	(0.18)	(0.27)	(0.29)	(0.35)	(0.19)
Robust	-0.39	-0.37	-0.43	-0.43	-0.27	-0.23	-0.22	-0.28
	(0.29)	(0.30)	(0.45)	(0.25)	(0.31)	(0.33)	(0.46)	(0.26)
Num. obs.	2679	2679	2679	2679	2678	2678	2678	2678
Eff. N Left	874	841	699	1242	917	819	635	1251
Eff. N Right	930	880	701	1344	966	847	632	1365
Eff. N Left BC	1144	1118	640	1242	1165	1146	683	1251
Eff. N Right BC	1227	1197	639	1344	1250	1228	685	1365
BW (h)	12.95	12.06	8.99	32.41	13.76	11.58	7.94	35.63
BW Bias Corr.	22.30	20.71	8.06	32.41	23.77	22.48	8.78	35.63
Order	1	1	1	1	1	1	1	1
Order Bias Corr.	2	2	2	2	2	2	2	2

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

2.B.2 Varying Local Polynomial Order

Table 2.B.2: Local Polynomials Varying Order - Non Parametric

	Log Workdays					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.41	-0.37	-0.39	-0.40	-0.36	-0.31
	(0.21)	(0.26)	(0.32)	(0.35)	(0.42)	(0.47)
Bias-Corrected	-0.39	-0.33	-0.42	-0.43	-0.34	-0.28
	(0.21)	(0.26)	(0.32)	(0.35)	(0.42)	(0.47)
Robust	-0.39	-0.33	-0.42	-0.43	-0.34	-0.28
	(0.25)	(0.29)	(0.35)	(0.37)	(0.44)	(0.49)
Num. obs.	2679	2679	2679	2679	2679	2679
Eff. N Left	812	1006	1061	1169	1151	1175
Eff. N Right	842	1058	1138	1263	1237	1271
Eff. N Left BC	1061	1165	1167	1236	1207	1223
Eff. N Right BC	1138	1254	1255	1336	1301	1321
BW	11.40	16.28	18.46	24.16	22.89	24.77
BW Bias Corr.	18.46	23.91	23.98	31.02	27.91	29.53
Order	1	2	3	4	5	6
Order Bias Corr.	2	3	4	5	6	7
Controls	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

Table 2.B.3: Local Polynomials Varying Order - Non Parametric

	Log Pay					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.33	-0.32	-0.35	-0.40	-0.48	-0.46
	(0.24)	(0.30)	(0.35)	(0.38)	(0.46)	(0.51)
Bias-Corrected	-0.31	-0.30	-0.40	-0.45	-0.48	-0.45
	(0.24)	(0.30)	(0.35)	(0.38)	(0.46)	(0.51)
Robust	-0.31	-0.30	-0.40	-0.45	-0.48	-0.45
	(0.29)	(0.34)	(0.38)	(0.41)	(0.48)	(0.54)
Num. obs.	2678	2678	2678	2678	2678	2678
Eff. N Left	783	964	1059	1163	1145	1165
Eff. N Right	804	1011	1135	1249	1228	1252
Eff. N Left BC	1033	1118	1170	1235	1201	1210
Eff. N Right BC	1098	1196	1263	1332	1290	1303
BW	10.79	15.06	18.38	23.59	22.41	23.84
BW Bias Corr.	17.54	20.69	24.26	30.83	27.19	28.17
Order	1	2	3	4	5	6
Order Bias Corr.	2	3	4	5	6	7
Controls	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

Table 2.B.4: Local Polynomials Varying Order - Non Parametric

	Log Materials					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.83^{*}	-0.88^{*}	-0.91	-0.94	-0.92	-0.83
	(0.35)	(0.42)	(0.52)	(0.59)	(0.68)	(0.76)
Bias-Corrected	-0.88^{*}	-0.85^{*}	-0.94	-0.97	-0.90	-0.82
	(0.35)	(0.42)	(0.52)	(0.59)	(0.68)	(0.76)
Robust	-0.88^{*}	-0.85	-0.94	-0.97	-0.90	-0.82
	(0.41)	(0.48)	(0.57)	(0.63)	(0.72)	(0.79)
Num. obs.	2670	2670	2670	2670	2670	2670
Eff. N Left	760	977	1042	1124	1138	1161
Eff. N Right	772	1031	1120	1218	1227	1259
Eff. N Left BC	1011	1126	1152	1197	1196	1209
Eff. N Right BC	1072	1220	1244	1297	1296	1311
BW	10.32	15.64	17.93	21.59	22.37	24.12
BW Bias Corr.	16.81	21.75	23.34	27.55	27.53	28.79
Order	1	2	3	4	5	6
Order Bias Corr.	2	3	4	5	6	7
Controls	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

Table 2.B.5: Local Polynomials Varying Order - Non Parametric

	Log Projects Completed					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.48^{**}	-0.52^{**}	-0.53^{*}	-0.56^{*}	-0.58^{*}	-0.69^{*}
	(0.16)	(0.20)	(0.22)	(0.25)	(0.28)	(0.32)
Bias-Corrected	-0.51^{**}	-0.53^{**}	-0.56^{*}	-0.59^{*}	-0.60^{*}	-0.71^{*}
	(0.16)	(0.20)	(0.22)	(0.25)	(0.28)	(0.32)
Robust	-0.51^{**}	-0.53^{*}	-0.56^{*}	-0.59^{*}	-0.60^{*}	-0.71^{*}
	(0.18)	(0.22)	(0.24)	(0.26)	(0.29)	(0.33)
Num. obs.	2679	2679	2679	2679	2679	2679
Eff. N Left	757	912	1041	1133	1170	1159
Eff. N Right	767	966	1112	1222	1264	1247
Eff. N Left BC	1017	1068	1163	1219	1234	1215
Eff. N Right BC	1074	1141	1250	1312	1332	1310
BW	10.12	13.69	17.72	21.79	24.25	23.37
BW Bias Corr.	16.83	18.64	23.60	28.97	30.61	28.49
Order	1	2	3	4	5	6
Order Bias Corr.	2	3	4	5	6	7
Controls	YES	YES	YES	YES	YES	YES
FE	YES	YES	YES	YES	YES	YES

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

2.B.3 Varying Global/Parametric Polynomials

Table 2.B.6: AIC for Parametric Polynomials (Baseline Spec, NO controls NO FE)

Polynomial Order	Log Workdays	Log Pay	Log Materials	Log Projects Completed
1	13156.58	13620.36	16096.14	12074.93
2	13150.93	13617.84	16094.36	12076.55
3	13153.17	13621.30	16094.28	12077.00
4	13156.95	13624.58	16098.26	12080.25
5	13160.90	13628.51	16102.21	12082.41
6	13164.14	13631.16	16105.28	12081.77

2.C Serious Charges

2.C.1 Serious Charges Balance Tests

Table 2.C.1: Candidate Balance Tests for Serious Charges

	Wealth	Liabilities	Age	Mem. National Party
Conventional	-0.03	0.11	-1.32	-0.09
	(0.18)	(0.79)	(1.07)	(0.05)
Bias-Corrected	-0.02	0.06	-1.01	-0.09
	(0.18)	(0.79)	(1.07)	(0.05)
Robust	-0.02	0.06	-1.01	-0.09
	(0.22)	(0.94)	(1.23)	(0.07)
Num. obs.	2504.00	2509.00	2506.00	2509.00
Eff. Num. obs. Left	693.00	703.00	704.00	721.00
Eff. Num. obs. Right	710.00	725.00	730.00	749.00
Eff. Num. obs. Left Bias Corr.	916.00	918.00	972.00	938.00
Eff. Num. obs. Right Bias Corr.	1002.00	1004.00	1085.00	1034.00
BW (h)	9.86	10.06	10.14	10.55
BW Bias Corr. (b)	16.15	16.19	18.72	17.18
Order (p)	1.00	1.00	1.00	1.00
Order Bias Corr. (q)	2.00	2.00	2.00	2.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Figure 2.C.1: Candidate characteristics Balance Tests for Serious Charges

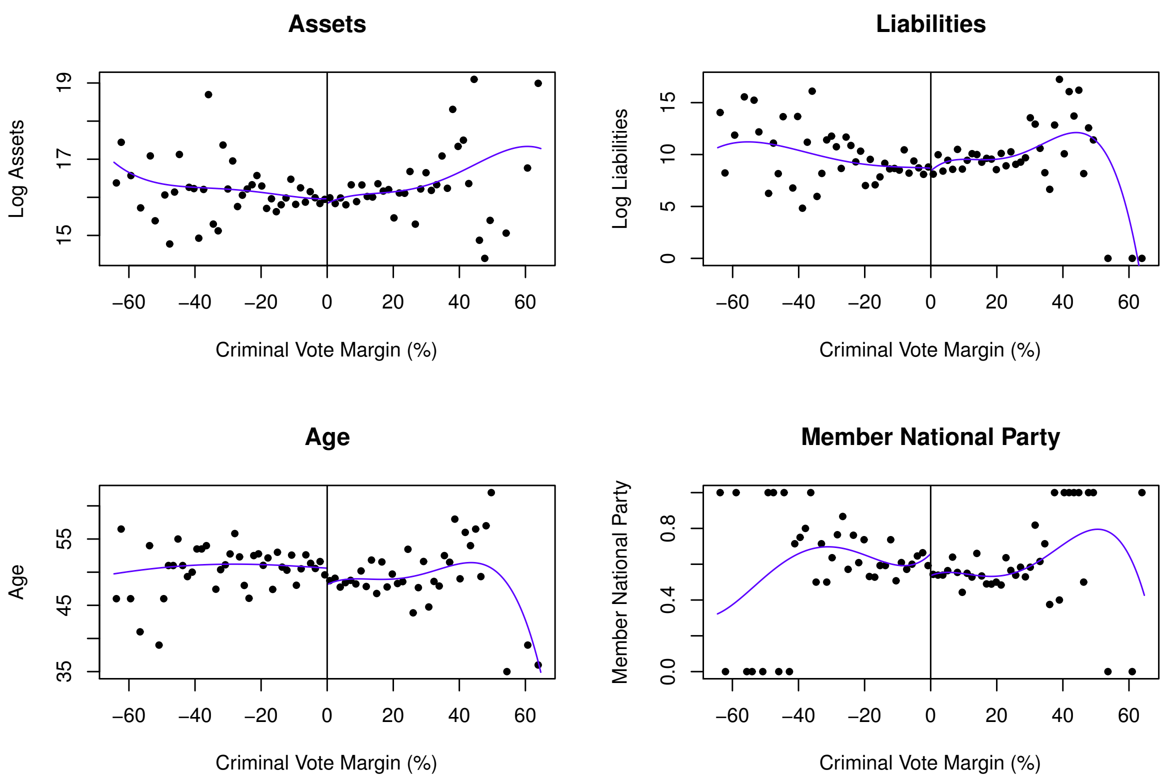
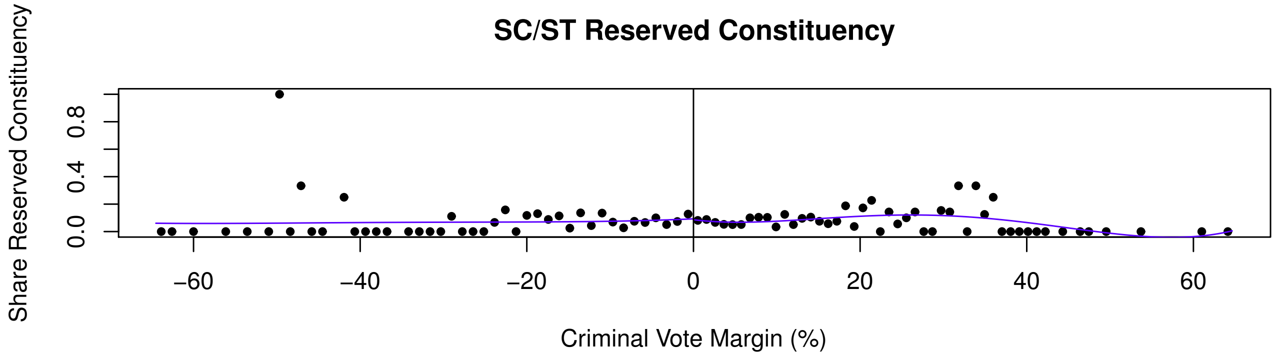


Figure 2.C.2: Constituency Balance Tests for Serious Charges



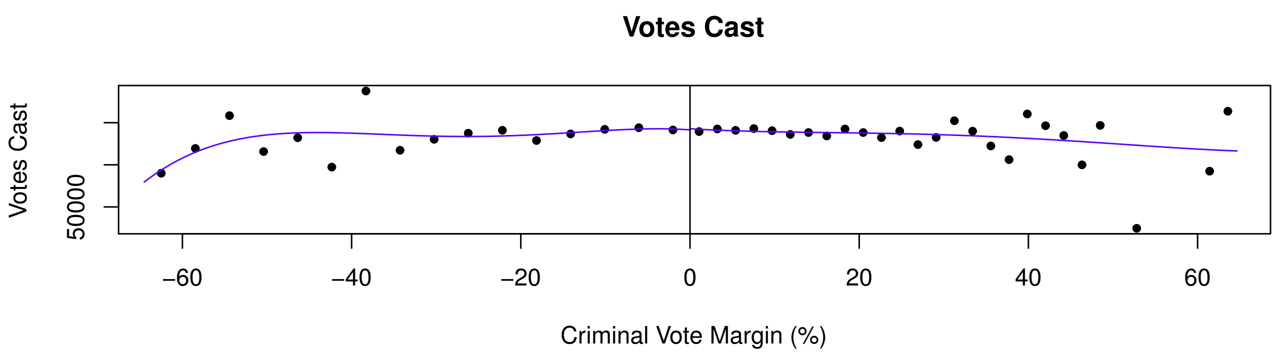


Table 2.C.2: Constituency Balance Tests for Serious Charges

	Votes Cast	Reserved
Conventional	-638.56	-0.04
	(4356.79)	(0.03)
Bias-Corrected	-739.75	-0.04
	(4356.79)	(0.03)
Robust	-739.75	-0.04
	(5114.37)	(0.04)
Num. obs.	2509.00	2509.00
Eff. Num. obs. Left	661.00	687.00
Eff. Num. obs. Right	677.00	702.00
Eff. Num. obs. Left Bias Corr.	892.00	921.00
Eff. Num. obs. Right Bias Corr.	975.00	1009.00
BW (h)	9.21	9.73
BW Bias Corr. (b)	15.42	16.33
Order (p)	1.00	1.00
Order Bias Corr. (q)	2.00	2.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.C.2 Serious Chrages Varying Polynomials for Non-parametric models

Table 2.C.3: Serious Charges- Local Polynomials Varying Order - Non Parametric

	Log Work	Log Work	Log Work	Log Work	Log Work	Log Work
Conventional	-0.49^{*}	-0.48^{*}	-0.50	-0.54	-0.56	-0.63
	(0.22)	(0.24)	(0.32)	(0.36)	(0.44)	(0.48)
Bias-Corrected	-0.50^{*}	-0.48^{*}	-0.50	-0.56	-0.55	-0.64
	(0.22)	(0.24)	(0.32)	(0.36)	(0.44)	(0.48)
Robust	-0.50	-0.48	-0.50	-0.56	-0.55	-0.64
	(0.26)	(0.27)	(0.35)	(0.38)	(0.47)	(0.50)
Num. obs.	2216.00	2216.00	2216.00	2216.00	2216.00	2216
Eff. Num. obs. Left	678.00	916.00	915.00	954.00	914.00	948
Eff. Num. obs. Right	702.00	1014.00	1012.00	1068.00	1010.00	1052
Eff. Num. obs. LBC	876.00	1012.00	991.00	1012.00	962.00	990
Eff. Num. obs. RBC	967.00	1128.00	1100.00	1130.00	1077.00	1100
BW (h)	11.33	21.12	21.05	24.92	20.97	23.92
BW Bias Corr. (b)	18.87	32.74	28.79	33.02	25.72	28.71
Order (p)	1.00	2.00	3.00	4.00	5.00	6
Order Bias Corr. (q)	2.00	3.00	4.00	5.00	6.00	7

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 2.C.4: Serious Charges- Local Polynomials Varying Order - Non Parametric

	Log Pay	Log Pay	Log Pay	Log Pay	Log Pay	Log Pay
Conventional	-0.38	-0.42	-0.42	-0.52	-0.75	-0.77
	(0.25)	(0.30)	(0.34)	(0.38)	(0.45)	(0.45)
Bias-Corrected	-0.39	-0.41	-0.43	-0.56	-0.78	-0.80
	(0.25)	(0.30)	(0.34)	(0.38)	(0.45)	(0.45)
Robust	-0.39	-0.41	-0.43	-0.56	-0.78	-0.80
	(0.30)	(0.33)	(0.36)	(0.39)	(0.47)	(0.46)
Num. obs.	2214.00	2214.00	2214.00	2214.00	2214.00	2214.00
Eff. Num. obs. Left	636.00	806.00	912.00	951.00	899.00	985.00
Eff. Num. obs. Right	645.00	869.00	1009.00	1062.00	991.00	1093.00
Eff. Num. obs. LBC.	831.00	944.00	992.00	1012.00	958.00	1019.00
Eff. Num. obs. RBC.	902.00	1047.00	1103.00	1132.00	1069.00	1145.00
BW (h)	10.13	15.67	20.94	24.49	20.12	28.28
BW Bias Corr. (b)	16.79	23.47	29.25	33.42	25.28	36.34
Order (p)	1.00	2.00	3.00	4.00	5.00	6.00
Order Bias Corr. (q)	2.00	3.00	4.00	5.00	6.00	7.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 2.C.5: Serious Charges- Local Polynomials Varying Order - Non Parametric

	Log Materials	Log Mats.	Log Mats	Log Mats	Log Mats	Log Mats
Conventional	-0.82^{*}	-0.89	-0.94	-0.91	-1.21	-1.20
	(0.40)	(0.47)	(0.65)	(0.67)	(0.90)	(0.90)
Bias-Corrected	-0.87^{*}	-0.85	-1.00	-0.94	-1.28	-1.24
	(0.40)	(0.47)	(0.65)	(0.67)	(0.90)	(0.90)
Robust	-0.87	-0.85	-1.00	-0.94	-1.28	-1.24
	(0.49)	(0.54)	(0.72)	(0.72)	(0.96)	(0.94)
Num. obs.	2212.00	2212.00	2212.00	2212.00	2212.00	2212
Eff. Num. obs. Left	642.00	849.00	846.00	967.00	912.00	991
Eff. Num. obs. Right	655.00	935.00	928.00	1080.00	1021.00	1108
Eff. Num. obs. LBC	832.00	956.00	925.00	1009.00	956.00	1015
Eff. Num. obs. RBC	906.00	1071.00	1031.00	1133.00	1072.00	1145
BW (h)	10.39	17.71	17.60	26.35	21.42	29.60
BW Bias Corr. (b)	16.93	25.36	22.23	33.42	25.45	35.73
Order (p)	1.00	2.00	3.00	4.00	5.00	6
Order Bias Corr. (q)	2.00	3.00	4.00	5.00	6.00	7

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

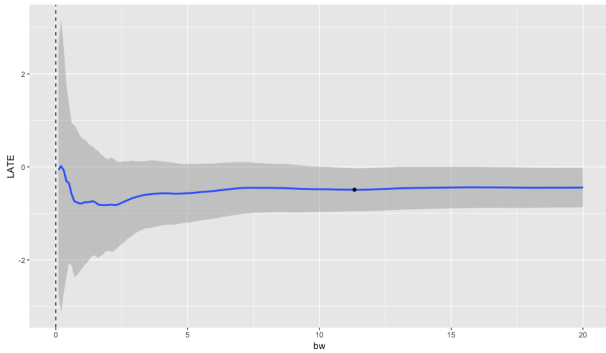
Table 2.C.6: Serious Charges- Local Polynomials Varying Order - Non Parametric

	Log Projects	Log Proj.	Log Proj.	Log Proj.	Log Proj.	Log Proj.
Conventional	-0.46^{**}	-0.53^{**}	-0.55^{*}	-0.59^{*}	-0.65^{*}	-0.70^{*}
	(0.17)	(0.20)	(0.24)	(0.27)	(0.31)	(0.32)
Bias-Corrected	-0.51^{**}	-0.54^{**}	-0.57^{*}	-0.61^{*}	-0.67^{*}	-0.73^{*}
	(0.17)	(0.20)	(0.24)	(0.27)	(0.31)	(0.32)
Robust	-0.51^{*}	-0.54^{*}	-0.57^{*}	-0.61^{*}	-0.67^{*}	-0.73^{*}
	(0.20)	(0.23)	(0.27)	(0.28)	(0.33)	(0.33)
Num. obs.	2216.00	2216.00	2216.00	2216.00	2216.00	2216
Eff. Num. obs. Left	596.00	777.00	833.00	920.00	916.00	983
Eff. Num. obs. Right	600.00	836.00	905.00	1023.00	1013.00	1093
Eff. Num. obs. LBC	809.00	909.00	933.00	988.00	970.00	1020
Eff. Num. obs. RBC	870.00	1004.00	1035.00	1096.00	1078.00	1146
BW (h)	9.16	14.64	16.88	21.68	21.11	28.01
BW Bias Corr. (b)	15.73	20.57	22.57	28.37	26.21	36.18
Order (p)	1.00	2.00	3.00	4.00	5.00	6
Order Bias Corr. (q)	2.00	3.00	4.00	5.00	6.00	7

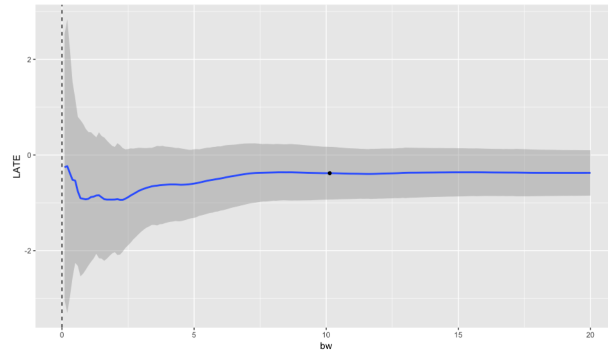
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.C.3 Serious Charges Bandwidth Sensitivity

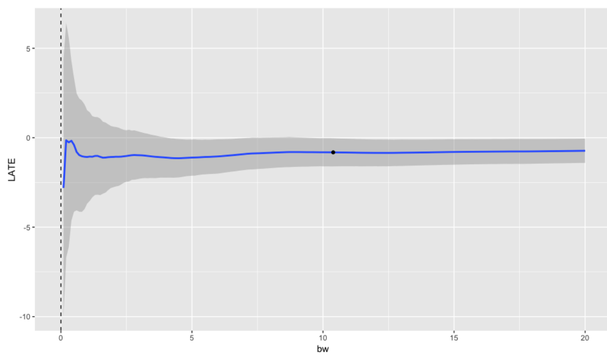
Figure 2.C.3: Serious Charges Sensitivity Analysis - LATE for Varying Bandwidths- Baseline with Fixed Effects and RDRobust data driven BWS



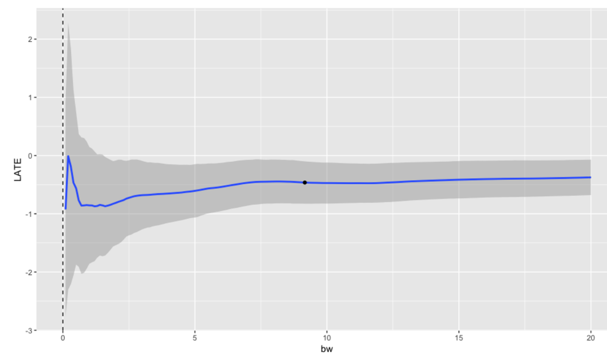
(a) Log Workdays



(b) Log Pay



(c) Log Materials

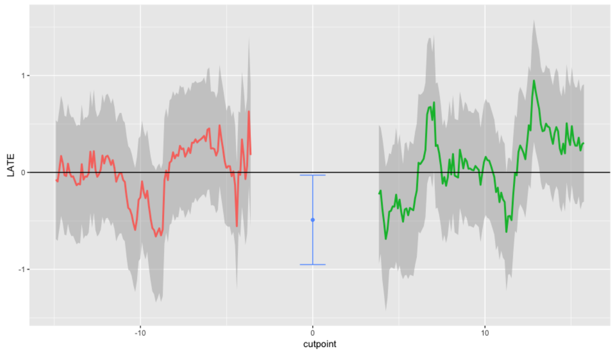


(d) Log Projects Completed

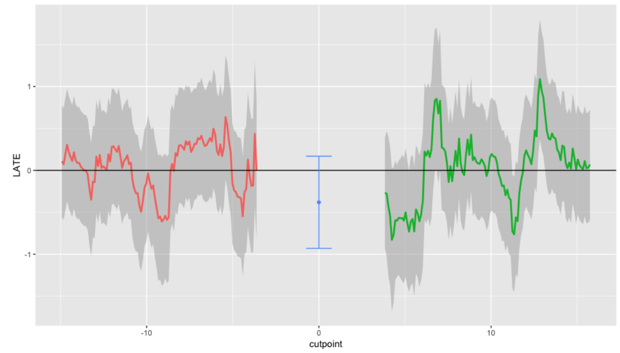
Note that RD estimates are non-parametric linear polynomials from the RDDTools package using the data-driven bandwidth selector from the RDRobust package. This leads to slightly different standard errors than those calculated under the RDrobust package (e.g. Tables above). However the point estimates remain the same.

2.C.4 Serious Charges Placebo Tests

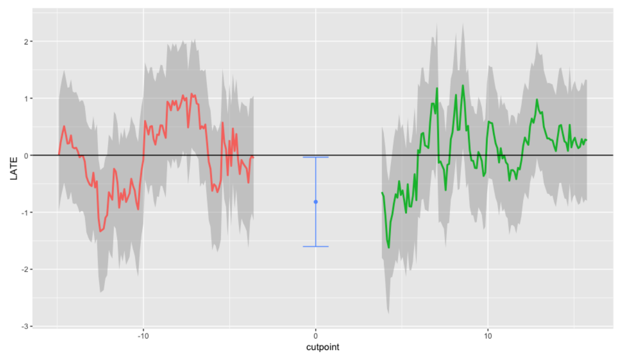
Figure 2.C.4: Serious Charges Placebo Tests - LATE for Varying Cutpoints- Baseline with Fixed Effects and RDRobust data driven BWS



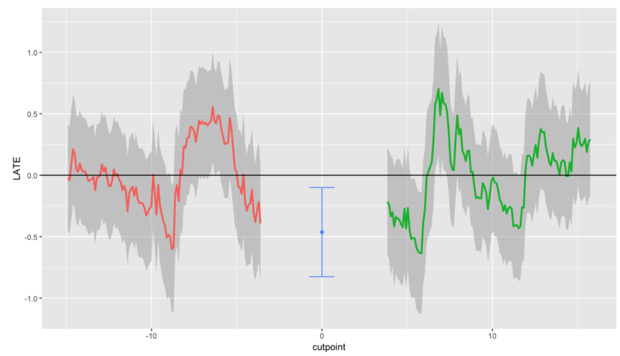
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Projects Completed

Note that RD estimates are non-parametric linear polynomials from the RDDTools package using the data-driven bandwidth selector from the RDRobust package. This leads to slightly different standard errors than those calculated under the RDrobust package (e.g. Tables above). However the point estimates remain the same.

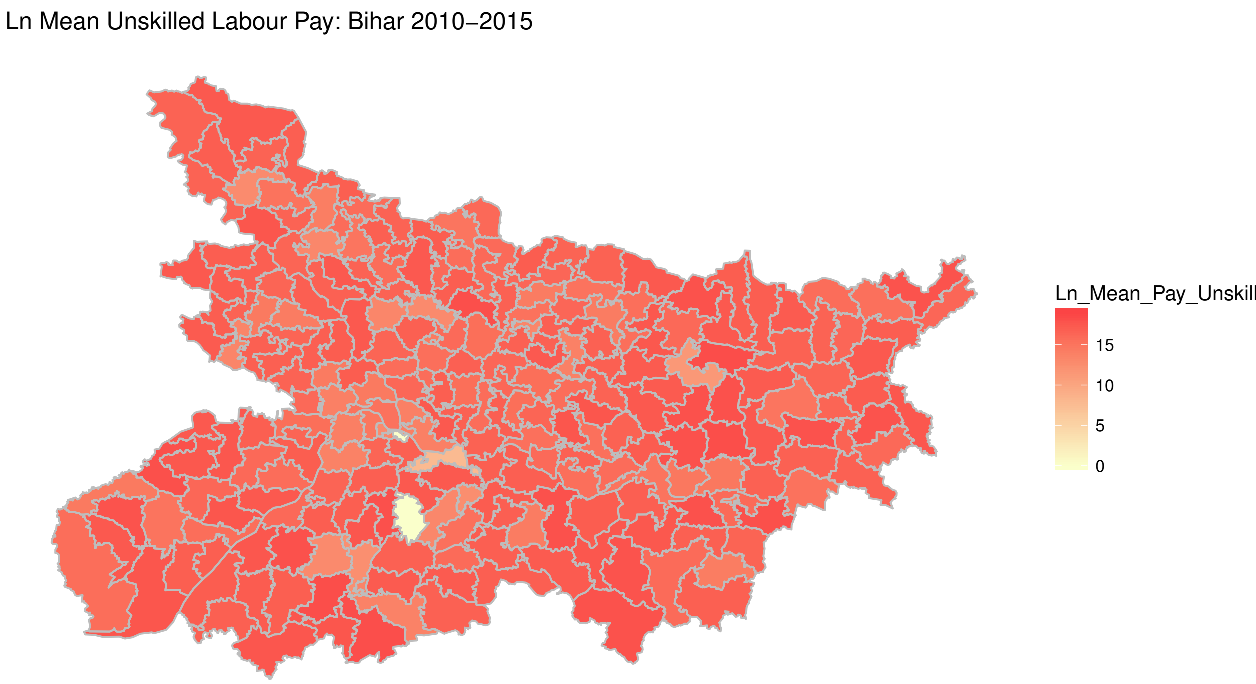
2.D State-Years in RD Sample

Table 2.D.1: State Legislative Elections in RD Sample

	State	# Constituencies	Election Year
1	andhra pradesh	169	2009, 2014
2	arunachal pradesh	12	2004, 2009, 2014
3	assam	64	2006, 2011, 2016
4	bihar	273	2005, 2010, 2015
5	chhattisgarh	38	2008, 2013
6	delhi	88	2008, 2013, 2015
7	goa	23	2007, 2012
8	gujarat	104	2007, 2012
9	haryana	73	2005, 2009, 2014
10	himachal pradesh	49	2007, 2012
11	jammu kashmir	15	2008, 2014
12	jharkhand	96	2005, 2009, 2014
13	karnataka	131	2008, 2013
14	kerala	202	2006, 2011, 2016
15	madhya pradesh	126	2008, 2013
16	maharashtra	384	2004, 2009, 2014
17	manipur	5	2007, 2012
18	meghalaya	5	2008, 2013
19	mizoram	6	2008, 2013
20	nagaland	3	2008, 2013
21	odisha	138	2004, 2009, 2014
22	puducherry	29	2006, 2011, 2016
23	punjab	66	2007, 2012
24	rajasthan	90	2008, 2013
25	sikkim	14	2009, 2014
26	tamil nadu	237	2006, 2011, 2016
27	tripura	16	2008, 2013
28	uttar pradesh	338	2007, 2012
29	uttarakhand	35	2007, 2012
30	west bengal	324	2006, 2011, 2016

2.E Maps

Figure 2.E.1: Variation in Pay across Bihar Assembly Constituencies



2.F Unlogged Estimates

Table 2.F.1: RD Robust

	Workdays	Pay	Materials	Projects Comp.
Conventional	-241463.62	-11542589.81	-6656203.51	-360.47
	(135792.14)	(13349696.42)	(6042734.37)	(221.94)
Bias-Corrected	-270744.86^{*}	-11293911.30	-6828284.92	-402.32
	(135792.14)	(13349696.42)	(6042734.37)	(221.94)
Robust	-270744.86	-11293911.30	-6828284.92	-402.32
	(159048.89)	(15729776.77)	(7039976.62)	(264.23)
Num. obs.	2679.00	2678.00	2670.00	2679.00
Eff. Num. obs. Left	734.00	767.00	844.00	828.00
Eff. Num. obs. Right	739.00	775.00	890.00	862.00
Eff. Num. obs. LBC	1001.00	1012.00	1103.00	1080.00
Eff. Num. obs. RBC	1053.00	1065.00	1181.00	1159.00
BW (h)	9.66	10.36	12.28	11.79
BW Bias Corr. (b)	16.07	16.66	20.33	19.26
Order (p)	1.00	1.00	1.00	1.00
Order Bias Corr. (q)	2.00	2.00	2.00	2.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

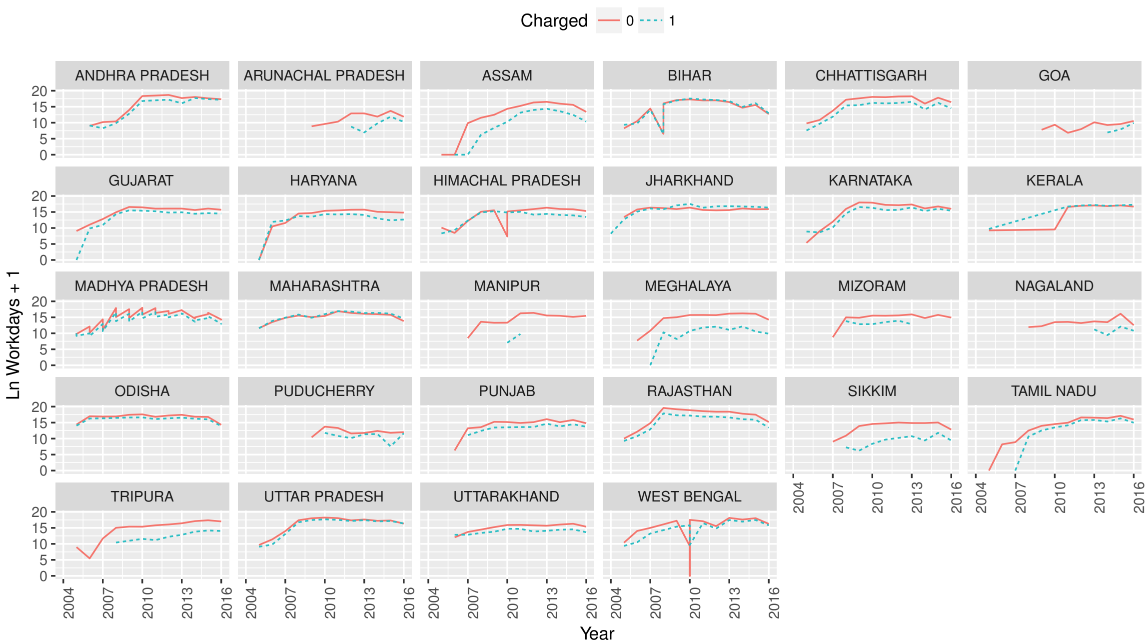
BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

2.G Variation in NREGS Outcomes by State and MLA type

I replicate state level variation in NREGS performance in Figures 1 through 4. These plots aggregate each NREGS outcome to the state-level, depending on if the sitting MLA in a given constituency faced one or more criminal charges (blue dashed line) or was uncharged (red solid line). Consistent with other studies, the plots show a general increase over time in program expenditures and project completion (Sukhtankar 2016). Interestingly in the raw data, clean politicians (red line) consistently outperform charged politicians (blue line) in NREGS delivery.[44] There are a few notable exceptions to this overall trend. Constituencies that elect criminally charged politicians seem to fare as well, if not better, in Bihar, Jharkhand, Uttar Pradesh, Maharashtra and Kerala. The first three states are known for their NREGS underperformance and abundance of criminally charged politicians. However, Kerala is somewhat of the odd state out, having the highest human development of any Indian state.

[44]This data is for the entire sample and is not restricted to mixed elections a la the RD sample. I also include all charges and do not restrict the definition of a charge to those of only a serious nature.

Figure 2.G.1: Variation in Workdays by State and Criminal status of MLA



In order to highlight the disparity between accused and unaccused politicians these plots do not divide outcomes by population. As such, populous states like Bihar and Uttar Pradesh seem to perform better than they would on a per capita basis. The logarithmic scale also flattens variation between states.

Figure 2.G.2: Variation in Pay by State and Criminal status of MLA

In order to highlight the disparity between accused and unaccused politicians these plots do not divide outcomes by population. As such, populous states like Bihar and Uttar Pradesh seem to perform better than they would on a per capita basis. The logarithmic scale also flattens variation between states.

Figure 2.G.3: Variation in Materials Expenditure by State and Criminal status of MLA

In order to highlight the disparity between accused and unaccused politicians these plots do not divide outcomes by population. As such, populous states like Bihar and Uttar Pradesh seem to perform better than they would on a per capita basis. The logarithmic scale also flattens variation between states.

Figure 2.G.4: Varation in Projects completed by State and Criminal status of MLA

In order to highlight the disparity between accused and unaccused politicians these plots do not divide outcomes by population. As such, populous states like Bihar and Uttar Pradesh seem to perform better than they would on a per capita basis. The logarithmic scale also flattens variation between states.

2.H Estimates from Various R Packages

Table 2.H.1: RD Package

	Log Workdays	Log Pay	Log Materials	Log Projects Completed
LATE	-0.40	-0.42	-0.95	-0.63
	(0.40)	(0.48)	(0.73)	(0.35)
Half-BW	-0.40	-0.36	-0.50	-0.76
	(0.57)	(0.71)	(1.05)	(0.47)
Double-BW	-0.32	-0.15	-0.71	-0.35
	(0.30)	(0.36)	(0.51)	(0.26)
Obs LATE	769.00	679.00	790.00	611.00
Obs Half-BW	402.00	353.00	424.00	311.00
Obs Doulbe-BW	1411.00	1250.00	1441.00	1135.00
BW LATE	4.54	3.91	4.72	3.49
BW Half-BW	2.27	1.95	2.36	1.75
BW Doulbe-BW	9.07	7.82	9.44	6.99

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

Table 2.H.2: RDD Tools

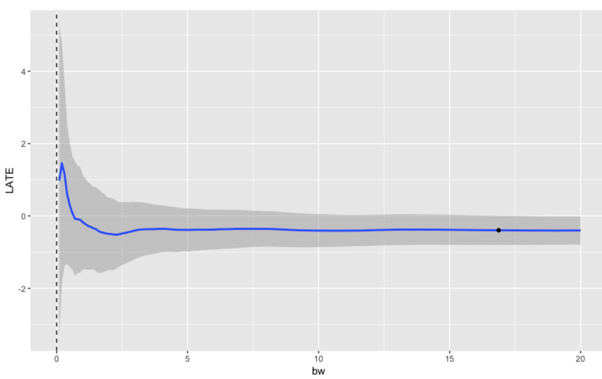
	Log Workdays	Log Pay	Log Materials	Log Projects Completed
Estimate	-0.01	-0.03	-0.06	-0.01
	(0.17)	(0.18)	(0.29)	(0.13)
No. Obs	2679.00	2678.00	2670.00	2679.00
Order	1.00	1.00	1.00	1.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

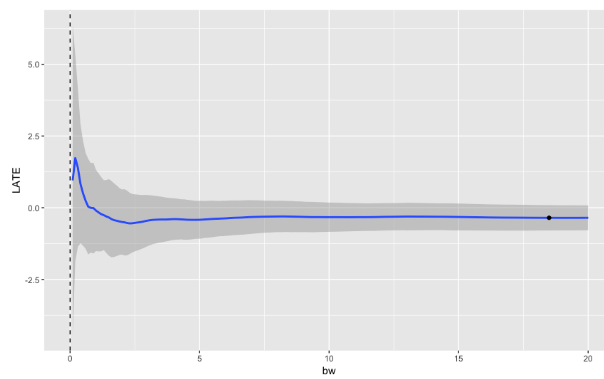
BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

2.I Estimates from Varying Bandwith Sizes

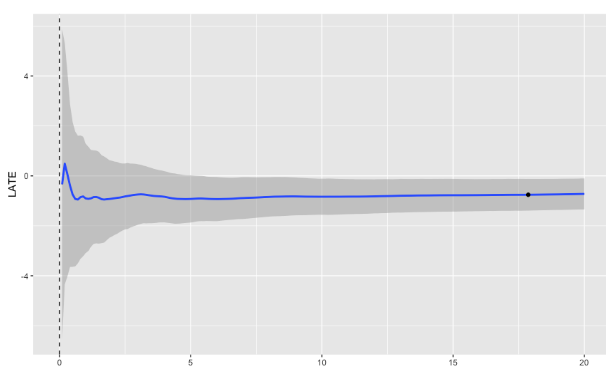
Figure 2.I.1: Sensitivity Analysis - LATE for Varying Bandwidths- Baseline with FE and rddtools



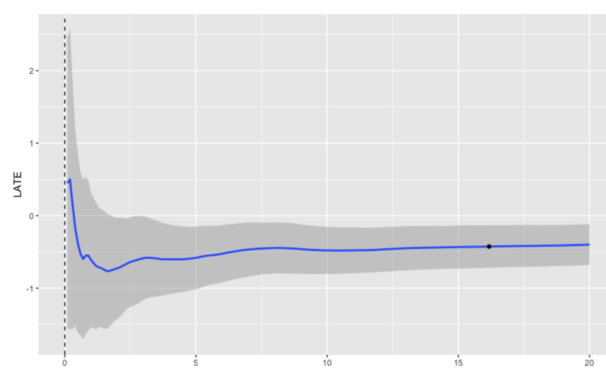
(a) Log Workdays



(b) Log Pay

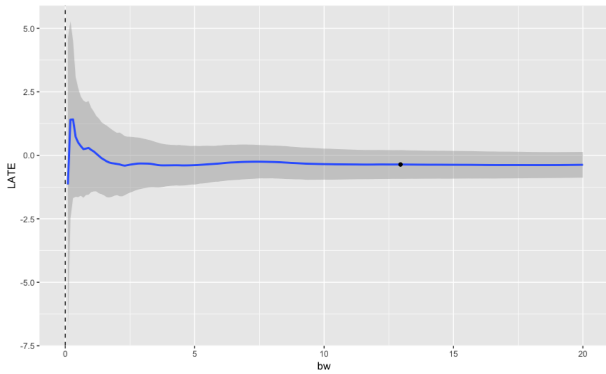


(c) Log Materials

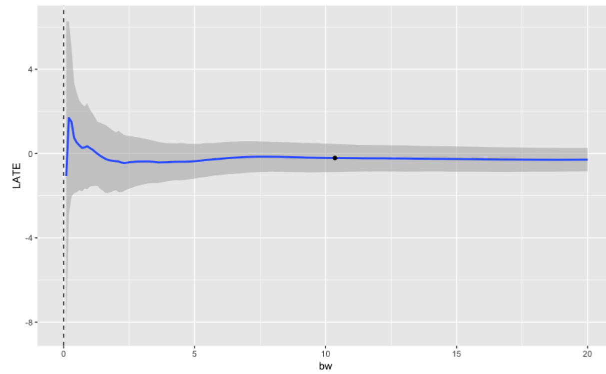


(d) Log Projects Completed

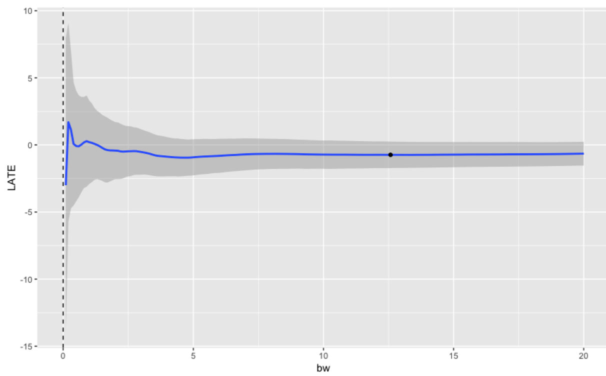
Figure 2.I.2: Sensitivity Analysis - LATE for Varying Bandwidths - Baseline with RDRobust data driven BWS



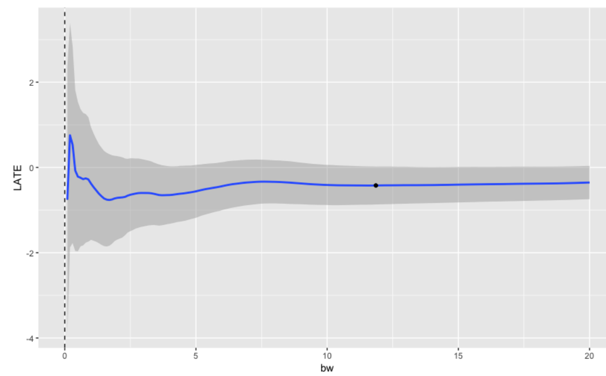
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Projects Completed

Note that RD estimates are non-parametric linear polynomials from the RDDTools package using the data-driven bandwidth selector from the RDRobust package. This leads to slightly different standard errors than those calculated under the RDrobust package (e.g. Tables above). However the point estimates remain the same.

