Do criminally charged politicians deliver? Evidence from India's National Rural Employment Guarantee Scheme.

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Abstract

Despite intense political competition, candidates facing criminal charges are routinely elected across India at higher rates than clean candidates. A standard explanation for criminal candidates electability is that they "get things done." Once elected, how do "criminal" politicians perform in office? To test if criminally accused politicians harm (or improve) benefit delivery at the local level, I construct a novel dataset detailing the criminal histories, wealth and electoral results of all state legislative candidates in India from 2003-2016. I combine the candidate dataset with original data on the geo-locations of over 20 million public works projects from India's largest anti-poverty scheme, the National Rural Employment Guarantee (NREGS). Using a regression discontinuity design, I estimate the causal impact of electing a criminally accused politician on the distribution of NREGS projects, pay and employment. I find suggestive evidence that constituencies electing a criminally accused politician complete fewer NREGS projects, with estimates ranging from a 34% to 40% reduction. I do not find evidence that criminally governed constituencies engage in more NREGS corruption. Overall, these findings undercut a key explanation for criminal politicians continued success.

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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 (Association for Democratic Reform 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 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 and 2011b, Vaishnav 2017, authors' fieldwork).

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

[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 (Winters and Weitz-Shapiro 2013, Boas et al. 2018).

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.

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. 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 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. 2014). 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.[3] 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

[3] see Appendix B for sensitivity Analysis.

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 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 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)[4] and lower overall economic activity (Prakash et al. 2016). Criminally charged politicians may also be less interested 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 2018).[5]

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,[6] 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

[4]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.

[5]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).

[6]For evidence see Kruks-Wisner 2015, Witsoe 2012, and Berenschot 2011

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.

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, therefore, be endogenous to benefit delivery.[7] 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

[7] 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.

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

$$\begin{aligned} &NREGS_{i,s,t} = \alpha_s + \beta_t + \tau Criminal_{i,s,t} + f(CMV_{i,s,t}) + e_{i,s,t} \\ &\forall CMV_{i,s,t} \in (0-h, 0+h) \end{aligned} \tag{1}$$

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.

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.[8] 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.[9] 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 candidates).[10] Overall, the full RD sample considers 31% of the elections in the entire dataset.[11] 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 1 presents the number of mixed elections that fall within a given bandwidth of competition.[12]

[8]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.

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

[10]347 elections are dropped because I was unable to match the affidavit for either the winner or runner-up, or the election was uncontested.

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

[12]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).

Table 1: Mixed Election Observations for Varying Bandwidths

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

3.2 NREGS Background and Data

Employment guarantees have a long history in India.[13] 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 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 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 → Block → District → State) and funds flow back down (State → District → Block → 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

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

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,”[14] 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 et al. 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).[15] Mounting empirical evidence suggests NREGS is hardly programmatic in its application but instead serves political ends (Dasgupta 2106, Gulzar and Pasquale 2017).[16]

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). 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 1986). 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

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

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

[16]For constituency level variation in NREGS pay in Bihar see Figure 17 in Appendix E.

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.

3.2.1 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 22 in appendix 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 asset 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.
- *Assets*: The sum total of NREGS projects completed during the MLAs 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.[17] 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 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 assets. Thus, geotagging acts as a partial post completion audit on asset 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.

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

[17] I am in the process of matching census blocks to blocks in my dataset, which will allow me to more test for balance across population and constituency characteristics related to NREGS demand. For a full list of controls from the census data see Table 8 in Appendix A

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 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.[18] 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.

3.2.2 Defining Criminality

I use affidavit data I scrapped from the Association for Democratic Reform[19] to code criminality after matching affidavits to election outcomes by candidates names.[20] 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

[18]To alleviate this concern I plan to re-run the analysis with the more complete village level data on wages and employment.