2.J Estimates from Varying Local Polynomials

Table 2.J.1: Local Polynomials Varying Order - Non Parametric

	Log Workdays					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.36	-0.32	-0.26	-0.30	-0.33	-0.38
	(0.25)	(0.33)	(0.40)	(0.45)	(0.51)	(0.56)
Bias-Corrected	-0.39	-0.25	-0.28	-0.35	-0.32	-0.37
	(0.25)	(0.33)	(0.40)	(0.45)	(0.51)	(0.56)
Robust	-0.39	-0.25	-0.28	-0.35	-0.32	-0.37
	(0.29)	(0.37)	(0.43)	(0.47)	(0.53)	(0.58)
Num. obs.	2679	2679	2679	2679	2679	2679
Eff. N Left	874	981	1039	1115	1128	1162
Eff. N Right	930	1031	1111	1194	1217	1249
Eff. N Left BC	1144	1175	1159	1201	1198	1214
Eff. N Right BC	1227	1264	1245	1293	1284	1305
BW	12.95	15.62	17.68	20.55	21.50	23.53
BW Bias Corr.	22.30	24.40	23.34	27.30	26.55	28.36
Order	1	2	3	4	5	6
Order Bias Corr.	2	3	4	5	6	7

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

Table 2.J.2: Local Polynomials Varying Order - Non Parametric

	Log Pay					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.25	-0.17	-0.18	-0.26	-0.45	-0.53
	(0.27)	(0.38)	(0.44)	(0.49)	(0.57)	(0.64)
Bias-Corrected	-0.27	-0.11	-0.23	-0.33	-0.47	-0.53
	(0.27)	(0.38)	(0.44)	(0.49)	(0.57)	(0.64)
Robust	-0.27	-0.11	-0.23	-0.33	-0.47	-0.53
	(0.31)	(0.42)	(0.48)	(0.52)	(0.61)	(0.68)
Num. obs.	2678	2678	2678	2678	2678	2678
Eff. Num. obs. Left	917	959	1049	1139	1127	1156
Eff. Num. obs. Right	966	1007	1120	1225	1215	1241
Eff. N Left BC	1165	1128	1164	1221	1190	1206
Eff. N Right BC	1250	1216	1249	1316	1282	1297
BW (h)	13.76	14.95	17.92	22.05	21.44	23.23
BW Bias Corr.	23.77	21.48	23.64	29.27	26.11	27.58
Order	1	2	3	4	5	6
Order Bias Corr.	2	3	4	5	6	7

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 2.J.3: Local Polynomials Varying Order - Non Parametric

	Log Materials					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.74	-0.76	-0.68	-0.77	-0.79	-0.79
	(0.44)	(0.59)	(0.72)	(0.82)	(0.93)	(1.02)
Bias-Corrected	-0.86	-0.67	-0.71	-0.84	-0.79	-0.77
	(0.44)	(0.59)	(0.72)	(0.82)	(0.93)	(1.02)
Robust	-0.86	-0.67	-0.71	-0.84	-0.79	-0.77
	(0.51)	(0.66)	(0.79)	(0.87)	(0.98)	(1.07)
Num. obs.	2670	2670	2670	2670	2670	2670
Eff. N Left	854	974	1027	1107	1120	1155
Eff. N Right	912	1029	1098	1188	1215	1248
Eff. N Left BC	1157	1139	1145	1188	1189	1204
Eff. N Right BC	1249	1230	1236	1283	1283	1304
BW (h)	12.58	15.54	17.53	20.47	21.44	23.52
BW Bias Corr.	23.67	22.70	22.97	26.38	26.40	28.24
Order	1	2	3	4	5	6
Order Bias Corr.	2	3	4	5	6	7

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

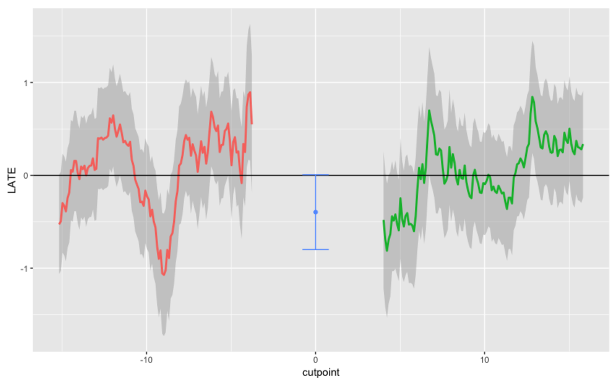
Table 2.J.4: Local Polynomials Varying Order - Non Parametric

	Log Projects Completed					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.42^{*}	-0.41	-0.39	-0.47	-0.63	-0.90^{*}
	(0.21)	(0.27)	(0.31)	(0.35)	(0.39)	(0.43)
Bias-Corrected	-0.48^{*}	-0.38	-0.41	-0.51	-0.66	-0.94^{*}
	(0.21)	(0.27)	(0.31)	(0.35)	(0.39)	(0.43)
Robust	-0.48^{*}	-0.38	-0.41	-0.51	-0.66	-0.94^{*}
	(0.24)	(0.30)	(0.34)	(0.37)	(0.41)	(0.44)
Num. obs.	2679	2679	2679	2679	2679	2679
Eff. N Left	831	927	1028	1086	1120	1131
Eff. N Right	868	981	1091	1166	1199	1219
Eff. N Left BC	1133	1103	1157	1198	1200	1205
Eff. N Right BC	1220	1177	1242	1284	1288	1298
BW	11.86	14.10	17.29	19.47	20.81	21.59
BW Bias Corr.	21.70	20.05	23.26	26.59	26.91	27.56
Order	1	2	3	4	5	6
Order Bias Corr.	2	3	4	5	6	7

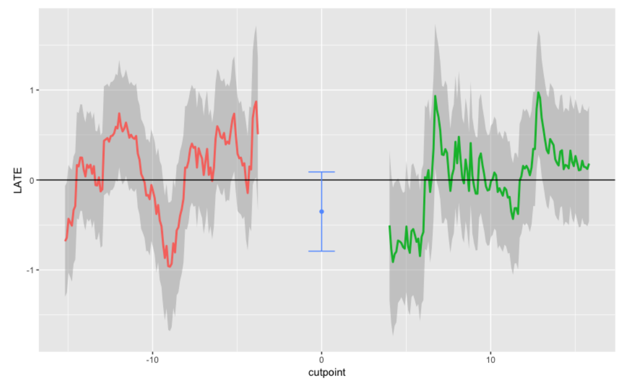
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.K Placebo Tests

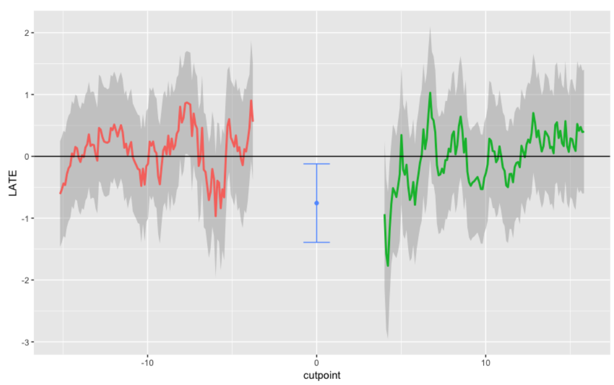
Figure 2.K.1: Placebo Tests - LATE for Varying Cutpoints- Baseline with FE and rddtools



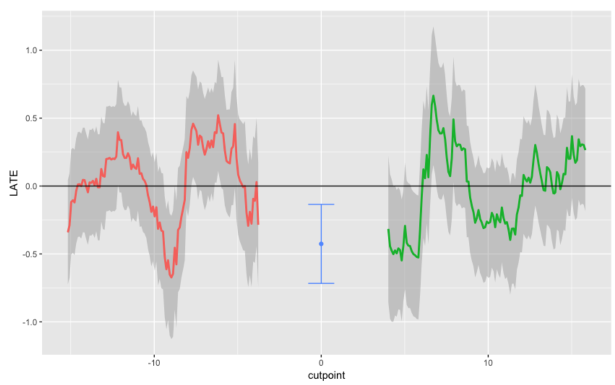
(a) Log Workdays



(b) Log Pay

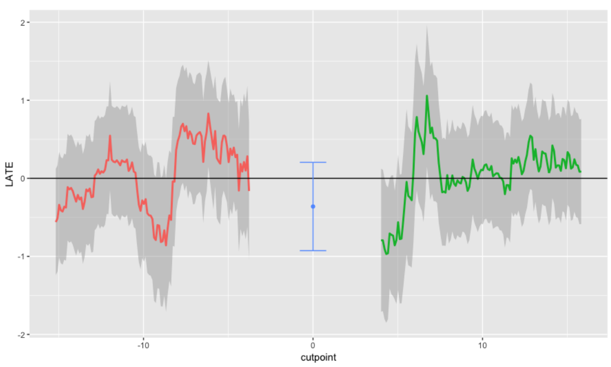


(c) Log Materials

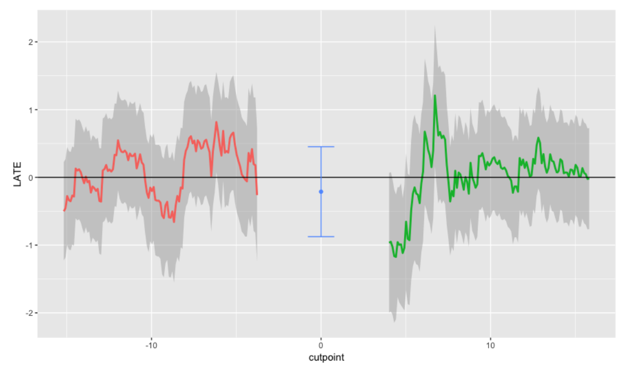


(d) Log Projects Completed

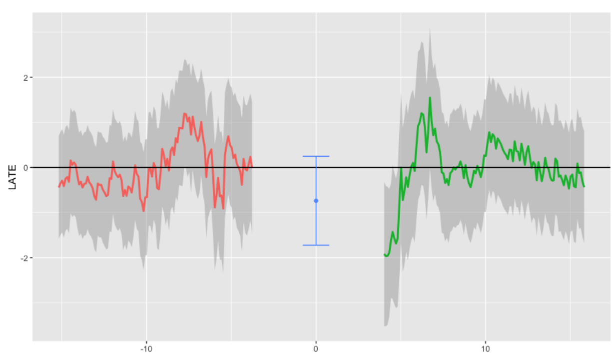
Figure 2.K.2: Placebo Tests - LATE for Varying Cutpoints- Baseline with RDRobust data driven BWS



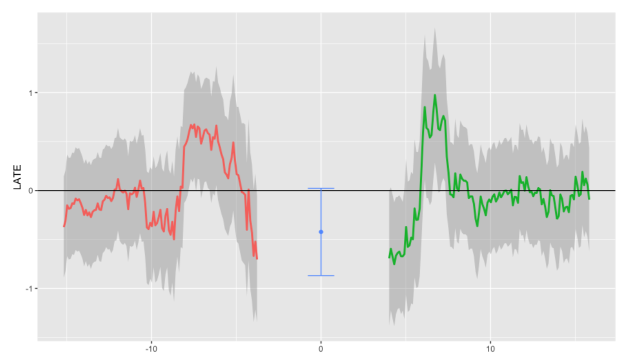
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Projects Completed

Note that RD estimates are non-parametric linear polynomials from the RDDTools package using the data-driven bandwidth selector from the RDRobust package. This leads to slightly different standard errors than those calculated under the RDrobust package (e.g. Tables above). However the point estimates remain the same.

2.L Financial Years

Table 2.L.1: Financial Years: RD Robust

	Log Workdays	Log Pay	Log Materials	Log Projects Comp.
Conventional	-0.15	0.06	-0.43	-0.15
	(0.09)	(0.17)	(0.23)	(0.09)
Bias-Corrected	-0.16	0.06	-0.46^{*}	-0.16
	(0.09)	(0.17)	(0.23)	(0.09)
Robust	-0.16	0.06	-0.46	-0.16
	(0.10)	(0.20)	(0.27)	(0.10)
Num. obs.	14664.00	14663.00	14655.00	14664.00
Eff. Num. obs. Left	4112.00	4328.00	4545.00	4112.00
Eff. Num. obs. Right	3997.00	4186.00	4459.00	3997.00
Eff. Num. obs. LBC	5412.00	5609.00	5867.00	5412.00
Eff. Num. obs. RBC	5537.00	5737.00	6125.00	5537.00
BW (h)	9.43	10.09	10.99	9.43
BW Bias Corr. (b)	14.70	15.84	17.76	14.70
Order (p)	1.00	1.00	1.00	1.00
Order Bias Corr. (q)	2.00	2.00	2.00	2.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 2.L.2: Financial Years - RD Robust

	Log Workdays			Log Pay			Log Materials			Log Projects Comp.		
Conventional	−0.07	−0.07	−0.05	0.06	0.06	0.03	−0.43	−0.43	−0.36	−0.15	−0.15	−0.19*
	(0.12)	(0.13)	(0.13)	(0.17)	(0.17)	(0.17)	(0.23)	(0.23)	(0.23)	(0.09)	(0.09)	(0.09)
Bias-Corrected	−0.07	−0.06	−0.05	0.06	0.07	0.02	−0.46*	−0.45	−0.39	−0.16	−0.17	−0.20*
	(0.12)	(0.13)	(0.13)	(0.17)	(0.17)	(0.17)	(0.23)	(0.23)	(0.23)	(0.09)	(0.09)	(0.09)
Robust	−0.07	−0.06	−0.05	0.06	0.07	0.02	−0.46	−0.45	−0.39	−0.16	−0.17	−0.20*
	(0.15)	(0.15)	(0.15)	(0.20)	(0.20)	(0.20)	(0.27)	(0.28)	(0.28)	(0.10)	(0.10)	(0.10)
Num. obs.	14664	14664	14664	14663	14663	14663	14655	14655	14655	14664	14664	14664
Eff. obs. Left	4562	4431	4392	4328	4326	4328	4545	4412	4306	4112	4104	4233
Eff. obs. Right	4491	4333	4242	4186	4177	4186	4459	4305	4177	3997	3967	4085
Eff. obs. LBC	5901	5764	5688	5609	5575	5575	5867	5683	5547	5412	5374	5550
Eff. obs. RBC	6158	5935	5846	5737	5702	5702	6125	5845	5675	5537	5518	5676
BW	11.08	10.59	10.38	10.09	10.02	10.06	10.99	10.48	10	9.43	9.32	9.78
BW Bias Corr.	17.89	17.13	16.47	15.84	15.72	15.71	17.76	16.50	15.53	14.70	14.57	15.52
Order	1	1	1	1	1	1	1	1	1	1	1	1
Order Bias Corr.	2	2	2	2	2	2	2	2	2	2	2	2

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 2.L.3: Financial Years - Unlogged - RD Robust

	Workdays	Pay	Materials	Projects Completed
Conventional	-25717.29^{*}	555401.66	-641148.02	-38.86
	(11983.44)	(1342128.86)	(717902.30)	(23.32)
Bias-Corrected	-28281.25^{*}	893645.70	-678326.66	-43.21
	(11983.44)	(1342128.86)	(717902.30)	(23.32)
Robust	-28281.25^{*}	893645.70	-678326.66	-43.21
	(13992.77)	(1541121.22)	(823311.97)	(27.94)
Num. obs.	14664	14663	14655	14664
Eff. Num. obs. Left	3805	3769	4340	4081
Eff. Num. obs. Right	3587	3539	4202	3890
Eff. Num. obs. LBC.	5109	5227	5757	5303
Eff. Num. obs. RBC.	5287	5361	5917	5448
BW (h)	8.37	8.22	10.20	9.15
BW Bias Corr. (b)	13.37	13.81	16.98	14.18
Order (p)	1	1	1	1
Order Bias Corr. (q)	2	2	2	2

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

2.M Wages and Employment per Project

While point estimates of NREGS provision are consistently negative across numerous specifications, only *Projects Completed* remains statistically significant across all of them. Thus, it could be the case that while accused MLAs complete fewer projects, they do not perform worse on metrics voters care about (namely employment and wages). However, when analyzing only serious charges I find a reduction in overall material expenditure and employment. This evidence is more consistent with a narrative that charged politicians generally underperform in providing access to NREGS. To shed some light on this, I compare employment and wages per project completed (see Table below). Do constituencies governed by a seriously accused MLA employ more workers and increase wage payments per project completed? One concern may be that certain types of projects are more costly or require more workers. However, wage rates are standardized within NREGS projects types. A technical assistant verifies the labor hours and progress made on project construction against a governmental benchmark. Therefore, for a given type of project, wages paid per day and the number of workers needed should be similar.[45]

The table below demonstrates that projects completed in seriously accused constituencies witness higher levels of employment and pay per project. Since I do not observe actual hours worked or if wages reach NREGS laborers, I can not adjudicate between whether this indicates improved worker outcomes or increased leakage. In other words, more workdays and higher labor expenditures could represent over-reporting or ghost-workers, with the excess rents captured by bureaucrats and/or politicians. An alternative explanation could be that constituencies governed by a seriously accused MLA happen to engage in more expensive or difficult projects. However, under the regression discontinuity design project type should not systematically vary with the criminal status of the MLA.[46] Finally, the results are only statistically significant in the model adding fixed effects and controls. However, this does not result from a reduction in standard errors but instead a dramatic increase in the size of point estimates. The large jump in coefficient size and simultaneous increase in standard errors, after adding controls, are indicative of model misspecification. Given these caveats, I take the results of this analysis as minimally suggestive and exploratory for now. I investigate alternative, qualitatively informed, measures of NREGS corruption in the subsequent section.

[45]While this is the formal vetting process, as noted above the ground level experience of NREGS can diverge dramatically from the formal process.

[46]I plan to test this more formally by checking for balance across project types in accused and unaccused constituencies.

Table 2.M.1: Serious Charges Log Pay and Work per Project

	$\frac{LnWorkdays}{LnProject}$		$\frac{LnPay}{LnProject}$	
Conventional	0.01	0.28^{*}	0.18	0.59^{*}
	(0.14)	(0.12)	(0.18)	(0.23)
Bias-Corrected	0.04	0.31^{*}	0.24	0.66^{**}
	(0.14)	(0.12)	(0.18)	(0.23)
Robust	0.04	0.31^{*}	0.24	0.66^{*}
	(0.16)	(0.14)	(0.20)	(0.26)
Num. obs.	2221	2216	2219	2214
Eff. Num. obs. Left	591	583	602	584
Eff. Num. obs. Right	595	590	606	591
Eff. Num. obs. Left Bias Corr.	834	826	877	830
Eff. Num. obs. Right Bias Corr.	906	894	970	899
BW (h)	8.96	8.89	9.28	8.91
BW Bias Corr. (b)	16.74	16.49	18.94	16.72
Order (p)	1	1	1	1
Order Bias Corr. (q)	2	2	2	2

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

For the models including fixed effects, outcomes are the residuals after controlling for state and election year. BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of the threshold. The Bias Corrected estimates try to measure and remove the bias introduced by the polynomial estimation of the true regression function (Cattaneo et al. 2018). BW Bias Corr. gives the bandwidth for the bias corrected estimate which also changes given the new bias corrected estimate. Order and Order Bias Corr. provide the polynomial order for the regression on either side of the threshold.

Chapter 3

Criminal Crosshairs: Do Criminal Politicians Target Co-partisans?

3.1 Introduction

One explanation for criminal politicians' continued electability is their ability to connect voters to services. Voter surveys and informal interviews suggest that criminal politicians are elected because "they get things done" (Vaishnav, 2017). However, what it means to "get things done," remains bundled, nebulous and empirically unverified. Generally, getting things done refers to constituency service and benefit delivery (Vaishnav (2017), author's interview). As Michelutti (2014) puts it, criminal politician's core competencies are to "protect and provide" (Michelutti, 2014). By combining violence with generous service delivery, criminals leverage their Machiavellian attributes to "defend and represent the community" (Hansen, 2005). To take one example, consider Pappu Yadav a well known and well liked criminal Member of Parliament from Bihar. Pappu uses his home in Delhi to house over 100 migrants from Bihar, often making grand, public displays of his own generosity.[1] As an MLA in Bihar, Pappu mobilized muscular and political networks to pressure public servants from doctors to District Magistrates to provide for his constituents.[2] In this

[1] Papu has decades of accolades detailing how he turned muscle into constituency service.Interview with reporter in Patna, Bihar. Author's interviews.

[2] Interview with reporter in Patna.

chapter, I look specifically at criminal politicians' ability to deliver government benefits to their supporters.[3] Are criminal politicians the robin hoods of Bollywood legend, protecting constituents from an ineffective and indifferent bureaucracy?

On balance, the empirical evidence suggests criminals do not "get things done" by improving general welfare or economic development (Chemin, 2012; Prakash et al., 2019).[4] In the previous chapter, I find little evidence that criminal politicians' prowess extends to government service delivery. If anything, criminal politicians under-provide state resources and are linked to increased rent extraction (Chapter 2; Asher and Novosad, 2016). Why then are criminals consistently referred to as effective politicians, when the empirical record finds perverse impacts post-election? One oversight of empirical tests to date is that they measure performance at the aggregate constituency-level, while MLAs compete in single member districts. Perhaps criminals' comparative advantage rests on targeting core voters who push them to plurality. In other words, criminal MLAs do not need to appease every constituent. Instead, developing a core base of supporters can be sufficient- alongside party and caste votes- to consistently win elections. Do criminal politicians privilege their own?

I take up this question using granular geotagged data on India's National Rural Employment Guarantee Scheme. By matching the delivery of NREGS projects and benefits to polling stations I can adjudicate if criminal Members of the Legislative Assembly are better at delivering to core supporters relative to clean politicians.

NREGS provides a useful setting to analyze whether criminal MLAs deliver state resources to their supporters. First, despite ostensibly being a demand driven program, there is consistent evidence of NREGS rationing (Sukhtankar et al., 2016), with the degree of rationing varying across states (Imbert and Papp, 2011). Second, NREGS is highly politicized with resources distributed

[3]I leave testing criminal politicians' provision of constituency service outside of government benefits for chapter 4.

[4]For a qualitative example, consider a BJP party-worker in Bihar noting that the local MP was far more connected than the local criminal MLA and could bring larger infrastructure projects to his village like a recently installed transformer. Arun Yadav, the criminal MLA did not have this kind of clout.

via partisan channels (Dunning and Nilekani, 2013; Dasgupta, 2016; Das, 2015). Third, the Indian government pushed digitization and transparency initiatives resulting in granular, geotagged data across the sub-continent. Finally, Members of the Legislative Assembly serve as linchpins in the allocation of projects and access to program benefits. In short, NREGS provides a platform for comparing precise targeting within a program MLAs can manipulate.

If targeting occurs on the basis of partisan ties, it's feasible that criminal politicians are better equipped to efficiently target state resources. For instance, Vaishnav (2017) finds that parties are more likely to field criminal politicians in constituencies where ethnic divisions are salient and therefore a criminal politicians' capacity to protect and provide may carry more weight. Drawing on previous literature and fieldwork, I identify and systematically test three unique assets that could explain criminal politicians' targeting advantage: *Money*, *Muscle* and *Networks*. Criminal politicians tend to be wealthier, on average, and may leverage these assets to bribe bureaucrats or otherwise push NREGS resources to their supporters. Second, criminals' reputation for violence can secure fealty from bureaucrats and brokers facilitating access to NREGS resources. By kicking a sclerotic bureaucracy into gear and minimizing local leakage, criminals can efficiently maximize their targeting efforts. Finally, I argue that criminal politicians' dominance over the local, illegal and legal economy creates dense political networks. By remaining rooted in local communities instead of seeking fortunes in urban areas, criminal politicians nurture networks of economic and political dependence. Theoretically, given these close communal ties, criminal politicians should be better situated to understand- and then meet- supporters' needs.

This chapter provides a critical test of criminal politicians as "community warriors" (Vaishnav, 2017). As Vaishnav recounts, criminal politicians are seen as parochial protectors providing for their own in-groups. In turn, the community warrior hypothesis suggests that criminal politicians may not deliver better governance overall, but are particularly effective in bringing particularistic goods to their fiefdoms. To translate, criminal politicians should be more effective at targeting co-partisans. Previous studies focused on constituency wide results of consumption (Chemin, 2012), economic development (Prakash et al., 2019), or broad public goods (Prakash et al., 2019). Alternatively, by considering within constituency targeting of a key anti-poverty program this study directly tests qualitative theories of criminal politicians as sons of the soil providing for their communities.

By aligning data and theory I overcome several shortcomings of prior studies. First, if criminal politicians are parochial protectors this suggests testing distributional consequences and not aggregate governance outcomes. Second, co-partisan targeting aligns with MLAs re-election incentives. Members of the Legislative Assembly are elected in single-member districts in highly competitive elections[5] requiring a bare plurality. Within this institutional framework, targeting state resources to a core stable of supporters may be an efficient path to victory, rather than undertaking broad development work for the constituency at large. Third, mapping micro-level NREGS data to polling station results measures outcomes and political support at the appropriate unit of analysis, the village. NREGS is a program that voters care about and MLAs can influence by targeting villages. Whereas, constituency wide development indicators are noisy measurements of MLAs political effort and effective governance.

3.1.1 Primary Results

Overall, criminal MLAs increase the total provision of NREGS projects, employment and wages to a typical polling station. In addition, I find evidence that criminals are better at targeting NREGS to core supporters. At competitive polling stations, Criminal MLAs are predicted to deliver 2.8 more NREGS projects than clean politicians (a 25% increase compared to typical polling stations). However, in core areas where an MLA wins the polling station by more than 25%, Criminals are expected to deliver 3.6 more projects, on average (a 33% increase compared to typical polling stations). In other words, while criminal politicians are expected to deliver more NREGS resources independent of political competition, they outperform clean politicians by greater margins in core areas of support.

[5] Average margin of victory in my sample is 7%

This evidence is consistent with criminal MLAs targeting core areas where supporters are readily identifiable and and geographically concentrated- reducing the chance of benefits spilling over to opposition voters. Recently, Bussell (2019) argues that local, group-based goods increase the likelihood of targeting since the marginal returns to political effort are larger relative to helping individuals. Similarly, Andrew Harris and Posner (2019) argue that in order to minimize spillovers, politicians target goods when supporters are geographically separated from opposition voters. Given MLAs imperfect control over who is employed on an individual project, they may be more likely to allocate effort to projects located in villages with near universal support. In these villages, both project and employment benefits are likely to accrue to supporters, maximizing benefits to partisans and minimizing spillover to non-partisans.

3.1.2 Mechanisms Investigation

To understand why criminal politicians may be associated with an increase in NREGS delivery, I investigate three mechanism-driven hypotheses (*money, muscle and networks*). Using data on the personal assets of each MLA, I test if wealthy criminal politicians drive the delivery and targeting of NREGS benefits. However, I find a precisely estimated null association between wealth and NREGS targeting. Moreover, criminal politicians advantage in *delivering* NREGS is *decreasing* in wealth. In other words, criminal politicians' deep pockets do not explain their predicted positive NREGS performance. Second, to test the muscle hypothesis, I disaggregate criminality based on the nature of politicians' charges. Criminals' facing violent charges systematically predict improved NREGS delivery. In sum, money may by you a seat at the electoral table, but to get things done, muscle matters.

I find additional support for the networks hypothesis. Core villages ruled by criminals disproportionately benefit form increased NREGS projects and expenditures. Superior NREGS targeting in core areas, perhaps somewhat tautologically, is consistent with identifiable and efficient supporter networks. To understand who benefits most from these extra resources, I drill down one step further and tease apart supporter networks. Leveraging granular information on individual NREGS projects I shed light on whether excess funds flow primarily to voters or local contractors. On one hand, MLAs may target voters directly with larger projects that increase employment and wage payments. On the other, MLAs may maximize a projects material expenses to increase the available rents for local politicians and contractors (Das and Maiorano, 2019a).[6] In exchange, brokers can reward MLAs with votes and/or kickbacks.[7]

As noted in the primary results, more *total* NREGS resources are allocated to core villages. However, on a per project basis, NREGS spending does not differ based on polling station political support nor by the criminal status of the MLA. NREGS projects are fundamentally similar on observable funding dimensions across criminal and clean constituencies. That is, criminal politicians do not disproportionately reward core villages by allocating larger, or more expensive, projects in these areas. Projects located in core villages do not employ more voters or pay out larger wages. Nor do core projects reward brokers by increasing material expenditures. Criminals simply award more projects to core areas than clean politicians.

3.1.3 Overview of Empirical Strategy

To increase external validity and explore heterogeneity in NREGS targeting, I include six Indian states and nine state elections.[8] My analysis is constrained to the six states where I can identify

[6]This strategy is inspired by Das and Maiorano (2019) who find little evidence of partisan targeting of wages, but increased material costs for projects in core areas. However, their analysis does not align the relevant political unit (villages) MLAs would target with NREGS spending. Instead, Das and Maiorano (2019) analyze constituency-level outcomes and thus focus on whether the Indian National Party's party-wide strategy targets swing or core constituencies across the state. Das and Maiorano (2019) is also limited to Andhra Pradesh, generally seen as one of the better functioning states where political influence may be curtailed since citizens know their rights and social audits are more routinely employed. Most NREGS study suffer from a similar ecological fallacy. At the other end of the spectrum, studies that use survey data at the individual-level tend to focus on co-partisan ties between village politicians and NREGS recipients. And, thus do not account for the role that MLAs play in altering NREGS distribution *prior* to funding reaching the village. My study overcomes these problems by directly mapping individual NREGS projects to villages that MLAs would consider as the relevant political unit for targeting.

[7]Graft has increasingly turned towards the material expenses on NREGS projects since wages are paid directly towards workers bank accounts (Jenkins and Manor, 2017). Hiring local contractors to provide the materials is also one way to reward local elites in the MLAs' network. In return, MLAs may receive campaign financing in the run up to elections (Sukhtankar and Vaishnav, 2014; Das and Maiorano, 2019a; Jenkins and Manor, 2017). Targeting brokers has the added appeal of easing the monitoring problem of this clientelistic exchange, assuming brokers are effective in delivering votes.

[8]See Table 3.A.1 in Appendix 3.A.

polling station geo-locations (Assam, Himachal Pradesh, Kerala, Tamil Nadu, Uttar Pradesh, West Bengal). Despite this limitation, my sample covers over 400,000 polling stations and 494 million citizens (36% of India's population).[9] Overall, my "convenience" sample still captures heterogeneity in wealth, public goods provision, geography, NREGS outcomes and political predictors. For example, Kerala and Tamil Nadu rank above the median in GDP per capita whereas Uttar Pradesh and Assam rank near the bottom. Forty-six percent of Uttar Pradesh MLAs faced declared criminal cases, whereas only 11% of Assams' politicians reported charges. Furthermore, I capture political and regional variation across the sample. During the study period from 2007 to 2017, the national BJP party gained strongholds in the northeastern states of Uttar Pradesh and Assam. Whereas regional parties dominated politics in Tamil Nadu and West Bengal to the South and West. At the same time, all MLAs compete in single member district plurality elections. Thus, I can explore variation in targeting under consistent electoral rules.

Given the likely confounding between political support and allocation of NREGS projects, I undertake several methodological precautions to minimize misspecification and researcher degrees of freedom. First, I map polling stations and NREGS projects to census village shapefiles. This allows me to adjust for demographic predictors of NREGS demand. NREGS guarantees a local minimum wage for hard manual labor, which encourages self-targeting. Program demand is strongest among households that are poorer, less educated or members of marginalized scheduled caste and scheduled tribes (Dutta et al., 2014). Second, I demonstrate that OLS results predicting partisan targeting of NREGS are highly sensitive to model choices. Using a "multi-verse"[10] approach, I test for partisan targeting by criminal MLAs across 288 different OLS models. Depending on model choices, the data are consistent with both large positive and negative effects of criminality on NREGS provision. Point estimates range from a 1% to 280% increase in NREGS wages provided in criminal constituencies relative to clean constituencies. To address this model

[9]Polling station results are dropped for urban areas as NREGS is only allocated to rural villages. I discuss polling station data construction in further detail in the data description and Appendix 3.D.

[10]i.e. Analyzing variation across alternative reasonable data coding and modeling choices (see Steegen et al. (2016) for a detailed description of multi-verse analysis).

dependency, I adopt a machine learning approach, testing predictions on a random sample of, unseen holdout data. In effect, I run a self replication on new data and compare the machine learning models' predictive power for NREGS delivery after including or excluding the variable indicating MLAs' criminal status.[11] Specifically, I use Kernel Regularized Least Squares on the randomly selected training and testing data. KRLS is helpful as it regularizes coefficients, reducing overfitting while still allowing for a flexible model not constrained to standard assumptions of linearity. Given that my main hypothesis is an interaction between the criminal status of the MLA and the co-partisanship of a polling station, KRLS represents a natural choice for data driven detection of interactions while providing interpretable results (Hainmueller and Hazlett, 2014).

3.2 Context: NREGS Targeting and Criminal Politicians in India

3.2.1 MLAs and NREGS Targeting

Before examining criminals' unique targeting advantages, I first outline how MLAs of all stripes can intervene in NREGS disbursal. Members of the Legislative Assembly hold both formal and informal levers of control over NREGS fund flows. MLAs can use these powers to bend the bureaucracy to their desires.

First, and most formally, MLAs sit on the the block council.[12] The block council both approves the NREGS labor budget and monitors project implementation (MoRD 2019).[13] [14]

[11]This allows an estimate of how well the model generalizes to new, unseen data (i.e. the generalization error, (Cranmer and Desmarais, 2017). If the model fits poorly on the testing data this indicates likely misspecification or that the model may be missing some crucial part of the data generating process. In addition machine learning helps minimize overfitting that could occur if I only considered how well the model performed on training data (as is common in the literature).

[12]The block is the middle tier in India's bureaucracy and government, in-between larger districts and lower-level villages.

[13]NREGS projects and labor requests originate in village meetings. Gram-panchayats submit their villages' prioritized project list to an NREGA Block Programme Officer for scrutiny and technical certification. Block Programme Officers consolidate all village pancahayat project requests into a single block plan (MoRD 2014). See `https://nrega.nic.in/Circular_Archive/archive/Roles_responsibilites.pdf`. After all projects in the block plan are certified by the technical department, the plan is forwarded to the block council for final approval. The approved block plan is subsequently passed up the bureaucratic ladder to the district, which bundles block plans into an overarching district plan for state certification. Only after each tier of the bureaucratic hierarchy approves the bundled project plans does money flow back down the bureaucratic chain.

[14]`https://nrega.nic.in/Netnrega/WriteReaddata/Circulars/2390Annual_Master_`

MLAs block council seat and subsequent relationships with block bureaucrats provides one entry point into controlling NREGS outcomes. Despite the block representing a middle tier in India's bureaucracy and governance, blocks and block bureaucrats hold immense power over the approval of NREGS projects and therefore the disbursal of program funding (Jenkins and Manor 2017).[15] For instance, Block Development Officers (BDOs)[16] act as "veto players" whose signature is required before engineers and other officials can begin work on NREGS projects. BDOs and other block bureaucrats are ultimately responsible for measuring, auditing, certifying and disbursing project funds (Jenkins and Manor, 2017).[17]

Thus, BDOs occupy a sweet spot- close enough to the village to interact with individual project backers but powerful enough to veto any project before it can even get off the ground.[18] For example, one of the villages I visited during fieldwork in August of 2018, dug several irrigation canals using NREGS funding. The projects in this village were "managed" by a middleman.[19] In order to initiate these projects the middleman developed a close working relationship with the BDO to ensure funding requests were quickly approved.

Circular_2019-20.pdf

[15]Or as Maiorano (2014) puts it "There is little the state government can do to block/release funds to specific [blocks]. MLAs can influence the allocation of funds within their constituencies, though."

[16]Technically, Block Programme Officers are the bureaucratic point person for NREGS projects, with a rank equal to that of BDOs. However, Programme Officer positions can be left unfilled with duties falling to the Block Development Officer. In either case, the BDO is ultimately responsible for all development work within the block. I use the terms interchangeably here.

[17]With these far reaching powers, the BDO role can be extremely remunerative. During my fieldwork I met with a local Block Development Officer who had initially taken the position due to its immense control over NREGS implementation and the rents accompanying that power. However, during his tenure NREGS switched funding mechanisms, bypassing blocks and depositing payments directly into laborers bank accounts (Banerjee et al., 2016). This made it more difficult to skim wage payments and demand kickbacks. At the time of the interview, the BDO was considering resigning due to the position's paltry salary when not subsidized by NREGS rents. Still, it would be remiss to lay all the blame for corruption and leakage at the feet of block bureaucrats. BDOs face political pressure from MLAs and high-ranking bureaucrats. At the same time, block offices are under-resourced and understaffed, making it difficult to carry out their multitude of responsibilities (Dasgupta and Kapur, 2020).

[18]Block bureaucrats also face minimal accountability constraints. Block level politicians are relatively weak considering that higher level politicians like the MLA sit on the block council. At the same time, most of NREGAs transparency initiatives are focused on village level politicians (Jenkins and Manor, 2017).

[19]Middlemen and contractors are formally outlawed under NREGA. However there is qualitative evidence that middlemen continued to operate and extract rents from the program, especially early on in NREGA implementation. Contractors pull is thought to have waned over time due to increased transparency measures and technological changes that distributed wage payments directly to workers bank accounts (Banerjee et al., 2016) I leave out any identifying information.

MLAs that are capable of controlling block level bureaucrats secure a key chokepoint in redirecting projects towards their preferred voters. In this sense, an MLAs' informal powers of persuasion are perhaps more important for controlling NREGS distribution relative to their seat on the block council. MLAs can pressure block bureaucrats primarily through two mechanisms: 1) threatening transfers, and 2) direct confrontation. First, MLAs can credibly threaten to transfer BDOs, derailing their careers and livelihoods (Iyer and Mani, 2012). Bussell (2015) documents that 86% of MLAs claim to have the power to transfer lower level bureaucrats (much higher than the self-reported transfer powers of block council presidents or other lower level politicians). Similarly, Bussell (2015) reports that BDOs have an average tenure of just 22 months, indicating frequent transfers.[20] However, transfers are no simple matter. They require the involvement of higher level politicians and bureaucrats, and tend to be employed more as a tool of last resort (Interview Patna, 2017). Instead, an MLA may lean on a series of increasingly muscular tactics to bend the bureaucracy to their will. Below, I outline how criminals leverage muscle (as well as money and networks) to control the bureaucracy and distribute resources to their preferred targets.

3.2.2 Criminal Politicians and NREGS Targeting

Are criminals better equipped to target supporters? I argue that criminals' arsenals of money, muscle and networks helps them to control the bureaucracy, identify supporters, and target resources.[21] While these three resources are self-reinforcing to some degree, I consider how each independently aids in targeting NREGS. Overall, I expect criminal politicians to deliver more NREGS resources, especially to their core supporters. For further discussions on criminal politicians attributes, see

[20] A ward member I interviewed recalled the multitude of block transfers over the past five years. Though the ward member cautioned that MLAs may value bureaucratic competence over loyalty since development work could translate directly to votes (author's interviews 2017).

[21] Vaishnav (2017) and Hansen (2005) emphasize money and muscle as core ingredients in criminals' electoral success. These resources were consistently mentioned in my own field interviews (2017, 2018). In addition, I highlight a superior access to networks rooted in criminal politicians illegal economies that create networks of dependencies between voters, bureaucrats, politicians and the criminal enterprise. At the same time, lucrative illegal economies create enough wealth to contest elections, while allowing the politicians to remain rooted in the community strengthening their community and network bonafides. In Chapter 4 I emphasize how the nature of the illegal economy enables politicians to remain rooted in communities in order to buttress their reputations as effective problems solvers over time.

the argument laid out in Chapter 1.

Money

Is money the root of criminals' political power? Criminal enterprises present one viable revenue stream for cash strapped parties. Indeed, criminal politicians are wealthier on average, according to self-reported net-worth (Vaishnav, 2017). For my sample, serious criminal politicians report being about 27% wealthier than clean politicians, on average.[22] In Tamil Nadu, Jharkhand and Bihar, rents from illegal mining activities run by criminal syndicates wind their way into party coffers (Singh and Harriss-White, 2019; Jeyaranjan, 2019).[23] In turn, these funds are used for vote buying, campaign expenditures or other party outlays Jeyaranjan (2019).

Once in power, criminal politicians can flex their financial muscles to solve constituent problems and deliver resources like NREGS. In particular, money can turn on the NREGS tap by lubricating intransigent bureaucrats (Maiorano, 2014). For instance, local politicians are sometimes required to pay bribes and incur travel expenses to visit block bureaucrats before NREGS projects are approved (Maiorano, 2014; Jenkins and Manor, 2017). Secondly, the very nature of criminal enterprises often necessitates establishing contacts within the bureaucracy. For aspiring MLAs involved in illegal enterprises like sand mining, " the highest single cost component... consists of 'protection' fees" paid to bureaucrats and the police to ensure the smooth operation (Michelutti, 2019). This money, paid to political protectors or party coffers "directly and indirectly sustains local political machines" (Michelutti, 2019).

Criminal politicians' liquid assets can bind bureaucrats to them, cultivate a reputation as an effective fixer, and pay or forgo bribes to ensure service delivery. If criminal politicians' primary advantage in NREGS delivery is due to their superior access to money I would expect targeting to be concentrated among wealthier criminal politicians. On the other hand, access to cash may shelter politicians from the demands to deliver services. Instead of complementing service deliv-

[22] Criminal politicians have average assets of 26,171,352 Rs. Whereas clean politicians average 20,622,816 Rs.

[23] In order to protect their rents, Vaishnav (2017) demonstrates how criminals' vertically integrated into politics forgoing their role as middlemen and vote fixers for political patron.

ery, money may substitute for it. Under this scenario, wealthier politicians would perform worse on delivering NREGS overall. I test this hypothesis using data on MLAs assets reported from candidate affidavits. As outlined below, I compare NREGS targeting between criminal and clean politicians at different levels of wealth.

Muscle

Criminal politicians' violent nature underpins their ability to target supporters. Bureaucrats and brokers are well aware of the violent deeds and political killings orchestrated by local mafiosos. Refusing a request from a local Don may entail consequences beyond career concerns and transfer threats. To add descriptive meat to the theoretical bones of why muscle matters, I provide examples from two criminal politicians in Bihar. In particular, I detail how their muscle-power translates to effective service delivery on the ground for supporters.

Mohammed Shahabuddin is one of the more prominent criminal politicians in India (Vaishnav 2017). After winning two state legislative elections in the 1990s, Shahabuddin subsequently secured a Member of Parliament seat in the Siwan constituency of Bihar in 1996.[24]. At the time, Shahabuddin was a well known "muscleman" facing over 30 criminal charges "[including] allegations of murder, kidnapping and possessing illegal arms and explosives etc."[25] Still, it would sell Shahabuddin short to paint him as a one-dimensional gangster. Heeding Machiavelli's advice to be both feared and loved, Shahabuddin orchestrated a parallel state (Vaishnav 2017). Akin to Arun Yadav, Shahabuddin would hold his own jantar durbar to redress grievances, resolve land disputes and lean on bureaucrats to improve service delivery (Vaishnav 2017; fieldwork interview with former MLA; India Today 2016).[26] For instance, Shahabuddin demanded that doctors charge no more than 100 Rs for clinic visits (interview with former MLA). As Vaishnav (2017 notes)

[24] `https://www.hindustantimes.com/india/shahabuddin/story-cPcnLvjzZj8jjLaKhW89AM.html`

[25] `https://web.archive.org/web/20030429101015/http://www.pucl.org/reports/Bihar/2001/shahabuddin.htm`

[26] `https://www.indiatoday.in/fyi/story/mohammad-shahabuddin-criminal-record\-bihar-siwan-all-you-need-to-know-340582-2016-09-12`

"the MP's 'phone call to any government officer... was a non-negotiable order."' A former two time MLA noted that Shahabuddin "provided justice, where justice was extremely slow." In other words, where courts and police had failed, Shahabuddin stepped in, translating muscle-power into effective governance for his supporters.

Similarly, Pappu Yadav, a Bihari MLA from the 1990s to early 2000s, employed muscular tactics to bring bureaucrats to heel. Pappu Yadav has a rap sheet nearly as long as Shahabuddin's, at one point serving a life sentence for the murder of his political opponent, from which he was eventually acquitted (Vaishnav 2017). Almost immediately after his release, Pappu issued a dictat ordering government health workers to lower fees. He burst into local clinics in Saharasa "to detect anomalies"' and intimidate doctors.[27] In Siwan, Pappu often held shows of strength, organizing rallies to pressure the District Magistrate (a high level bureaucrat). In fact, Pappu had enough pull to force the District Magistrate or doctor to pay attention to poorer segments of his constituency (Interview with reporter in Patna 2018, Vaishnav 2017).