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

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

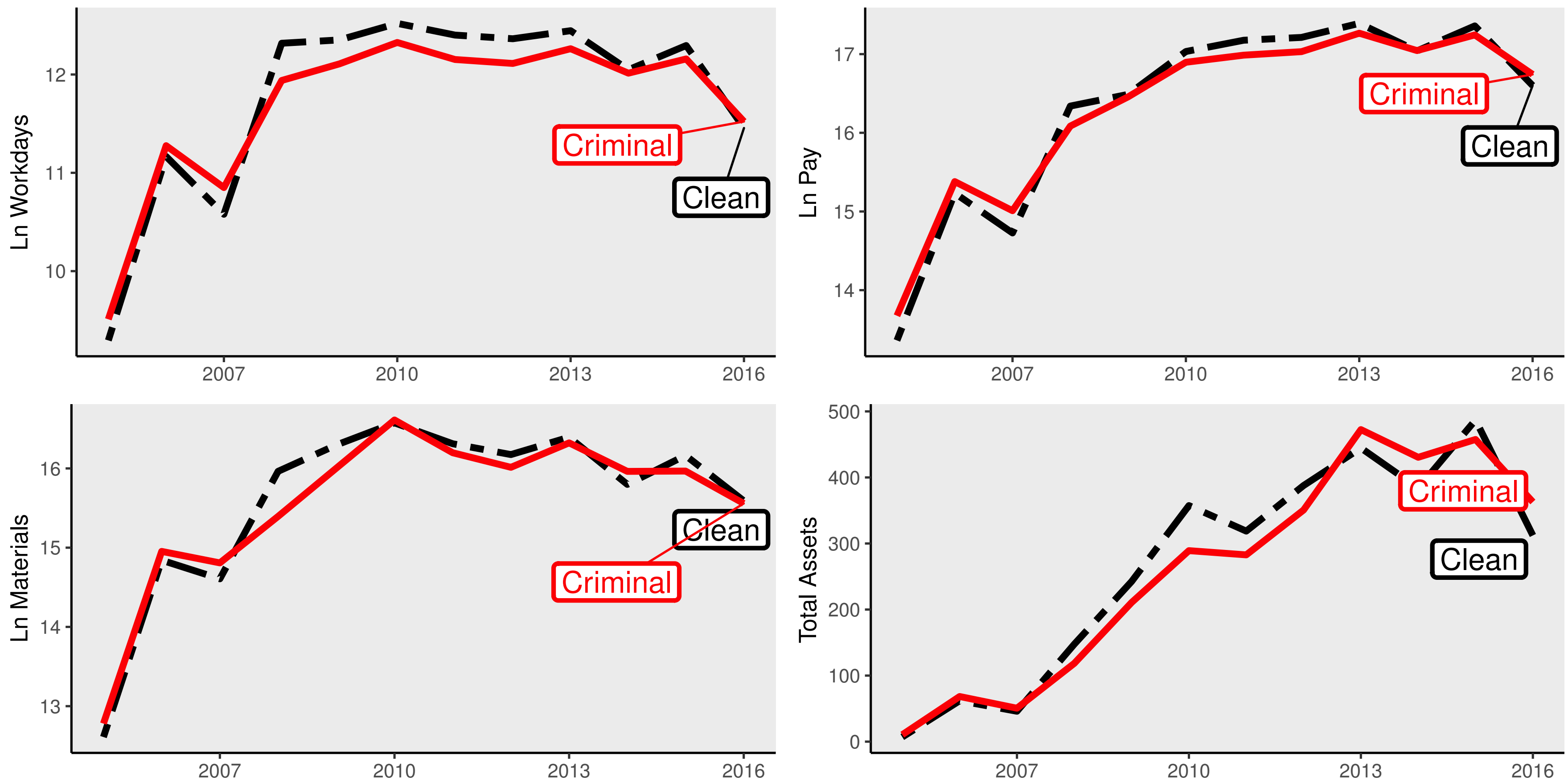
violence such as murder, attempted murder, rape, or committing grievous physical harm.[21]

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 "criminal" politicians. I follow the coding rules set forth by the Association for Democratic Reform (2014b) 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.

[21] 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 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.

Despite NREGS's ostensibly universal guarantee, the program is known for wide variation in performance and implementation (Sukhtankar 2016).[22] Figure 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 *Total Assets*, 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.

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

[22]For comparisons of state-level variation in NREGS outcomes between criminal and clean constituencies see Appendix 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 et al. (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.[23] 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.[24] 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.[25] In short, it appears unlikely that criminal candidates can systematically sort themselves

[23]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.

[24]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

[25]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,

into the category of bare winners.

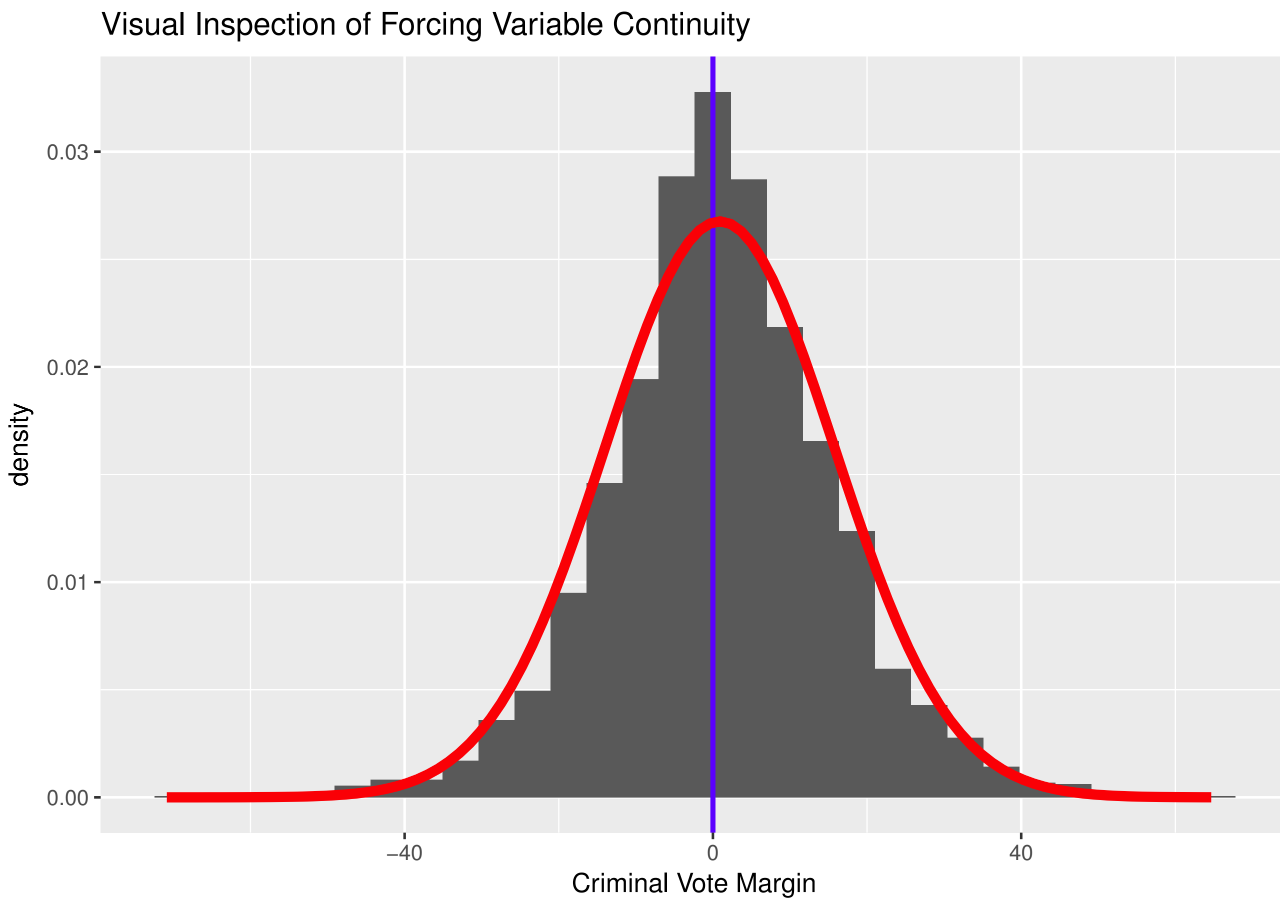
Beyond these theoretical considerations, I directly test for candidate sorting by inspecting the density of the forcing variable (*Criminal Vote Margin*, see figure 2). If criminal politicians are indeed sorting into the bare winning column this should create a noticeable discontinuity at the cut-point. In other words, there will be more criminal candidates just to the right of the 0 threshold than just to the left. Figure 5 provides a visual check by plotting the density of *Criminal Vote Margin*. It is not indicative of criminal candidate sorting at the threshold.