I test the extent to which muscle matters for targeting by comparing the distribution of NREGS between violent and non-violent criminal politicians. Violent criminals are coded based on whether they face a charge for a violent crime (discussed further in the Data section). In sum, *Money* and *Muscle* strengthen voters perceptions that MLAs have the ability to hold up their end of the deal and deliver on core resources. Voters may see their vote as an investment in a social security net, supporting politicians as a way to mitigate risk (Vaishnav 2017). However, voters need to have some confidence that candidates can actually deliver on their promises to bring local goods back to the village. Money, muscle and the accompanying reputation can strengthen a candidate's credibility in the eyes of voters.

[27] https://www.thehansindia.com/posts/index/National/2014-09-19/Mafia-MP-Pappu-Yadav-fixes-fees-of-doctors/108253

Networks

Criminality fosters political network development in several ways. First, the illegal nature of criminal enterprises requires weaving bonds of trust between MLAs, lower-level politicians and the bureaucracy. The desire for silence and loyalty means criminal networks tend to rely on familial ties, which may be stronger than networks predicated on caste (Michelutti, 2019). The threat of violence also prevents political networks from fraying. Fearing violent repercussions, lower-level actors may be less likely to break ranks and flea to another political patron. At the same time, village politicians may initially lend support to local dons they deem credible in delivering clientelistic goods. NREGS provides a ripe opportunity for MLAs to demonstrate their clout and pull voters into their network. For example, Jenkins and Manor describe how the delivery of votes by a village politician was conditioned on the prior delivery of NREGS projects.

> “Part of the bargain was that the [MLA] would have to demonstrate his ability to fulfill his commitment by leaning on block authorities to bring jobs to the hamlet in the the months *before* the election. Because the BJP, the candidate’s party, was at the time in control of the state government, this was theoretically within his power. However, no jobs appeared within the stipulated timeframe. The local *Bhil* community, through their broker, informed the candidate’s operatives that they would not be voting for the BJP...” (Jenkins and Manor (2017) p. 77)

Second, criminality strengthens political networks by enriching politicians, bureaucrats and voters. For example, in Sandesh, illegal sand mining represents the dominate economic activity. Sand mining provides kickbacks to bureaucrats and employment for voters (e.g. via jobs in excavation and transportation). The economic fate of many in Sandesh is directly tied to the continued success of Arun Yadav and his natural resource extraction empire. In turn, Arun Yadav can leverage these increased contacts and control over the bureacuracy to deliver state resources.

Finally, by extracting wealth from rural areas where industry is absent, criminals solve the problem of generating enough money to contest elections, while simultaneously remaining connected to the local community. For instance, protection rackets and illegal mining can be far more remunerative than agriculture. Moreover, once criminals have a stranglehold on the illegal economy they often expand to dominate the legal economy. This allows criminals to invest both time and money in strengthening network bonds through repeated demonstrations of local bonafides. For example, voters know that Arun Yadav is available any time if they need help accessing job cards for NREGS (Interview 2017).

By understanding voters concerns and then having the capacity to solve them, criminals political networks may be primed as efficient targeting machines. In other words, criminal politicians may be more effective at targeting partisans and translating infrastructure projects to votes. I test the networks hypothesis using polling station results. Put simply, I compare the distribution of NREGS funds and projects in criminal and clean constituencies to co-partisan polling stations.[28]

Do MLAs target brokers or voters?

Further, qualitative and quantitative evidence is divided over how political networks precisely operate. Do MLAs target local elites or voters themselves? To shed light on if brokers or voters disproportionately profit in core villages, I leverage granular information on funding patterns from individual NREGS projects. In particular, I am interested in comparing if criminal politicians structure their networks differently. Is there evidence indicating that criminals funnel NREGS resources to local elites or voters at different rates than clean politicians?

Following, (Das and Maiorano, 2019a) I tease apart MLAs targeting networks noting that NREGS wages accrue directly to voters whereas material expenditures can reward local elites

[28]To be clear, networks do not facilitate the distribution of NREGS to core supporters under all conditions. While NREGS can promote employment and wages for India's poorest segments it may alienate large land holders or other employers of casual village labor as it bids up the local reservation wage rate. Thus MLAs who derive support from these local elites may be wary of forcing the scheme without compensating local elites in some way. Which incentive wins out, depends on if votes from program generation are greater than votes generated by maintaining and greasing relationships with local elites. NREGS targeting is thus dependent on who is in the MLAs support network. If MLAs rely on local landholders and farmers then NREGS is an ineffectual form of patronage as this undermines landholder profits. To partially address these concerns, I adjust for agricultural measures at the village level using census data.

(Das and Maiorano, 2019a).[29] This leads to two distinct sub-hypotheses for the *network hypothesis*. First, if projects in core areas tend to employ more workers and pay higher wages, this would suggest that MLAs targeting efforts are concentrated on voters. That is voters are the ones who directly benefit from the increase in employment and wages, allowing politicians to claim credit for NREGS distribution. To be clear, I do not directly observe whose pockets NREGS funds eventually line. However, since wage payments are direct deposited in workers' bank accounts, all else equal, increased employment rewards voters.

Second, if projects in core areas report increased material expenditures this would be consistent with a strategy of MLAs rewarding co-partisan local elites. After NREGS wage payments converted to direct deposits, graft has increasingly targeted material expenditures (Jenkins and Manor (2017), fieldwork 2018). Therefore, material expenditures are likely to reward village politicians, with MLAs targeting local elites in exchange for vote banks and/or kickbacks. One way politicians can skim rents from NREGS material funds is by colluding with contractors.[30] Recent work in India suggests that politicians privilege firms and contractors they share close connections with (Lehne et al., 2018; Sukhtankar, 2012). Additionally, politicians may skim rents by over-invoicing a projects material budget (Das and Maiorano, 2019a). For example, a BJP partyworker explained one scam leading to the collapse of a massive water tank on the same day the tank was inaugurated. The contracting firm supplied cheap cement while reporting inflated invoices. In turn, contractors, officials and politicians skimmed excess funds from the material budget.[31]

[29]Das and Maiorano (2019) employ a similar strategy and find little evidence of partisan targeting of wages, but increased material costs for projects in core areas. However, the analysis does not align the relevant political unit MLAs would target with NREGS spending. Most NREGS study suffer from this ecological fallacy. I move beyond Das and Mairoano by more precisely measuring targeting using polling station results. In short my study overcomes the ecological inference problem by directly mapping individual NREGS projects to the relevant political unit of villages that MLAs would target. Das and Mairoano analyze the entire constituency focusing on whether the Indian National Congress targets swing or core constituencies across the state. Das and Maiorano (2019) is also limited to Andhra Pradesh generally seen as one of the better functioning states where political influence may be curtailed via increased citizen knowledge of program benefits and routine social audits. Second studies that use individual survey data on NREGS tend to focus on co-partisan ties between village politicians and NREGS recipients. Thus, these studies do not account for the role that MLAs play in altering NREGS distribution *prior* to funding reaching the village.

[30]See previous chapter for further examples of corruption in NREGS expenditures.

[31]This water tank was not built with NREGA funding but money from a similar scheme started by Bihar's Chief Minister Nitish Kumar. Nitish Kumar had emphasized bringing water connection to every village so there was a focus

If politicians act purely strategically to reward supporters, I would expect more expensive projects that increase employment and wage payments in core-areas. On the other hand, if MLAs look to reward local elites in exchange for votes or kickbacks, I would expect projects in core areas to have greater material expenditures.[32] I then test if these tendencies differ between criminal and clean politicians.

In sum, criminals' assets of money, muscle and networks aid in targeting state resources to supporters. Money is useful for greasing the wheels of the bureaucracy. Muscular politicians can physically threaten bureaucrats and brokers, adding weight to their demands. Networks indicate that politicians are both more attune to constituent needs and have contacts in the bureaucracy who can rectify voters issues.

3.3 Data Construction

To determine if criminal politicians more effectively target NREGS, I combine three main datasets: NREGS outcomes, polling station results, and MLAs criminality and characteristics. Each of these datasets is comprised of multiple sources. I document the data construction in the following section with additional information in the appendices. The construction of this dataset is particularly useful for investigating whether criminal politicians "get things done" by aligning the delivery of a resource voters care about and politicians can manipulate with a measure of voter political support.

3.3.1 Primary Outcomes

To measure the distribution of NREGS projects, funds and employment I collect original data on the location and details of over 20 million NREGS projects from India's Ministry of Rural Development.[33] These data provide the geographic location (latitude and longitude) for NREGS projects across all states from 2007 to 2017. Each individual NREGS project record includes

on building water tanks at this time. Officials would procure shoddy materials and pocket the difference.

[32]Hiring local contractors to provide the materials is also one way to reward local elites in the MLAs network. With the contracting firm making campaign donations in the run up to elections (Vaishnav and Sukhtankar, Das and Maiorano, Jenkins and Manor, fieldwork).

[33]Data available here: `https://bhuvan-app2.nrsc.gov.in/mgnrega/mgnrega_phase2.php`

longitude and latitude coordinates, the project start and end date, funding distributions by the fiscal years and project details (e.g. name, type and an image of the asset). While the NREGS dataset covers all of India, in this chapter I limit my analysis to states where I can obtain polling stations location data. Overall, I observe 3,538,247 projects across nine state elections.[34] Table 3.A.1 in Appendix 3.A lists the state elections included in my sample.

From the NREGS data I construct four primary outcomes. *Workdays* are the total number of labor person-days spent constructing the NREGS asset. *Pay* are the total wages paid out to a laborers on the project. *Materials* are the total material expenditures incurred by the project. I also count the total number of NREGS projects located at a polling station or village: *Projects*. Project funds and labor are measured by the fiscal-year. Seventy-five percent of projects are completed in under a year. For projects that span more than one fiscal year, I aggregate the outcomes across years to the project-level. In other words, NREGS outcomes measure the total workdays, pay and materials for each project.[35] Further, to measure partisan targeting, I assign each project to a polling station. Subsequently, I aggregate project outcomes over their assigned polling station for a given MLA's term (generally five years). For my primary models, the final unit-of-analysis is thus the polling-station-election-term.

3.3.2 Measuring Political Support

I collect original data on polling station results for all MLA candidates at 409,147 polling stations from the Indian state election commissions' Form 20s.[36] Form 20s include polling station results, polling station ID and candidates name and party.

To identify polling station locations I scrapped longitude and latitude coordinates from the

[34]As detailed in the empirical section I split my sample into training and testing data. The training data includes 1,777,811 NREGS projects.

[35]Details on this process and assigning projects to electoral cycles are provided in Appendix **??**.

[36]To the best of my knowledge, this is the most comprehensive collection of polling station data for state legislative elections in India. Susewind (2020) provides comprehensive polling station returns for MPs and some state elections. Bussell (2019) uses Susewind's polling station data for legislative elections in Karnataka. When polling station results, names or locations were unavailable from state election websites I accessed this data via internet archive. Data sources are available from the author upon request.

Election Commission of India's polling station locator in 2016.[37] The polling station location data includes the polling station name and ID. However, polling station ids are election specific and the Election Commission updates the polling station locator as new state and general elections occur. The Election Commission does not identify which election year the polling stations represent, making it nearly impossible to match by polling station ID.[38] Given this limitation, I fuzzy match polling station locations to polling station results by name, within constituencies.[39] In effect, to obtain the final polling station dataset, I link polling station results to polling station locations by polling station name.[40] In sum, obtaining a complete set of polling station results, names and locations for elections between 2007 and 2017, restricts my sample to nine state elections.

Subsequently, I combine the finalized polling station dataset with the NREGS datset using geo-cooridnates. To measure if MLAs target core areas I map NREGS projects to the nearest polling station by longitude and latitude. Following the Election Commission of India's voting mandates, for my preferred models, I select a 2 kilometer catchment area around polling stations. Projects that are further than two kilometers from every polling station remain unassigned. I adopt this conservative approach to ensure that NREGS benefits match voters political support as closely as possible. The two kilometer radius is motivated by how India's election administrators assign polling stations to villages. First, the Election Commission of India mandates that "no voter should ordinarily travel more than 2 kms" (ECI 2006). Thus, polling stations correspond closely to villages. Second, each village with more than 300 electors and a suitable building is required to have its own polling station (ECI 2006). The maximum number of electors per polling booth is capped at 1500 (ECI 2008). On average, 1300 people live in a given village (Indian Census 2011). Un-

[37] I am indebted to Aaron Rudkin for help in scraping polling station location data from `http://psleci.nic.in/Default.aspx`.

[38] The most reasonable assumption is that location data refers to the most recent election for each state when I collected the data in 2016. However, as some elections occurred in 2016 it is not clear if locations are for the upcoming or previous elections. Also, some states most recent elections were the national parliamentary elections of 2014. Which have different polling station ID numbering.

[39] Further details can be found in Appendix **??**.

[40] Where available, I collected polling station names from state election commission website and matched these to polling station results by polling station ID.

less the village is tiny or lacks a suitable building there should be a one to one matching between villages and polling stations. In turn, there should be high correspondence between polling station results and an MLAs true level of support for citizens served by any NREGS project within the 2km radius. Moreover, NREGS serves rural residents, reducing concerns that polling stations tightly overlap as they do in urban settings. In any case, I assign projects to their nearest polling station, noting that voters are assigned to the nearest polling station in their village as well.

Finally, to include census controls, I map NREGS projects and polling stations to 2001 village census shapefiles. Thus, polling stations and their assigned projects are matched to village demographics for the villages that they serve. Thus for most analyses, I can include village controls, polling stations results and the assigned NREGS projects outcomes.[41] The census data is also useful for identifying and removing urban polling stations. Since NREGS is only available in rural areas I drop all urban polling stations. To identify urban polling stations, I leverage two pieces of information. Polling stations with NREGS projects within two kilometers are by definition located in rural areas where the program operates. However, polling stations without NREGS projects within two kilometers pose a challenge. On one hand, these polling stations may be in rural areas but simply failed to receive a project during that electoral cycle. On the other, these polling stations may actually be in urban areas and therefore ineligible to receive NREGS funding. To avoid conflating targeting with urban vs. rural voting patterns, I use the 2001 census coding of rural and urban areas to identify urban polling stations.

3.3.3 Identifying Criminality: Constructing the Candidates Dataset

The primary hypothesis compares differential rates of targeting between criminal and clean MLAs. Data on MLA candidate characteristics, party affiliation and constituency results is taken from

[41]Note, that I do not aggregate NREGS outcomes to 2001 village shapefiles, since these village boundaries could change over time. Instead , I use the village data as a proxy for development and NREGS demand at given polling station election term. In other words, projects are still assigned to polling stations and I merge in village demographic and development data from 2001 census to proxy for demand.

the Trivedi Center for Political Data's Lok Dhaba database.[42] The TCPD database provides the most comprehensive and linked coverage of Indian elections, forming the universe of candidates in my analysis. To identify criminal MLAs I supplement the TCPD data with candidate affidavits scrapped from the Association for Democratic Reform.[43] I code MLAs as criminal if their signed affidavit lists any serious charge.[44] To combine the comprehensive elections database with the criminal affidavits, I fuzzy match candidates within constituency by name, party and age.[45] Overall, I match 100% of the candidates in the criminal charges dataset and 74% of all possible candidates from the universe of candidates in the TCPD database (see Appendix 3.C.1 for further discussion on these points).[46] While a higher match rate would be ideal, for this chapter, I am only interested in winning MLAs who can control the distribution of NREGS and so do not need to match every candidate. Among winning MLAs, I match 99% to their criminal affidavits.

Finally, to test if criminals engage in more partisan targeting, I match the winners from the MLA affidavit dataset to polling station results by candidate name and party. Overall, I link 99% of winning MLAs to their criminal histories and polling station performance for the nine state elections analyzed below. In sum, the final dataset consists of 1,763 victorious MLAs (their characteristics, demographics, criminal charges and wealth) how they performed across 409,147 polling stations and more than 3.5 million NREGS projects generated during their electoral term.

3.3.4 Control Variables

Primary controls variables are taken from the 2001 Indian census abstract. One important confounder is worker demand for the NREGS program. It could be the case that criminals tend to

[42] The database maintained by Ashoka University can be accessed here: `http://lokdhaba.ashoka.edu.in/`

[43] All MLA candidates are required to submit signed affidavits detailing any criminal charges, the candidate's assets and education level. ADR digitizes these candidate affidavits, which I scrapped and cleaned from here: `http://myneta.info/`.

[44] See previous chapter for definition of serious charges.

[45] Parties will often run dummy candidates with similar names to their main challenger to siphon votes from competitors. Some candidates also have common names. This makes it difficult to uniquely identify candidates with common names even within constituencies. By matching on party and age, I am able to uniquely identify candidates from both databases.

[46] Most of the non matches result from small time, independent candidates who receive less than 2% of the vote.

operate in less developed areas where NREGS demand is likely high. To attempt to adjust for NREGS demand I use data from the 2001 census. There are over 150 variables in the Indian socio-economic census. Following Gulzar et al. (2020) I create indices that are likely to be predictive of NREGS demand. Demand tends to be highest among women, SC/ST and marginal agricultural laborers (Dutta et al., 2014). Using village level data from the 2001 Indian census I adjust for a villages number of women, SC/ST and seasonal agricultural laborers, among other factors that are predictive of program demand. The complete list of indices can be found in Gulzar et al. (2020).

Second, since prior literature highlights the importance of political alignment with the ruling party in receiving NREGS funds, I code MLAs as members of the ruling coalition or opposition. Lacking a definitive source on ruling parties and coalitions in state legislatures, I construct my own from primary sources. A detailed description of coalition coding and sources is available in Appendix 3.A, Table 3.A.1.

3.3.5 Summary Statistics

Summary Statistics are calculated using only the training data sample to keep information from the testing data leaking into the analysis. I randomly sample 50% of the dataset, conditioning on MLA criminality to include in the training data.[47] The final training dataset- consisting of NREGS outcomes, polling station results, MLAs criminal affidavits and 2001 census controls- covers 120,141 polling stations. Mirroring the rest of Indian politics, political competition at polling stations is quite intense. Rural polling stations show a high level of political competition, with an average margin of victory of 5% or 31 votes for victorious MLAs (see table 3.3.1).

[47]Further, summary statistics are provided for only rural polling stations in table 3.3.1. For the full sample of training data see Appendix 3.D.

Table 3.3.1: Summary Statistics: Rural Polling Stations Only

Statistic	N	Mean	St. Dev.	Min	Max
Workdays	120,141	4,138	12,968	0	668,108
Pay	120,141	676,926	2,207,588	0	124,842,795
Materials	120,138	160,413	613,356	0	39,815,554
Projects	120,141	11	30	0	1,060
Votes cast for Winning MLA	120,141	273	164	0	1,622
Total Votes Cast at Polling Station	120,139	644	244	0	18,870
Winning MLA Vote Share (PS)	118,159	42	19	0	100
Winning MLA Margin of Victory (PS)	117,903	5	30	−100	99
Margin of Victory (Constituency)	120,141	10	9	0	81
Vote Share (Constituency)	120,141	42	9	18	87
Number of Criminal Cases	116,499	3	6	0	43

Political competition and NREGS outcomes summary statistics for rural polling stations (i.e. only rural polling stations across 9 state assembly elections). Descriptive statistics are for training sample only. Political competition variables are calculated for victorious MLAs.

3.4 Descriptive and Graphical Analysis

3.4.1 Political Competition in State Assembly Elections

In order for politicians to target co-partisans, supporters need to be sufficiently separated geographically from opposition voters (Andrew Harris and Posner, 2019). To investigate if MLA supporters meet this pre-requisite for targeting, Table 3.4.1 and Figure 3.4.1 display the distribution of polling station political competition by criminality and across states, respectively. Since NREGS projects are local public goods that can be targeted to villages (or even within villages) it is sufficient that a portion of polling stations are firmly in the MLAs camp- as is the case across each state in Figure 3.4.1.

The summary statistics in Table 3.3.1 provides descriptive statistics of political competition for rural polling stations in the training data. Polling stations demonstrate high levels of political competition with an average margin of victory of 5%. Put differently, victorious MLAs win a typical polling station by 31 votes, on average.[48] To benchmark these trends, consider that prior studies

[48]Rural polling stations show a slightly higher level of political competition than urban areas. Including all polling

view assembly constituencies with +/- 10% margin of victory as "swing constituencies" (Vaishnav, 2012; Das and Maiorano, 2019a). For comparison, from 2004-2014 victorious Members of Parliament won by an average margin of victory between 10 and 20% (Auerbach, 2015). Similarly, Min (2012) finds an average margin of victory of 14.5% for all legislative assembly elections between 1991 and 2003. For a more direct comparison, in the training sample I find that MLAs have an average margin of victory of 11% across assembly constituencies.[49] Taken together, polling stations display high levels of political competition, even for the notoriously competitive Indian political landscape.[50]

Still, victorious MLAs win about 25 percent of polling stations by more than a 25 percent margin (see Table 3.4.1). In other words, there is a substantial subset of core villages providing ample opportunities for NREGS targeting with minimal leakage. On average, assembly constituencies have about 138 rural polling stations. Thus, for a typical constituency, there are roughly 35 polling stations where the winning MLA dominates the local landscape.

Finally, focusing on the average masks variation in polling station political competition across states. Figure 3.4.1 compares the distributions of margin of victory across Indian states, illuminating variation in micro-level political competition.[51] All else equal, states with greater spread exhibit less political competition. States with probability mass concentrated around 0 exhibit relatively higher levels of political competition (e.g. West Bengal). Assam and Uttar Pradesh show a greater spread of political competition indicating relatively more core and opposition strongholds. In these states, targeting programs to core villages should be relatively easier given the separation of core and support polling stations (see Andrew Harris and Posner (2019)). On the other hand states like West Bengal and Kerala exhibit relatively higher degrees of political competition with

stations (i.e. urban and rural) the average margin of victory is 7% or 45 votes. For reference and the full sample of polling station political competition see Table 3.D.1 in Appendix 3.D

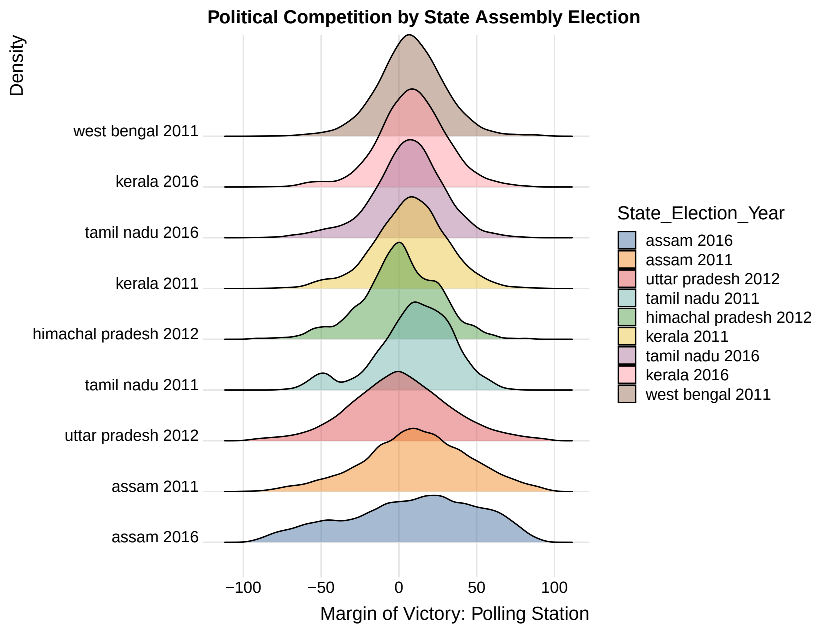
[49]See Table 3.D.2 in 3.D.

[50]To be fair, this is somewhat of an artificial comparison, given that winning candidates can't lose constituencies but can lose polling stations. Moreover, it does not account for the distribution of polling station results which I discuss below.

[51]For distributions of alternative measures of political competition by state see Appendix 3.D, Figure 3.D.9.

polling stations margin of victory clustered more tightly around zero. Interestingly, Tamil Nadu shows strong areas of opposition support in 2011 but not 2016.[52]

Figure 3.4.1: Polling Station Political Competition by State- Rural Sample



States sorted by the standard deviation of the polling station margin of victory

3.4.2 Political Competition and Criminality

Table 3.4.1: Quartiles of Polling Station Margin of Victory by Criminal Status of MLA

	Criminality	Q1	Median	Mean	Q3	Std. Dev.
1	Clean	-9.12	7.86	7.12	24.52	28.52
2	Criminal	-10.44	7.33	6.49	24.83	28.98

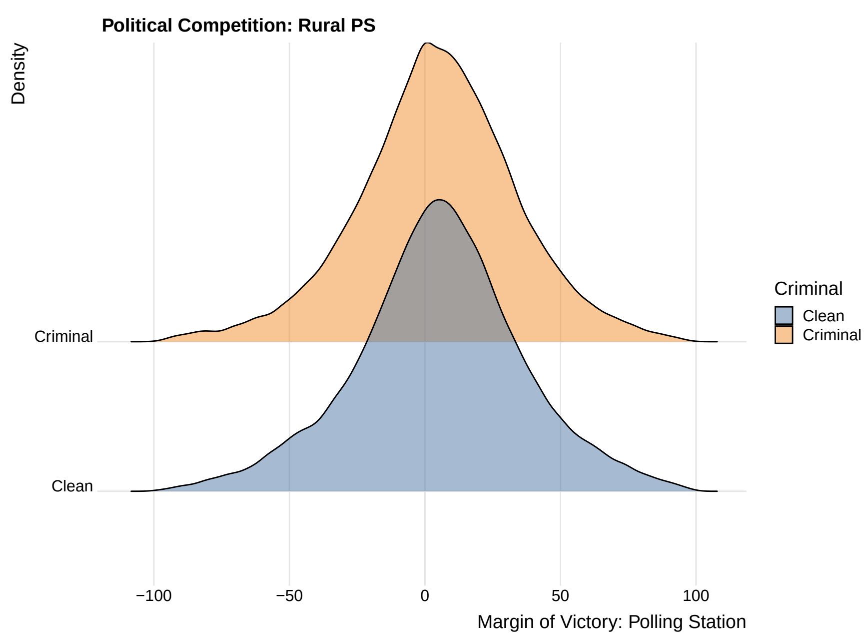
Do criminals blow away the competition? Figure 3.4.2 illustrates the distribution of polling station margin of victory comparing criminal to clean politicians.[53] Overall, the distributions of political competition between criminal and clean appear visually very similar. Given the large sam-

[52] Assam has similar spikes of opposition strongholds in both elections, see vote share in Figure 3.D.9a of Appendix 3.D.

[53] For full sample including urban polling stations and alternative measures of political competition, see Appendix 3.D.3.

ple size, however, I reject equality between the two distributions[54] Criminal political constituencies are slightly more competitive, on average (mean of 6.5 vs. 7.1 in clean districts)[55] with nearly identical standard deviations. Still, 25% of polling stations are won by more than 25%, indicating that there are some secure strongholds for winning MLAs to target across these 9 state elections.[56] However, MLAs are winning the vast majority of their polling stations by just a handful of votes. At the same time there is not a large presence of opposition strongholds (though this varies across states). Winning MLAs lose only 25% of polling stations by (roughly) more than 10%. Overall, the picture that emerges is one of high levels of political competition at polling stations in both criminal and clean constituencies- with little substantive difference between their distributions.

Figure 3.4.2: Polling Station Political Competition by Criminality- Rural Sample



[54] Kolomogorov-Smirnov test returns approximate p-value < 0.001, p-value = 5.426e-10.
[55] ttest of difference in means significant at p < 0.001, t = -4.5777, df = 132260, p-value = 4.706e-06
[56] Table 3.4.1 presents quartiles for the distribution of political competition in criminal and clean constituencies.

3.5 Empirical Estimation

In light of the graphical analysis, I now build up to more complex models which consider that criminal politicians likely operate in different political contexts and constituencies compared to clean politicians. To more systematically investigate the relationship between criminal status and targeting I turn first to OLS models that attempt to adjust for confounders between criminality and NREGS distribution. After reporting OLS results, I motivate the use of Kernel Regularized Least Squares as a way to relax OLS assumptions.

To estimate the relationship between criminality and targeting using OLS, I assign all NREGS projects to their nearest polling stations, subject to a maximum distance of two kilometers. Projects further away than two kilometers I do not consider as serving villagers voting at that polling station. The unit of analysis is the polling station-election term. Put simply, I count the number of projects targeted to a polling station catchment area and aggregate NREGS wages and employment days within these same locales. I model NREGS outcomes as a function of the interaction between incumbent margin of victory at an individual polling station and MLA criminality, controlling for village level demand, MLA characteristics and constituency political predictors. Controls include:

- **Census village indices**
  - Indexes proxy for village NREGS demand.
  - Demand tends to be highest among women, SC/ST and marginal agricultural laborers (Dutta et al., 2014).
  - For example, using village level data from the 2001 Indian census I adjust for a villages number of women, SC/ST and seasonal agricultural laborers among other factors that are predictive of program demand.
  - For the full list of census controls and index construction see (Gulzar et al., 2020).
- **MLA personal and political characteristics:**

- *Age, Sex, Assets, Liabilities*[57]

- *INC*: Indicator for whether MLA belongs to the Indian National Congress[58]

- *Opposition*: indicator for whether MLA belong to the opposition or ruling party coalition.

- **Constituency Measures of Political Competition:**

  - Assembly constituency margin of victory (%)[59]

  - Assembly constituency turnout (%)

My preferred OLS model can be represented as:

$$\begin{aligned} NREGS_{b,t} = \alpha + \beta_1 Voteshare_{b,t} + \beta_2 Criminal_{b,t} + \beta_3 Criminal_{b,t} * Voteshare_{b,t} + \\ \mathbb{X} + StateElection_t + e_{b,t} \end{aligned} \tag{3.1}$$

Where subscript b refers to a polling booth and t refers to the election term and $\mathbb{X}$ is the battery of aforementioned controls. If criminal politicians deliver more NREGS benefits to their core supporters relative to clean politicians, then I would expect a positive coefficient on the interaction term. If criminal politicians simply deliver more NREGS benefits overall but do not necessarily target co-partisans, then $\beta_2 Criminal$ should be positive. Standard errors are clustered at the assembly constituency level. I included state election cycle fixed effects to capture variation across states and over time in NREGS program implementation (Sukhtankar et al., 2016). NREGS outcomes of labor expenditures (*Pay*), workdays *(Workdays)*, and material expenditures (*Materials*)

[57] Assets and liabilities are taken from candidate self-reported affidavits dataset. I log these skewed income variables.

[58] (Gulzar and Pasquale, 2017) indicate that, at least initially, voters affiliated NREGS with the INC. Congress created the program and congress party members lobbied hard to claim credit for its success Dasgupta (2016). To adjust for these party effects, I use an indicator for whether or not the MLA is a member of the Indian National Congress

[59] It could be the case that incentives for targeting are only present in competitive MLA races. Safe seats on the other hand may not illicit targeting efforts from MLAs who feel they can maintain their incumbency without increasing efforts to target political resources.

are logged transformed. The count of projects (*Projects*) assigned to a polling station remain untransformed.

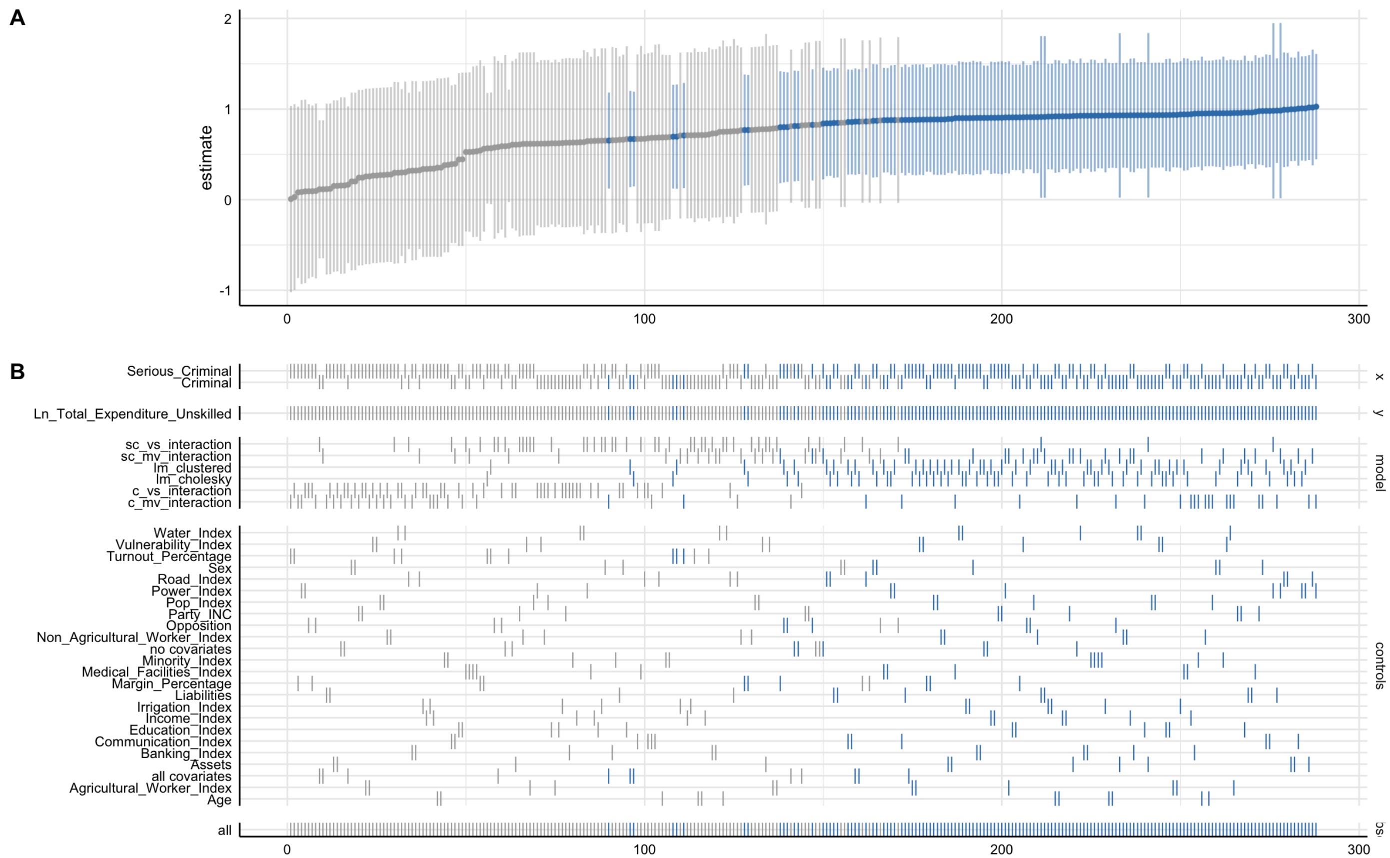
Given researcher degrees of freedom in choosing outcomes, controls, interactions, coding, among a myriad of other researcher specific decisions, the number of possibly analyses can quickly balloon, leading to overconfidence in results (Simmons et al., 2011). To investigate how outcomes depend on data, specification and model choices I take a multiverse approach (Steegen et al., 2016). Put simply, there are many reasonable and defensible coding, cleaning and modeling decisions. For example, measuring polling station political competition as vote share or total votes may more precisely capture the idea that politicians target where their supporters are more numerous. However, if politicians care about minimizing spillovers then margin of victory may be the more precise measure of the underlying latent concept of political competition. Second, demand for NREGS is likely to be non-linear in demographics and may include interactions between inter-sectional identities (e.g. women from scheduled castes). But which interactions to include? Or, what is the best threshold for declaring matches and non-matches in polling station or candidate names that minimizes false positives and false negatives? Instead of simply moving a handful of alternative analyses to robustnesss and sensitivity appendices, I explicitly compare how results hinge on analytic choices.

To be clear, I am still providing only a brief tour of the modeling sausage factory. But I hope by bringing this choice set of defensible decisions to the forefront of the analysis I can embrace uncertainty and avoid over-hyping results dependent on an arbitrarily selected model.

Figure 3.5.1 estimates the association between criminality and NREGS pay for 288 different OLS models. These models vary how the dependent variable is measured (i.e. any criminal charge or only serious charges), adjustors included, interactions between criminality and political competition and the type of standard error estimator used.[60] Overall, the takeaway message is that the is wide variation in effect estimates based on researcher degrees of freedom. Depending on the type

[60]See table notes in Figure 3.5.1 for model and data permutations.

Figure 3.5.1: Specification Curve: NREGS Pay



Coefficient estimates for criminality are reported in Panel A. The outcome is logged *Pay* and is constant throughout. Panel B indicates the specific model choices made. Ticks indicate the specific model choices with the corresponding results above in Panel A. For instance, the first grouping in Panel B indicates different choices of coding criminality (i.e. either including all charges or only serious charges). Models have been written so that coefficients are marginal effects of criminality to make comparisons between models that include and exclude interactions possible. The third block indicates model choices that include serious criminal interacted with different measures of polling station political competition- either vote share (vs) or margin of victory (mv). The next block includes models with no interactions but different types of standard errors (clustered or cholesky deccomposition). Alternative codings of criminality including all charges and interactions with polling station political competition are similarly estimated. The fourth block from *Water Index* to *Age* varies which controls are included. These models are all estimated on the complete training dataset (noted by the *all* in final block of Panel B). This represents a final fork not taken in the garden of forking paths. All models could be estimated on subsets of the data e.g. different states which would exponentially explode the number of models and comparisons.

of model, measurement of criminality and controls included, estimated coefficients for Criminality can range from 0.01 to 1.03 (i.e. for the unlogged coefficients these estimates range from 1% to 280%) increase in NREGS pay when switching the criminal status of the MLA. For some models, the data are even consistent with negative associations between criminality and NREGS pay. Figures 3.E.1 and 3.E.2 in Appendix 3.E further explore what drives these variations in effect estimates. Overall, difference in estimates are driven by model choices (e.g. whether the model includes interactions) and residual variance. Including different adjustors matters relatively less in explaining effect variation (see Figure 3.E.2). Given the unexplained residual variance, model dependence and the likely non-linear interplay between independent variables and variance in OLS estimates, I try alternative strategies to tie my hands in model specification. In particular, Kernel Regularized Least Squares provides one path to more agnostic modeling.

3.5.1 Kernel Regularized Least Squares

KRLS, combined with sample splitting, provides a number of advantages over defaulting to a preferred OLS specification accompanied by robustness checks. First, KRLS is agnostic about functional form (i.e. how features combine to predict NREGS outcomes). Where OLS assumes additivity and linearity, KRLS allows for a flexible functional form search (Hainmueller and Hazlett, 2014). In turn, this reduces the likelihood of misspecification bias resulting from strict OLS assumptions. This is particularly helpful given that my primary hypothesis is an interaction between criminality and political competition. It is unlikely that the only interaction between predictors is those that I explicitly theorize. With dozens of predictors specification searches for the "correct" interactions among all potential interactions quickly explode. I deploy two methodological shields to guard against model misspecification. First, I rerun models using KRLS to allow a more agnostic approach to choosing the model's functional form. KRLS also regularizes predictors which penalizes large or overly complex models. Second, I set aside half the polling station observations to test model predictions. The hold out set allows a comparison of model performance and can help adjudicate if criminality is predictive of NREGS delivery on unseen data- in effect, acting as type of self-replication.

However, allowing KRLS to flexibly fit the data can lead to overfitting (i.e. fitting noise in the training data leading to poor prediction on new data). In other words, the flexible function while reducing misspecification bias can lead to greater variance in estimates, in a classic bias-variance tradeoff. KRLS regularizes coefficients by penalizing model complexity leading to better predictions on test data (Hainmueller and Hazlett, 2014). Regularization is particularly useful in this setting given the large amount of data and wide variation in NREGS outcomes. If the criminal status of MLAs is not predictive of NREGS provision the coefficient on criminality should be shrunk towards zero. In all cases I build exploratory models on the training data and test their accuracy on the test set. Thus, I can check if the model findings in the training set capture systematic relationships between political predictors and NREGS outcomes of interest on unseen data.

Finally, KRLS allows for interpretable estimates and easily communicated quantities of interest (Hainmueller and Hazlett, 2014). While other machine learning models (e.g. Random Forrest) can flexibly fit data and regularize estimates, communicating results and conducting hypothesis tests proves more difficult. KRLS reports pointwise marginal effects and error bounds similar to OLS. In addition, KRLS reports marginal effects for each data point, aiding exploration of heterogeneous effects and interactions. KRLS is flexible (avoiding OLS assumptions leading to misspecification bias), regularized (avoiding overfitting) and interpretable (reporting communicable results).[61] At the same time, KRLS is not a magic bullet and does nothing to alleviate omitted variable bias. Model estimates are only as useful as the data they flexibly fit. Even with the rich set of covariates there are still possible unobserved confounding between the criminal status of MLAs and NREGS delivery (e.g. bureaucratic capacity). I therefore do not make causal claims when discussing reported "effects" below. Having motivated the advantages of KRLS I turn now to reporting results.

[61] These advantages come at computational cost. I overcome KRLS' slow computing using brute force via Amazon Web Services cloud computing.

Table 3.6.1: KRLS: Serious Criminal

	Pay	Workdays	Materials	Total Projects
Serious Criminal	0.80^{***}	0.57^{***}	0.68^{***}	3.05^{***}
	(0.06)	(0.05)	(0.05)	(0.36)
Polling Station Margin of Victory	-0.00	-0.00^{***}	-0.00^{***}	-0.02^{**}
	(0.00)	(0.00)	(0.00)	(0.01)
Age	0.03^{***}	0.01^{***}	0.02^{***}	0.08^{***}
	(0.00)	(0.00)	(0.00)	(0.01)
Sex	-0.01	0.06	-0.01	0.93
	(0.09)	(0.08)	(0.08)	(0.54)
Party_INC	0.44^{***}	0.29^{***}	0.60^{***}	-1.04^{*}
	(0.07)	(0.07)	(0.07)	(0.47)
Opposition	-0.67^{***}	-0.35^{***}	-0.46^{***}	-1.88^{***}
	(0.05)	(0.05)	(0.05)	(0.33)
Assets	-0.00^{***}	-0.00^{***}	-0.00^{***}	-0.00^{***}
	(0.00)	(0.00)	(0.00)	(0.00)
Obs.	53800.00	26963.00	53908.00	26938.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

3.6 Results

Table 3.6.1 and Figure 3.6.1 present KRLS sample-average marginal effects for all NREGS outcomes (i.e. *Pay, Workdays, Materials, Total Projects*). For indicator variables, coefficients represent first differences. Due to computational cost, I estimate KRLS models on a random 50% subsample of polling stations for the *Pay* and *Materials* outcomes. *Total Projects* and *Workdays* are estimated from a random 25% subsample. This translates to approximately 54,000 and 28,000 polling station observations for the 50 and 25% subsamples respectively. For full results, including estimates for census and constituency predictors see Appendix 3.F.1.