More formally, I conduct a McCrary test for sorting at the threshold (see figure 3). The test is inconsistent with the hypothesis that criminal candidates are more likely to win in close elections (p-value = 0.58).[26]

electoral procedures, counting and result announcement." (Mahmood and Ganguli 2017)

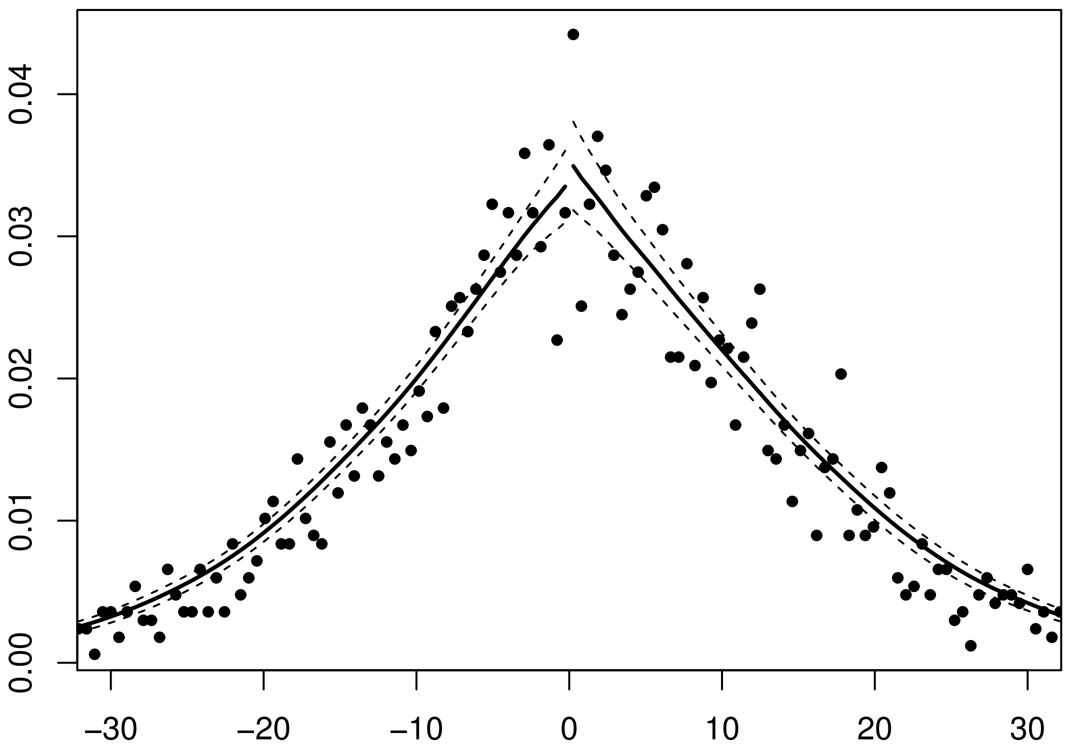
[26]Eggers et al. 2015 and Prakash et al. 2016 also fail to find evidence of MLA sorting in India.

Figure 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.

Figure 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.

4.1 Balance Tests and Controls

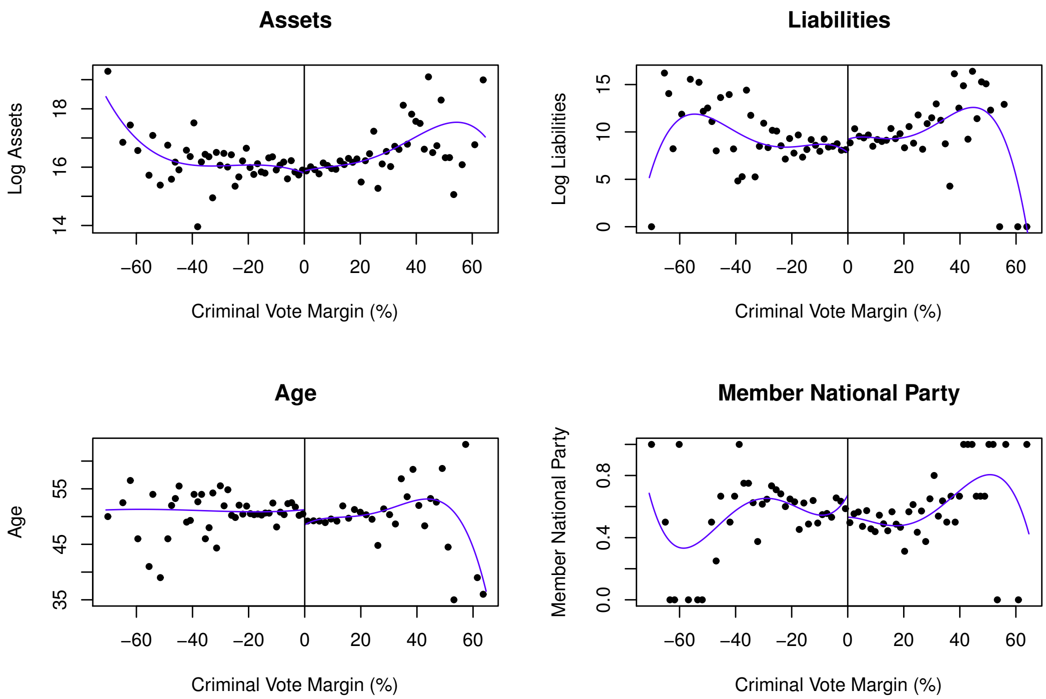
A second implication of the regression discontinuity design is that if treatment assignment is quasi-random at the threshold, then treatment and control groups should be balanced on observable and unobservable characteristics. Treated constituencies that elect a bare criminal winner should look similar 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.[27] Overall, treatment and control units seem relatively well balanced across both constituency and candidate characteristics (see Figures 4 & 5 and Tables 2 & 3). However, bare criminal winners are less likely to be a member of a National Party.[28] 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 C.1 Table 16 and Figure 13).[29]

[27]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 US, Brazil and India see Ferreira and Gyourko 2014, Brollo and Troiano 2016 and Brown 2017, respectively; for RD comparisons of candidates' criminality in India see Chemin 2012, Prakash et al. 2015 and Nanda and Pareek 2016) and that this does not mean that treatment and control units will be unbalanced on other covariates.

[28]While this could arise due to chance, I will use the remaining variables listed in Appendix A Table 8 to adjudicate if there is indeed evidence of imbalance between treated and control candidates.

[29]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 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 local, 4th order polynomials on either side of the cutpoint. Bandwidths are estimated using a mean squared error optimal bandwidth selector (Calonico et al 2015)

Table 2: Balance across Candidate Characteristics

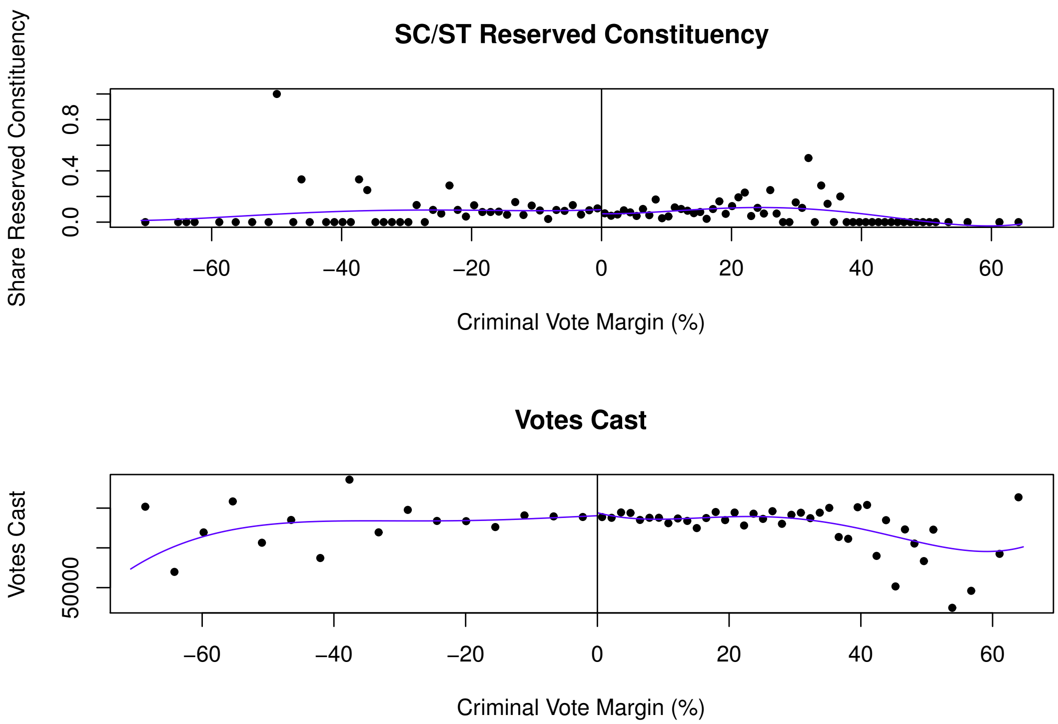
	Log Assets	Log Liabilites	Age	Member Nat. Party
Accused	0.08	1.37	-1.49	-0.12*
	(0.16)	(0.7)	(1.0)	(0.05)
Obs.	3047	3052	3049	3052
BW est.	10.32	10.56	9.8	10.16