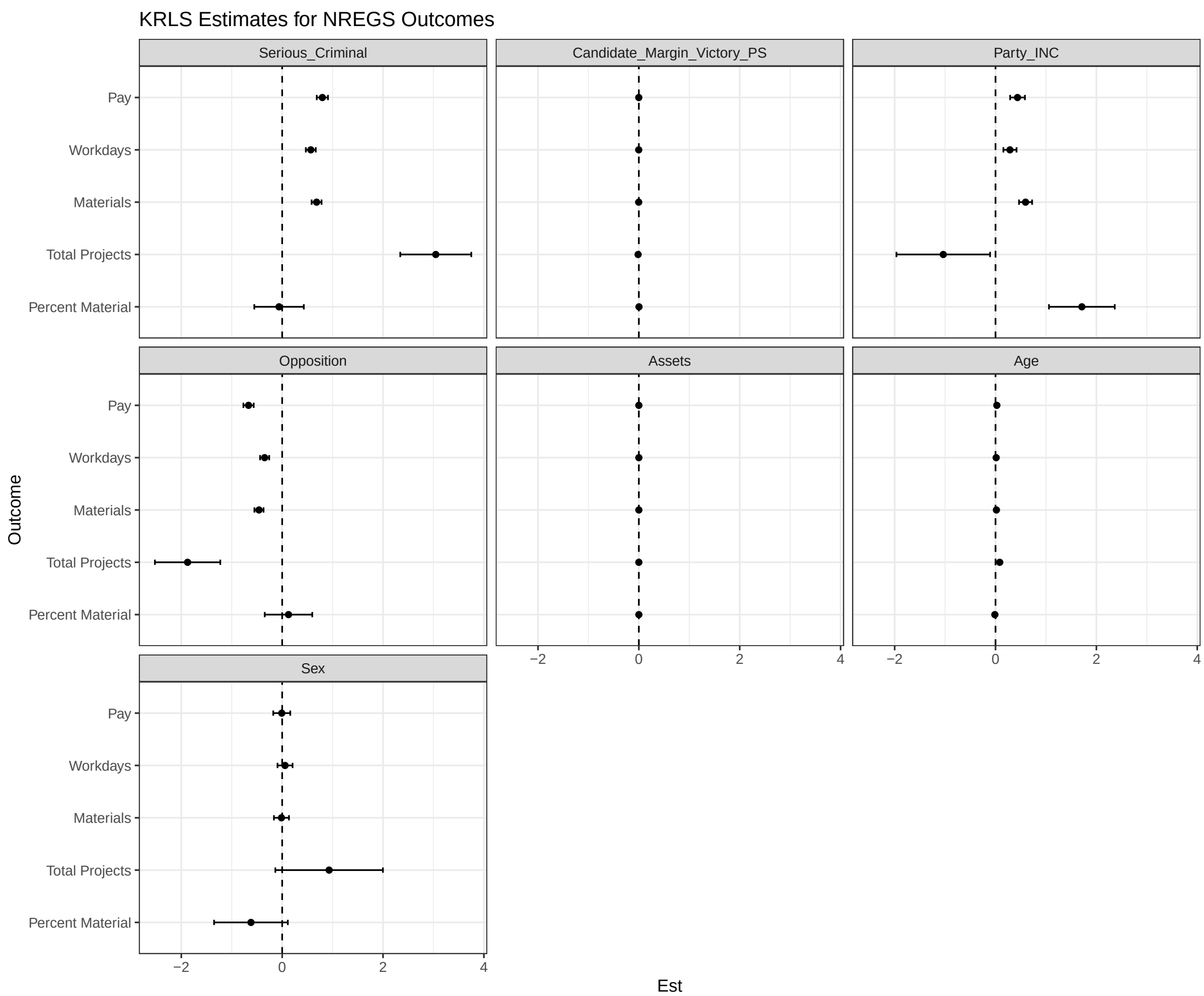
Consider the first panel of Figure 3.6.1 which displays first-difference estimates between Serious Criminals and all other MLAs along with 95% confidence intervals. Serious criminals are associated with an increase in NREGS provision across all four main outcomes. In other words constituencies governed by criminal MLAs are predictive of more NREGS projects, more NREGS employment (e.g. pay and workdays) and greater material expenditures (see columns 1-4 of Table

3.6.1). On average, serious criminals are associated with delivering 3 more NREGS projects to each polling station during their electoral term. At the same time, serious criminals increase pay, employment and material expenditures relative to clean politicians. Interestingly, polling station political competition shows a precisely estimated zero correlation with NREGS provision (top row second panel of Figure 3.6.1). In other words, there is no evidence that MLAs target NREGS resources to core villages. The lack of targeting when considering both types of MLAs together, is consistent with previous work by (Das and Maiorano, 2019a).[62] Still, it could be that there are differential targeting rates between criminal and clean politicians that net out when combined.

Other personal candidate characteristics show precisely estimated null associations with NREGS delivery. An MLAs *Wealth, Age* and *Sex* are not predictive of NREGS provision. However, party politics are predictive of NREGS delivery. Consistent with previous findings by Gulzar and Pasquale (2017) and Dasgupta (2016) Indian National Congress' MLAs are associated with increased NREGS provision relative to other parties' politicians. Recall that the INC developed and implemented NREGS with voters seeing the scheme as a "Congress program." On the other hand, opposition MLAs are less likely to deliver NREGS resources (a la findings from (Bohlken, 2018) regarding MPLADS).

[62]However Das and Maiorano (2019a) find increased targeting of material expenditures as a reward for local contractors in Andhra Pradesh. I examine this alternative explanation in more detail in the discussion below, but note that I find no evidence of greater material expenditures for core polling stations. Nor do NREGS projects increase material expenditures as a percentage of overall project costs in areas that support the MLA (Das and Maiorano (2019a)'s preferred measure of rewarding contractors).

Figure 3.6.1: KRLS Marginal Effect Estimates on NREGS Outcomes



3.6.1 Criminal Politicians and NREGS Targeting Results

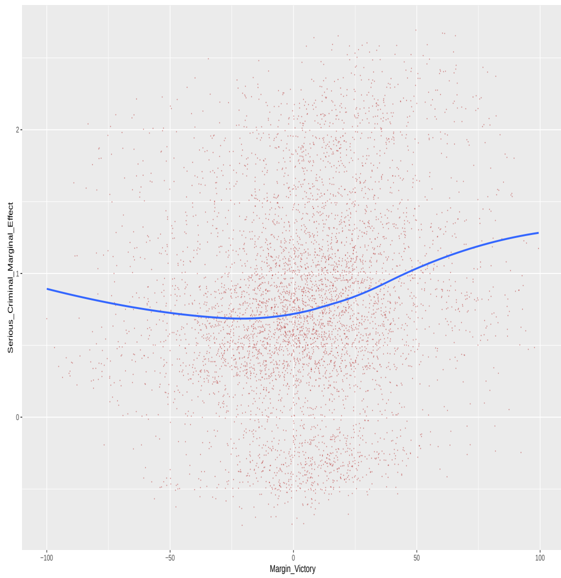
Is the association between MLA criminality and NREGS provision moderated by political competition? To test the targeting hypothesis I consider the interaction between the criminal status of the MLA and political support.

An added benefit of KRLS is the ability to examine effect heterogeneity. In particular, KRLS allows researcher to investigate non-linearities and interactions using pointwise marginal effects. Figure 3.6.2 examines the core hypothesis of differential targeting between criminal and clean politicians. KRLS calculates pointwise marginal effects (or in the case of dichotomous variables, first differences) for each predictor (Hainmueller and Hazlett, 2014). In my analysis this results in first difference estimates of the predicted effect of criminality on NREGS delivery for each polling station. Thus, I can compare predicted NREGS outcomes between criminal and clean MLAs across the range of polling station political competition. Figure 3.6.2 displays first difference estimates comparing serious criminals to clean politicians for varying polling station margins of victory. Each point represents a first difference estimate of the effect of criminality on NREGS outcomes for an individual polling station at a given margin of victory. For visual clarity, I randomly select 20 percent of polling stations to show the distribution of the raw point estimates (about 5,400 polling stations, i.e. 20% of 27,000 polling stations). I then fit a loess curve to illustrate the general trend in effect estimates across the range of political competition.

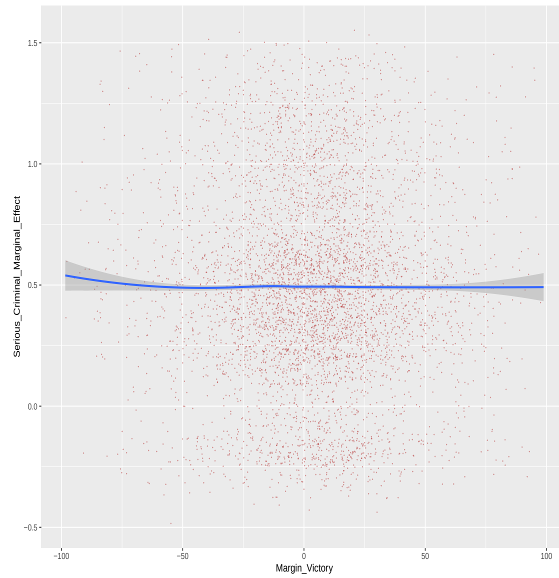
The loess curves shows positive- if small- increases in NREGS provision when moving from competitive to core polling stations. In other words, as polling station margin of victory increases, the predicted difference in NREGS delivery between criminal and clean politicians increases. The loess curve is consistently above zero marginal effect, indicating Criminals deliver more NREGS across the entire range of polling station political competition. Moreover, this is especially true among core support areas as there is a noticeable (if small), positive trend above a 25% margin of victory. These results hold across all NREGS outcomes. To make this more concrete, consider competitive polling stations where the winning MLAs margin of victory is between -10 and +10 percent. On average, Criminal MLAs are predicted to deliver 2.8 more NREGS projects than clean politicians. However, in core areas where an MLA wins by more than 25%, Criminals are expected to deliver 3.6 more projects, on average. In other words, criminal politicians are predicted to deliver nearly one additional project over their already superior performance when we consider only core polling stations. Overall, there is consistent evidence across all NREGS outcomes that criminals are associated with more NREGS delivery, on average, across the entire range of polling station political competition (i.e. the loess curve never falls below 0 for an estimated first difference). Moreover, criminals are predicted to outperform clean politicians when they dominate the local polling stations (except for NREGS employment see panel B). This evidence is consistent with criminal politicians having a comparative advantage in targeting core supporters.

Figure 3.6.2: First Differences of Criminality moderated by Polling Station Political Competition

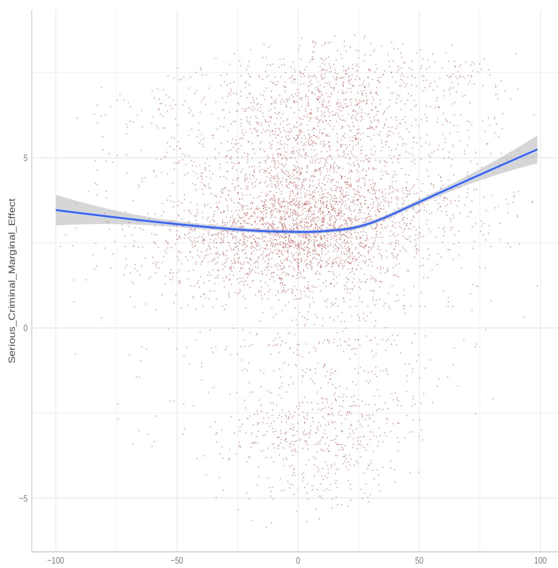
(a) Pay



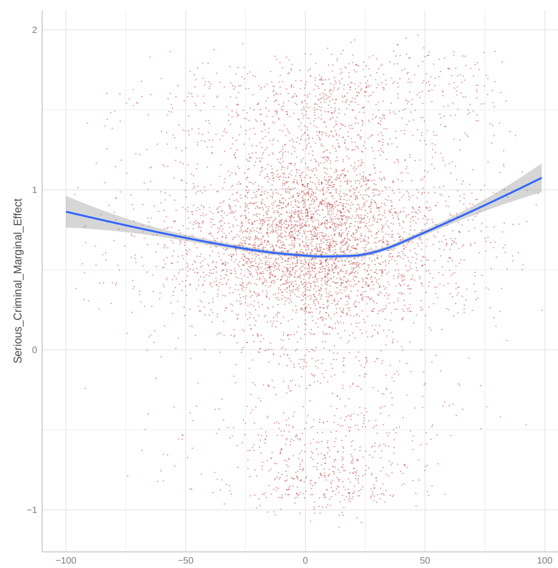
(b) Workdays



(c) Total Projects



(d) Material Expenditure



The x-axis in all figures represents polling station margin of victory. In other words, moving from left to right is moving from opposition to core polling stations. The y-axis represents First Differences for the serious criminal indicator. Each point estimate represents the first difference for an individual polling station estimate. Points above 0 (i.e. the vast majority) indicate that criminal politicians are estimated to be associated with superior NREGS delivery at that polling station. The loess curves sloping upward indicate that serious criminals outperform clean candidates by greater margins in core polling stations (i.e. where criminals have a larger margin of victory). These trends are indicative of superior targeting on behalf of criminal politicians.

3.7 Discussion

In this section, I consider if criminal politicians advantages in money, muscle and networks may explain their positive association with delivering more NREGS resources. In addition, I break apart supporter networks, to investigate if increased NREGS benefits flow primarily to voters or local contractors. By comparing individual NREGS projects in core and opposition areas, I test if differential spending is funneled to voters via increased wages or to local contractors via material expenditures. Third, I consider whether results depend on constituency-wide political competition. Finally, I demonstrate that the main results hold when testing KRLS models on an unseen holdout sample of polling stations.

3.7.1 Money

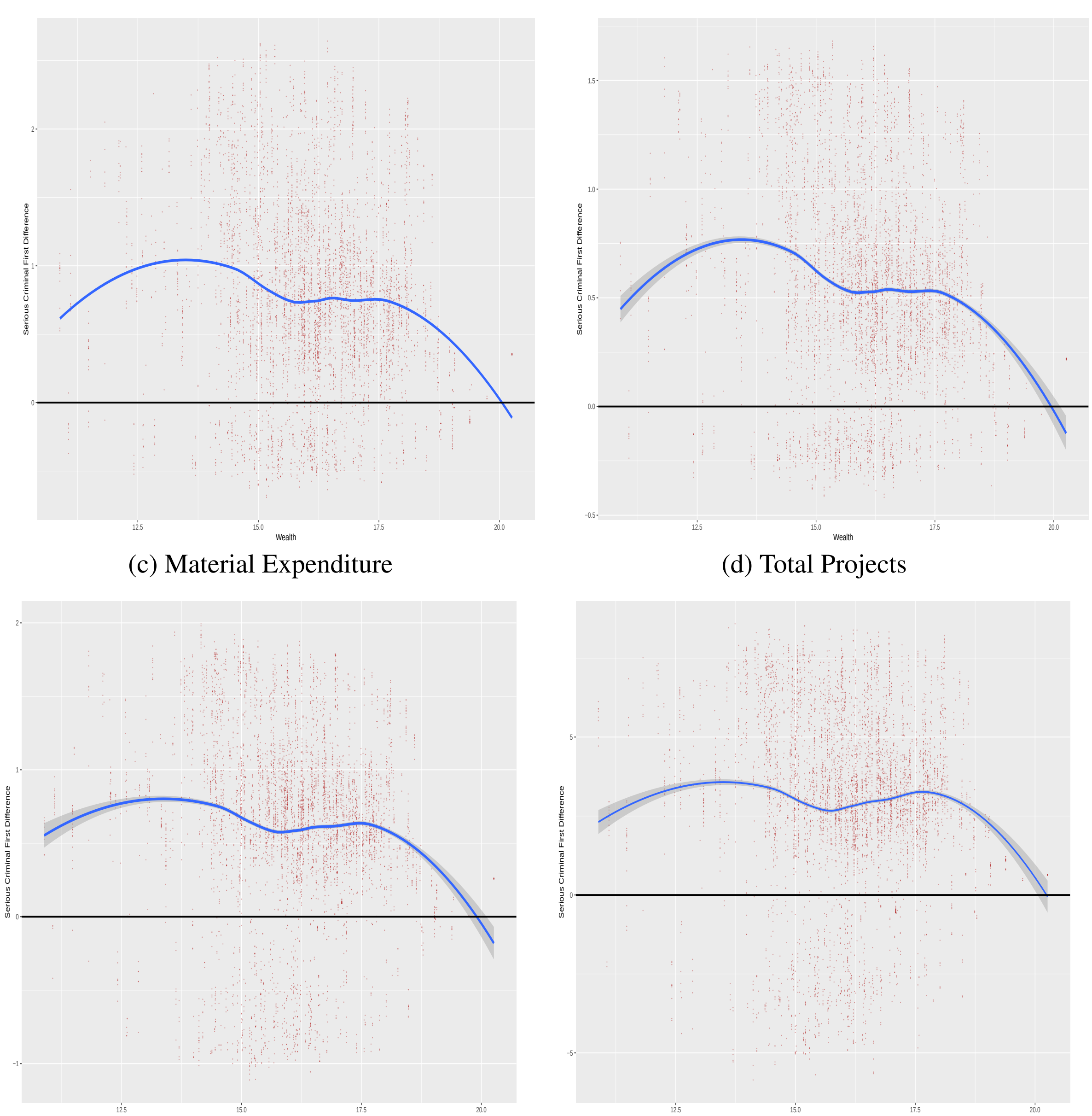
Do criminal politicians liquid assets drive superior NREGS delivery? Criminal politicians tend to be wealthier on average (Dutta and Gupta, 2014). Money may grease the wheels of bureaucracy, enabling NREGS funds to flow (Maiorano, 2014). Marcesse (2018) finds that local politicians are sometimes required to pay bribes out of pocket before NREGS projects are approved. Alternatively, money may enmesh voters in networks of dependence, enabling NREGS to flow down pre-existing channels. While the previous models controlled for politicians' assets and liabilities, Figure 3.7.1 explicitly examines the wealth mechanism. I leverage KRLS pointwise estimates to examine heterogeneity in the effect of criminality by the level of politician wealth. Figure 3.7.1 plots the pointwise first differences of criminality by MLAs logged wealth. Each point represents the predicted effect of criminality on NREGS resource delivery to a polling station. If criminal MLAs superior NREGS performance is driven by wealth, I would expect an increase in the effect of criminality moving across the wealth spectrum (x-axis). However across all NREGS outcomes, the first difference effect of Criminality on NREGS is *decreasing* in criminals' wealth. In other words, while wealthy criminals still perform slightly better than wealthy but clean MLAs, the biggest difference in predicted NREGS performance is among poorer politicians. In short, the association between criminal MLAs and improved NREGS delivery is driven by poorer politicians.

This does not mean that wealth is unimportant to political survival or resource delivery. For example, politicians may substitute cash payments for targeting effort. Instead of delivering state resources, wealthy MLAs may rely on superior campaign spending or personalized constituent service to carry them to victory. However, the overall effect of wealth on NREGS delivery is a precisely estimated zero (see *Total Projects* in Table 3.6.1). Whereas, a substitution hypothesis would predict a strong negative effect of wealth on NREGS delivery as poorer politicians compete with the only tool in their toolbox.

In sum, the evidence is inconsistent with the money hypothesis. Wealth may still enable criminal's to solve other constituent problems (e.g. paying for weddings, temples or school fees as witnessed at Arun Yadav's janata durbar). However, I find no evidence that access to liquid funds is associated with criminal MLAs predicted improved delivery of NREGS resources.

Figure 3.7.1: First Differences of Criminality moderated by Wealth

(a) Pay (b) Workdays (c) Material Expenditure (d) Total Projects



The x-axis in all figures represents logged wealth. In other words, moving from left to right is moving from lower levels of wealth to higher levels of wealth. The y-axis represents First Differences for the serious criminal indicator. Each point estimate represents the first difference for an individual polling station estimate. Points above the black line at 0 (i.e. the vast majority) indicate that criminal politicians are estimated to be associated with superior NREGS delivery at that polling station. However the downward slopping loess curves indicate that wealthier criminal politicians tend to deliver similar levels of NREGS benefits as clean politicians. However, among poorer politicians, criminals demonstrate superior NREGS delivery.

3.7.2 Muscle

Criminal politicians with a capacity for violence can credibly threaten bureaucrats to redirect resources towards their political needs. Given that MLAs violent reputations may precede them, sometimes a simple phone call will suffice to bend the bureaucracy to a politician's will (Vaishnav (2017), fieldwork 2017). In short, criminals supply of muscle could improve their ability to target resources like NREGS. To test the *Muscle* hypothesis, I use the Indian Penal Code to identify MLAs charged with violent crimes. I define *violent* charges as those being both serious in nature (i.e. max sentence of at least two years and non-bailable) and demonstrating a capacity for violence.[63] Types of violent crime include charges associated with murder, attempted murder, homicide, assault (including sexual assault), rape, kidnapping, armed robbery, extortion and arson.[64] For example, I code IPC section 324 "Voluntarily causing hurt by dangerous weapons or means" as indicative of a violent criminal politician. However, IPC section 323 "Punishment for voluntarily causing hurt" is coded as non-violent. The maximum punishment for violating IPC 323 is one year and the offense is bailable. Therefore I do not code it as "serious crime." Following Vaishnav (2012) I do not code charges related to rioting as violent. Even though these types of charges may demonstrate a capacity for violence, political opponents may try to trump up charges against politicians holding rallies. Therefore, these politically motivated charges are less likely to capture the underlying latent concept of a "Violent Criminal Politician."

Table 3.7.1 compares OLS results for Violent (Panel A) and Non-Violent (Panel B) criminals.[65] Non-violent criminals are all MLAs facing a serious charge not coded as violent. Column 1 shows the marginal effect of criminality on NREGS pay for rural polling stations. The previously

[63] Indian Penal Code descriptions are taken from `https://www.latestlaws.com/`. My coding is similar to the Association for Democratic Reforms definition of violent crimes (myneta.org) and follows inspiration from (Asher and Novosad, 2016)

[64] There are 144 unique IPC charges in my training data sample, 36 of which (25%) I code as violent. The Indian Penal Code is divided into chapters with most of the violent charges pertaining to Chapter 16 "Of Offenses Affecting the Human Body."

[65] I use OLS instead of KRLS to explore this mechanism due to computational cost constraints in rerunning KRLS models after recoding criminality twice for both Violent and Non-violent charges. For full results, including control variable coefficients see Tables and coefficient plots in Appendix 3.F.2

discussed result that criminal politicians are associated with increased NREGS delivery is driven entirely by violent criminals (see bold coefficients in Panel A compared to bold coefficients in Panel B). Violent criminal politicians have a consistent, positive association with NREGS delivery regardless of the outcome analyzed (see columns 1-4). The difference in coefficients between Violent and Non-violent criminals is statistically significant across all four outcomes. The models also include an interaction between violent criminality and polling station margin of victory to test for differential targeting (see third row of Panel A and B for interaction coefficients). Neither Violent nor Non-Violent MLAs show an increased proclivity to target core polling stations. While OLS may not pick up the subtle non-linearities found in prior KRLS models, the results still indicate the strong positive association between criminality and NREGS outcomes is attributable to politicians charged with *Violent* crimes. In sum, the data is more consistent with the *Muscle* hypothesis, that criminals violent nature is associated with improved resource delivery. In other words, money is fungible but muscle matters.

Table 3.7.1: Muscle Mechanism Test: Comparing Violent and Non-Violent Criminals

Panel A: Violent Criminals

	Pay	Materials	Workdays	Total Projects
Violent Criminal	$\mathbf{1.15}^{***}$	$\mathbf{0.96}^{**}$	$\mathbf{0.77}^{***}$	$\mathbf{3.79}^{**}$
	(0.34)	(0.29)	(0.22)	(1.16)
Margin of Victory	-0.00	0.00	-0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Violent Criminal x MoV	-0.00	-0.00	0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.01)
Controls	Yes	Yes	Yes	Yes
Obs.	107894.00	107891.00	107894.00	107894.00
Adj. R-Squared	0.08	0.06	0.08	0.04

Panel B: Non-Violent Criminals

	Pay	Materials	Workdays	Total Projects
Non-Violent Criminal	$\mathbf{-0.30}$	$\mathbf{-0.31}$	$\mathbf{-0.19}$	$\mathbf{-0.24}$
	(0.45)	(0.38)	(0.28)	(1.56)
Margin of Victory	-0.00	-0.00	-0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Non-Violent Criminal x MoV	0.01	0.01	0.01	0.00
	(0.01)	(0.01)	(0.00)	(0.02)
Controls	Yes	Yes	Yes	Yes
Obs.	107894.00	107891.00	107894.00	107894.00
Adj. R-Squared	0.08	0.06	0.08	0.04

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$ Standard errors in parentheses are clustered by Assembly Constituency.

Standard errors in parentheses are clustered by Assembly Constituency.

3.7.3 Networks: Contractor Raj?

The KRLS results from section 3.6 show that criminals have a proclivity to target core polling stations with NREGS resources. Who benefits from additional NREGS funding? On one hand, more wages and workdays will likely benefit voters who labor on NREGS projects. On the other, rents from material expenditures likely accrue to local contractors and politicians (Das and Maiorano (2019a), fieldwork). Do MLAs reward local elite or voters in their networks? To investigate this question, I conduct a project-level analysis. That is, instead of aggregating NREGS projects up to the polling station, I compare individual projects in core and opposition areas. Specifically, I ask if NREGS projects assigned to supporter polling stations are fundamentally different than projects assigned to opposition polling stations?

In this setup, the logic is that criminal politicians prioritize projects in their core areas funneling greater payments and employment to their supporters. In part, the project level analysis recognizes that placement of NREGS projects is endogenous to local demand for employment. Some villages will simply desire more NREGS projects. Still, politicians might be able to influence the money that flows to individual projects by pressuring block level bureaucrats to fast track certain projects and district officials to release more money to criminals' core areas. Second, a project level analysis can provide some suggestive insights into criminal politicians' proclivity to channel extra resources towards voters or local elites. If projects allocated to core-areas fundamentally benefited voters I would expect these projects to have higher levels of employment and wages. On the other hand if networks in core-areas facilitate a "contractor-raj," where rents accrue to local elites, I would expect greater material expenditures and a higher percentage of total project expenditures allocated towards materials.

To investigate if MLAs prioritize voters in core polling stations I compare labor expenditures (*Pay*) and employment (*Workdays*) generated per project. Conversely, if MLAs privilege local elites, I would expect greater material expenditures on projects. I use *Material Expenditures* and

Percent Material to measure if certain NREGS projects funnel more money towards contractors and villages politicians. Local elites, politicians and contractors tend to skim rents from the material portion of the project and may pay kickbacks to bureaucrats and higher level politicians for this privilege (Das and Maiorano (2019a), Fieldwork). *Percent Material* measures the share of project spending dedicated to materials. By law, NREGS projects are capped at 40% material expenditure to ensure that at least 60% of project spending is directed towards laborers. This regulation was put in place specifically to deter rent seeking by local contractors. By interacting polling station margin of victory with criminality I can compare if NREGS projects differ on these funding measures in supporter and opposition areas.

Figure 3.7.2: Slope Estimates for Candidate Covariates (NREGS Projects)

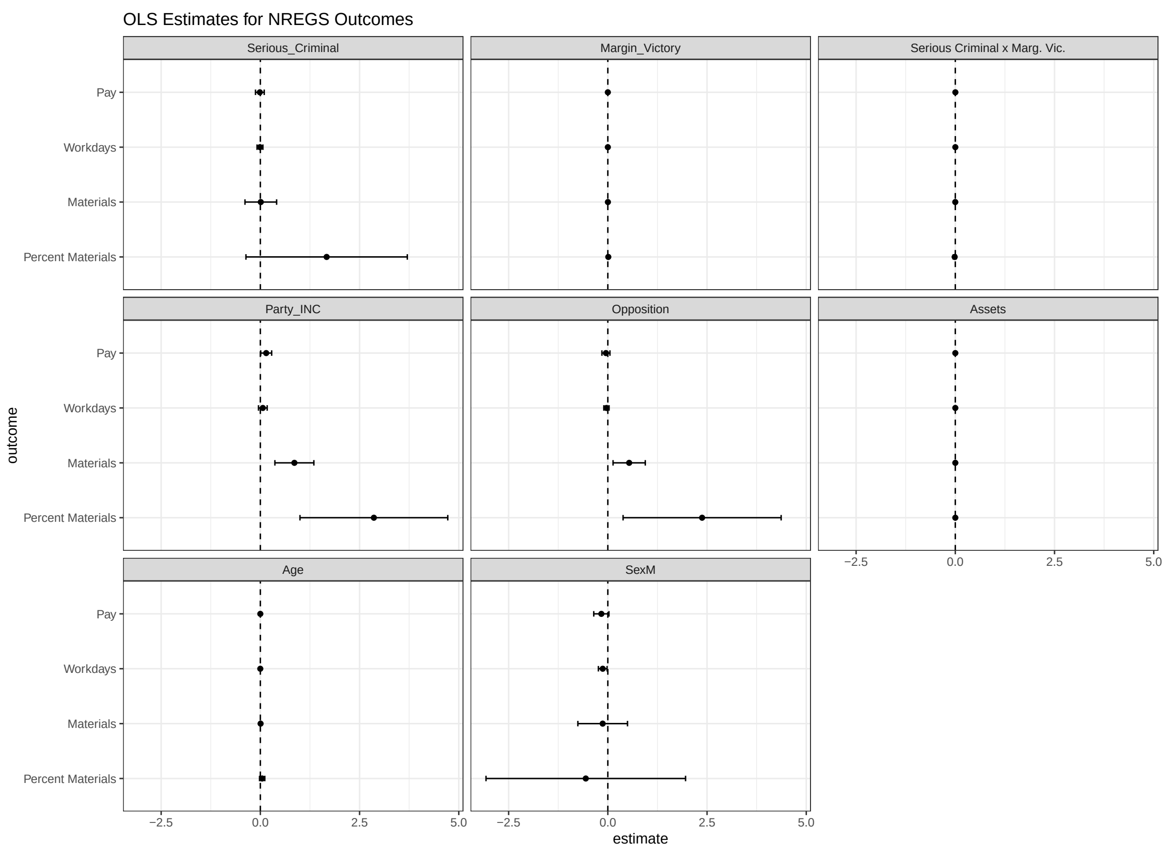


Figure 3.7.2 displays coefficient estimates and 95% confidence intervals from over 1,000,000 NREGS projects. I do not find systematic evidence that NREGS fundings differs per project based

on the criminality of the MLA, polling station political competition nor their interaction (see Figure 3.7.2 top row).[66] In fact, criminality, polling station margin of victory, and their interaction are all precisely estimated zeros for both labor and material spending (see top row of panels in Figure 3.7.2). Projects are essentially the same on funding dimensions in criminal and clean constituencies. Likewise polling station political competition has no effect on altering project funding or employment. In other words, while I can not definitively partial out whether local elites or voters gain the most from any individual project, NREGS projects in core-areas are qualitatively similar to those in opposition areas.[67] Projects in core polling stations are not more expensive (see Pay and Materials outcomes in Figure 3.7.2). Nor do they privilege contractors by increasing the share of material expenditure (see Percent Material outcome in Figure 3.7.2).

In sum, NREGS projects are remarkably consistent across candidate characteristics and political competition. Larger, more expensive and more graft prone projects are not specifically allocated to core support areas. Recall from the main results that criminal constituencies see an overall increase in the number of projects and NREGS benefits, with these gains concentrated in core polling stations. It does not seem to be the case that "better" projects are located in criminal support areas at the expense of opposition or swing areas. Instead, criminals are associated with increased NREGS delivery, allocating more projects and thus more funds to all types of polling stations relative to clean politicians. Simply put, criminal politicians just deliver more NREGS funding.

[66] For full results, including controls, see coefficient plots in Appendix 3.F.3. For predicted effects of the interaction between criminality and polling station margin of victory for project-level outcomes see figures in the same Appendix.

[67] It is possible that contractors are able to extract more from projects located in core-areas but if so this is not a function of the project observables.

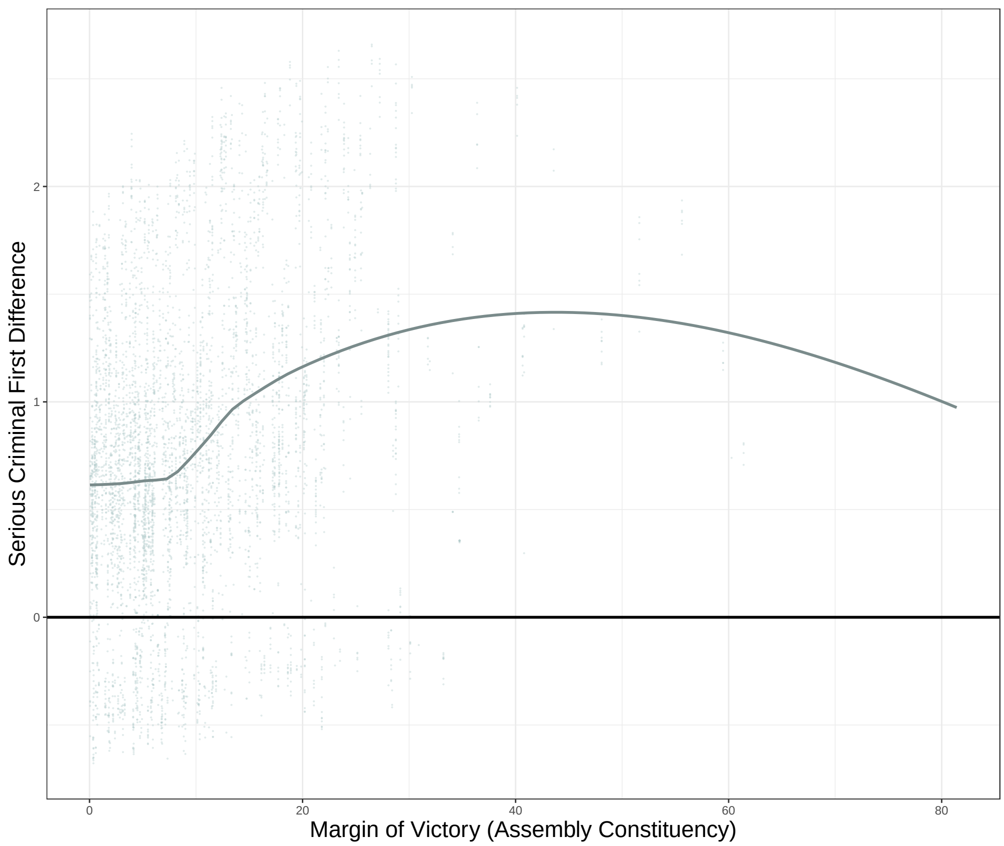
3.7.4 Assembly Constituency Political Competition

Do the results depend on how tight the MLA race is? It could be the case that incentives for targeting are only present when competition is fierce and MLAs fear losing their seats. In settings of high political competition, incumbents may use every tool in their toolbox to ensure victory. Whereas in safe seats, MLAs do not feel pressured to extend extra effort to target supporters. Qualitative fieldwork and quantitative evidence suggests that MLAs must address constituent concerns and provide personalized service if they want a seat at the electoral table and will be quickly rebuked if they fail to make these overtures (see Bussell (2019)). However, in competitive constituencies MLAs may feel added pressure to make every effort in rewarding previous supporters to keep those voters in their stable and turning out. In other words, does stronger evidence for targeting emerge when looking at competitive constituencies where incentives for targeting increase?

In Figures 3.7.3 and 3.7.4, I compare results between competitive and safe seats. While prior models controlled for assembly constituency political competition (either using margin of victory or effective number of parties), here I explicitly explore whether evidence of NREGS targerting differs across safe and contested constituencies. Specifically, Figure 3.7.3 plots the predicted pointwise first differences for Criminality against *Assembly Constituency* margin of victory for NREGS *Pay*. Moving from left to right across the x-axis is moving from competitive to safe seats, comparing the predicted polling station effect of criminality on NREGS labor expenditures. The positive LOESS trend (cyan line) predicts that criminals outperform clean MLAs by an even wider margin in safe seats. While criminals are predicted to deliver more NREGS resources across the entire spectrum of Assembly Constituency political competition, on average, this advantage is expected to increase in safer seats. In fact almost all of the individual pointwise first differences predict a positive effect of criminality when the Assembly Constituency margin of victory is greater than 20%.

Second, I compare differential polling station targeting in competitive and safe constituencies

Figure 3.7.3: Marginal Effect of Criminality on NREGS by Assembly Constituency Margin of Victory



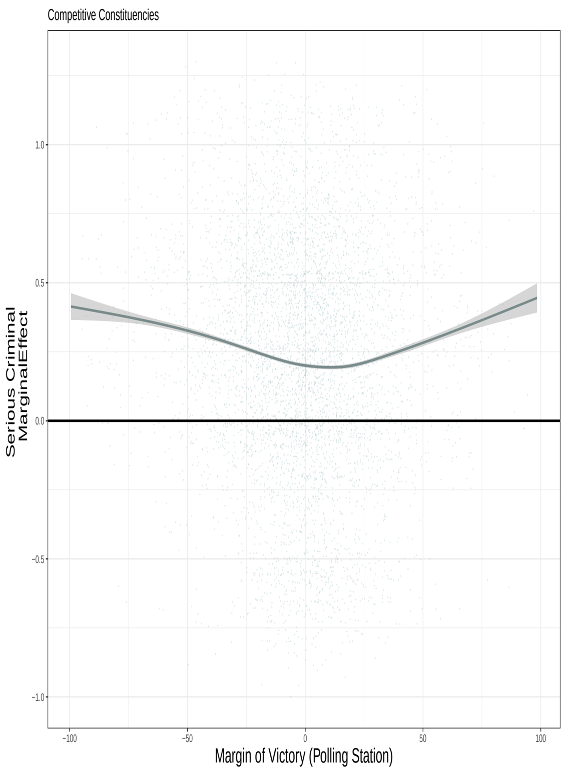
(Figure 3.7.4 parts A and B, respectively). I consider all constituencies where the margin of victory is less than 10% to be competitive, and those greater than 10% to be safe. Subsequently, I break apart each type of constituency to investigate targeting by polling station political competition. Thus, I can compare targeting trends in competitive and safe constituencies. Each figure plots the pointwise predicted effects of criminality against *polling station* margin of victory. In other words, these graphs compare if there is more evidence for differential targeting between criminal and clean politicians in competitive versus safe constituencies. For now, I focus on a single NREGS outcome, *Pay*. The overall trends in predicted effects are similar for competitive and safe assembly constituencies. Interestingly, the predicted effects of criminality are much larger, on average, for safe constituencies (part B) relative to competitive constituencies, across the spectrum of polling station competition. Put simply, while criminals are expected to deliver more NREGS wages, on average, the predicted difference between criminal and clean MLAs is much greater in safe seats. This result is consistent with the finding from Figure 3.7.3, that criminals are predicted to perform especially well in safe constituencies. Second, the targeting trends are similar in both safe and competitive seats. Criminals predicted advantage in delivering wages increases when moving from toss-up polling stations to core areas of support. Albeit, there is also an upward trend for opposition polling stations.

Disaggregating the analysis by Assembly Constituency competition can also help reconcile the findings in this chapter with those from the regression discontinuity analysis. Recall that the regression discontinuity found either imprecise or negative effects of criminality on NREGS delivery. Whereas, in this chapter Criminal MLAs are associated with increased access to NREGS funds. However, this positive effect of criminality is driven primarily by safe seats. Whereas, the regression discontinuity is constrained to compare extremely close elections by design. In other words, the RDD estimates the local average treatment effect of criminality to be negative. But when considering safe seats far away from the RD threshold, criminal politicians' apparent advantages materialize.

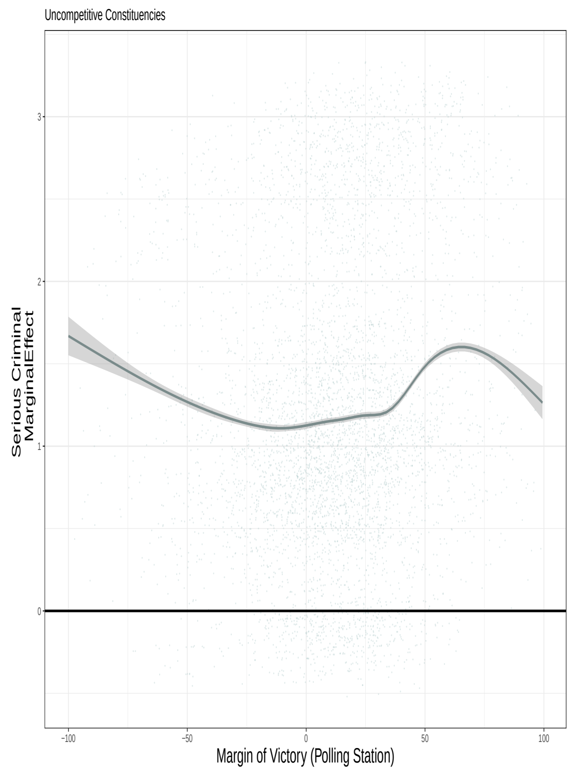
One potential explanation to reconcile these findings is that in competitive seats all types of candidates are incentivized to deliver. Any benefits of a criminals' type are washed out, either by the selection of similarly effective clean competitors or because parties focus efforts on these close battlegrounds. At the same time, political competition is endogenous to the effectivenes of MLAs in office. In other words, the relationship could be reversed with criminals winning votes exactly in the constituencies where they are most effective at delivering NREGS resources and meeting voters needs. While my analysis considers prospective targeting, rewarding voters for votes in the prior election, these time trends are difficult to untangle with observational data. Further research is required to ascertain if the "effect" of criminal politicians is consistently moderated by political competition. In appendix 3.F.4 I provide some suggestive evidence that clean politicians drop off in their targeting efforts more rapidly as the safety of their seat increases. However, the predicted interaction effects of criminality and assembly constituency margin of victory are not significantly different across politician type.

Figure 3.7.4: Targeting in Competitive and Safe Constituencies

(a) Pay Competitive Constituencies



(b) Pay Safe Constituencies



GAM with 95% confidence intervals. Random subsample of 10 percent of polling stations from random 50% subsample of training data.

3.7.5 Comparing Model Predictions on Test Data

I turn now to comparing the out of sample predictive power of KRLS models. Specifically, I compare models with and without a variable for Serious Criminality. I fit the models on the training data and then predict NREGS pay outcomes for the testing dataset. The holdout test set randomly samples 50% of the constituencies from the overall dataset (i.e. the 50% of constituencies not included in the training data). If criminal politicians are a strong driver of NREGS delivery, then models including the serious criminal variable should perform better. However, I find nearly identical predictive accuracy, regardless of whether the Serious Criminal variable is withheld. I estimate a 6.64 root mean squared error for each model (i.e. a measure of how far off the model prediction of NREGS pay is from the actual value of NREGS pay for a given polling station). Overall, including the Serious Criminal does not improve the models out of sample predictions. Which is understandable, since the other variables in the model include demographic and development measures which are known to be highly predictive of demand for a minimum wage employment program like NREGS. MLA criminality likely explains little additional variation in light of these other controls.

Second, I check if the result showing increased delivery for criminal politicians replicates for the testing data. Indeed the positive and significant coefficient on criminality is reproduced in the test sample (see Table 3.5, column 2). However, the effect of criminality does attenuate by about half for the holdout testing data. By replicating these baseline results, I help reduce uncertainty that the relationship between criminality and NREGS provision is a spurious artifact of the training sample.

3.8 Conclusion

One core explanation of criminal politician's electoral success is that they deliver for their own communities. Party leaders may view muscular politicians and caste defenders as strong candidates in constituencies dominated by parochial concerns (Vasihnav 2017). If criminal politicians'

Table 3.7.2: KRLS: Train vs. Test

	Train: Pay	**Test: Pay**
Serious Criminal	0.80^{***}	0.47^{***}
	(0.06)	(0.08)
Polling Station Margin of Victory	-0.00	0.00
	(0.00)	(0.00)
Obs.	26963.00	26726.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

This table compares the original first difference estimate of Serious Criminality for the training data to the same model run on the holdout testing data

primary advantage comes from exploiting cleavages and promoting a muscular "politics of dignity" (as Vaishnav alleges) then we should expect criminals to engage in more NREGS targeting. Or as Vaishnav (2017, p. 63) puts it "politicians associated with criminal activity...acquire political status on the basis of their ability to manipulate the state to divert benefits to a narrowly defined community."[68] In this chapter, I provide evidence that criminal politicians are associated with delivering more benefits to their own co-partisan communities.

In fact, I find that the predicted difference in NREGS delivery between criminal and clean politicians is greatest when the polling station margin of victory is above 25%. In other words, in ultra-core areas, criminal politicians are associated with far better delivery. This is consistent with recent work showing that targeting is partially dependent on the geographical concentration of supporters (Andrew Harris and Posner, 2019). At the same time, the evidence is consistent with criminal politicians taking a more expansive strategy. That is, I find that, relative to clean politicians, criminals improve NREGS allocation for a broad base and not only among supporters.

Why might criminal politicians more reliably target supporters? I leverage personal data on Members of the Legislative Assembly and rich information on individual NREGS projects to test potential mechanisms that could explain an increased propensity to target core supporters. Drawing

[68]In North India in particular, Vaishnav (2017) notes that caste based gangs stepped in and substituted for the state providing public goods where the state had failed.

on previous literature and fieldwork, I identify and systematically test three unique assets that could explain criminal politicians targeting advantage: *Money*, *Muscle* and *Networks*. I find the most evidence for the *Muscle* channel, with tentative support for the *networks* hypothesis. Conversely, the data is not consistent with the *Money* hypothesis where wealthy criminals deliver better NREGS outcomes.

Finally, I ask if results differ based on how tight the overall MLA race is? I split my sample into competitive and safe assembly constituencies to determine if incentives for targeting co-partisans are stronger when the overall race narrows. Criminals' superior NREGS provision is driven by safe constituencies. This finding provides one potential reconciliation for the negative and null effects of criminality found in the regression discontinuity chapter. Simply put, the RDD design only considers extremely competitive races, where the margin of victory is close to zero.[69] Whereas, in this chapter, I find the strongest, positive impacts of criminality on NREGS provision in safe seats-far away from the regression discontinuity threshold of zero margin of victory.

In addition to the core findings, I provide the first descriptive evidence of polling station competition for MLAs in six Indian states. I find that India's notoriously competitive elections replicate at the very local level, with an average margin of victory of 7% or less than 25 votes. Though this masks variation across states. Overall, this indicates that some states have greater political polarization at the micro-level and may be more amenable to politicians discerning and targeting supporters.

Finally, I use several methods to improve predictions and limit model misspecification while using observational data. In observational settings one default may be to model outcomes and conduct robustness tests to check specification sensitivity. However, I show that even for relatively few alternative specification decisions the number of potential models explodes and results can

[69]To be clear, this chapter looks at only a subsample of states, but the finding is consistent with a moderating result of political competition across both chapters. For instance, it could be the case that in close races, clean politicians face strong incentives to increase NREGS provision so any innate benefit to criminality is trumped by extra effort from clean MLAs. Also, technically speaking, in some models the RDD draws strength from every constituency race, increasing weight for races close to the threshold.

vary widely. As data becomes more readily available and computational costs decrease, sample splitting and machine learning models present one fruitful way forward. By testing models on holdout data we can increase our confidence that results are more reliable and likely to replicate.