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 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 local, 4th order polynomials on either side of the cutpoint. using a mean squared error optimal bandwidth selector (Calonico et al 2015)

Table 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).

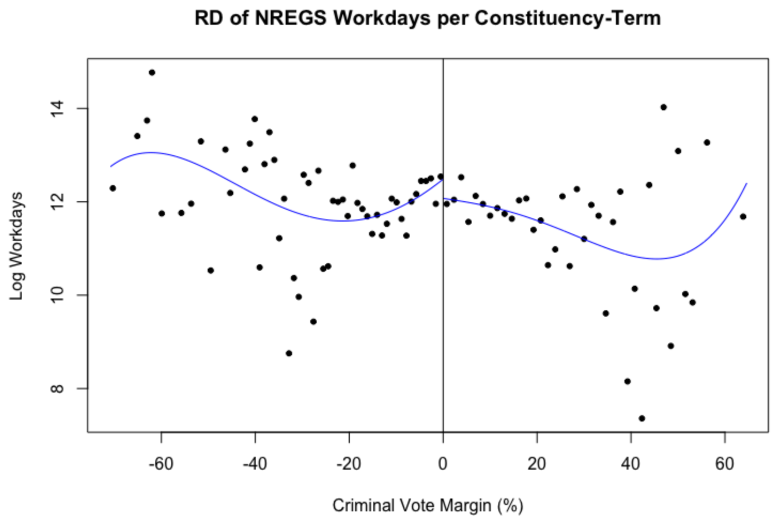
5 Results

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, Assets*). The outcomes are logged transformed, with sample means grouped in evenly spaced bins.[30] 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.[31] 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 6, the blue regression lines are estimated separately for treatment and control units (accused and unaccused) using a global, fourth order polynomial. 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 assets 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 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.

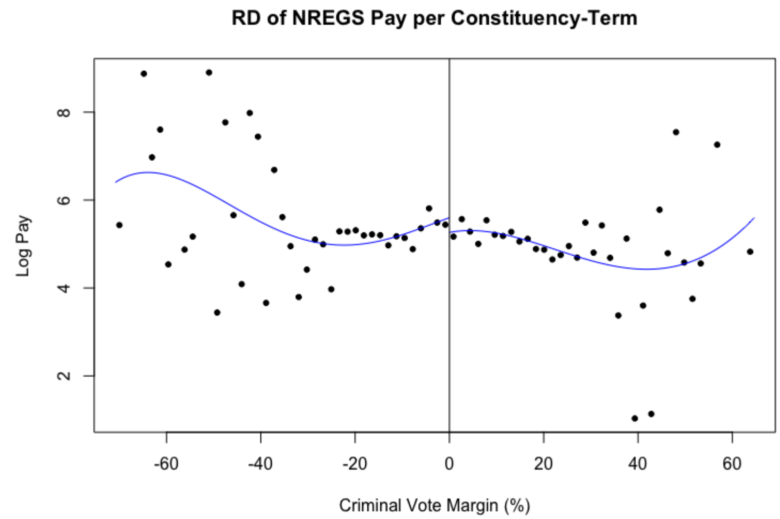
[30]The number of bins is determined separately for treatment and control candidates by a data-driven approach introduced in Calonico, Cattaneo, Titiunik 2015. Specifically, I use the "mimicking variance evenly-spaced method [with] spacings estimators." to select the number of bins