Appendix

3.A Coding Ruling and Opposition Parties

Table 3.A.1: Party Coalitions for State Legislatures in Sample

State	Election Year	Ruling Coalition	Opposition	Other
Assam	2011	INC	AGP, AIUDF, BJP, AITC, IND	BOPF
Assam	2016	NDA: BJP, BOPF, AGP	AIUDF, INC	IND
Himachal Pradesh	2012	INC	BJP	HLP, IND
Kerala	2011	UDF: INC, MUL, SJD, KEC(J), KEC(M), KEC(B), KRSP	LDF: CPM, CPI, JD(S), NCP, RSP	IND
Kerala	2016	LDF: CPI, CPM, NCP, NSC, KEC(B), JD(S), C(S)	UDF: INC, IUML, KEC(M), CMPKSC, KEC(J)	NDA: BJP
Tamil Nadu	2011	AIADMK, DMDK, CPI, CPM, MAMAK, PT, AIFB, AIDMK	DMK, INC, PMK	IND
Tamil Nadu	2016	ADMK	IUML, INC, DMK	IND
Uttar Pradesh	2012	SP	BSP, INC, SP, RLD, PECP, BJP, IEMC, NCP, QED, IND	
West Bengal	2011	AITC, INC, SUCI	CPIM, CPI, RSPI, AIFB, RCP, SP, DSP(P), BBC, WPI	GOJAM, IND

This table only includes parties that won a seat in an assembly election. I did not code parties that failed to win. Independent Candidates are considered as the opposition except when I could find definitive evidence that these candidates contested as part of the ruling alliance (e.g. in Kerala). Coalition partners are inclusive of both pre-election partners and partners in government as it was difficult to find evidence of the later.

3.B NREGS Projects

Table 3.B.1: NREGS Project Types and Project Codes

	Project Category	Project Code	Total	Percentage
1	Irrigation Facility	IF	405259	22.80
2	Rural Connectivity	RC	287821	16.19
3	Rural Sanitation	RS	222621	12.52
4	Land Development	LD	195322	10.99
5	Water Conservation	WC	160491	9.03
6	Water Harvesting	WH	98803	5.56
7	Flood Protection	FP	83216	4.68
8	Drought Proofing	DP	78894	4.44
9	Irrigation Canals	IC	73115	4.11
10	Maintenance of Rural Public Assets	OP	22173	1.25
11	Drinking Water	DW	2279	0.13
12	BNRGSK Resource Center	SK	814	0.05
13	Fisheries Related	FR	454	0.03
14	Anganwadi Center Construction	AV	443	0.02
15	Playground Construction	PG	313	0.02
16	Coastal Areas	CA	205	0.01
17	Food Grain	FG	19	0.00

Sources: MGNREGA Operational Guidelines 2013. https://nrega.nic.in/Circular_Archive/archive/Operational_guidelines_4thEdition_eng_2013.pdf

3.C Data Matching and Merging

3.C.1 Match Rates for TCPD Candidate Results and ADR Criminal Charges

Match rates are based on all state elections (i.e. the study group included in the previous chapter).

Matching just by Candidate Name, Constituency state and year matches 73% of all candidates' affidavits, though this likely has some duplicates. By fuzzy matching, I increase the match rate to 100 percent of all affidavits and 74 percent of all potential candidates. The median match rate for an election year is 95 percent across all three thresholds. The table and boxplots below summarize the distribution of candidate matches between the elections/results and charges dataset.

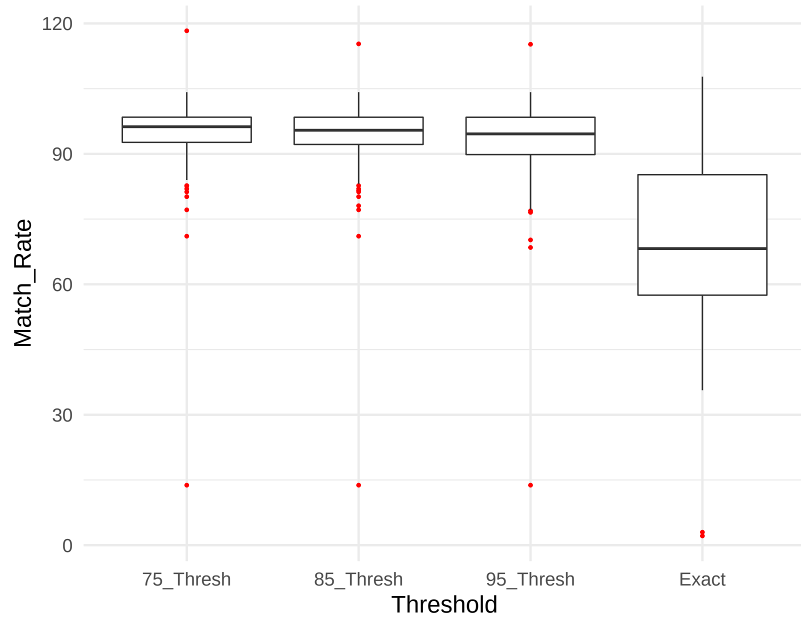
Table 3.C.1: Match Rates for Candidate Results and Criminal Charges

Statistic	N	Mean	St. Dev.	Min	Max
95_Thresh	77	92.1	12	13.8	115.2
85_Thresh	77	93.1	11.5	13.8	115.3
75_Thresh	77	93.8	11.4	13.8	118.3
Exact	77	68.8	18.9	2.1	107.8

Median is 95% for state elections across all 3 thresholds. Overall, I match 100% of all candidate affidavits and 74% of all candidates from the TCPD Lok Dhaba data. Note this excludes bye-elections

Note also that this is more stringent than just matching them by party within an assembly constituency. I could match the names of the assembly constituency and then match on party for major parties though this would not work for independent candidates. Still matching on assembly constituency, candidate name, party and age creates a more stringent criteria for correct matches. I still achieve a median match rate of 95% and match 100% of all affidavits.

Figure 3.C.1: Candidate Results and Criminal Records Match Rates for State Elections



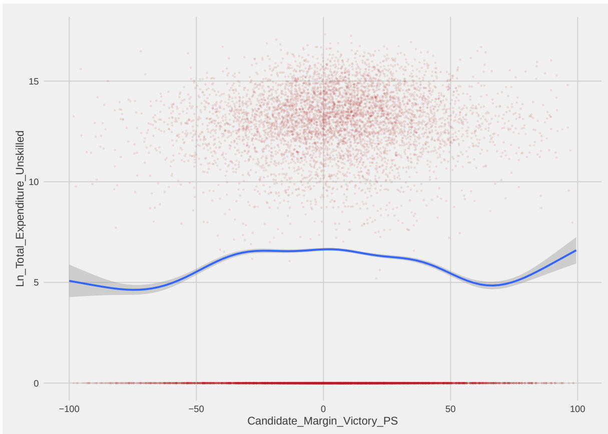
Median is 95% for state elections across all 3 thresholds. Overall, I match 100% of all candidate affidavits and 74% of all candidates. Note this excludes bye-elections

3.D Political Competition

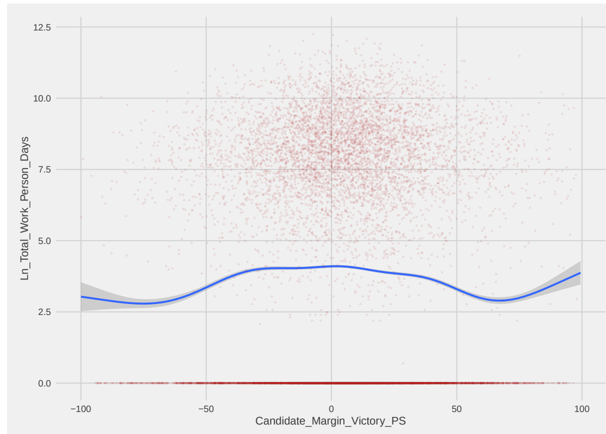
3.D.1 Graphical Analysis of Political Competition and NREGS Distribution

Here I display the raw data comparing the distributions of NREGS outcomes by polling station competition and criminal status of the MLA. Figure 3.D.1 below plots NREGS outcomes (log(*pay*), log(*workdays*), log(*materials*), *total projects*) against the range of polling station political competition. The loess smoother represents the trend of NREGS provision across varying levels of polling station political competition for all 120,141 rural polling stations. I randomly sample 5,000 polling stations to give a sense of the raw data distribution. However, trend lines are fit to the entire training dataset. Each point indicates an individual polling station and the total NREGS resources distributed during an MLAs' five year electoral term. In general the distribution of NREGS resources shows some evidence of targeting swing polling stations (though the main takeaway is that the trends appear flat across the range of political competition). Second there is perhaps a slight uptick in expenditures and employment in areas where the MLA is extremely dominant (i.e. where the margin of victory is greater than 75%).

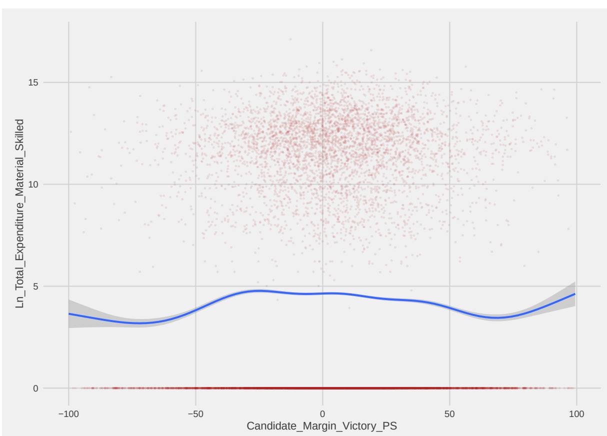
Figure 3.D.1: NREGS vs. Political Competition



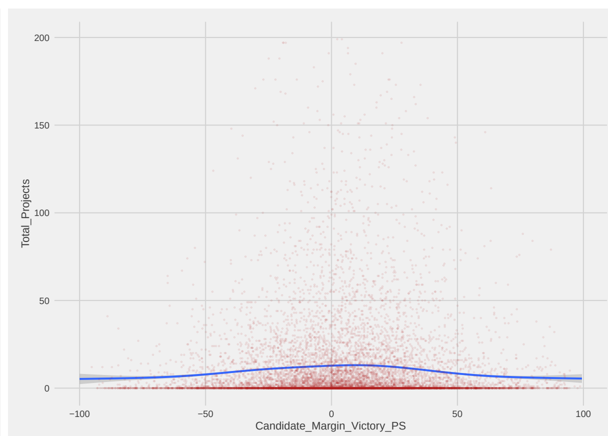
(a) Labor Expenditures



(b) Work Days



(c) Material Expenditures



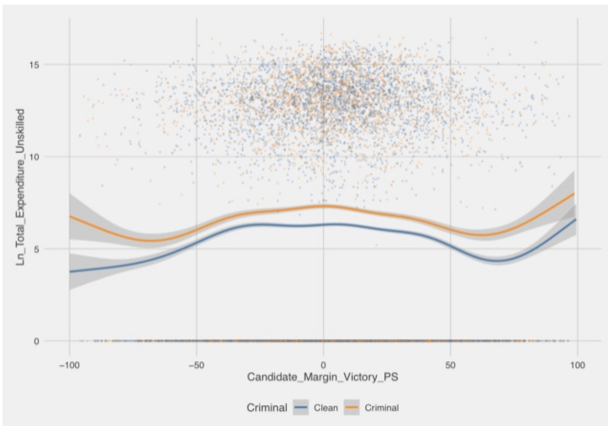
(d) Total Projects

NREGS outcomes are on the y-axis and polling station political competition is on the x-axis.

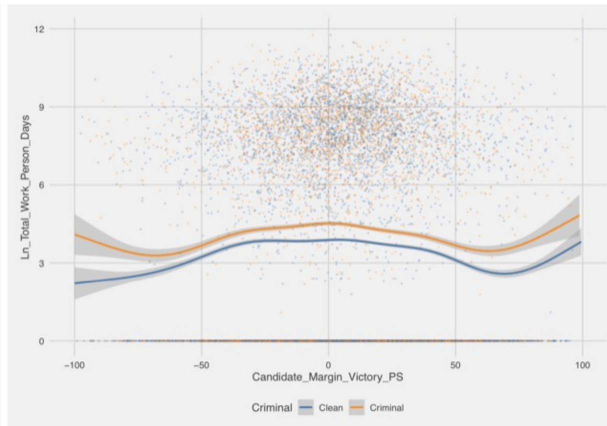
Breaking out the same graphs by criminal status of the MLA reproduces the same overall pattern for both types of politicians (Figure 3.D.2). There is perhaps a slight tendency towards favoring swing polling stations. There is no strong visual pattern of co-partisan targeting, though again perhaps a slight uptick in NREGS resources flowing to areas where the MLA is most dominant. Overall, the loess curves show fairly flat trends for expenditures and employment across polling station competition, with a slight uptick above 75%. Interestingly, criminal MLAs are associated with higher rates of pay and workdays across the entire range of polling station political competition. In other words, criminal politicians are associated with delivering more NREGS employment regardless of the local level of political competition. Criminals deliver more total projects overall as well. Finally, for material expenditures (Figure 4c) there is a discernible separation at extreme levels of support, with criminal politicians associated with greater material expenditures. Delivering more material expenditures to core areas would be consistent with rewarding local elites rather than voters (Das and Maiorano (2019a)).

Obviously, this visual investigation is preliminary and I do not want to over-interpret potentially random fluctuations, even with the large number of observations. For example, replicating the graphs for each state individually shows little difference between criminal and clean constituencies in the distribution of NREGS resources (see figures in Appendix **??**). Likewise, no state shows dramatic evidence of co-partisan targeting. Instead, the difference in criminal constituencies receiving more NREGS resources is only visually apparent after aggregating across states. To more systematically adjust for potential confounders across political constituencies and types I turn now to modeling the distribution of NREGS resources.

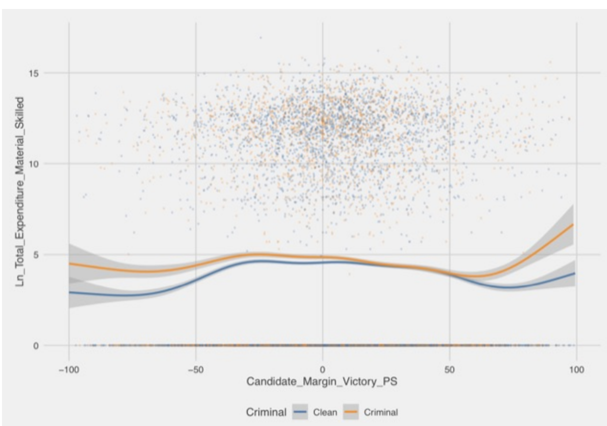
Figure 3.D.2: NREGS vs. Political Competition by Criminality



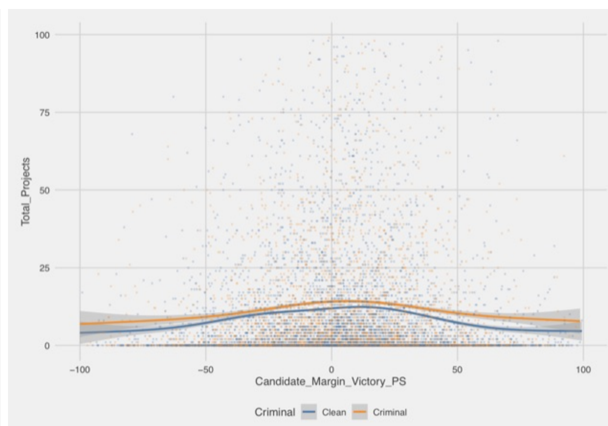
(a) Labor Expenditures



(b) Work Days



(c) Material Expenditures



(d) Total Projects

NREGS outcomes are on the y-axis and polling station political competition is on the x-axis. The orange line is the loess smooth of NREGS distribution across polling station competition in criminally governed constituencies. The blue line displays the loess smooth for clean governed constituencies.

3.D.2 Urban and Rural Polling Stations

Table 3.D.1: Polling Station Political Competition: Full Sample

Statistic	N	Mean	St. Dev.	Min	Max
PollingStation_Vote_Percent	203,327	44.5	17.8	0.0	100.0
Candidate_Margin_Victory_PS	202,974	6.9	28.5	−100.0	99.4
Votes_PS	205,431	292.8	160.6	0.0	5,194.0
Total_Votes_perPS	205,429	652.4	237.9	0.0	18,870.0

Political competition summary statistics for polling stations across full sample (i.e. rural and urban polling stations in 9 state assembly elections). Descriptive Statistics are for the training sample only.

Table 3.D.2: Assembly Constituency Political Competition: Full Sample

Statistic	N	Mean	St. Dev.	Min	Max
Margin_Percentage	872	11.1	9.7	0.04	81.4
Vote_Share_Percentage	872	45.6	9.3	18.5	87.4

Political competition summary statistics for assembly constituencies across full sample (i.e. rural and urban polling stations in 9 state assembly elections). Descriptive Statistics are for the training sample only.

Figure 3.D.3: Full Sample: Density of Polling Station Margin by State

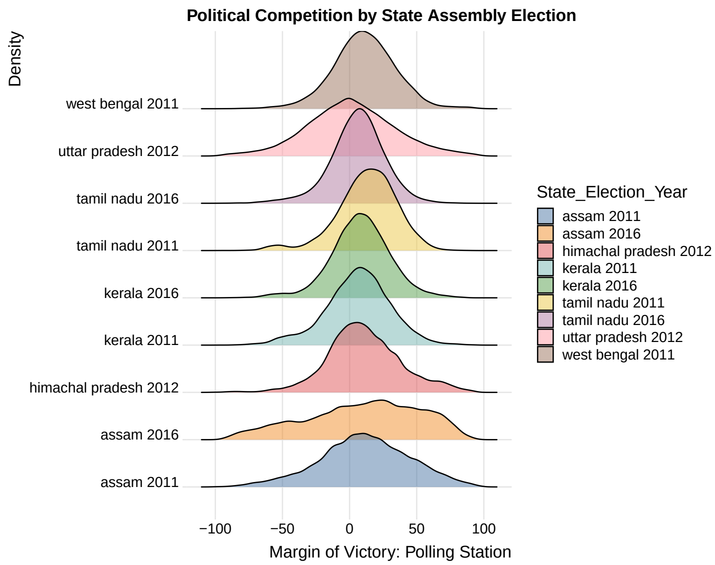


Figure 3.D.4: Full Sample: Density of Polling Station Vote Share by State

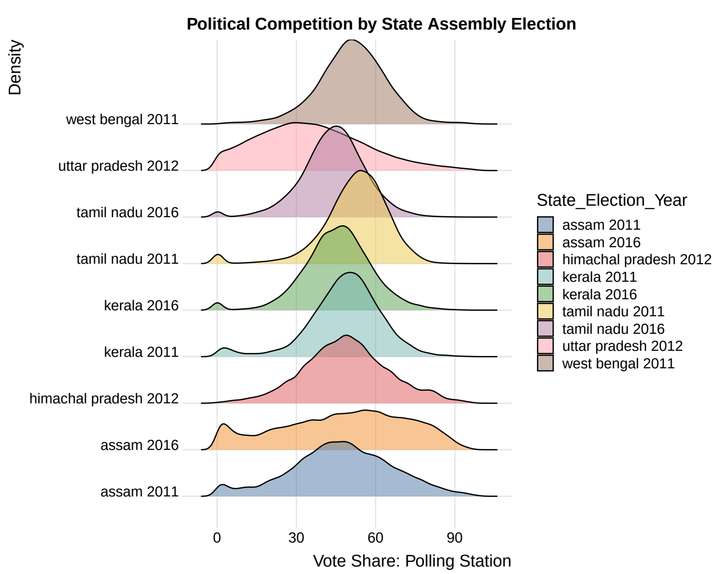


Figure 3.D.5: Full Sample: Density of Polling Station Votes Totals by State

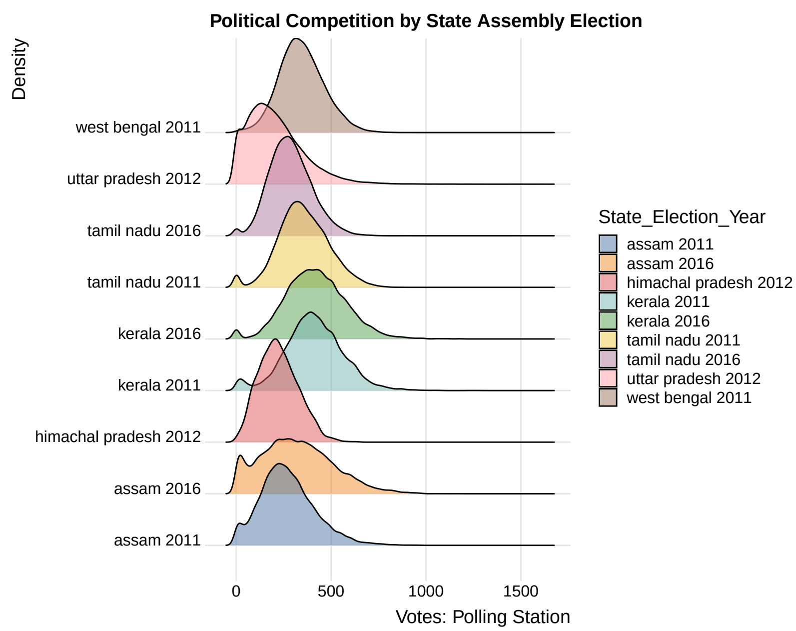


Figure 3.D.6: Density of Polling Station Margin of Victory by Criminality: Full Sample

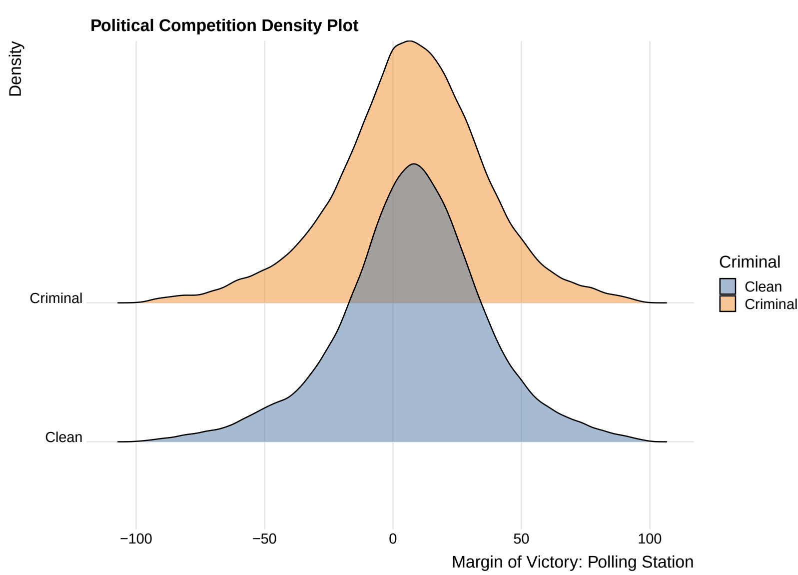


Figure 3.D.7: Density of Polling Station Vote Share by Criminality: Full Sample

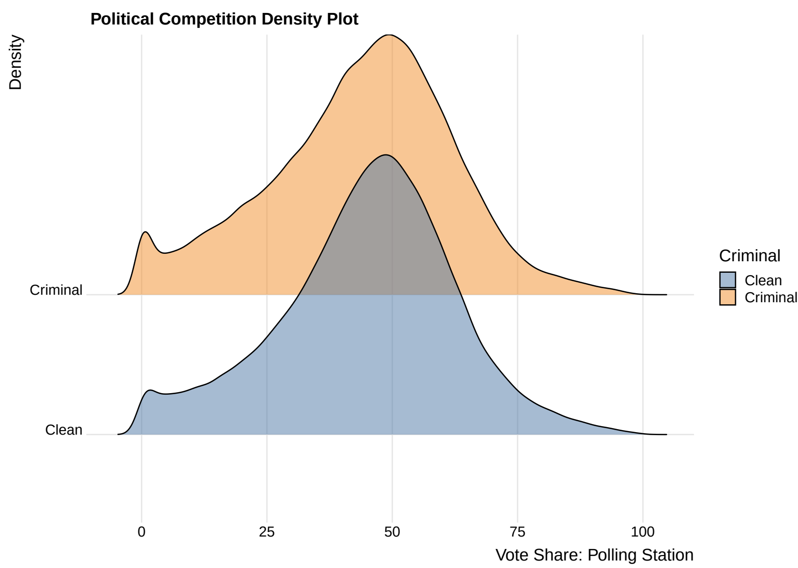
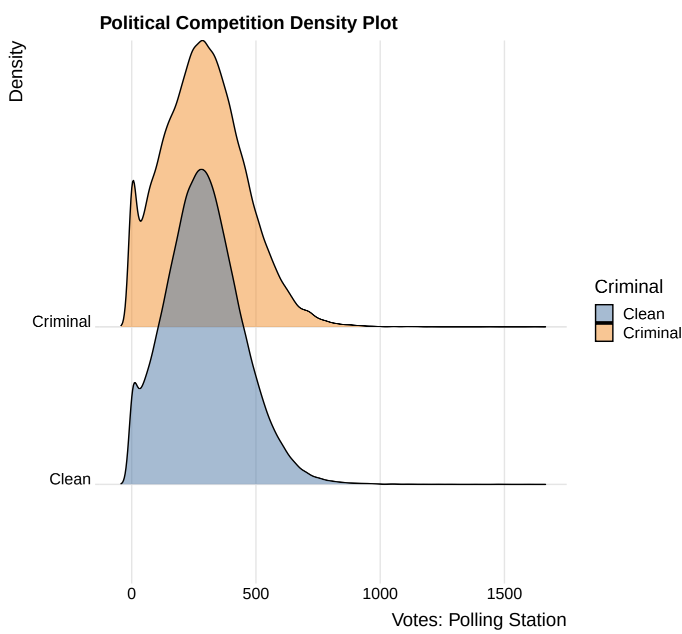
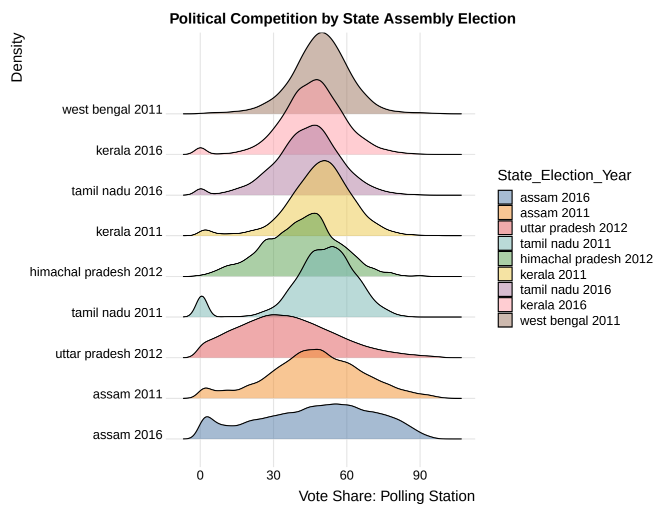


Figure 3.D.8: Density of Polling Station Total Votes by Criminality: Full Sample

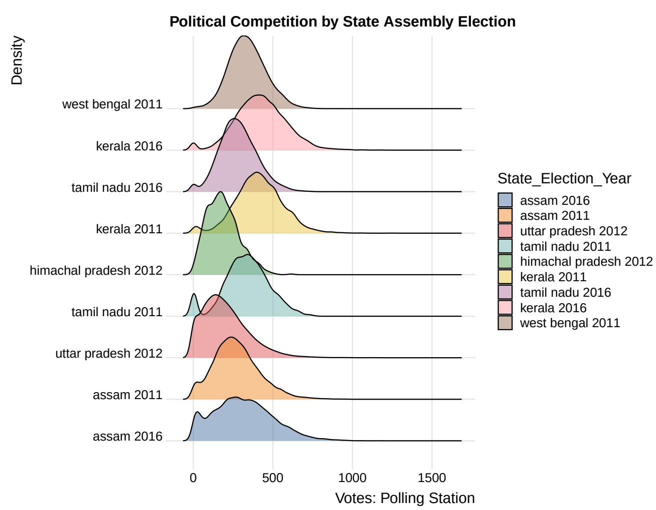


3.D.3 Rural Polling Stations Only

Figure 3.D.9: Statewide Variation in Polling Station Political Competition- Rural polling stations

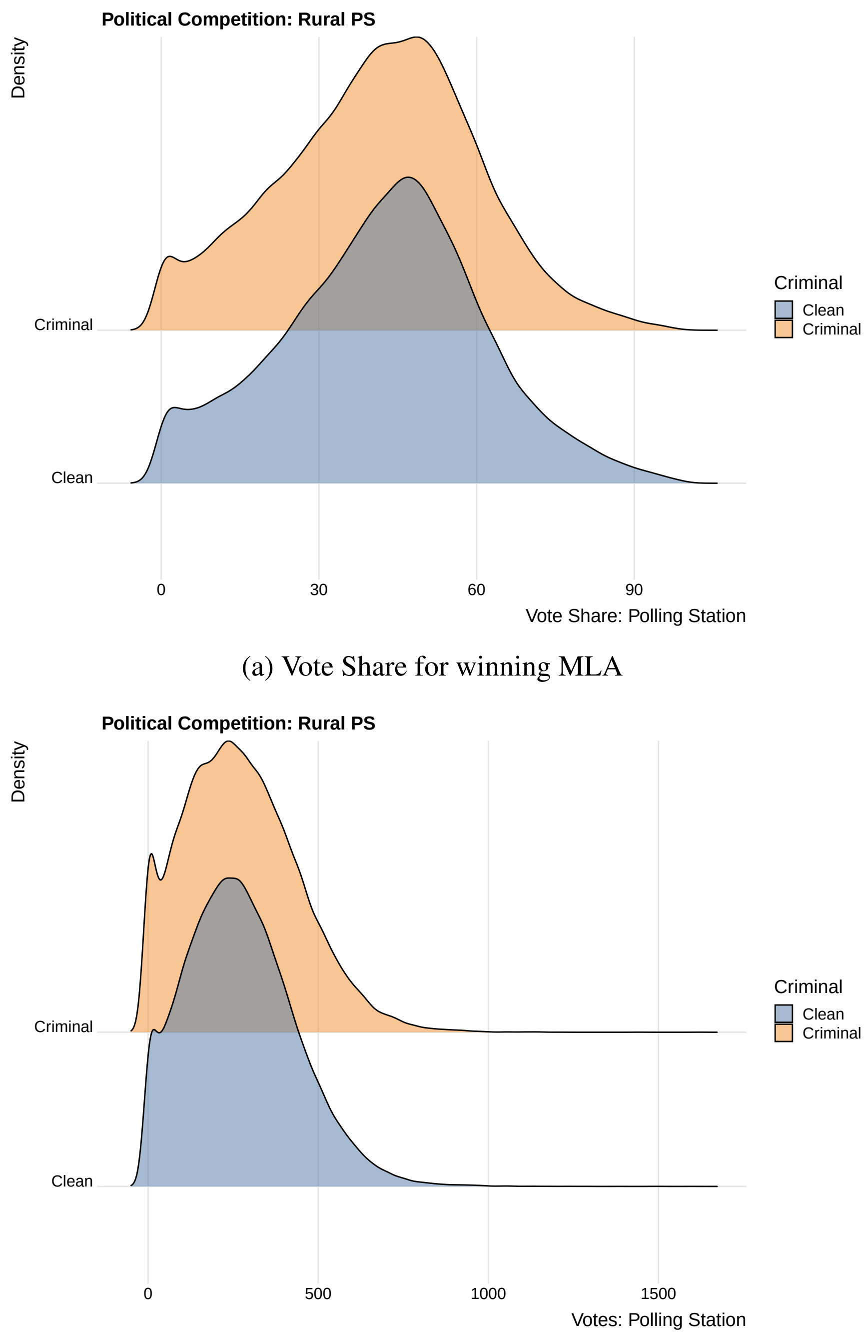


(a) Vote Share for winning MLA



(b) Total votes for winning MLA

Figure 3.D.10: Distribution of Polling Station Political Comp. by Criminal status of MLA



(a) Vote Share for winning MLA

(b) Total Votes for winning MLA

3.E Multiverse Analysis

Figure 3.E.1: Summary of Coefficient Estimates for Criminality based on different model choices

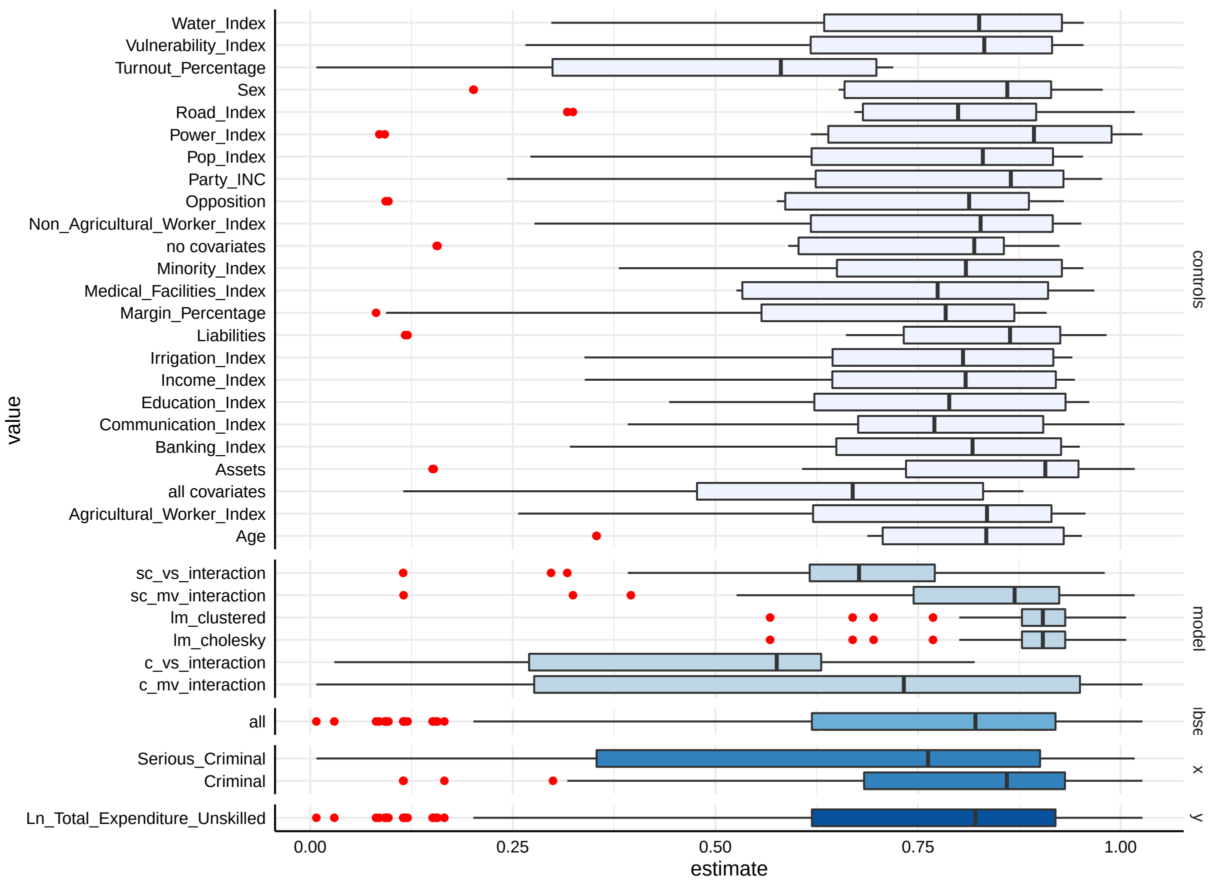
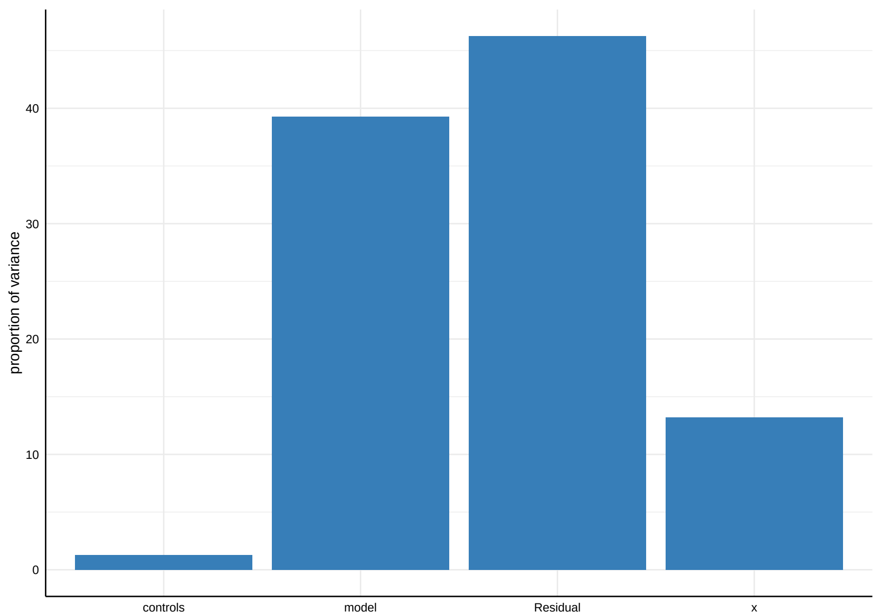


Figure 3.E.2: Variance Decomposition of Model Estimates



3.F Supplemental Results

3.F.1 KRLS

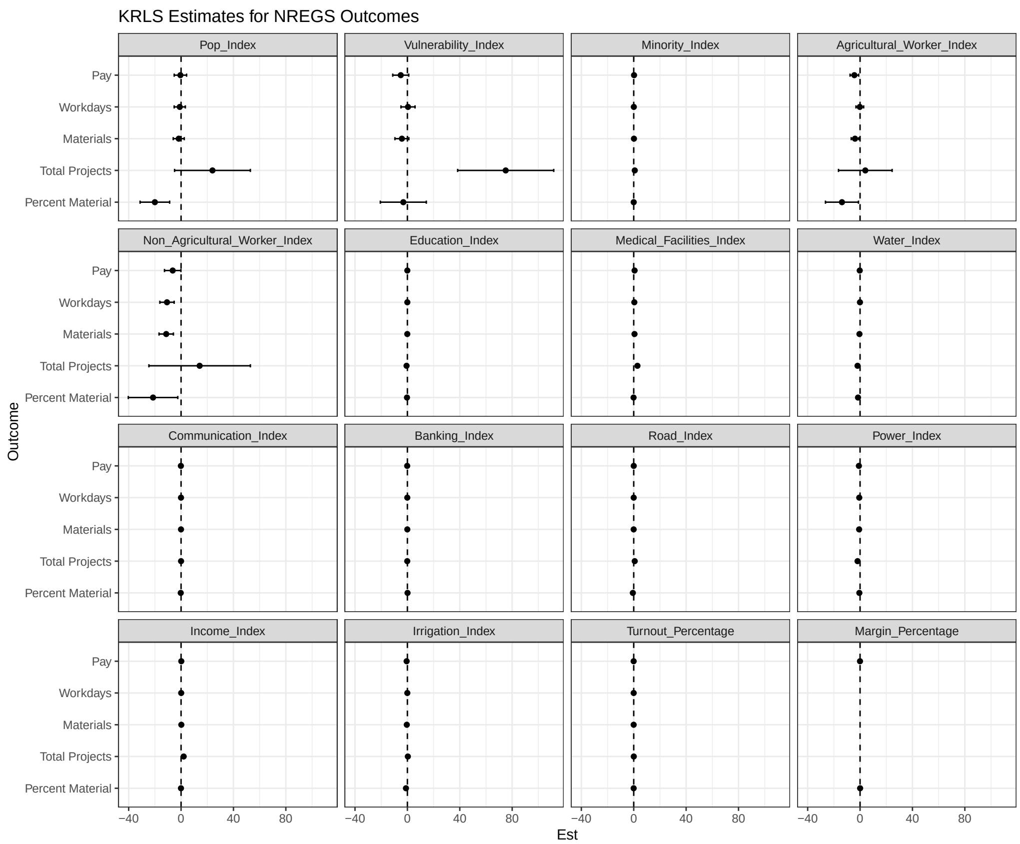


Table 3.F.1: KRLS: Serious Criminal

	Pay	Workdays	Materials	Total Projects	Percent Material
Serious Criminal	0.80^{***}	0.57^{***}	0.68^{***}	3.05^{***}	-0.06
	(0.06)	(0.05)	(0.05)	(0.36)	(0.25)
Political Competition (PS)	-0.00	-0.00^{***}	-0.00^{***}	-0.02^{**}	0.00
	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Age	0.03^{***}	0.01^{***}	0.02^{***}	0.08^{***}	-0.01
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Sex	-0.01	0.06	-0.01	0.93	-0.62
	(0.09)	(0.08)	(0.08)	(0.54)	(0.37)
Party INC	0.44^{***}	0.29^{***}	0.60^{***}	-1.04^{*}	1.71^{***}
	(0.07)	(0.07)	(0.07)	(0.47)	(0.33)
Opposition	-0.67^{***}	-0.35^{***}	-0.46^{***}	-1.88^{***}	0.13
	(0.05)	(0.05)	(0.05)	(0.33)	(0.24)
Assets	-0.00^{***}	-0.00^{***}	-0.00^{***}	-0.00^{***}	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Liabilities	-0.00^{***}	-0.00^{*}	-0.00^{***}	-0.00^{**}	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Pop_Index	-0.51	-0.99	-1.77	23.98	-20.00^{***}
	(2.41)	(2.17)	(2.15)	(14.76)	(5.76)
Vulnerability_Index	-5.08	0.46	-4.21	75.05^{***}	-3.11
	(3.12)	(2.74)	(2.72)	(18.71)	(8.96)
Minority_Index	0.25^{***}	0.07	0.17^{***}	0.79^{**}	-0.01
	(0.05)	(0.05)	(0.04)	(0.31)	(0.15)
Agricultural_Worker	-4.34^{**}	-0.19	-3.84^{**}	4.01	-13.81^{*}
	(1.67)	(1.50)	(1.49)	(10.45)	(6.42)
Non_Agricultural_Worker	-6.38^{*}	-10.74^{***}	-11.34^{***}	14.18	-21.39^{*}
	(3.20)	(2.79)	(2.80)	(19.77)	(9.64)
Education_Index	-0.05	-0.04	-0.08	-0.64	-0.32
	(0.07)	(0.07)	(0.07)	(0.46)	(0.25)
Medical_Facilities_Index	0.66^{***}	0.47^{***}	0.60^{***}	2.84^{***}	-0.13
	(0.09)	(0.08)	(0.08)	(0.55)	(0.27)
Water_Index	-0.24^{***}	-0.07	-0.46^{***}	-1.92^{***}	-1.55^{***}
	(0.06)	(0.05)	(0.05)	(0.36)	(0.19)
Communication_Index	-0.14^{*}	-0.04	-0.05	0.01	-0.25
	(0.07)	(0.06)	(0.06)	(0.42)	(0.21)
Banking_Index	-0.15^{**}	-0.08	-0.06	-0.12	0.12
	(0.05)	(0.04)	(0.04)	(0.30)	(0.19)
Road_Index	0.01	-0.02	-0.05	0.76^{**}	-0.67^{***}
	(0.04)	(0.04)	(0.04)	(0.28)	(0.13)
Power_Index	-0.89^{***}	-0.61^{***}	-0.71^{***}	-1.90^{***}	-0.53^{**}
	(0.04)	(0.04)	(0.04)	(0.26)	(0.17)
Income_Index	0.19^{***}	0.10^{***}	0.21^{***}	2.00^{***}	-0.08
	(0.03)	(0.02)	(0.03)	(0.31)	(0.05)
Irrigation_Index	-0.57^{***}	-0.05	-0.46^{***}	0.35	-1.10^{**}
	(0.12)	(0.12)	(0.11)	(0.69)	(0.36)
Turnout_Percentage	-0.06^{***}	-0.04^{***}	-0.04^{***}	0.05^{**}	-0.05^{***}
	(0.00)	(0.00)	(0.00)	(0.02)	(0.01)
Margin_Percentage	-0.04^{***}				0.04^{**}
	(0.00)				(0.02)
ENOP		-0.10^{***}	-0.13^{***}	-1.22^{***}	
		(0.03)	(0.02)	(0.18)	
Obs.	53800.00	26963.00	53908.00	26938.00	50840.00

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

3.F.2 Muscle Mechanism Test

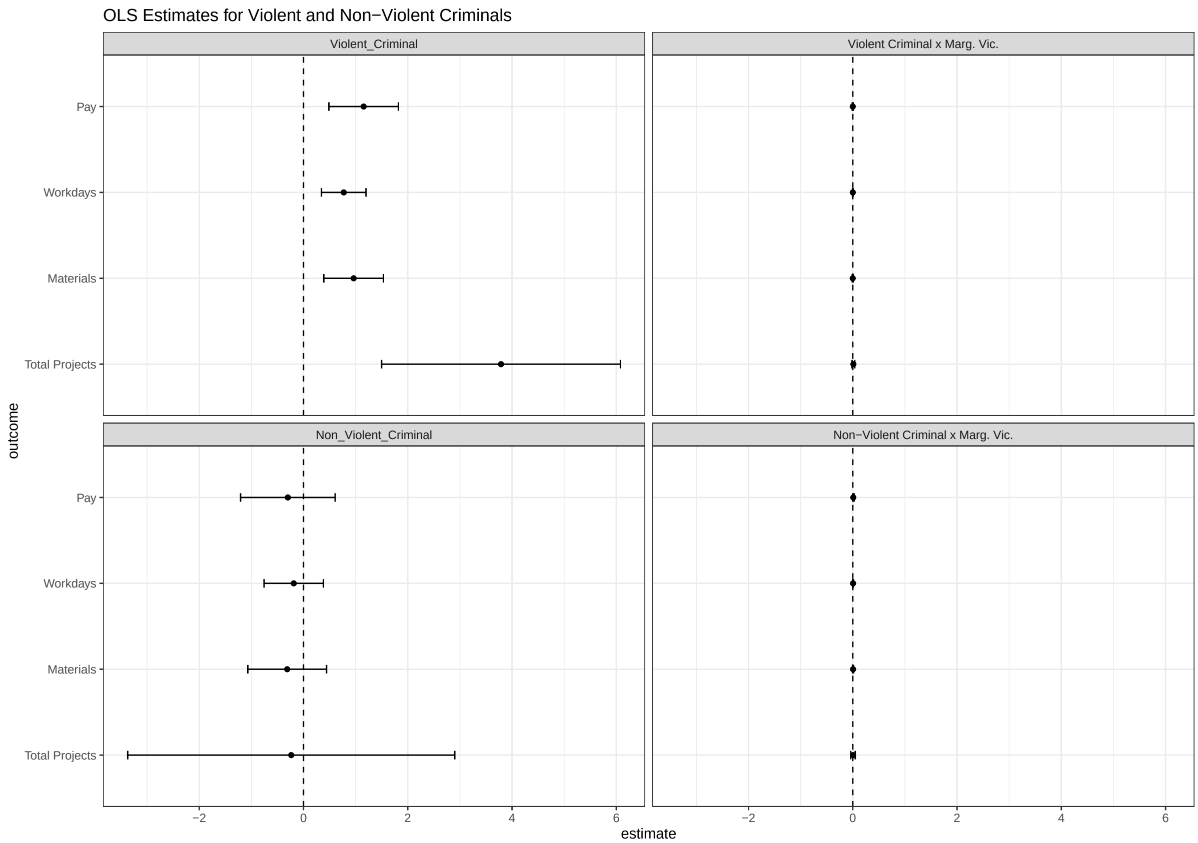


Table 3.F.2: OLS: Violent Criminal

	Pay	Materials	Workdays	Total Projects
(Intercept)	12.46^{***}	8.72^{***}	7.46^{***}	2.19
	(1.09)	(0.92)	(0.69)	(3.85)
Violent Criminal	1.15^{***}	0.96^{**}	0.77^{***}	3.79^{**}
	(0.34)	(0.29)	(0.22)	(1.16)
Political Competition (PS)	-0.00	0.00	-0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.04^{**}	0.03^{**}	0.02^{**}	0.12^{**}
	(0.01)	(0.01)	(0.01)	(0.04)
SexM	-0.45	-0.34	-0.31	-0.15
	(0.48)	(0.42)	(0.30)	(1.48)
Party_INC	0.49	0.67^{*}	0.23	-2.18
	(0.35)	(0.32)	(0.22)	(1.58)
Opposition	-0.53	-0.61^{*}	-0.39^{*}	-2.06^{*}
	(0.29)	(0.24)	(0.18)	(0.97)
Assets	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Liabilities	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Pop_Index	16.02^{***}	10.49^{***}	9.77^{***}	13.25
	(3.24)	(3.07)	(2.05)	(18.81)
Vulnerability_Index	-13.42^{***}	-9.22^{***}	-8.25^{***}	-20.31
	(2.70)	(2.61)	(1.74)	(14.98)
Minority_Index	0.06	0.03	0.04	0.08
	(0.06)	(0.07)	(0.04)	(0.43)
Agricultural_Worker_Index	-0.02	-0.90	0.14	8.05
	(1.15)	(0.94)	(0.74)	(5.58)
Non_Agricultural_Worker_Index	-1.66	-1.71	-0.83	14.13
	(1.35)	(1.20)	(0.87)	(7.31)
Education_Index	-0.22^{**}	-0.16^{*}	-0.16^{**}	-0.99
	(0.08)	(0.08)	(0.05)	(0.58)
Medical_Facilities_Index	0.16	0.16	0.11	0.58
	(0.10)	(0.08)	(0.06)	(0.46)
Water_Index	-1.43^{***}	-1.48^{***}	-0.93^{***}	-6.01^{***}
	(0.22)	(0.18)	(0.14)	(0.82)
Communication_Index	0.43^{**}	0.24	0.25^{**}	1.03
	(0.13)	(0.13)	(0.09)	(0.71)
Banking_Index	-0.21^{*}	-0.06	-0.12^{*}	0.42
	(0.09)	(0.07)	(0.06)	(0.39)
Road_Index	1.24^{***}	0.66^{***}	0.77^{***}	4.24^{***}
	(0.16)	(0.12)	(0.10)	(0.62)
Power_Index	-0.85^{***}	-0.54^{***}	-0.53^{***}	-1.04
	(0.16)	(0.13)	(0.10)	(0.61)
Income_Index	0.02	0.02	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.05)
Irrigation_Index	-0.01	-0.00	-0.01	-0.09
	(0.09)	(0.08)	(0.06)	(0.43)
Turnout_Percentage	-0.10^{***}	-0.06^{***}	-0.06^{***}	0.10^{*}
	(0.01)	(0.01)	(0.01)	(0.04)
Margin_Percentage	-0.05^{**}	-0.04^{**}	-0.04^{***}	-0.26^{***}
	(0.02)	(0.01)	(0.01)	(0.05)
Violent_Criminal:Candidate_Margin_Victory_PS	-0.00	-0.00	0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.01)
Obs.	107894.00	107891.00	107894.00	107894.00
Adj. R-Squared	0.08	0.06	0.08	0.04

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 3.F.3: OLS: Non Violent Criminal

	Pay	Materials	Workdays	Total Projects
(Intercept)	13.35^{***}	9.46^{***}	8.05^{***}	4.95
	(1.07)	(0.91)	(0.67)	(3.74)
Non Violent Criminal	-0.30	-0.31	-0.19	-0.24
	(0.45)	(0.38)	(0.28)	(1.56)
Political Competition (PS)	-0.00	-0.00	-0.00	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Age	0.03^{**}	0.02^{*}	0.02^{**}	0.10^{*}
	(0.01)	(0.01)	(0.01)	(0.04)
SexM	-0.18	-0.11	-0.13	0.69
	(0.47)	(0.42)	(0.30)	(1.46)
Party_INC	0.40	0.59	0.17	-2.48
	(0.35)	(0.32)	(0.22)	(1.59)
Opposition	-0.65^{*}	-0.71^{**}	-0.47^{*}	-2.48^{**}
	(0.29)	(0.24)	(0.18)	(0.96)
Assets	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Liabilities	-0.00	-0.00	-0.00	-0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Pop_Index	16.81^{***}	11.11^{***}	10.29^{***}	15.83
	(3.25)	(3.04)	(2.06)	(18.66)
Vulnerability_Index	-14.09^{***}	-9.76^{***}	-8.70^{***}	-22.62
	(2.71)	(2.60)	(1.74)	(14.89)
Minority_Index	0.06	0.03	0.04	0.06
	(0.06)	(0.07)	(0.04)	(0.43)
Agricultural_Worker_Index	0.16	-0.75	0.26	8.60
	(1.18)	(0.96)	(0.76)	(5.72)
Non_Agricultural_Worker_Index	-1.64	-1.69	-0.82	14.25
	(1.41)	(1.21)	(0.91)	(7.39)
Education_Index	-0.23^{**}	-0.17^{*}	-0.16^{**}	-1.02
	(0.08)	(0.08)	(0.05)	(0.58)
Medical_Facilities_Index	0.17	0.17^{*}	0.12	0.62
	(0.11)	(0.08)	(0.07)	(0.46)
Water_Index	-1.52^{***}	-1.56^{***}	-0.99^{***}	-6.32^{***}
	(0.22)	(0.18)	(0.14)	(0.83)
Communication_Index	0.42^{**}	0.23	0.25^{**}	1.01
	(0.13)	(0.13)	(0.09)	(0.72)
Banking_Index	-0.22^{*}	-0.07	-0.12^{*}	0.39
	(0.09)	(0.07)	(0.06)	(0.39)
Road_Index	1.25^{***}	0.67^{***}	0.78^{***}	4.27^{***}
	(0.16)	(0.12)	(0.10)	(0.63)
Power_Index	-0.80^{***}	-0.50^{***}	-0.49^{***}	-0.87
	(0.16)	(0.13)	(0.10)	(0.61)
Income_Index	0.02	0.02	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.05)
Irrigation_Index	-0.02	-0.00	-0.01	-0.11
	(0.09)	(0.08)	(0.05)	(0.43)
Turnout_Percentage	-0.10^{***}	-0.07^{***}	-0.06^{***}	0.08
	(0.01)	(0.01)	(0.01)	(0.04)
Margin_Percentage	-0.05^{**}	-0.04^{**}	-0.04^{***}	-0.26^{***}
	(0.02)	(0.01)	(0.01)	(0.05)
Non_Violent_Criminal:Candidate_Margin_Victory_PS	0.01	0.01	0.01	0.00
	(0.01)	(0.01)	(0.00)	(0.02)
Obs.	107894.00	107891.00	107894.00	107894.00
Adj. R-Squared	0.08	0.06	0.08	0.04

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

3.F.3 Networks Mechanism Test

Figure 3.F.1: Slope Estimates for Census Covariates (NREGS Projects)

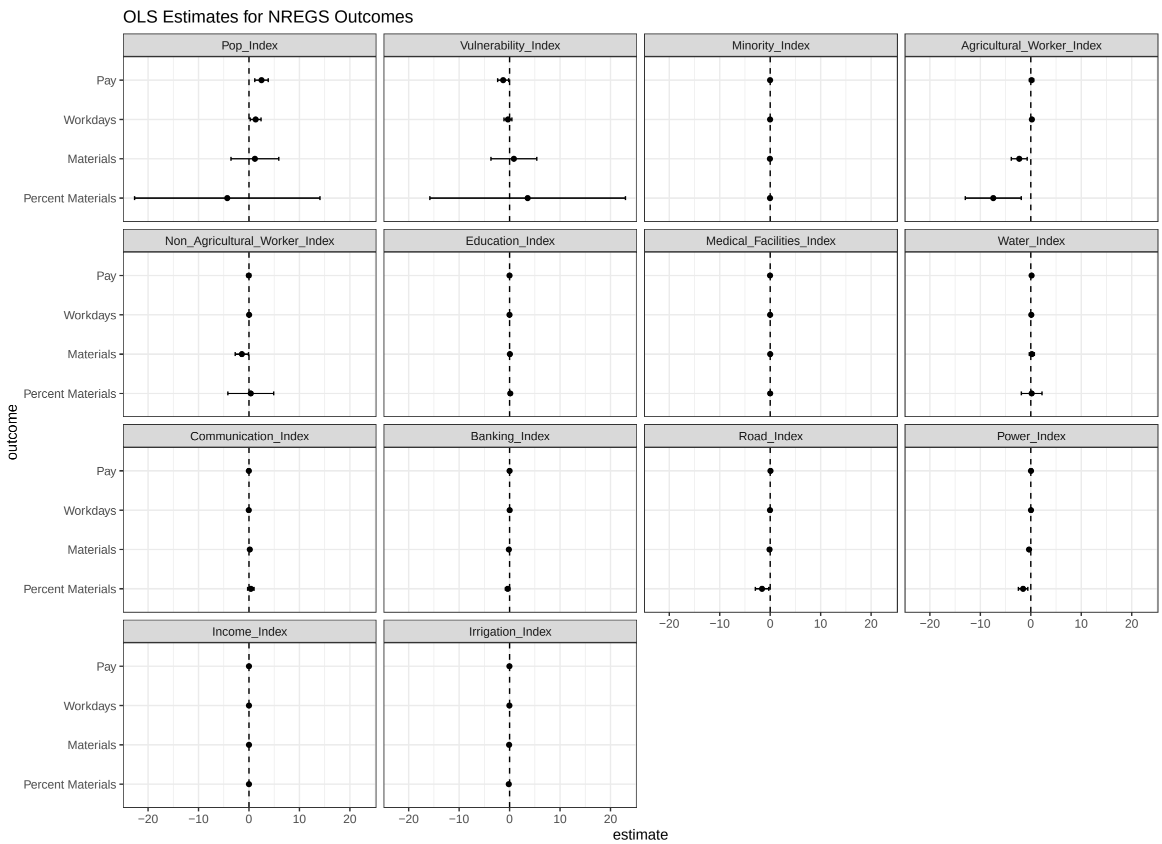


Figure 3.F.2: Slope Estimates for Constituency Covariates (NREGS Projects)

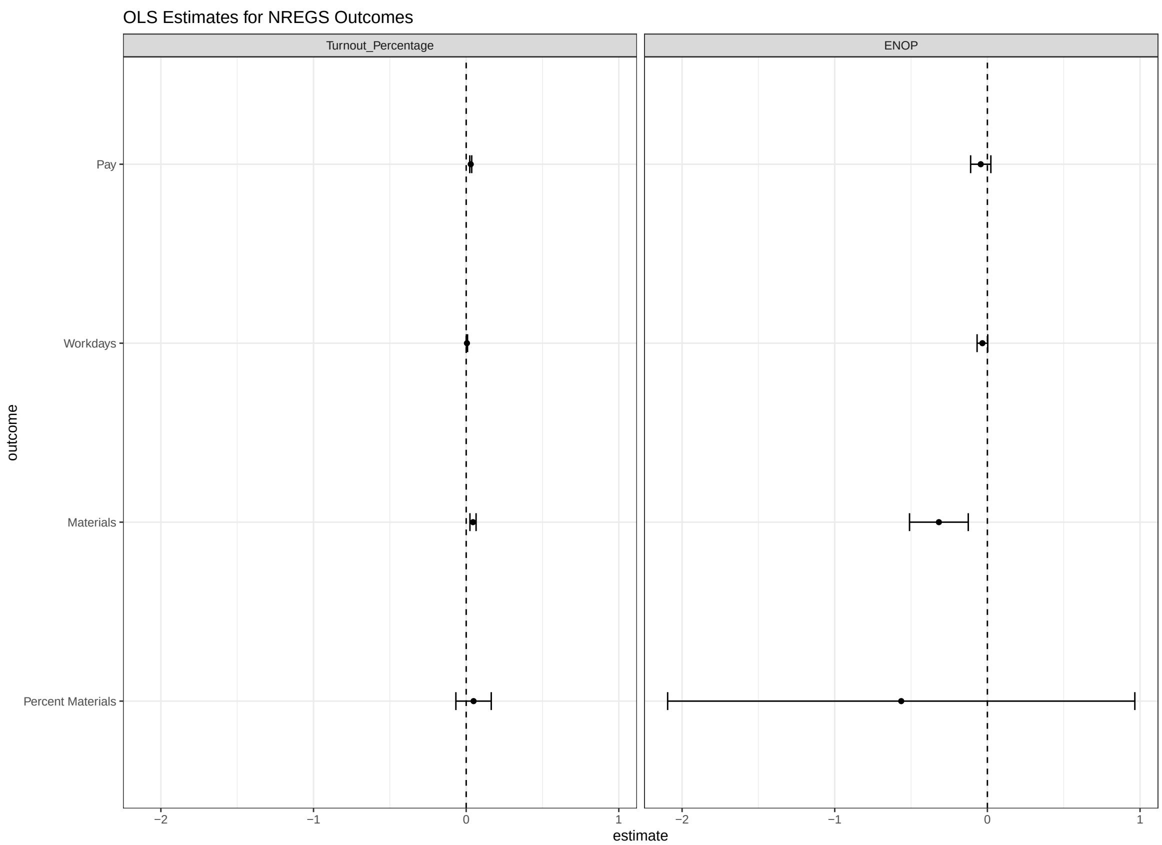
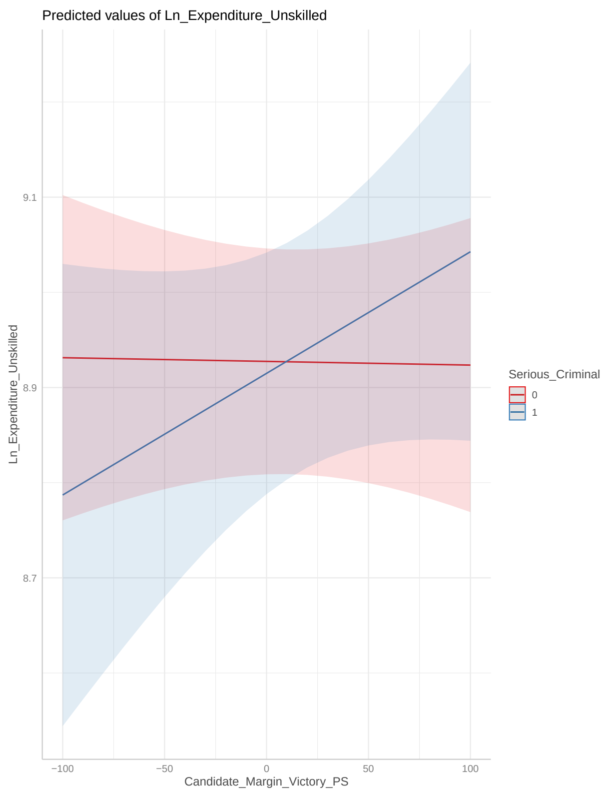
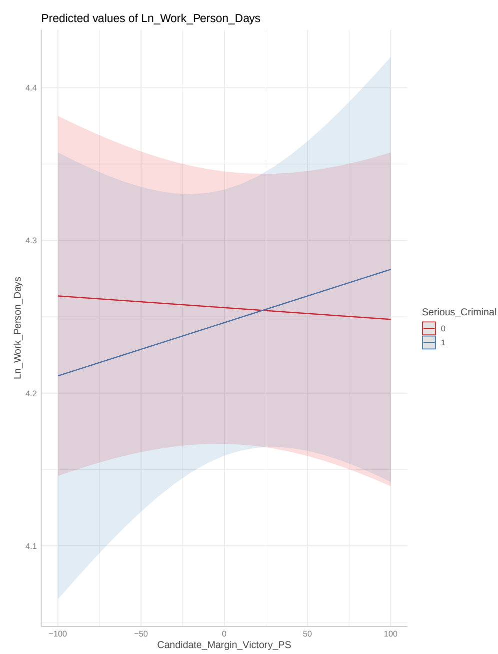


Figure 3.F.3: Marginal Effects of Criminality moderated by Polling Station Political Competition (OLS Estimates)



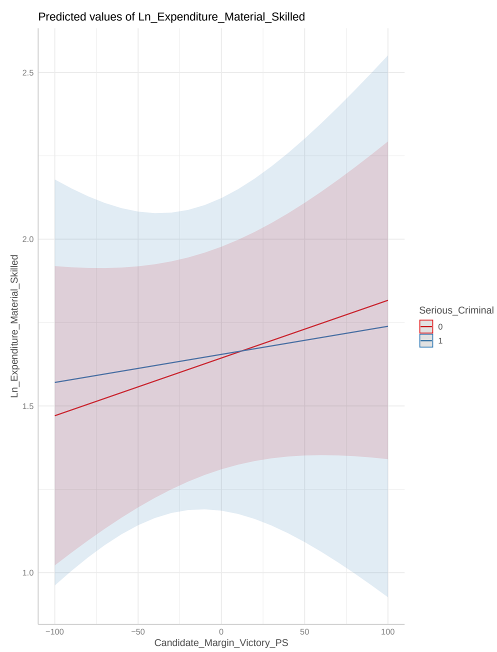
(a) Project Labor Expenditure



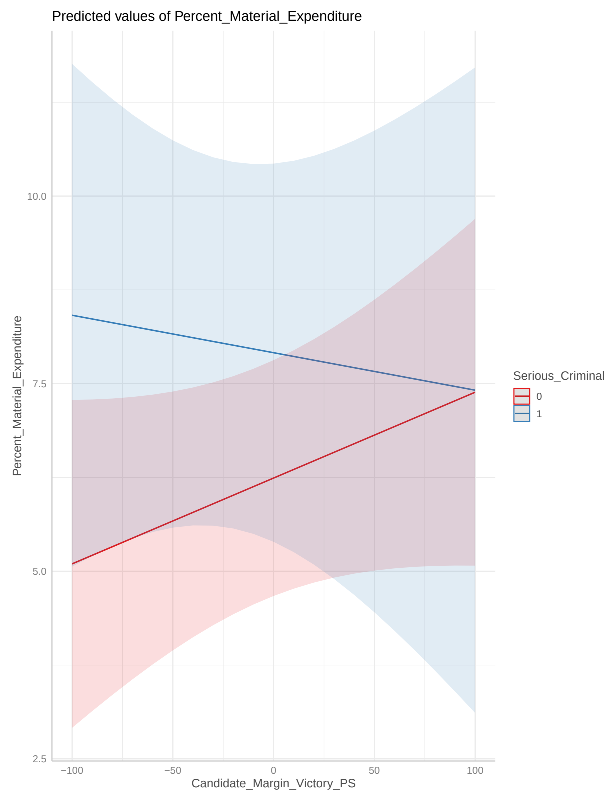
(b) Project Workdays

Blue lines are the predicted effects of Criminal MLAs. Red lines are predicted effects of Clean MLAs. Standard errors are clustered at the Assembly-Constituency-Election Level.

Figure 3.F.4: Blue lines are the predicted effects of Criminal MLAs. Red lines are predicted effects of Clean MLAs. Standard errors are clustered at the Assembly-Constituency-Election Level.

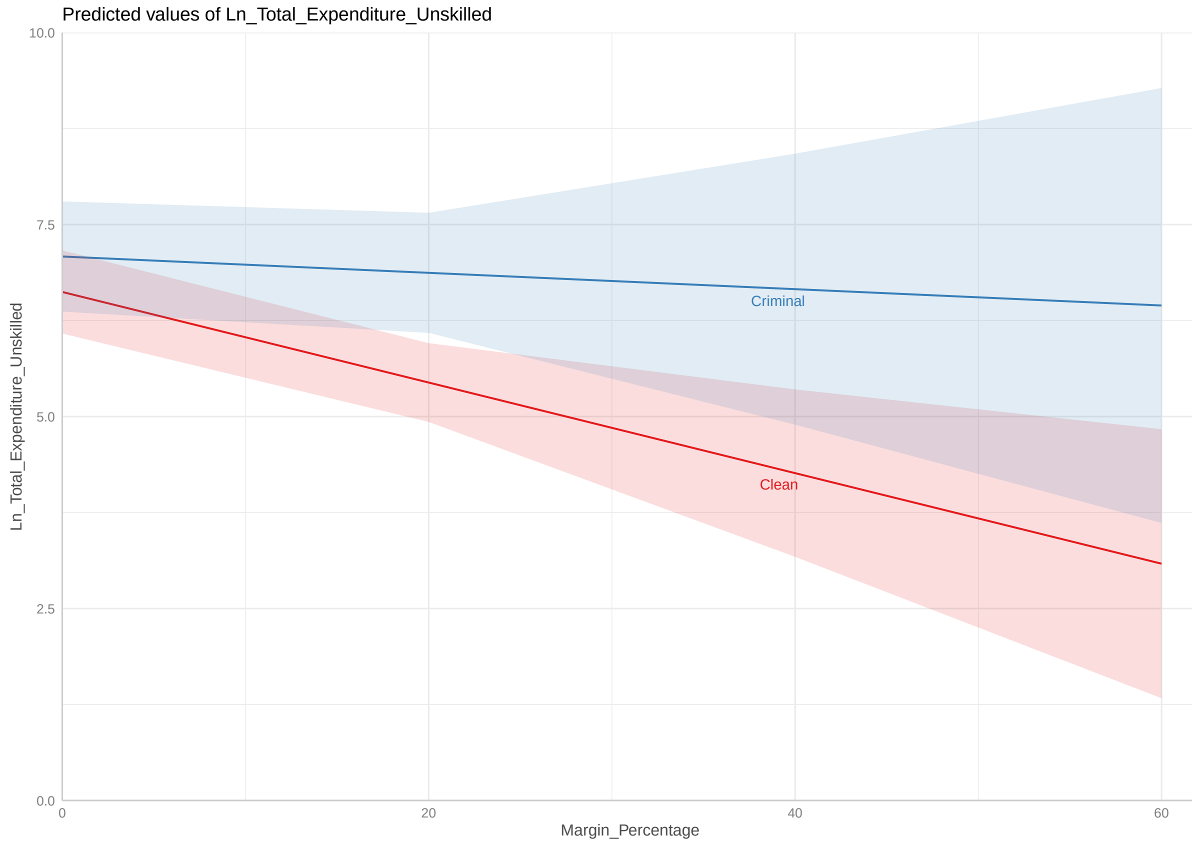


(a) Material Expenditure

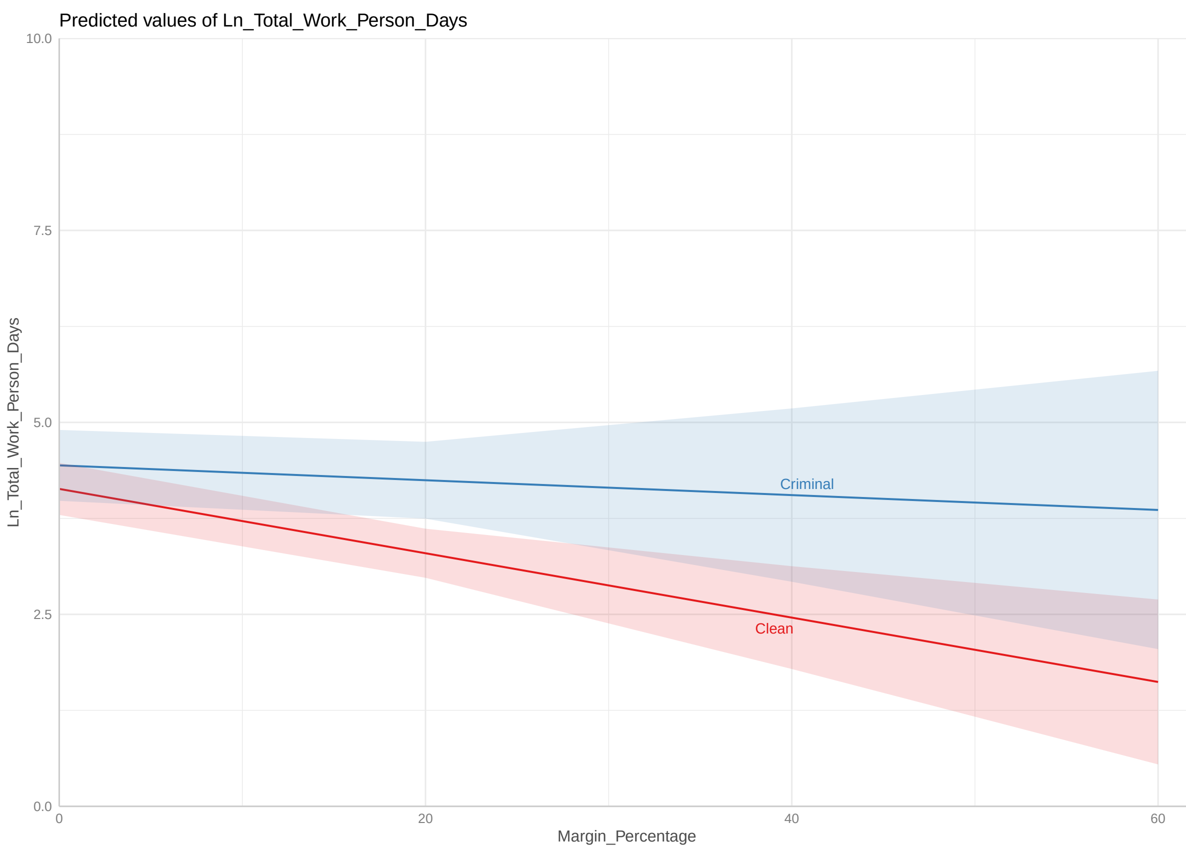


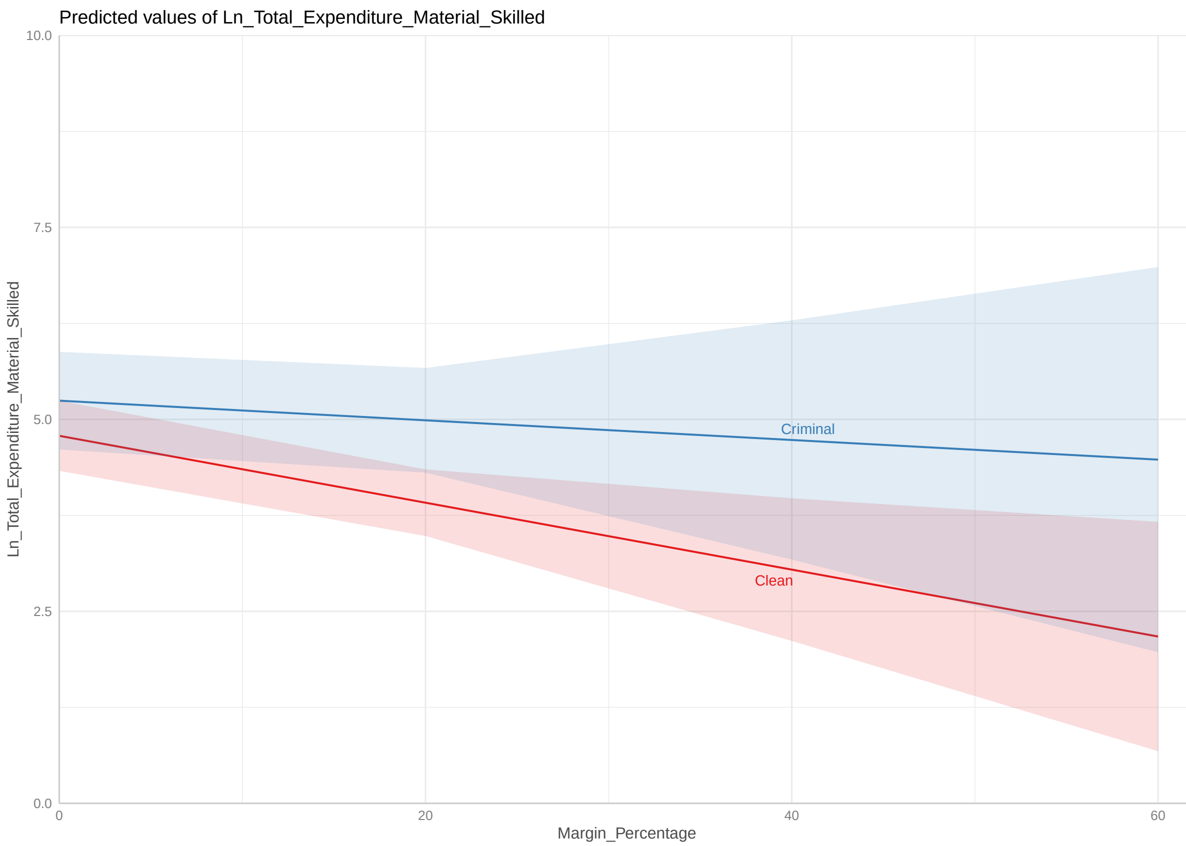
(b) Project share of Material Expenditure

Test



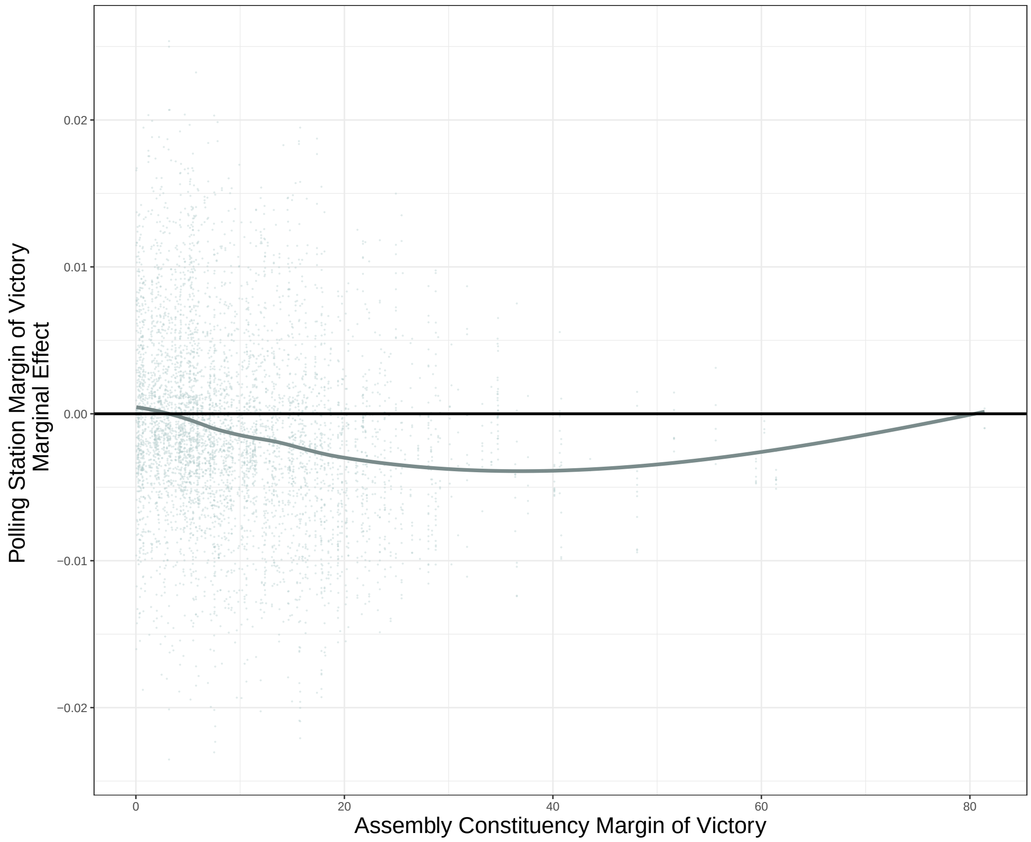
3.F.4 Assembly Constituency Political Competition

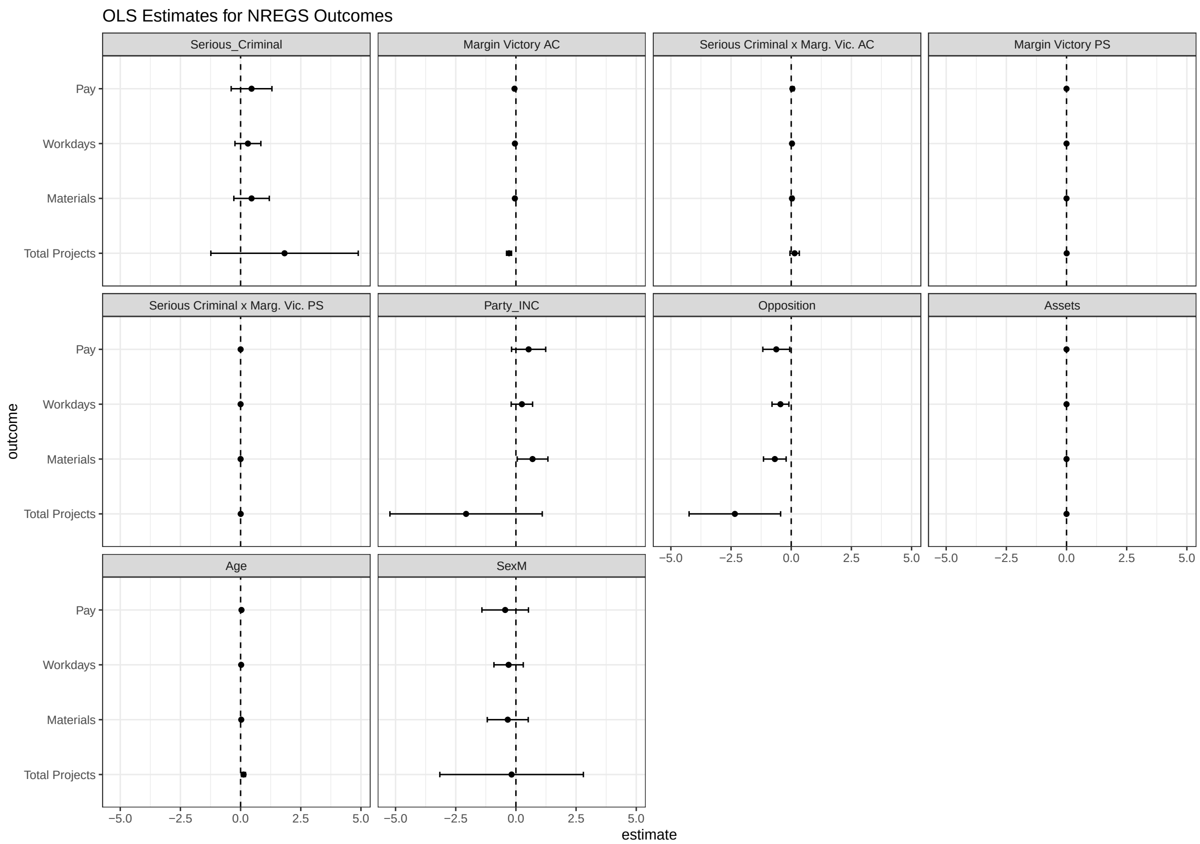

Predicted values of Ln_Total_Work_Person_Days
10.0
7.5
5.0
2.5
0.0
Ln_Total_Work_Person_Days
Criminal
Clean
0
20
40
60
Margin_Percentage

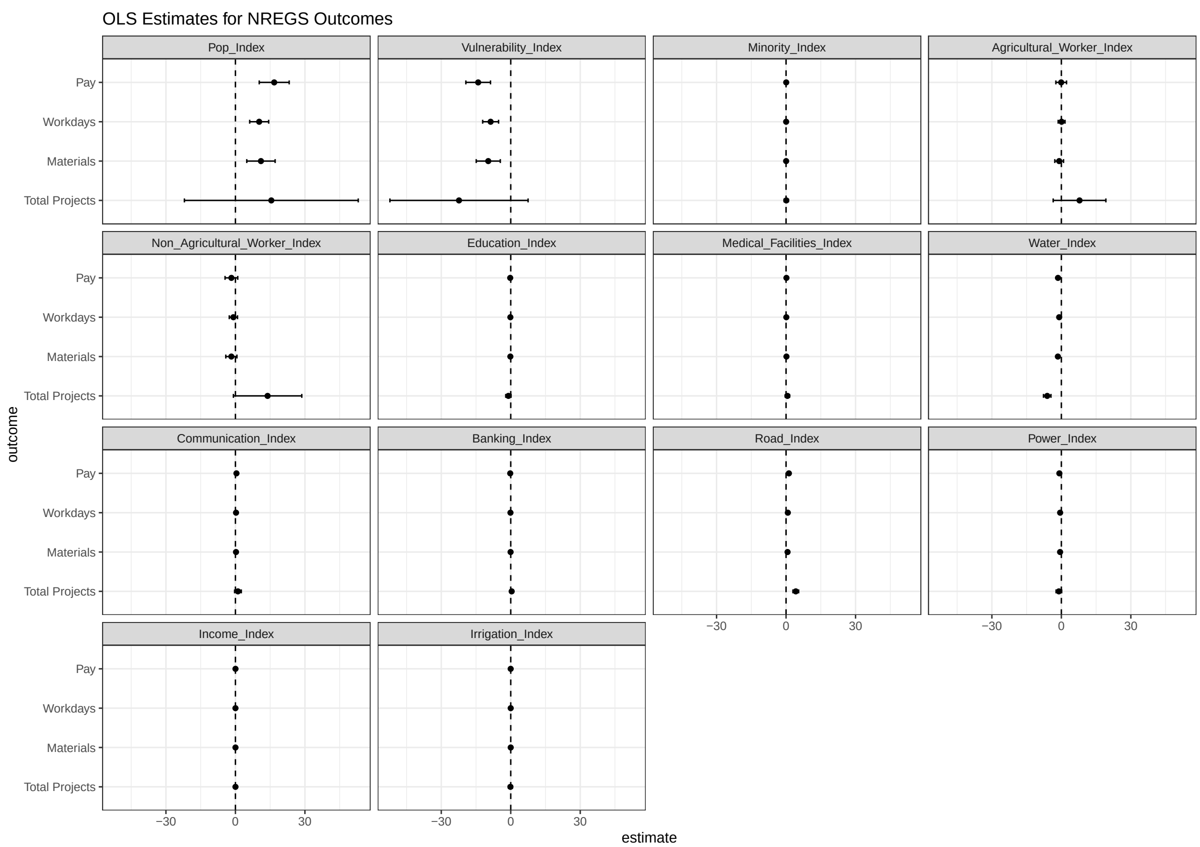


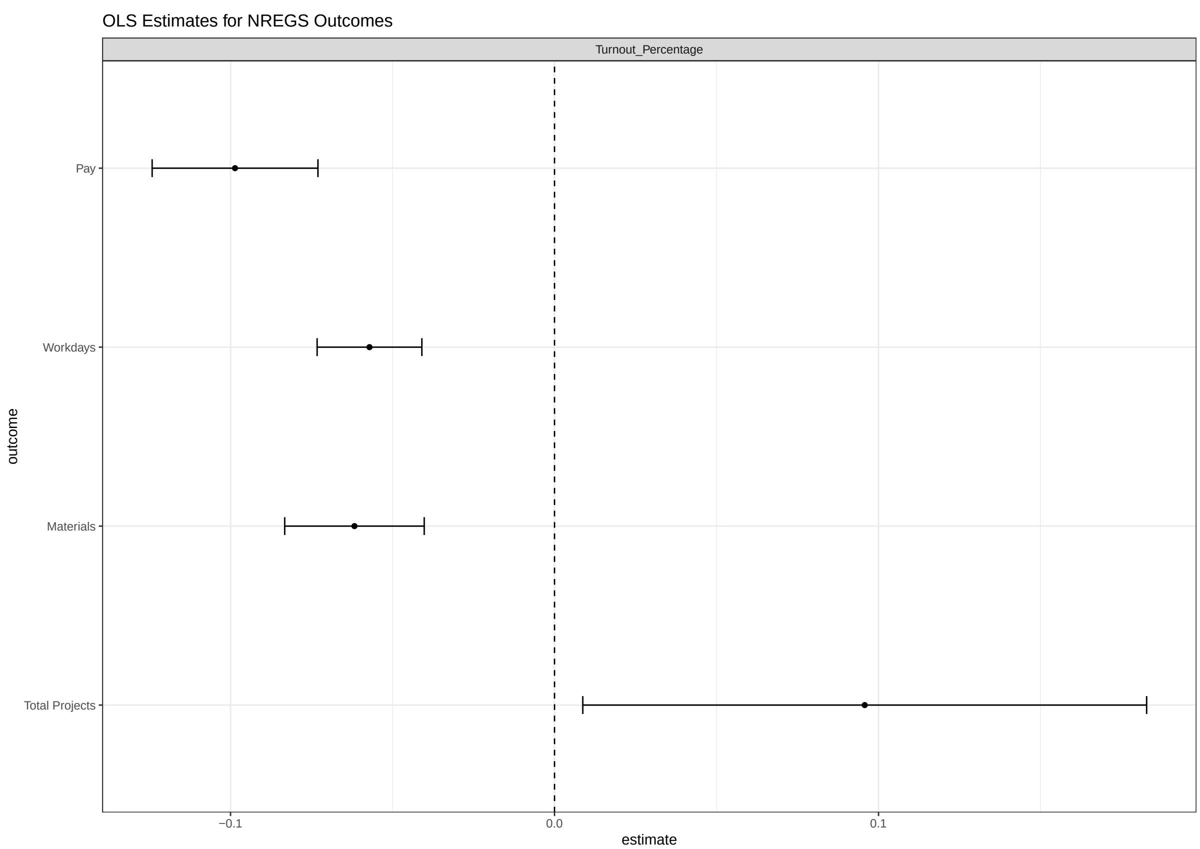
Predicted values of Ln_Total_Expenditure_Material_Skilled
10.0
7.5
5.0
2.5
0.0
Ln_Total_Expenditure_Material_Skilled
Criminal
Clean
0
20
40
60
Margin_Percentage

Figure 3.F.5: Slope Estimates for Constituency Covariates (NREGS Projects)




OLS Estimates for NREGS Outcomes
Serious_Criminal
Margin Victory AC
Serious Criminal x Marg. Vic. AC
Margin Victory PS
Serious Criminal x Marg. Vic. PS
Party_INC
Opposition
Assets
Age
SexM
Pay
Workdays
Materials
Total Projects
outcome
estimate
−5.0
−2.5
0.0
2.5
5.0


OLS Estimates for NREGS Outcomes
Pop_Index
Vulnerability_Index
Minority_Index
Agricultural_Worker_Index
Non_Agricultural_Worker_Index
Education_Index
Medical_Facilities_Index
Water_Index
Communication_Index
Banking_Index
Road_Index
Power_Index
Income_Index
Irrigation_Index
Pay
Workdays
Materials
Total Projects
outcome
−30
0
30
estimate


OLS Estimates for NREGS Outcomes
Turnout_Percentage
Pay
Workdays
Materials
Total Projects
outcome
−0.1
0.0
0.1
estimate

Chapter 4

Wedding Service: Do Criminal Politicians Deliver Constituency Service?

4.1 Introduction

Do criminal politicians deliver benefits outside of government programs? The previous chapters found mixed evidence regarding criminals' ability to distribute NREGS. In part, this discrepancy may be explained by heterogeneous effects depending on the level of constituency political competition, with criminals under providing in close races but over providing in safe seats. Still, criminal politicians retain a predictable advantage at the polls (Aidt et al., 2011; Vaishnav, 2017; Sircar, 2018a). Perhaps criminal politicians' electoral intangibles derive from services outside of government funded welfare programs. While NREGS is an important program, it represents only one benefit in the eco-system of resources at politicians' disposal. Criminal politicians' distributive strategy may focus on other channels. In particular, previous literature highlights the importance of constituency service for MLAs (Bussell, 2019; Kruks-Wisner, 2018). Further, qualitative evidence suggests that criminals engage heavily in solving everyday problems, patronizing temples and attending community events (Berenschot, 2011a; Vaishnav, 2017).

In this chapter, I argue that MLA candidates face a tradeoff between spending time conducting constituency service in villages, and accumulating wealth in cities. Both constituency service and wealth accumulation are vital for winning elections. Yet, candidates may find it difficult to optimize across these two dimensions at the same time. This may be especially true in rural and peri-urban constituencies with less dynamic economies that limit wealth generation. I argue that criminal politicians are uniquely suited to solving this fundamental problem. By leveraging their muscle-power, criminals can extort wealth from the local economy that is inaccessible to clean candidates. In turn, this provides criminals with the funds necessary to credibly contest elections and the time necessary to conduct constituency service. Conversely, candidates who try to accumulate wealth outside the constituency may fail to provide sufficient constituency service and subsequently be discounted by voters.

Before discussing challenges in systematically measuring constituency service and my identification strategy, I first lay out each part of this argument in detail.

4.1.1 The Role of Constituency Service in Winning Elections

Voters expect politicians to provide constituency service (Auerbach, 2019; Bussell, 2019; Piliavsky, 2014; Vaishnav et al., 2019). MLAs report receiving requests from thousands of constituents "on days when they are *in their home constituecies*" (Bussell, 2019 p. 53, emphasis added). Face-to-face constituency service of this magnitude is time consuming. Bussell (2019) finds that MLAs spend one-fifth of their time meeting with citizens, which is a greater share of time than they attribute to policy or office work. Similarly, many interviewees during my fieldwork argued that engaging in "social-work" can make or break candidates' campaigns. For example, one mukhiya (village president) I interviewed claimed that the most important trait MLAs must possess is a demonstrated respect for voters. The best way for politicians to demonstrate this respect he said is "by making themselves available." The same mukhiya went on to elaborate that Arun Yadav (the local criminal MLA from Sandesh) is seen as an effective and available leader while Sanjay Tiger (the losing "honest" candidate for the Sandesh MLA seat in 2015) fails to embody either of these traits. Arun Yadav, famously can be reached by phone anytime, day or night. Candidates who base themselves in cities and state capitals will have fewer opportunities to engage in this work. Given voters expectations for constituency service, absentee politicians may suffer am electoral penalty.

Can politicians split the difference? That is, can they spend some time in cities and visit the constituency when they can? Perhaps substituting face-to-face service with an unlimited cell plan to field requests from afar? Indeed, some politicians prefer to live in state capitals for reasons beyond wealth generation (e.g. to access better services and schools, or simply to be closer to their office and legislative work). To take one example, Sanjay Tiger often made his home base in Patna, Bihar's capital. Sandesh is only 60km outside of Patna making this seem like a viable strategy. Yet, bumpy roads and traffic jams make the trip regularly take over 2-3 hours one way.[1] Even periodic travel to far flung constituencies can be difficult, especially if you are trying to visit multiple villages. Secondly, state legislators represent hundreds of thousands of voters. Not all of their requests can be handled over the phone (e.g. attending weddings, paying respects to the deceased, or intervening immediately to obtain a hospital bed). If politicians make infrequent trips back home, their absence- and the resultant backlog of requests- will surely be noticed. Trying to provide constituency service infrequently and from afar is unlikely to be a winning strategy with voters.

I argue that, to win elections, candidates need to demonstrate some form of constituent problem solving before ever holding office- which likely requires long-term and repeated relationship building. When politicians prove they are repeatedly "available" and capable of solving constituents problems, voters may interpret this as a strong indicator of a candidate's likely behavior in office. At a minimum, effective constituency service requires candidates to posses an abundance of time to spend in their home district.

At the same time, economic opportunities in villages can be limited, at least relative to more dynamic Indian cities. A candidate who spends all their time in villages sitting down, speaking with constituents, may find it hard to develop the necessary capital to credibly contest elections. That is, candidates can face a trade-off between boning up on constituency service bonafides and

[1] This can easily stretch to longer travel times if you need to reach a remote part of the constituency or visit multiple villages.

developing a sufficient pot of money to buy a party ticket and fund campaigns.

4.1.2 The Role of Money in Winning Elections

Wealth is practically a pre-requisite for contesting expensive MLA elections. Several forces in Indian politics push towards self-financing candidates. First, party tickets represent the most viable path toward winning elections. Parties prefer deep-pocketed candidates who can help finance campaigns across the party ticket (Vaishnav, 2017). All else equal, parties tend to nominate wealthier individuals. Second, party power is highly centralized in India. Lower-level legislators have little say over party policy positions. Party centralization reduces incentives for firms or lobbyists to pour money into individual MLA campaigns. Instead, politicians must fend for themselves (Sircar, 2018a).

While wealthier candidates are more likely to win in India, debate continues over how cash actually helps candidates (Vaishnav 2017, Bjorkman 2014, Sircar 2018). Does cash directly buy votes or do wealthy candidates buy party tickets in safe seat constituencies? In either case, the costs of entering the electoral arena are substantial.

It is difficult to pin down the precise amount of campaign spending. Formal spending is capped for MLA elections in the range of about $30,000 to $45,000 (Bussell, 2018). However, anecdotal evidence suggests that actual outlays are much larger. Estimates of true campaign spending often eclipse 10 times the legal limit (Chauchard, 2018).[2] Nevertheless, politicians likely spend more on constituency service over their five years in office and in reputation building activities prior to being elected (as compared to campaigns which last a matter of weeks). For example Arun Yadav claimed to have donated up to 20 million rupees (about $270,000 USD) in less than two years after his election.[3] These funds went towards everything from weddings and funerals, to temple construction and small cash payments.[4] Using cash in this manner facilitates relationship building,

[2] In a particularly extreme example, the AIADMK in Tamil Nadu lavished 50-75,000 potential swing voters with with 1,000 INR each (Jeyaranjan, 2019).

[3] Albeit, this boast should be taken with a heaping dose of salt as Arun also claimed to be a great wrestler before promptly being thrown on his backside by a local youth.

[4] Arun claimed that he bequeaths larger amounts to about 40-60 constituents every week and showed me the ledger

demonstrates candidates' generosity, highlights financial connections and proves their ability to get things done. For example, Bjorkman (2014) argues that during a municipal election in Mumbai, cash gifts served not to buy votes but instead fostered trust in on-going relationships between voters and politicians. In short, money helps build critical political networks before campaigns begin, and maintains these networks after votes are tallied.

Money, therefore, serves a dual purpose. On one hand, money is necessary to gain the attention of party big-wigs and win a party ticket. On the other, cash serves as a crucial input into constituency service networks. Thus, to effectively engage in constituency service politicians must have an abundance of both time and money. This is precisely where I argue the tradeoff binds. Candidates who opt to seek fortunes far from the constituency risk losing ties with local political networks. To be both deep-pocketed and ever-present in the constituency requires finding a way to accumulate wealth within the district. However, at least in some constituencies, opportunities to raise enough capital to fund a campaign may be few and far between.

4.2 Criminals in the Constituency

The fundamental problem candidates face is making money while remaining rooted in the constituency. I argue that criminals solve this problem by using coercive force to take over the local illegal and legal economy. Politicians chasing money in major metropolises may be limited in their ability to develop a ground game. Whereas, criminal politicians can more easily expropriate wealth directly from the constituency. While some voters surely hold the expropriation I describe below against criminal politicians, criminals lavish redistribution machines mitigate this blowback.

4.2.1 Criminal Politicians and Local Wealth Generation

Criminals' coercive power provides an advantage in solving the wealth-accumulation vs. constituency-service tradeoff. By leveraging their muscle, criminals can establish protection rackets and squeeze wealth out of the local economy that is otherwise inaccessible to clean candidates. Protection

he keeps as proof.

rackets are central to how mafias make money around the world. When the state lacks a complete monopoly on violence and fails to provide core, muscle-backed services, such as protecting property rights, contract enforcement and dispute adjudication, criminals may step in to fill this void (Gambetta, 1996). During my fieldwork, natural resource extraction and protection rackets were commonly cited occupations for criminal politicians in Bihar.

Criminal politicians' protection driven funding advantage may be especially large in rural or peri-urban economies where fewer legitimate opportunities for wealth generation exist. Despite massive economic growth over the past few decades, nearly half of India's workforce remains employed in agriculture. Yet, agriculture accounted for only 17.5% of India's overall GDP in 2016 (Deshpande, 2017). This represents a large economic shift from over 50% of GDP in the 1950s. India's rural economy continues to diversify with a rising share of manufacturing and more rural workers employed in non-farm activities. However, these non-farm activities consist of occupations in handicrafts, transportation and small shops (World Bank, 2017). As one independent MLA candidate opined while gesturing across wide open agricultural fields "can you show me any industry in Bihar that has created 1,000 jobs? Show me the industry" (fieldwork 2017). Instead, wealth is concentrated among large landholders, those with access to government contracts and illegal enterprises. Often, the largest funds flowing through the constituency are from state owned monopolies and state development initiatives. I argue that criminals can access this pot of money through coercive force. Whereas, everyday entrepreneurs can not.

Consider the following examples that speak to how protection rackets confer a natural advantage to criminal politicians for delivering constituency service while still accumulating wealth. When the State's monopoly over violence is incomplete criminals can hone in on these opportunities by establishing protection rackets and illegal enterprises. For example, Anant Singh- one of Bihar's most notorious criminal politicians- derived most of his wealth from providing protection for the National Thermal Power Plant Corporation (NTPC). A three time MLA from Mokoma, Anant Singh was an established criminal, with an extensive rap sheet but little money. When the power plant broke ground in this still heavily agricultural constituency, the NTPC became the biggest economic player in the area by far. The power plant was one of only two such power stations in the state. However, the power plant was susceptible to extortion from local brigands, especially during construction. In exchange for protection money, Anany Singh stepped in and dealt with the smaller crooks to allow construction to continue unimpeded (fieldwork 2018).

State monopolies over natural resources provide another opportunity for criminals to expropriate wealth. In the 1980s, much of India witnessed a real estate development boom (Jeyaranjan, 2019). This led to increased demand for sand, a core ingredient in concrete construction. Lucrative sand extraction cartels popped up in response to the sky rocketing demand for the natural resource. In Tamil Nadu, seeing the potential for rents, politicians swooped in "investing their own resources, they co-opted local politicians as junior partners in sand ventures. " (Jeyaranjan, 2019). No bid contracts were provided to cartel loyalists and money flowed primarily to state politicians but also to local politicians and bureaucrats in charge of oversight in the form of kickbacks. Maintaining control over the sand cartels thus required maintaining political power.

A similar story characterizes the rise to power of Arun Yadav in Sandesh, Bihar. Arun is a high-level player in Bihar's "Sand Mafia." The MLA is one of 5-6 members heading the so-called sand syndicate. Illegal sand mining was already a massive enterprise before Arun arrived in Sandesh. However, Arun leveraged superior fire power to grab control of this racket. As Arun's bank account grew, he caught the eye of Lalu Yadav, the former Chief Minister of Bihar and political kingmaker. Lalu tapped Arun for the RJD ticket leading to Arun's eventual MLA victory. Once in power, Arun expanded his empire beyond illegal sand mining. Like other MLAs, he is known to skim rents from government contracts, especially in the road and construction sectors. Arun Yadav does not so much substitute for the state, as insert himself directly in to it. Arun now comports himself more as a legitimate businessman. He owns a milk production factory with over 600 head of cattle, huge tracts of land, and other local businesses. Though illegal sand mining is by far his greatest asset. Arun dominates nearly every facet of the Sandesh economy (fieldwork 2017 and 2018).

More systematically, Asher and Novosad (2016) find that exogenous shocks to the price of mining minerals cause an increase in the election of criminal politicians. Moreover, when mineral prices spike, politicians in office are charged with more crimes.

4.2.2 Criminal Politicians and Constituency Service

Once criminals are deeply embedded in the local economy they can supercharge their constituency service provision and local network creation. As Vaishnav (2017) notes, criminals first contested elections as a way to vertically integrate their personal power with their political power. In other words, criminals sought a way to stop merely being the muscle for some political patron who reaped all the in-office benefits from criminals cracking skulls. In a similar vein, I argue that criminals engage in politics as a way to vertically integrate the protection of their lucrative illegal enterprises. Criminals have several incentives to engage in constituency service even though they may be the dominant economic and political player in the area. First, as in the case of illegal sand mining, criminal enterprises may be heavily dependent on local labor. Plying constituents with services helps keep the whole operation running smoothly. Second, by assimilating into the State, criminals neutralize (at least in part) their greatest threat and violence competitor: the State itself. Therefore, providing constituency service to win elections becomes a key strategy in maintaining criminals' economic and political power.

Establishing trusted political networks with voters is far from a single-shot game. Networks require routine investments in local communities over time. Blessed with both time and money, embedded criminals can engage in repeated constituency service in order to protect their own empires. In an ethnographic exploration of North Indian Sand and Oil mafias, Michelutti (2019) explains the importance of mafiosos "invest[ing] locally in strengthening their political careers and/or the ones of emerging local leaders as strategies to cultivate immunity. The prerogative is to keep their residential 'territory' secure." In other words, criminal politicians remain rooted in their local communities both to keep watch over their lucrative enterprises and to cultivate relationships, with politicians, bureaucrats and locals to protect these assets.

To reiterate the argument presented in Chapter 1, criminality fosters political networks via three channels. First, criminal enterprises have a ready-made core network of co-conspirators (often family members) and a business fundamentally centered on trust. Second, the criminal enterprise helps align the economic incentives of politicians, bureaucrats and voters with that of the criminals. For example, in the case of the thermal power plant, politicians saw their massive infrastructure project come to fruition, bureaucrats and construction workers stop getting harassed on the job, and voters received improved power supply. All it cost was some protection money paid to Anant Singh. Third, as outlined above, criminals' muscle-power allows them to solve the tradeoff between wealth generation and spending time with the community.

In sum, to build reputable constituency service networks, politicians must demonstrate more than munificence during campaigns. Repeated demonstrations of empathy within villagers across the constituency are instrumental in building community ties. Criminals expropriative enterprises generate the wealth necessary to fund service networks, but also provide the time to strengthen network bonds. For example, criminal politicians like Pappu Yadav in Bihar, sit down to eat with voters, sharing their pain and problems. By sharing meals politicians demonstrate their social connections instead of just standing aloof from crowds at rallies (Interview with reporter, Patna 2018).[5] Ritualized, routine and repeated connections with local voters, builds a networks' connective tissue over time. In turn, criminal politicians can call on this personal vote to help protect them from authorities, win a party nomination and eventually an election (Interview with reporter in Patna, 2018). This qualitative evidence resonates with recent revisions in the clientelism literature emphasizing politicians doling out benefits in an effort to build credibility and bolster their reputations (Hicken and Nathan, 2020). In essence, delivering services to constituents can buy candidates a seat at the electoral table and voters have come to expect it. Those who fail in this

[5]To paraphrase one reporter (from an interview in 2018) regarding relations between MLAs and voters, "sharing meals together shows I'm close to you, I'm dear to you." By showing concern and "connectedness" to local people politicians can raise the status of a household or village. Connected politicians extend support to all types of households, regardless of caste or standing. The reporter went on to say "dalits may not have plastic chairs or even water or cup of tea, but these [criminal] politicians still sit with them and share their concerns, connecting with all types of local people."

core area of service work may hardly stand a chance come election time Bussell (2019). Here, criminal politicians who dominate local economies have an advantage in cultivating deep-rooted and durable networks. Control over the local illegal economy enables criminal politicians to repeatedly invest in reciprocal protection networks while also aligning voters' economic incentives with their own.

By combining local wealth generation with deep, embedded networks, criminals can increase the likelihood of winning office. Having laid out the theoretical expectations, I now turn to measurement issues in judging whether criminal candidates actually engage in more constituency service than clean candidates.

4.3 Measuring Constituency Service

4.3.1 Measurement Challenges

Do criminal politicians engage in more constituency service? And if so, do they reap greater electoral rewards as a result? Observational and survey data analyzing this causal pathway fall short on two fronts. First, there is a fundamental dearth of data regarding how candidates spend time and treasure (Vaishnav et al., 2019). In particular, there is a lack of data on candidates backgrounds, biographies and historical time use, especially for aspiring politicians. We know little about the degree of constituency investment candidates engage in over time, especially prior to being elected to office. Similarly, given the accusations of black money in political campaigns, details on precise spending and campaign operations remains a black box for Indian MLA candidates.[6]

Previous scholars have made great strides to fill these gaps. In particular, Bussell (2019) provides a comprehensive, multi-method study of constituency service in the Indian sub-continent. Combining survey experiments, micro-observational data and rich qualitative analysis Bussell identifies which citizens politicians are likely to serve and the conditions that lead to claim making.

[6] See Kapur and Vaishnav (2018). In particular, Bussell (2018) provides survey evidence on variation in sources of campaign financial support and gift giving across levels of Indian government and perceptions of which type of campaigns are fueled by "black money."

Still, quantitative evidence of constituency service focuses on elected politicians. If, as I argue, the primary value in communal investment derives from repeated and routine problem solving over time, the evidence to date elides whether victorious candidates engage in more of this behavior prior to gaining office. At the same time, the evidence base is typically cross-sectional, failing to consider how historical investments evolve over time. Thus, we can not identify variation in candidates' ability to provide communal investments and whether voters reward these skills as opposed to influxes of campaign cash.

The second and arguably thornier challenge is estimating causal effects when the relationship between candidate wealth, constituency service and electoral fate is likely endogenous. Wealthier candidates are more likely to spend lavishly in their communities but also more likely to be appointed to safe seats. Similarly, popular politicians are more likely to be invited to communal events and coveted by political parties. Two notable studies attempt to identify the causal effects of increased cash on politicians electoral fortunes. Sukhtankar and Vaishnav (2014) leverages shocks to cement prices as construction companies act as a primary source of campaign cash. Similarly, Asher and Novosad (2016) exploits price shocks to local mining deposits to show that additional mineral revenues causes an increase in the number of criminal politicians elected to office. However, they can not determine if this increase results from additional campaign finance for criminal candidates or from close associations between candidates and mining firms. Finally, while survey experiments manipulate aspects of voter (Bussell 2019) or candidate characteristics (Chauchard, 2016) they do not directly manipulate the provision of constituency service, let alone match this to observable election results.

4.3.2 Measuring Constituency Service with Wedding Supply Shocks

I address these challenges by focusing on a single form of constituency service- weddings. By narrowing the scope of my investigation, I bypass some of the difficulties in measuring and accounting for the various forms of constituency service that MLAs perform (though this strategy does have its drawbacks as I discuss below). In part, I focus on weddings as they form a core component of constituency service. One of the major social expenditures that candidates can make directly to individual constituents is to pay for marriage expenses, which can cost several times families' annual income (Bloch et al., 2004). For example, MLA Arun Yadav noted that he sees an uptick in requests and cash outlays during wedding season. By attending weddings, politicians can provide generous gifts, enhance the status of the local community, and extend their network during a large, centralized gathering of constituents. In this sense, weddings are uniquely consequential in their economic and cultural importance relative to other types of constituency service. I elaborate further on the role of weddings in constituency service in the next section. Finally, I am able to approximate a natural experiment by leveraging a shock to the supply of weddings that occur during campaigns.

To overcome the aforementioned endogeneity challenges, I exploit the timing of the Hindu religious period of Chaturmas in relation to state legislative elections. I explain my identification strategy in greater depth in Section 4.4. Briefly, Chaturmas causes a large, negative shock in the number of weddings held during the four months when Vishnu is asleep and Hindus observe a more subdued and somber period. Elections that fall during Chaturmas, therefore, experience a negative, exogenous shock to demand for campaign spending and services that target wedding ceremonies. Election dates are arguably orthogonal to the Hindu wedding calendar. The Election Commission of India (ECI) determines the precise election and campaign schedule. Additionally, auspicious marriage days and wedding season vary year-over-year according to the Hindu lunisolar calendar. The primary structural break that delineates wedding season from non-wedding season is Chaturmas (which roughly runs from July to November) (Gupte, 1994). During this period there are very few Hindu weddings as it is a time of austerity, fasting and penance. I leverage this quasi-random shock as a discouragement for candidates to engage in one traditional form of constituency service during campaigns.

To clarify, my qualitative argument stresses criminal politicians' *long-term* and repeated investment in constituency service as the important point of differentiation. Still, there are reasons to believe that candidates who have previously invested in local communities should fare better from an uptick in wedding spending before elections. The communal importance and public nature of weddings offers candidates a chance to cement social ties and status within the community (Bloch et al., 2004; Rao, 2001). Politicians are also routinely expected to pay marriage expenses (fieldwork and Rao 2001). Voters may discount candidates who splash cash during the campaign only, thinking this candidate will not return after the election. In this case, the returns to paying for weddings may be higher for candidates who have proven themselves to be "sons of the soil" via longstanding ties and investment in the community. Therefore, paying for and attending weddings will provide a greater marginal vote return to candidates consistently investing in local political networks.

In an ideal experiment, I could randomly assign some candidates to increase constituency service, leaving others to serve as a valid counterfactual for how voters punish the absence of these efforts. Lacking these conditions, I leverage a shock to constituency service opportunities that coincides with campaigns. While I can not precisely manipulate access to constituency service, the religious period of Chaturmas does induce natural variation in the supply of weddings (a core form of constituency service) that occur during MLA campaigns. I exploit this natural shock to the opportunity for constituency investment, at a critical time, when acts of service may carry more weight in the mind of voters.

4.3.3 Drawbacks to Focusing on Weddings

Despite the potentially exogenous variation in constituency service created by Chaturmas, there are some drawbacks to my singular focus on weddings. First, this strategy fails to capture the full variety of constituency service and will miss other important channels of community investment that politicians engage in. The total basket of constituency service, and how the components of that basket varies across candidates and over time, presents a fruitful avenue for future research. Another concern is that politicians may just substitute towards other forms of constituency service during Chaturmas. For example, candidates may take their wedding investments and inject them into rallies. While rallies are not strictly a traditional form of constituency service outside campaign season, candidates often hand out food and other gifts to entice voters to show up. It's plausible that when faced with a dearth of weddings, candidates reinvest in rallies.

There are a few reasons to believe that simply putting money and time towards rallies (or other forms of constituency service) would not necessarily return the same bang for politicians' service buck. To be fair, I can not definitively rule out that criminal politicians' channel extra resources of time and money to other forms of constituency service. Still, strict substitution away from weddings may be difficult. First, weddings are a unique event in the type of crowd that they draw. Voters that may not necessarily show up to candidates' other events, will show up to their friends' wedding. For example, rallies tend to draw supporters. Whereas, weddings are more likely to draw a cross-section of society, including swing and opposition voters. In this sense, weddings serve as a type of free advertisement for voters who might not otherwise have a way to update their beliefs about the candidate.

Second weddings are a large public gathering that may provide the most efficient use of candidates' time during campaigns. Money may be more easily injected into other campaign expenses. However, outside of rallies, it is difficult to consider a form of constituency service that provides the same efficient use of candidates' time when reaching out to voters. Weddings provide a large, centralized pool of voters, the candidate is given a seat of honor, and candidates can demonstrate their generosity via gifts. Still, I can not empirically rule out that candidates are able to substitute fully towards other acts of service during Chaturmas.

In addition to aiding the identification strategy, concentrating on weddings is useful for the central role they play in constituency service. To further motivate the focus on one part of politicians' service eco-system, I lay out the importance of weddings both in communal life, and specifically as a form of constituency service.

4.3.4 Weddings as Constituency Service

Attending and paying for wedding expenses are cornerstones of politicians' constituency service. Weddings enable candidates to demonstrate their standing in the community and provide generous gifts. Constituency service, writ broadly, acts as a core tool in politicians' community representation (Bussell 2019). Still, within the wide array of service acts, weddings standout as a particularly fundamental ingredient. One Member of Parliament from Karnataka noted the electoral benefits associated with wedding attendance:

> "At weddings of our constituents or well-wishers, I can meet 1,000 to 2,000 people at one place. I attend at least 10-12 marriages a day and to save time, sometimes I go in the afternoon when all the rituals are over. My presence there is more important...The fear of losing the goodwill of voters makes us attend all these events..." - BJP MP P C Mohan (Ataulla, 2016)

Similarly, an independent MP candidate in Rajasthan offered "to pay for 2,100 weddings to win votes" (Campbell, 2014). Arguing that he was a "son of the soil, born and brought up here and... engaged in such activities for many years"' (Campbell, 2014). Albeit, these weddings were to be held after the election to skirt charges of illegal vote-buying. Other scholars have noted the trade-off candidates can face between legislative duties and constituency service, particularly wedding attendance (Jensenius and Suryanarayan, 2017). Put differently, not only are weddings important communal events, certain politicians may be more capable and credible at investing in these opportunities. Candidates can face a tradeoff in time and treasure of investing in weddings versus pursuing other political goals. For example, voters may assign credit to politicians for personal constituency service via wedding attendance and gift giving. Whereas, efforts put towards development work may prove difficult for credit claiming. An interview between Francesca Jensenius and a prominent politician puts these points succinctly:

> "On his desk there were piles and piles of thick envelopes. Following my gaze, he smiled and told me that these were weddings invitations, enough of them for him to attend several weddings every day. He said that even if he worked to improve roads or water, people did not credit him for it. On the other hand, if he came to their wedding or came to show respect when someone in the family had passed away, then he would be guaranteed their vote for a long time to come. In fact, he was convinced that if he attended weddings and did nothing else during his whole period in office that would ensure him re-election." - Jensenius (2011)

Weddings take on an outsize importance in generating communal bonds and serving as conspicuous displays of social status. Voters prize the attendance of elites to further enhance their own families standing within the local community.[7] For example, Rao (2001) argues that "publicly observable celebrations have two functions: they provide a space for maintaining social reputations and webs of obligation, and they serve as arenas for status enhancing competitions." In fact, enterprising wedding planners are known to print separate VIP invitations with political leaders photos to entice honored guests to come (Ataulla, 2016).

While voters clamor for the attendance of notable political figures in order to raise the salience and status of their own wedding, politicians efforts are expected to go beyond merely showing up (Times of India 2016). Large monetary gifts from high profile attendees are practically a prerequisite for weddings that can cost multiple times some families' annual income (Bloch et al., 2004).[8] Thus, deep pocketed and connected candidates conceivably have more to gain by investing in weddings.

[7]For a peculiarly American example consider Hillary Clinton's attendance at Donald Trump's wedding where she claimed "that her presence was the gift" (Times of India 2016).

[8]For example, "former chief minister HD Kumaraswamy and MLA BZA Zameer Ahmed Khan are known to foot the wedding expenditure of poor people" (Times of India 2016).

4.3.5 Chaturmas as a Natural Experiment

Given weddings central role in constituency service, do candidates attend fewer weddings outside of wedding season? To ensure the greatest difference between treatment and control campaigns, I focus on Chaturmas which severely curtails the supply of weddings for four months each year. During this time, politicians who thrive on providing constituency service lose a large opportunity to invest in communal bonds and differentiate themselves from other contenders. Typically the Hindu luni-solar calendar period of Chaturmas corresponds to late June through early November. The 11th day of Ashadh (the fourth month of the Hindu lunisolar calendar) signals when Vishnu goes to sleep and the the start of Chaturmas. At this time, there are no auspicious wedding days until Vishnu reawakens on the 11th day of Kartik (which roughly corresponds to October or November) (Agarwal, 2017).

While weddings form just one part of constituency service, the Chaturmas shock to wedding supply is sizable. For instance, the price of gold is known to fluctuate with the Indian wedding season (Menon, 2020). Gold is the main luxury in both elaborate wedding displays and dowry gifts (Menon, 2020). The gold worn by a bride quite literally serves as a status signal, with wedding expenses eclipsing up to six times a rural family's annual income (Bloch et al., 2004; McKenzie, 2015). In total, "India accounts for roughly a third of global gold demand, with half of that being spent on jewelry" for weddings (Garner, 2016). With up to 20 million weddings held annually, India's gold laden brides cause prices to spike. Commodity analysts routinely note the bull run on gold that occurs around the end of Chaturmas in anticipation of the upcoming wedding season (Wood and Wachman, 2010; Garner, 2016; Menon, 2020; Ghosal, 2015). Moreover, gold prices vary in response to the number of auspicious wedding days on the Hindu calendar (Jadhav, 2019; McKenzie, 2015).

The number of weddings held in a constituency in a given month is mechanically related to Chaturmas. While I can not definitively show which politicians alter their behavior in wedding attendance or constituency service, fluctuations in the price of gold speak to the magnitude of the wedding supply shock that occurs during these four months. In turn, I anticipate politicians' wedding attendance to be much higher during wedding season- with larger electoral rewards for politicians whose personalized constituency service forms the backbone of their appeal.[9] Chaturmas, therefore, provides a unique opportunity to study a quasi-random shock to the number of wedding celebrations held during campaigns and the accompanying opportunity for constituency service.

In sum, I argue that candidates view wedding attendance as integral to their election chances. Second, Chaturmas provides a negative shock to the supply of weddings. Putting the pieces together, campaigns that coincide with Chaturmas will have fewer weddings and social network opportunities than otherwise similar campaigns held during wedding season. If we think of candidates attending wedding ceremonies as the treatment, wedding season is the encouragement for this treatment (or discouragement in the case of Chaturmas). In other words, I observe intent-to-treat effects. To be clear, I do not think voters evaluate candidates' constituency service records solely on wedding attendance during campaigns. Instead, to the extent that voters are myopic, the dearth of weddings in the four months leading up to elections will harm candidates whose bread and butter is constituency service more than candidates whose strengths lie elsewhere. In other words, criminal politicians (who I argue are more likely to invest in long-term constituency service networks) will suffer a greater electoral blow. As discussed in the data section below, there are two distinct clusters of elections pre and post Chaturmas. Therefore, in the run-up to the post-Chaturmas elections, candidates will face a four month wedding drought and reduced opportunities to nurture networks. Even if Chaturmas does not completely overlap with the campaign window, candidates who stand to gain the most from wedding attendance will miss out on a golden opportunity.

[9] For example, one reporter claimed that Karnataka's politicians "Be it a member of Parliament or the legislature -or one even in the making - attending a minimum of five weddings a day during the marriage season is a given. Blessing newlyweds, offering condolences at funerals and gracing house warming functions or other rituals in their constituencies are must-dos for Karnataka's elected representatives."

4.4 Identification Strategy

Is campaign timing relative to Chaturmas "quasi-random?" The fundamental identification assumption is that the timing of elections relative to Chaturmas is quasi-random. In other words, whether a campaign is run during Chaturmas or wedding season is orthogonal to candidates' potential election outcomes. Elections held during Chaturmas should, therefore, be otherwise similar to elections held during wedding season.

Several electoral institutional features make the haphazard assignment of campaigns to Chaturmas more plausible. Under the 1951 Representation of the People Act, the Election Commission of India, an independent, federal body, determines the exact election dates and preceding campaign schedule. The ECI's mandate requires elections to be completed before the end of the current legislative sessions' five year term. Given that deadline, the ECI has some leeway in determining the precise election schedule and preceding campaign window. A typical ECI press release outlining the election schedule illustrates the quasi-random factors influencing election timing. In particular, the ECI prepares the election schedule "after taking into consideration all relevant aspects like the climatic conditions, academic calendars, festivals, prevailing law and order situation in the State, availability of Central Police Forces, time needed for movement, transportation and timely deployment of forces, and assessment of other ground realities" (ECI, 2013).

Second, larger states require multiple election phases, further altering the campaign season even among states that hold elections in the same month. Campaign windows can prove particularly lengthy in populous states, depending on the availability and logistics of moving independent police forces to bolster election safety (Khalil et al., 2019). Finally, the timing of Chaturmas itself varies from year to year based on the Hindu luni-solar calendar.

One particular concern could be that certain states happen to fall on the Chaturmas election cycle, holding elections every five years without any variation in treatment status. If this were the case, my strategy might identify only state specific differences in election outcomes, rather than a treatment effect of election timing per se. However, the leeway in ECI assigning election dates, year-over-year changes in Chaturmas timing, and idiosyncratic perturbations to states' electoral schedules, provide quasi-random overlap between campaigns and Chatrumas- even *within* states' election cycles.

Do Candidates and Parties Self-Select into Treatment?

In order for the ignorability of the Chaturmas treatment assignment assumption to hold, candidates must not self-select into election schedules. At first blush, self-selection of candidates into wedding or Chaturmas election windows may seem likely. For example, if candidates know they are going to take an electoral hit they may decide to not run, biding their time for a better opportunity. However, elections operate on five year cycles making it likely that candidates will face the same treatment status after wasting a half-decade. Second, party tickets- which represent the best path towards electoral victory- are controlled by party elites and notoriously competitive. There is no indication that parties consider Chaturmas in allocating nominations, though parties may do so indirectly by factoring in a candidates' popularity at the time of elections. Instead, parties prioritize candidates' wealth and caste considerations when handing out nominations (Vaishnav, 2017).

Governing parties that routinely call early elections represent a far greater potential for selection bias. If parties strategically select election timings, it could be that early elections are called when incumbents feel they can win comfortably. In this case, parties choose their electoral window for reasons unrelated to Chaturmas and the dearth of weddings. Since Chaturmas represents only $1/3$ of the year, strategic early elections could lead to higher margins of victory during wedding season and an artificial correlation between my hypothesized treatment effect and election outcomes.[10] Studies of political budget and business cycles in parliamentary democracies have to grapple with the endogeneity inherent in parties setting the election schedule. For example, in India, Khemani (2004) finds that 34% of state legislative elections between 1960 and 1992 were

[10]Though it is not immediately clear to me why this would disproportionately effect criminal as compared to clean candidates.

called in the middle of the legislative session. However, the prevalence of early elections in Indian states seems to be a relic of the Congress era. In my sample, only 2 out of 74 state legislative elections (less than 3%) were called early by the ruling party or coalition.

Even in the case of "early elections" the exact timing is not precisely controlled by parties and occurs generally for reasons orthogonal to Chaturmas. In late 2013, the BJP fell just short of a majority seat-share and failed to form a government. The upstart Aam Aadmi Party formed a coalition government alongside Congress ruling for a short-lived 49 days. After failing to table a preferred anti-corruption bill, Arvind Kejriwal the Aam Aadmi Party leader resigned, and Delhi entered into a period of President's rule by the federal government (Jain, 2014). Early elections were subsequently held in 2015. A more canonical example of strategic early elections occurred in 2009 in Haryana. The Haryana assembly elections were called four months early. The ruling Congress party wanted to capitalize on their strong showing in the Lok Sabha parliamentary elections five months prior, where they won 9 out of 10 seats (Kumar, 2019). Still, even in the rare cases where elections are called early based on estimations of popularity signals, it would not necessarily mean this strategy differentially favors criminal over clean candidates.[11]

One further case bolsters the idiosyncratic nature of state election timings and how states may switch their treatment status. In 2005, the Bihar legislative elections resulted in an hung assembly. After failing to form a coalition government, the Prime Minister imposed President's Rule until fresh elections were held in November that same year. With that fractured election, Bihar shifted its campaign cycle from February to October with the subsequent 2010 and 2015 elections falling during Chaturmas. These idiosyncratic outcomes lend plausibility to the as-if random assignment of elections and undermine the narrative that MLAs can choose their fate.

In short, election timing with respect to the Chaturmas window is plausibly exogenous across states. To clarify, states that hold campaigns during Chaturmas should be otherwise similar to states that hold campaigns outside of Chaturmas, except for the timing of the campaign and the re-

[11] As a robustness check, I re-run the main specifications after dropping these early election states

sultant negative shock to community events. To operationalize this test, I compare changes in vote share and win probabilities for criminal and clean politicians between campaigns that occur during Chaturmas or wedding season. This analysis will recover the causal effect of Chaturmas on vote share under the identifying assumption that state elections held during Chaturmas are otherwise similar to state elections held during wedding season, except for the timing of the election.

There are two primary shortcomings to this identification strategy. First, Chaturmas serves as a proxy for the true treatment I care about: MLAs local investment over time, especially as signaled via wedding attendance. To recap the primary theoretical argument, criminal politicians local wealth generation allows them to perform long standing community investments which voters reward. While voters may care about the long-term nature of these investments, I am unable to measure these inputs. Instead, I argue that Chaturmas will reduce a campaigns salience of constituency service, undermining politicians who rely on conspicuously displaying their community bonafides. Campaigns held during Chaturmas are, therefore, a proxy (at best) for the larger theoretical argument that longstanding community investment drives criminal politicians' electoral advantage. Put differently, the analysis can be thought of as an intent-to-treat effect where Chaturmas acts as a discouragement to the wedding attendance treatment. In this case, where I do not directly measure or vary wedding attendance (let alone overall constituency service), comparing vote share in Chaturmas and wedding season elections recovers only an intent-to-treat effect. As noted above, there are qualitative reasons to believe that criminals would both avail themselves of these communal opportunities and be disproportionately rewarded for this investment. However, I am unable to show that this is systematically true.

Second, any unmeasured confounders that are coterminous with Chaturmas would bias estimation. Most obviously, Chaturmas coincides with monsoon season in several states. I discuss threats to identification in more detail in the discussion section. For now, any causal interpretation would rest entirely on the plausibility of the ignorability assumption and accounting for time-varying confounders. For this reason, I consider my analysis as an exploration of a potential natural experiment without claiming confirmatory causal results.

4.5 Data

To contrast how criminal and clean state legislators perform during Chaturmas compared to wedding season I combine two primary sources of information. First, the candidate dataset- detailed in Chapters 2 and 3- identifies serious criminals and electoral performance. The primary outcomes of interest are candidate vote-share (*Vote Share*) and an indicator for whether or not the candidate won the election (*Winner*). In the main specifications, I interact the Chaturmas treatment with candidate's criminality. Thus, I can compare if criminal candidates suffer a greater electoral penalty during Chaturmas elections as compared to clean politicians. Second, to allocate state elections to treatment and control conditions, I collect new data on campaign windows. More specifically, this requires identifying both campaign windows and Chaturmas windows to determine if they coincide and the percentage of overlap.

4.5.1 Election and Campaign Timing

There is no definitive dataset on election dates for state legislatures let alone campaign time-frames. The Trivedi Center for Political Data houses the most authoritative source on Indian state legislative elections. However, the TCPD dataset lists the month votes were counted, but not when ballots were cast or candidates hit the campaign trail. This can lead to large discrepancies between the TCPD's "election month" and when candidates actually campaign. This divergence is especially troublesome for large states, where voting is staggered across constituencies resulting in over a month between the TCDPs election date and when the first ballot was actually cast. To identify plausible campaign windows, I collect data from the Election Commission of India's Press Notes which outline election schedules and sets key election dates.[12] The Election Commission of India determines the exact election dates and thus dictates the campaign schedule. I code "the date of issue" of the ECI press notification as the start of the campaign window.

[12] The ECI's election schedule Press Notes are available at `https://eci.gov.in/files/category/11-press-releases/`.

The campaign window *officially* starts immediately upon the release of the Press Note from the ECI. After release of the Press Note, campaigns are subject to the ECI's Model Code of Conduct and candidates begin to file their nomination papers. The Model Code of Conduct regulates campaign and government activities. For example, the ruling government is forbidden from staring new welfare programs or transferring civil servants. Similarly, the ECI heightens its observation of campaign spending and illegal electioneering activities like vote-buying. A natural alternative for the campaign start date would be the last day to file candidate nomination papers, which finalizes the list of candidates. However, I prefer the date of press notification issue for two reasons. First, party efforts begin prior to the notification period (e.g. parties identify candidates, PR firms and coalition partners) with "spending [intensifying] once the ECI fixes polling dates and candidates file their nomination papers" (Collins, 2018). Second, most major candidates and incumbents will anticipate receiving party tickets and may increase community investments prior to filing their nomination paperwork.[13]

I use the final day of polling to mark the end of the campaign window. In populous states, assembly elections are held in multiple phases to accommodate voters, logistics and ensure safe elections. Even if voting has finished in one part of the state, campaigns are allowed to continue for constituencies that have yet to go to the polls. For this reason, I consider the campaign period as extending from the date of the ECI press notification until the final day of voting.[14] In sum, I construct campaign time frames for 74 state legislative elections. Based on these starting and ending definitions, the typical campaign window lasts 55 days. Figure 4.5.1 plots the annual distribution of campaign weeks, with the shaded portion indicating the average Chaturmas start and

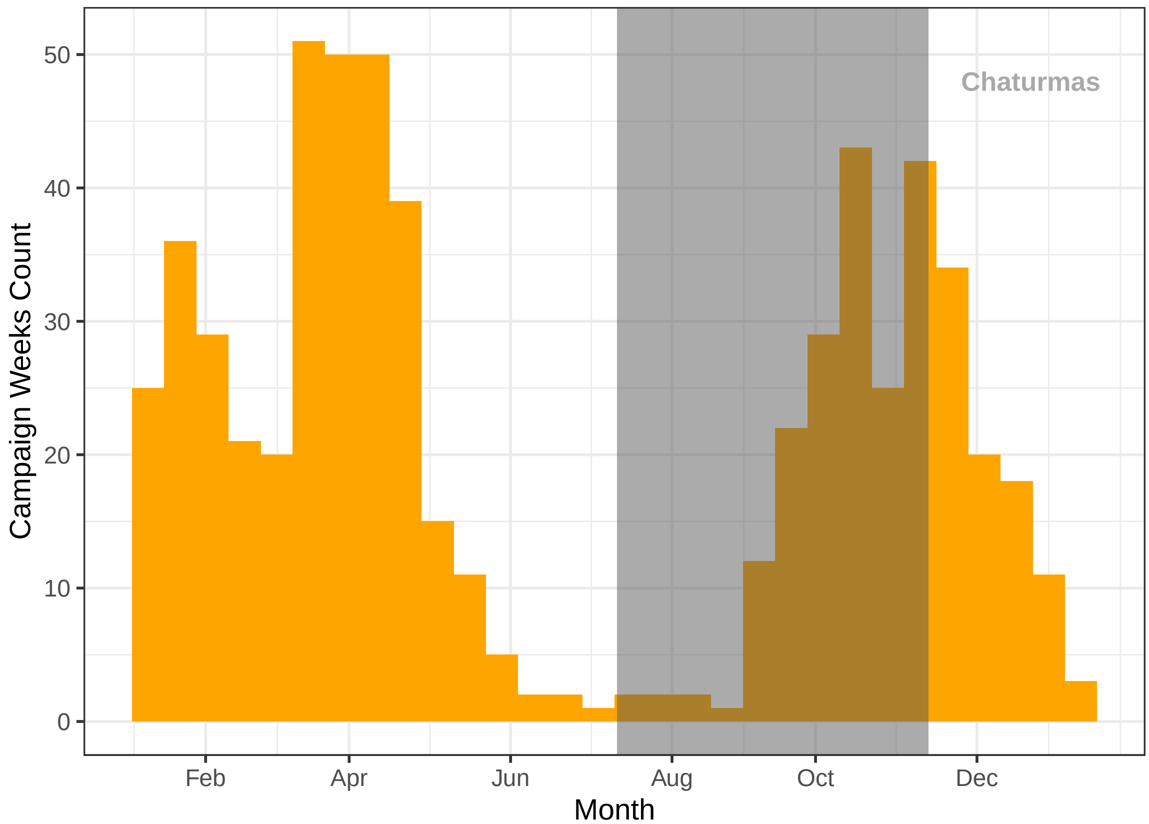
[13]The distribution of state legislative elections by week shows a generally bimodal pattern, with a group of state elections coming before Chaturmas, and a second group after Chaturmas (see Figure 4.5.1). Thus adjusting the campaign start date by one or two weeks (i.e. by using the nomination deadline vs. the press issue date) does not make a material difference in the state elections assigned to treatment and control.

[14]Future work could leverage this staggered roll-out within states to more precisely align overlaps of constituency level campaigns and Chaturmas dates. Allowing for variation within state will drastically increase sample size. On the other hand, these micro-lags in polling dates within the same month will produce a smaller shock in wedding supply relative to state elections that are held during the four month absence of weddings incurred under Chaturmas. For a design that exploits staggered, within-state election timing see (Khalil et al., 2019).

ending period between 2005 and 2016. Broadly speaking, state legislative elections are temporally bimodal, with one group of state elections occurring after Chaturmas and one before. I exploit this variation to construct the wedding shock during campaigns.

One other point of interest from Figure 4.5.1 is the dearth of elections in the summer. Due to the monsoon season few elections are held between June and September. This is likely in part due to the resulting logistical complications for moving security troops to ensure free and fair elections. This is potentially concerning as it illustrates that monsoon season is highly correlated with Chaturmas. I elaborate on the problems created by the fact that there is a strong correlation between monsoon season and Chaturmas in the discussion section. My primary strategy to address this issue is to adjust for rain shocks. I elaborate further in the discussion.

Figure 4.5.1: Distribution of State Election Campaigns



This figure displays the distribution of campaign weeks across the year, for state elections between 2005 and 2016. The shaded area indicates Chaturmas. Start and end dates for the Chaturmas represent the average start and end dates between 2005 and 2016. State elections appear to be bimodally dsitributed with one grouping of campaigns occurring early in the year and one towards the end.

4.5.2 Identifying Chaturmas and Wedding Season

Chaturamas typically runs from July to November, depending on the Hindu luni-solar calendar. On average, Chaturmas lasts 122 days, or just over 4 months. Chaturmas start and end dates are scrapped from `drikpanchang.com` which provides historical Hindu calendars. I construct the treatment indicator by combining the Chaturmas and campaign windows. To be fair, the binary coding of Chaturmas coarsens the treatment effect of the supply of weddings that co-occur with campaigns. A more granular treatment measure would be to count the number of auspicious Hindu wedding days during campaigns. However, there is no definitive list of auspicious dates and "even Hindu sages had different viewpoints on some of the elements considered while selecting auspicious marriage dates" (Drik Panchang).

In fact, the exact start and length of Hindu "wedding season," is somewhat arbitrary. Wedding Season generally occurs soon after Chaturmas and extends several months into the following year. For example, December is a popular wedding month in India. In this case, campaigns that start in November but move into December would water down any treatment effect from Chaturmas. Unfortunately, without a reliable way to code wedding season or auspicious wedding dates, I rely instead on the well defined Chaturmas period. Chaturmas start and end dates are dictated by the days when Vishnu enters and then arises from a four month slumber. These days are constant on the Hindu lunisolar calendar and only vary on the Gregorian calendar from year to year. In addition, I code the percentage of overlapping campaign and Chaturmas days. This continuous treatment variable should help avoid the contamination that occurs for campaigns that blur between the end of Chaturmas and the start of a broadly defined "wedding season." Combining the candidate dataset with the timing of campaigns and Chaturmas allows me to compare the relative performance of criminal and clean candidates when elections coincide with Chaturmas versus wedding season.

4.5.3 Covariates

While Chaturmas and wedding season elections should be otherwise similar, given the aggregated level of treatment, I include controls to increase precision. In particular, I include constituency and candidate adjustors from the candidates dataset. Candidate controls include sex, age, personal wealth, and whether the candidate is a member of a major party or running as an independent. Constituency-Election controls include turnout, whether the constituency is reserved for Scheduled Caste or Scheduled Tribe candidates, the number of electors and the number of candidates. Still, including adjustors presents a tradeoff in increasing precision versus inducing bias from conditioning on post-treatment variables (Kern and Hainmueller, 2009). In particular, turnout and the number of candidates are variables that could be influenced by the Chaturmas treatment as they occur after the ECI sets the election dates. As a robustness check, I report results with and without adjustment.

4.5.4 Balance Checks

Overall, there are 82,567 candidates running across 10,072 assembly-constituency elections. Of the 74 state legislative elections in the study group, 29 are assigned to treatment and 45 to control, based on their campaign timing relative to Chaturmas. While there is debate over the usefulness and interpretation of balance tests in experimental research, Dunning (2012) recommends including balance tests for natural experiments where the researcher does not control the assignment mechanism. Balance checks serve as one possible insight into as-if random assignment for natural experiments. I report balance checks between treatment and control state elections in Appendix 4.A. The coefficient estimates and 95% confidence intervals in Figure 4.A.1 are from independent linear models where treatment (i.e. being assigned to a Chaturmas election) is regressed on constituency, electoral and candidate characteristics. Elections where campaigns overlap with Chaturmas are not significantly more populous (as measured by the number of electors). Nor do they have higher turnout or more candidates running. Crucially, wealthy and criminal candidates are not significantly more likely to run during wedding season. If criminals were more likely to run during wedding season this could indicate selection into treatment. This helps to lend some credence- and one empirical check- that elections held during Chaturmas and wedding season are otherwise similar, and treatment assignment is as-if random. The one substantive difference in the balance tests is that more independent candidates run during Chaturmas. I control for candidate type (i.e. whether they are affiliated with a party or not) along with the other predictors in the balance tests in an attempt to adjust for imbalance between treatment and control electoral conditions.

4.6 Results

Do criminal politicians suffer an electoral penalty during Chaturmas? As an initial inspection, Figure 4.6.1a displays how vote share changes across election dates, relative to Chaturmas. I fit local regression smoothers to candidates vote share by criminality based on the week the state election was held (i.e. the week of the last day to cast a ballot in the state). The grey shaded area indicates the typical beginning and end dates for Chaturmas. Elections that occur during or after Chaturmas show a decrease in vote share relative to elections that take place during wedding season. However, this downturn is appreciably larger for criminal candidates. Thus, this visual evidence is consistent with criminals paying a larger penalty for campaigns held during Chaturmas relative to wedding season. Figure 4.6.1b provides an alternative view of these trends. Here, I plot the number of days from the close of an election campaign, relative to the start of Chaturmas, for that year. Again, campaigns that take place during wedding season show increased vote shares for criminal and clean candidates. Whereas, campaigns that end after Chaturmas, show a steep decline in vote share for criminal candidates.

One other point to note about Figure 4.6.1a is the relatively large increase in criminal vote share that occurs right before the start of Chaturmas. At the risk of over-interpreting trendlines, this evidence is actually consistent with my Chaturmas treatment effect argument. Elections that occur right before the start of Chaturmas have undergone the entirety of wedding season. Hence, candidates in these elections have had more weddings with which to demonstrate their routine community investment. Whereas, elections in January (with the campaign starting eight weeks prior) land right at the tail-end of Chaturmas when the lack of wedding attendance is fresh in voters' minds. Therefore this uptick is consistent with criminals as community warriors faring better after having the opportunity to demonstrate community investment over a longer period of time. Data listing the exact dates of auspicious wedding days could help investigate this trend in future work.

While the quasi random assignment of elections balances elections characteristics in expectation, it is still possible (especially for this small draw of 74 elections) that an increase in the number of candidates contesting these elections or drops in turnout may explain part of the dip in vote-share percentage. To adjust for potential confounders and imbalance, I explore models predicting vote share and margin of victory by interacting criminality with the Chaturmas treatment.

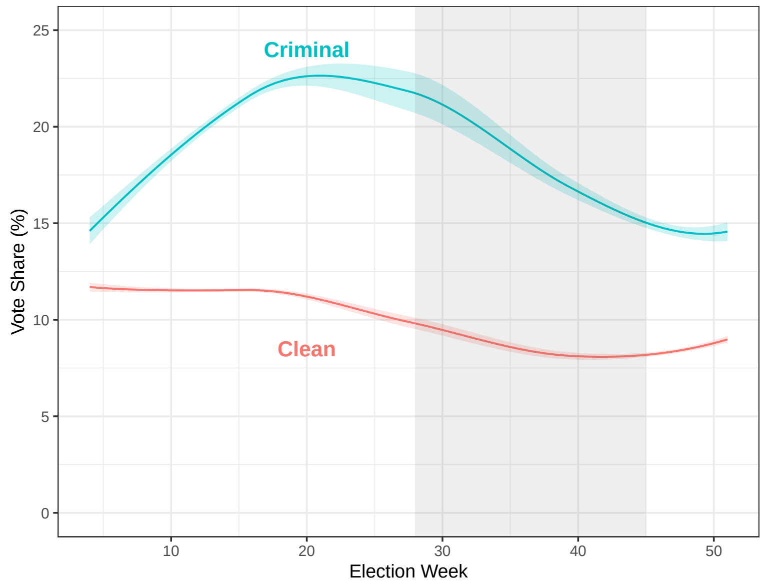
Column 1 of Table 4.6.1 interacts the Chaturmas treatment with candidates' criminality to predict vote shares. Here, the treatment is simply if at least one day of a candidate's campaign overlaps with the Chaturmas window. Standard errors are clustered at the state-level (i.e. the level of treatment assignment).[15] Chaturmas is associated with a decline in vote shares for both clean and serious criminal candidates. However, the decrease in vote share relative to wedding season is steeper (more negative) for serious criminal candidates. Serious criminals experience a nearly 6 percentage point decrease in vote share (from an average of 21% in elections held during wedding season to 15% when campaigns overlap with Chaturmas).[16] Moreover, this is nearly 2.5 percentage points more than the decrease expected for clean candidates between wedding season and Chaturmas. However, the disproportionate decrease for criminal candidates is only significant

[15]For results aggregated to the state election level see Appendix 4.B. Results have similar signs and magnitudes though remain statistically insignificant.

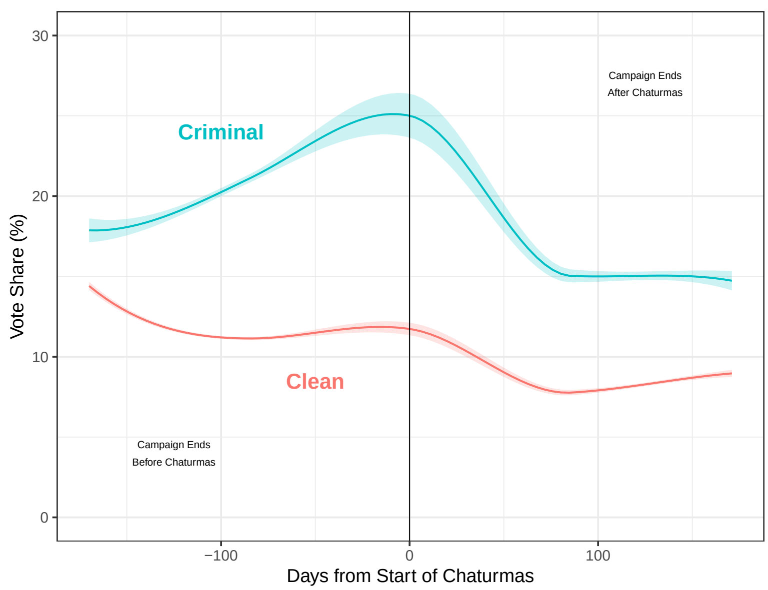
[16]These calculations are taken from the table displaying the full marginal effects of Chaturmas in Appendix **??**. This decrease for serious criminals is statistically significant at the 5% level.

Figure 4.6.1: Vote Shares by Criminality and Campaign Timing

(a) Candidate Vote Share vs. Election Timing



(b) Candidate Vote Share for Campaigns Ending Before vs. After Chaturmas



(a) plots the local regression smooth of candidate vote share by criminality across election weeks for state legislative elections between 2005 and 2016. Election weeks are based on the week where the last date of voting occurred in a given state election. The figure illustrates how vote share changes as a function of the week of the year when an election finished, broken out by the criminality of the candidate. The shaded portion of the graph indicates the typical time-frame for Chaturmas. The exact Chaturmas start and end dates are based on the average start and end dates between 2005 and 2016. (b) portrays the same information but reorients the graph to compare vote shares in campaigns that end after Chaturmas to those that end before Chaturmas. The zero point indicates the start of Chaturmas for a given year. I use a local regression smoother to trace the change in vote shares by criminality based on the number of days a campaign ended prior to, or after, the beginning of Chaturmas. Vote shares see a noticeable dip for criminal candidates in elections that occur after Chaturmas relative to those at the beginning of the year.

at conventional levels after candidate and constituency controls are included (see column 2).[17]

To increase the validity of as-if random assignment, I include both candidate and electoral constituency controls in columns 2 and 4. For example, candidates' wealth could be a potential confounder. Wealthier candidates are known both to perform better at the ballot box and the have a greater disposal to splurge on community social events such as a wedding. After adjusting for wealth and other candidate characteristics, Criminality still positively predicts vote-share and gets an added bounce during wedding season. Some of those adjustors could conceivably be considered post-treatment with the timing of election affecting how many candidates run or party allocation of tickets. For results with just pre-treatment candidate characteristics see Appendix 4.C.

Columns 3 and 4 report estimates from linear probability models after changing the dependent variable to an indicator for the winning MLA candidate.[18] Substantively speaking, the results are broadly the same as those for models with vote share as the dependent variable. Serious criminal candidates are 7 percentage points less likely to win an MLA seat when campaigns run during Chaturmas as opposed to wedding season. Which is a further 3 percentage point decline over that experienced by clean candidates. Albeit, the difference in slopes is not statistically significant at conventional levels. In sum, results are consistent with criminals performing worse in elections held outside of wedding season. Though this decrease is not large enough to statistically differentiate from the simultaneous decrease for clean candidates in all models.

4.7 Discussion

This identification strategy faces two primary challenges. First, I do not directly observe wedding attendance by candidates, let alone differential attendance across elections based on candidate criminality. In an absolute best case scenario, I identify the causal effect of holding a campaign

[17]This is somewhat to be expected given that precisely estimating interaction effects can require at least 16 times the sample size as identifying main effects (Gelman, 2018)

[18]I prefer linear probability models in this setting as opposed to logit to ease interpretation. At the same time, predicted probabilities do not fall outside of the 0,1 interval and I use heteroskedasticity-consistent standard errors. Thus, I address the main concerns about using the linear probability model in place of logistic regression.

Table 4.6.1: Serious Criminal x Chaturmas Treatment

	Vote Share	Vote Share	Winner	Winner
Serious Criminal	9.08^{***}	6.15^{***}	0.11^{***}	0.08^{***}
	(0.99)	(0.78)	(0.01)	(0.01)
Chaturmas	-3.16^{*}	-1.88^{*}	-0.04^{*}	-0.02^{*}
	(1.51)	(0.84)	(0.02)	(0.01)
Serious Criminal x Chaturmas	-2.44	-2.33^{*}	-0.03	-0.03
	(1.33)	(1.09)	(0.02)	(0.02)
Controls	No	Yes	No	Yes
N obs.	82,564	80,793	82,567	80,796
N clusters	74	74	74	74

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Controls adjust for both constituency and candidate characteristics. Candidate controls include age, gender, log(wealth + 1) and whether the candidate ran as an independent or on a party ticket. Constituency controls include the number of electors, whether the constituency is reserved for SC/ST, turnout (%), and number of candidates. Standard errors are clustered at the state-election level.

during Chaturmas versus wedding season on vote share. In other words, I measure an intent -to-treat effect, where Chatrumas serves as a discouragement. However, I fail to observe actual wedding attendance. I can not rule out politicians responding strategically and substituting other forms of constituency service, undermining inference for the primary mechanism I am interested in testing. Second, I am unable to adjust precisely for some confounders that regularly co-occur with Chaturmas. I discuss both limitations in turn. For now, I consider my analysis as an exploratory investigation. In addition, this chapter lays the groundwork for an identification strategy that could be more plausible given additional data collection or alternative research questions.

4.7.1 Strategic Politicians and Parties

Without measuring actual candidate behavior, it is possible that MLA candidates simply substitute other forms of constituency service for wedding attendance during Chaturmas. Still, it is not obvious that handing out cash or attending temple services are easily interchangeable with wedding attendance. Weddings are unique both from an economic and cultural standpoint. Economically speaking, weddings are a particularly burdensome expense. Dowries can cost twice the brides' family annual income, with total wedding expenses routinely sending families deep into debt (Menon, 2020; Bloch et al., 2004). Politicians play an integral part in raising the status of local households by attending services while simultaneously strengthening personal bonds and accruing cultural cache. The economic expense and status signaling associated with weddings underscore their central importance in village life (Rao, 2001). Therefore, it may be difficult for politicians to find a second-order substitute of similar economic and cultural significance where they can efficiently meet many constituents. On the other hand, weddings' represent just one mode of community investment and I can not precisely identify whether weddings outweigh the basket of other local opportunities that politicians could shift their time and money towards. Future data collection on how MLAs spend their time (especially during campaigns), the type and amount of community investments they engage in, would be illuminating in this regard. Combining data on community investments with the exogenous wedding season shock would more closely approximate an encouragement design to identify the effect of community investments on election outcomes.

Secondly, it is possible that parties act strategically in the aggregate, giving tickets to different types of candidates depending on their electability at the start of campaign season. I find this strategic response to be less plausible given the previous discussion on the limited number of early elections called during this period. Still, I re-run the analysis after dropping the early election results for Delhi in 2015 and Haryana in 2009. Re-running the analysis with only elections that reach their full term does not substantively alter the results, with a slightly larger negative effect estimated for criminal politicians during Chaturmas (see columns 3 and 4 of Table 4.7.1).

4.7.2 Coterminous Confounders

The biggest threat to identification plausibly comes from events that occur on the same annual cycle as Chaturmas. In this case, I could not disentangle the effect of wedding season from another event that happens at the same regular interval. Monsoon season is perhaps the most obvious cyclic confounder. While it seems unlikely that voters would disproportionately punish criminal

Table 4.7.1: Robustness Checks

	Percentage Overlap		**Dropping Early Elections**	
	Vote Share	Winner	Vote Share	Winner
Serious Criminal	5.95^{***}	0.08^{***}	6.12^{***}	0.08^{***}
	(0.68)	(0.01)	(0.79)	(0.01)
Chaturmas	-1.82	-0.02	-1.86^{*}	-0.02^{*}
	(1.01)	(0.01)	(0.85)	(0.01)
Serious Criminal x Chaturmas	-2.47^{*}	-0.03	-2.36^{*}	-0.03
	(1.18)	(0.02)	(1.11)	(0.02)
Controls	Yes	Yes	Yes	Yes
N obs.	80,793	80,796	78,958	78,961
N clusters	74	74	72	72

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

The treatment in columns 1 and 2 is the percentage of campaign days that overlap with Chaturmas. Columns 3 and 4 use the baseline treatment indicator for at least one day of the campaign overlapping Chaturmas but drop the two state elections that were called early and may indicate strategic selection of campaign timing. Controls adjust for both constituency and candidate characteristics. Candidate controls include age, gender, log(wealth + 1) and whether the candidate ran as an independent or on a party ticket. Constituency controls include the number of electors, whether the constituency is reserved for SC/ST, turnout (%), and number of candidates. Standard errors are clustered at the state-election level.

Table 4.7.2: Controlling for Rain Shocks

	Vote Share	**Winner**
Serious Criminal	6.65^{***}	0.08^{***}
	(0.86)	(0.01)
Chaturmas	-1.33	-0.02
	(0.91)	(0.01)
Serious Criminal x Chaturmas	-2.86^{*}	-0.04
	(1.14)	(0.02)
Controls	Yes	Yes
N clusters	74	74

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Controls are the same as in Table 2 with the addition of the rain shock variable. Standard errors are clustered at the state-election level.

candidates for weather events, in times of economic pain voters may turn to criminals for a safety net. That is, voters are more willing to make the tradeoff between a criminals miscreant deeds and their ability to ensure against the worst case economic outcomes, upwardly biasing the Chaturmas treatment effect. The timing of monsoons and states exposure to weather events varies across the country, which raises the possibility of adjusting for weather and agricultural confounding.

In table 3, I adjust for rainfall shocks using mean deviations from monthly state rainfall averages over the entire study period. Unfortunately the rain data is somewhat imprecise as it spans multiple smaller states. Still, it is encouraging that results are similar to the baseline specification and even significant in the case of vote share. However, lacking precise data on rain, I do not consider this potential confounder well adjusted for. I leave more precise monsoon measurement for future research.

Another way to partially mitigate the threat of contemporaneous shocks (outside of just monsoon season) is to exploit variation in campaign length and yearly changes in Chaturmas start and end dates. Even states that hold elections during the same month experience variation in the *extent* of campaign exposure to the Chaturmas window. Table 4.7.1 columns 1 and 2 repeat the baseline specification replacing the indicator for Chaturmas with the percentage a campaign days that overlap with Chaturmas.[19]

This measure is imperfect since small variations in overlap with Chatrumas is a weak shock to wedding supply. If voters are retrospectively myopic they may care less about a few days of missed weddings relative to the fresh memory of candidates' absent during an extended Chaturmas period.[20] Results are substantively similar using this alternative treatment coding.

4.7.3 General Equilibrium Effects

Do candidates attend the same weddings? One additional concern might be that city based candidates respond to constituency service types by shadowing and showing up at the same event. Or,

[19] Alternatively, one could exploit staggered election phases in large states or special by-elections that occur throughout the year- a strategy employed by (Baskaran et al., 2015).

[20] See Khemani (2001) for a discussion of under what circumstances Indian voters may act myopically.

similarly, that all candidates are invited and to the same weddings. Under this scenario, would the effect of Chaturmas net out to zero? Given hundreds of thousands of constituents and the compressed wedding season, candidates likely have some choice in the weddings they could attend. Still, there are better reasons why the wedding effect would not necessarily cancel out with politicians attending the same events. First, my argument is that criminal politicians are able to unlock the wedding vote bump due to their historical constituency investments. Voters discount politicians who only show up during the campaign. If anything, this mirroring strategy may serve to discredit the campaign opportunist. Voters would likely draw comparisons between the candidate they routinely see and the one who only shows up when an election is on the line.

4.8 Conclusion

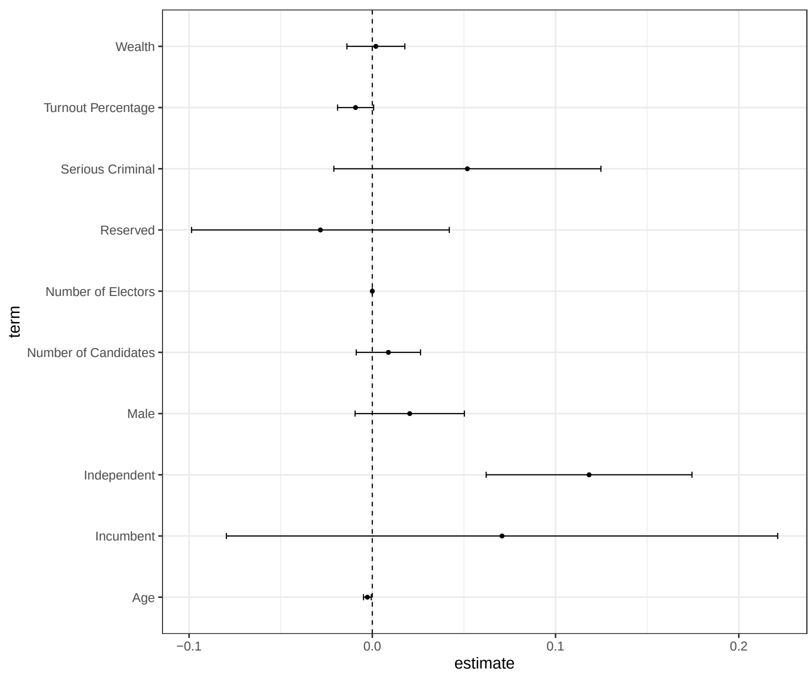
Drawing on 12 months of fieldwork, I argue that criminal politicians are uniquely suited to optimizing both constituency service delivery and wealth generation, within their home constituency. Criminals muscle-in on local economic activity and set up protection rackets. By extracting wealth from rural areas where industry is absent, criminals solve the problem of generating enough money to contest elections, while simultaneously remaining connected to the local community. For instance, protection rackets and illegal mining can be far more remunerative than even large agricultural landholdings. Moreover, once criminals have a stranglehold on the illegal economy they often expand to dominate the legal economy. This allows criminals to invest both time and money in strengthening network bonds through repeated demonstrations of local bonafides. For example, Arun Yadav routinely contributes to local temples, often paying for renovations. By re-investing in the community over time, criminals can ensure the continued compliance of local populations and strengthen political networks. Voters know who has repeatedly shown up at communal events and been available in their own time of need.

Given the difficulty in observing all forms of constituency service, to induce variation in constituency service delivery, I leverage a plausible exogenous shock to the number of weddings that co-occur with campaigns. By combining the MLA candidates dataset with original data on campaign timings in relation to Chaturmas, I am able to compare electoral outcomes for criminal and clean candidates in elections that fall during the Chaturmas treatment relative to the wedding season control. Leveraging this identification strategy, I find that criminal politicians are 11 percent less likely to be elected during Chaturmas when far fewer weddings are held. However, this decrease is only statistically significantly different from the clean candidates trend after controlling for candidate attributes. Even with the rich candidate dataset I do not observe actual candidate behavior regarding campaign investments or wedding attendance. Given these data limitations and with treatment assigned at the state level leading to a small sample of 74 state elections, I take these estimates as suggestive for now.

Appendix

4.A Balance Checks

Figure 4.A.1: Balance Check



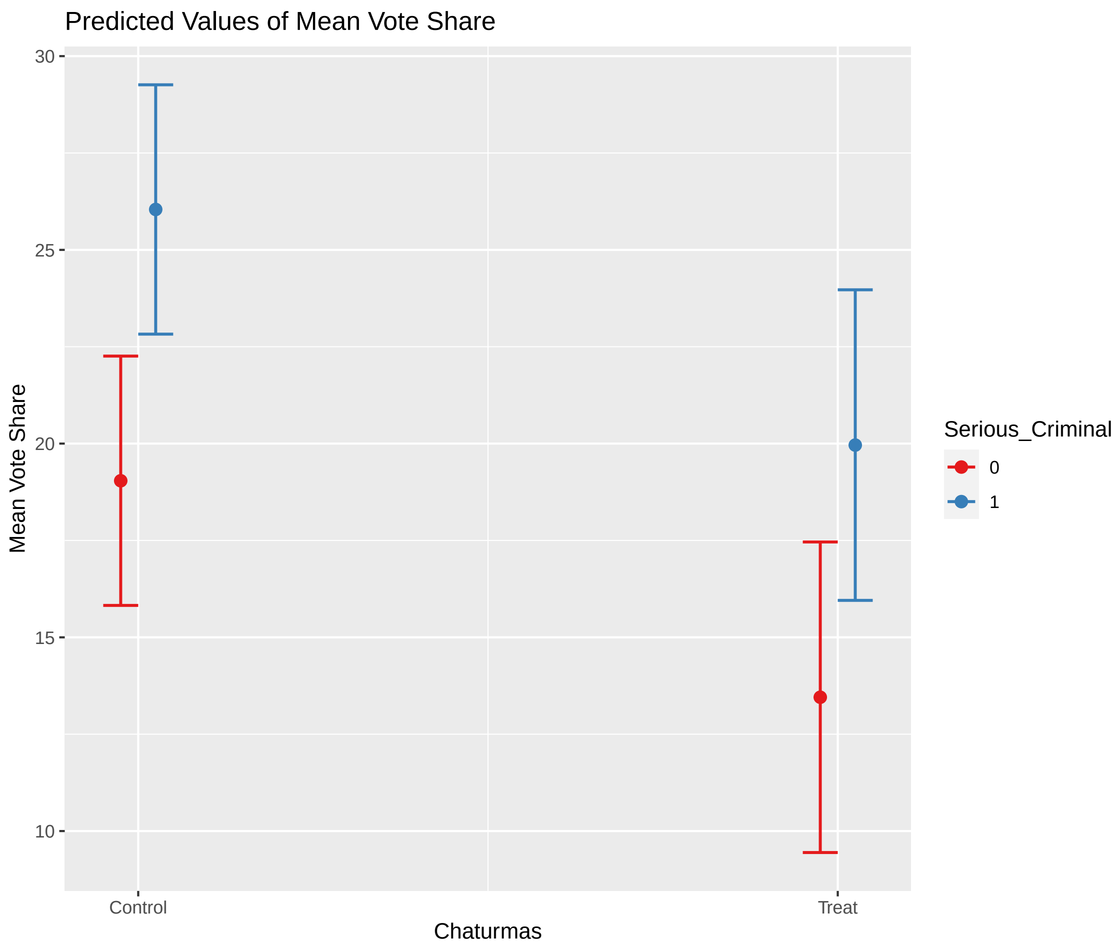
Plot of coefficient estimates and 95% confidence intervals from OLS models of treatment regressed on candidate and constituency predictors. Standard errors are clustered at state-election level (i.e. the level of treatment).

4.B Aggregate State Level Analysis

Table 4.B.1: State Election Regression Results

	Dependent Variable:
	State Election Mean Vote Share by Criminality
Serious Criminal	7.00^{***}
	(2.32)
Chaturmas	-5.59^{**}
	(2.62)
Serious Criminal x Chaturmas	-0.49
	(3.71)
Constant	19.04^{***}
	(1.64)
Observations	148
R^2	0.14
Adjusted R^2	0.13
Residual Std. Error	11.01 (df = 144)
F Statistic	8.02^{***} (df = 3; 144)
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

The dependent variable is the average vote share in the state election for serious criminals and clean candidates. Serious criminals are more likely to win elections and see a 6 percentage point decrease in vote share during Chaturmas. However this difference is not significantly distinguishable from clean candidate trends at conventional levels.



The dependent variable is the average vote share in the state election for serious criminals and clean candidates. Blue point estimates and 95% confidence intervals indicate serious criminals. Red point estimates and confidence intervals indicate clean candidates. Serious criminals are more likely to win elections and see a 6 percentage point decrease in vote share during Chaturmas. However this difference is not significantly distinguishable from clean candidate trends at conventional levels.

4.C Post Treatment Bias

These models reproduce the primary results in Table 1 after removing controls that occur after the campaign begins and could conceivably be affected by the Chaturmas treatment.

Table 4.C.1: OLS- Serious Criminal

	Vote Share	Winner
Serious Criminal	5.51^{***}	0.07^{***}
	(0.97)	(0.01)
Chaturmas	-2.07	-0.02
	(1.46)	(0.02)
Serious Criminal x Chaturmas	-2.47	-0.03
	(1.26)	(0.02)

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Chapter 5

Conclusion

5.1 Overview

In this dissertation I investigate the conditions under which criminal politicians distribute resources to their constituents. In turn, I ask whether superior state resource distribution or constituency service can explain criminals' enduring electoral success? In other words, what do criminal politicians deliver to voters? If anything? To gain traction on these questions, I focus on one of India's key anti-poverty initiatives and the worlds' largest public works program. Access to NREGS is enshrined in a fundamental human right to work. However, demand for the program exceeds supply. As I write, NREGS has been pushed to the brink as employment demand soars during the coronavirus pandemic. Voters across India turn to politicians for help in interfacing with the bureaucracy to access these benefits.

In addition to NREGS importance in the aggregate, there are theoretical reasons why I focus on the employment program. As described throughout the preceding chapters, NREGS benefits are highly desirable, politically manipulable and precisely measurable. NREGS represents a large flow of government funds reaching a wide variety of voters. Inherently, the size and availability of program benefits catch the eye of voters and politicians alike. At the same time, previous research documents how politicians can intervene to redirect benefits towards their own political goals (Gulzar and Pasquale, 2017; Dasgupta, 2016; Marcesse, 2018). Finally, NREGS provides fine grained, administrative geo-data across India. Put simply NREGS serves as a "most likely scenario," for an empirical test of whether or not criminal politicians intervene to redistribute resources towards their supporters.

In this dissertation, I present two key arguments. First, I contend that criminal politicians core assets of money, muscle and networks make them particularly suited to both deliver more state benefits and redirect fund flows towards co-partisans. Importantly, NREGS allows for a critical test of the theory that criminal politicians are community warriors, protecting and providing for their "in-group." Previous quantitative treatments of criminal politicians have focused solely on aggregate constituency outcomes. Leveraging NREGS geo-data I directly test if criminal politicians engage in differential targeting, within constituencies.

Second, I identify a trade-off that candidates face between accruing enough capital to fund campaigns and remaining rooted in the constituency to provide personalized service to voters. I argue that criminals' muscle-power allows them to sidestep this tradeoff and optimize on both dimensions. Muscle enables criminals to establish lucrative protection rackets in their home constituencies. In effect, protection rackets turn muscle into money. To protect this money, criminals invest in networks. Constituent service networks help criminal politicians maintain political power, which is useful for protecting their illegal enterprises.

To understand if criminal politicians deliver social welfare benefits and constituency service, I combine granular and exhaustive data on NREGS outcomes, polling station results and candidate affidavits. I use causal inference and machine learning designs to analyze this data and strengthen the validity of my estimates. While some of my findings conflict, two primary patterns emerge from the empirical analyses.

First, I provide new evidence that criminal politicians are associated with increased targeting of state resources to co-partisans. This hews closely to criminals' theoretical advantages stemming from their deep communal roots. By remaining embedded in the constituency, I argue that criminals are better positioned to identify, and then meet, supporters needs. Second, and perhaps unsurprisingly, I find criminals' core advantages in providing for constituents derives from their capacity for violence. Both qualitative and quantitative evidence speak to criminal muscle as a necessary input for both improved constituency service and benefit delivery. Quantitatively, I find that criminals with violent charges are associated with increased NREGS delivery. Whereas, non-violent criminals are not. In addition, drawing on in-depth fieldwork, I highlight the role of muscle in solving the constituency-service vs. wealth-generation trade-off.

5.2 Summary of Findings

In Chapter 2, I leverage state of the art causal inference techniques to estimate the impact of criminality on NREGS delivery. Relative to clean politicians, criminals underperform in the provision of NREGS, completing 475 fewer projects during their tenure. However, results remain inconclusive regarding whether criminals deliver NREGS wages and employment. Therefore, I can not rule out the possibility that criminal politicians may perform better on the primary dimensions that voters care about. Still, the cleanest causal estimate identifies a negative effect of criminal politicians on NREGS project delivery.

In this chapter, I construct a novel, qualitatively informed measure of corruption. Leveraging rich NREGS project data, I identify projects that are more susceptible to corruption based on how easy graft is to hide from engineering audits. I do not find that constituencies governed by criminal politicians are more likely to engage in graft-prone projects. This rules out a primary alternative explanation for the continued success of criminal politicians. Namely, that criminals deliver rents to lower level politicians in exchange for votes.

Lacking a smoking gun from the regression discontinuity analysis, in Chapter 3, I test whether criminal politicians are more efficient at targeting NREGS to co-partisans. I argue that if money and muscle help criminals deliver NREGS benefits, networks help them identify supporters and then meet their needs (e.g. providing access to NREGS job cards). Combining NREGS project geo-data with an original geo-dataset on polling stations results, I map NREGS benefits directly to micro pockets of political support. In this chapter, I find that criminal politicians both deliver NREGS benefits overall and demonstrate superior targeting relative to clean politicians. Secondly, I conduct a mechanisms investigation to understand if money, muscle or networks are responsible for superior NREGS delivery. I find the most support for the muscle mechanism. Criminal politicians facing violent charges deliver at higher rates than non-violent criminals.

To increase the validity of these results, I apply a machine learning approach and replicate the analysis on unseen holdout data. In the testing replication, I find that criminals still deliver more NREGS wages overall. Though, the effect of criminality attenuates by about half relative to the training data estimates.

How then do we think about reconciling the divergent NREGS delivery findings between the regression discontinuity and micro-targeting analyses? To investigate one potential path to unify these results, I disaggregate the micro-targeting analysis into competitive and safe constituencies. I find that superior NREGS delivery is primarily driven by criminals in safe seats. Recall that the regression discontinuity only compares criminal and clean candidates in competitive, coin-flip races. Therefore, heterogeneous effects based on the level of constituency political competition, provide one way to to reconcile these findings. At the risk of post-hoc theorizing, it could be the case that parties and candidates face stronger incentives to ensure smooth NREGS delivery in very tight races to ensure another seat in the legislature. This additional effort and party support may swamp any baseline advantage for criminal politicians.

However, political competition is not the only factor changing across these two analyses. In the targeting chapter, my sample is constrained to states for which I have polling stations results. Thus the analyses differ on both the degree of competition and geography. Moreover, in the targeting chapter I do not attempt to identify a causal effect. I can not rule out that the positive association between criminality and NREGS provision is the result of biased estimation.

Finally, in Chapter 4 I flip the question around to ask if voters reward criminal politicians for providing superior constituency service. This chapter recognizes that my prior analyses were limited by only analyzing a single distributive program. I discuss this limitation in greater detail in the next section. To partially alleviate my reliance on NREGS, in this chapter, I focus on criminal politicians provision of constituency service. My analysis exploits a plausibly exogenous shock to the opportunity for candidates to invest in constituency service during their campaigns. During the Hindu holiday period of Chaturmas, there are no auspicious wedding days, reducing the supply of a key form of constituency service. Campaigns that coincide with Chaturmas will not provide candidates the same opportunity to show up and pay for weddings. I argue that this exogenous shock should be particularly painful for criminal candidates who can demonstrate superior local bonafides at these events. I find that, relative to clean politicians, criminal politicians suffer a greater electoral penalty when elections are held during Chaturmas. However, this difference is only statistically significant for some models.

5.3 Limitations and Measurement Challenges

One limitation of this dissertation is that I focus on a single social welfare program (NREGS). However, the reality is that India runs a vast welfare state and politicians can optimize over a bevy of resources at their distributive disposal. In Chapter 4, I attempt to address this deficiency by examining the role that criminal politicians play in delivering constituency service that falls outside of NREGS. However, by focusing solely on weddings, I only add one small piece of the constituency service puzzle to my analysis. As Bussell (2019) notes, constituency service is often missing entirely from studies on distributive politics which tend to focus on social assistance programs or government fund flows. During my fieldwork, I found constituency service to be one of the main ways that criminal politicians curry favor with voters. However, constituency service presents difficulties from a measurement and causal identification perspective. I elaborate on these challenges and my attempts to address them in the subsequent section.

5.3.1 Measuring Constituency Service

There are several measurement challenges to identifying the impact of constituency service on electoral outcomes and whether criminal politicians are more likely to engage in these tactics. Bussell (2019) provides the most up to date, India-wide analysis of politicians' constituency service. Still, there is a lack of data on the historical evolution of constituency service, how spending changes over time and what candidates prioritize during campaigns. We know even less about MLA candidates who fail to win, raising questions about survivorship bias.

One important way that politicians can provide constituency service is via small cash transfers to constituents. Cash can solve a variety of small requests on demand. Several studies have utilized candidate affidavit data to investigate the role wealth plays in determining electability (Vaishnav, 2017; Sircar, 2018a). While the candidate affidavits dataset is a rich resource it falls short on actually identifying how wealth matters. First, data on candidates' personal assets are a stock not a flow. This elides how candidates' spending habits change over time and whether this variation drives a response from voters. Second, the assets data says nothing about how candidates alter their cash distribution strategies during campaigns. In other words, the affidavits data only allows comparisons *across* candidates and not *within* candidates.

Ultimately, given the amount of black money funding campaigns, the true level of election spending is likely unobservable (Kapur and Vaishnav, 2018). While campaign spending is limited by law, it is well known (and sometimes documented) that candidates spend above and beyond the paltry ceiling placed on election spending. In my own fieldwork, there were multiple rumors that one candidate was arrested during a campaign when trying to flee authorities with a heavy suitcase full of cash. Second, Bussell (2019) conducts a detailed survey of election spending. Candidates admit to spending more than campaign limits with the amount increasing. Still, despite this expansive and exhaustive data collection effort, Bussell's analysis unfortunately can not identify individual candidate spending. An actual accounting of election spending is perhaps impossible to collect via surveys. For respondents, accurately accounting for their own campaign spending habits could be tantamount to admitting to a felony.

To circumvent these measurement challenges, I exploit a novel natural experiment that induces variation in the supply of weddings. Weddings are a bundled form of constituency service since they consist of candidates donating both time and money to the event. However, like my singular focus on NREGS, constituency service is ultimately far broader than the one form I identify. One potential solution, for future research, would be to combine detailed administrative data on a distributive program (like NREGS) with survey evidence regarding the broader ecosystem of distributive resources available to politicians. For example, in this dissertation, I leverage the granular NREGS data and detailed politicians' affidavits to conduct a mechanism-based investigation of the specific criminal assets driving NREGS performance. While this micro-level work is revealing, my investigative spotlight is firmly fixed on only one program. Therefore, I leave open the possibility that criminals exhibit heterogeneous effects across a basket of distributive goods and constituency services. To not completely elide this limitation, my prior would be that criminals would perform better, on average, across this basket of distributive goods. As I have argued throughout the dissertation, criminals' assets of money, muscle and networks are useful for solving a variety of voter requests.

5.3.2 Measuring the Latent Concept of Criminality

One additional measurement issue requires attention. I use criminal charges to identify "criminal" politicians. While an obvious choice, charges are only a proxy for the underlying, latent concept of criminality. Some candidates are accused of only minor issues and should not be lumped in with the Arun Yadavs and Anant Singhs of the world. On the other end of the spectrum, some powerful dons may escape police scrutiny entirely. This misclassification is not overly detrimental to my analysis- since measurement error should attenuate the effects of criminality. Moreover, the more common occurrence seems to be that criminal politicians are slapped with charges for the crimes they commit but then have the power to delay court dates indefinitely (Vaishnav, 2012). Instead, these data problems speak to the larger issue of trying to capture latent criminality. Really what I am interested in measuring is the capacity for violence and, to some extent, candidates' ability to engage in extortion rings and protection rackets. Raw criminal power is a difficult concept to capture empirically.

Consider a more pernicious case that fell out of my fieldwork in Bihar. The politician Vijay Krishna exhibits all the characteristics of a canonical criminal MLA. He is deeply embedded in local villages and viewed as a strong and capable leader who provides for his constituents. In fact, Vijay Krishna is even serving time in jail for a serious criminal charge. However, the voters I interviewed were adamant that Vijay Krishna, while a capable leader, was not a strongman in the same sense as Arun Yadav. Vijay incurred his charge while covering up a murder that his son committed. While Vijay was present at the time of the murder, everyone was adamant that he had no role in the actual killing. Instead, in an effort to save his son, he covered up the murder by having the body dumped in the Ganges. While not a model citizen, Vijay does not exude the qualities of criminal politicians that I have described previously.

This is not an easy measurement issue to solve. No matter how I recode charges, Vijay Krishna would look every bit as much like the murderous Arun Yadav. While this may just be an edge case, it speaks to larger issues within both the affidavit data and the conceptualization of criminal politicians. One potential solution to better capture the latent concept of criminality would be a deeper dive and data collection effort on the sources of politicians' wealth, their connections to local firms and identification of protection rackets. This would enable me to identify if there is systematic differences between charged and uncharged politicians in how they fund political efforts and engage in constituency service. Local reporters are very knowledgeable about who the power players are in their area. Reporters' local knowledge could be leveraged to construct a measure of criminality that more closely aligns with the latent concept.

5.4 Implications for Future Research

One common recommendation for reducing political interference in state resource distribution is to professionalize the bureaucracy and push for programmatic distribution (Stokes et al., 2013). When applied to criminal politics in India, on its face, this strategy may seem like a useful lever for rooting out criminal politicians. If citizens can depend on the impartial bureaucracy for state resources, instead of politicians, then criminals' electoral appeal would disappear. However, one implication of my research is that simply improving the programmatic delivery of social welfare programs seems unlikely to curtail criminal politicians' distributive power. Insulating and professionalizing the bureaucracy may reduce criminals appeal to a degree. However, in Chapter 4 I argue that criminal politicians are perfectly capable of delivering constituency service that falls outside of formal state programs. In fact it may be a more fundamental break down in state capacity that allows criminal politicians to accrue political power via protection rackets in the first place. In turn, I argue that criminals leverage this power to make themselves economically and politically indispensable to a plurality of the constituency. Alternative options suggested by my research would be to either root out protection rackets, or at least, address how state owned monopolies can attract these rackets with the added consequence that criminals may eventually entangle themselves in the state directly.

Regarding the broader distributive politics literature, my research suggests continued efforts on identifying variation over time in how politicians provide constituency service, especially prior to taking office. This could be an important ground clearing data exercise in order to better understand how challengers successfully defeat incumbents without access to state resources. In this dissertation I have argued that politicians have different inherent capabilities for resource distribution. I add to the pool of important characteristics that can shape politicians distributive strategy. In particular, I argue for understanding how the *source* of politicians' wealth can influence incentives and capabilities for engaging in constituency service. Identifying how muscle-power is systematically transformed into wealth generation and redistribution is a criminally underrated avenue for future research.

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