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

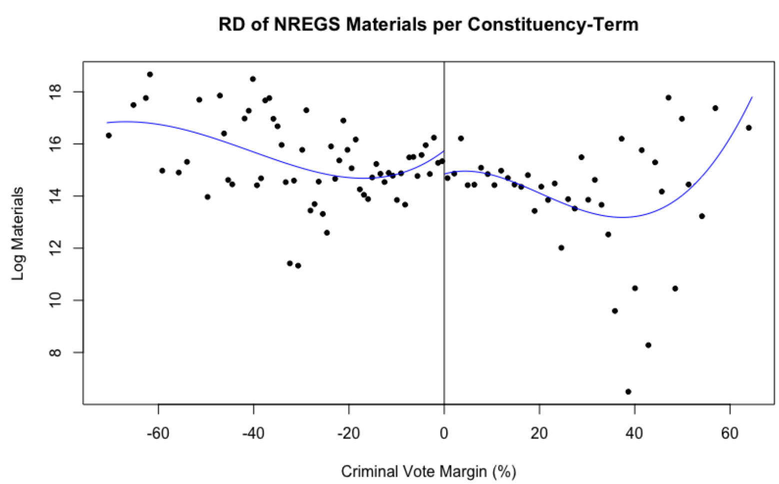
Figure 6: Main RD Plots - Baseline Specification



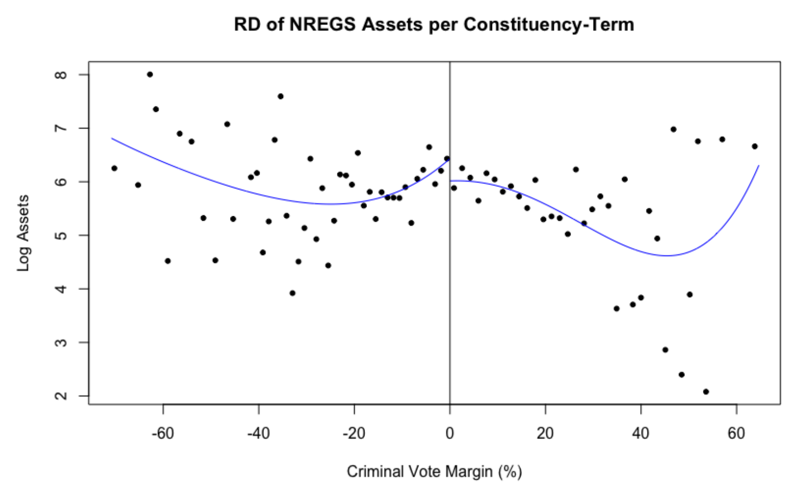
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Assets

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 4th order, global polynomial regression on either side of the cutpoint. All outcomes are transformed by ln(outcome +1).

Table 4: Baseline RD Estimates of Criminal Accusations on NREGS Outcomes

	Log Workdays	**Log Pay**	**Log Materials**	**Log Assets**
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 Bias Corr.	1144	1165	1157	1133
Eff. Num. obs. Right Bias Corr.	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. 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 4 displays point estimates and standard errors for the four logged NREGS outcomes. Whereas Figure 6 employed global polynomials here the discontinuity is estimated using local linear regressions with a triangular kernel in a window around the threshold.[32] 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

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

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 assets 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).[33] It is encouraging however that the point estimates do not change dramatically despite the bias correction and alternative bandwidth selection. Moreover, the *Assets* 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

[33] Convential 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 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)

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

	Log Workdays			**Log Pay**			**Log Materials**			**Log Assets**		
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 Bias Corr. (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 Bias Corr. (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

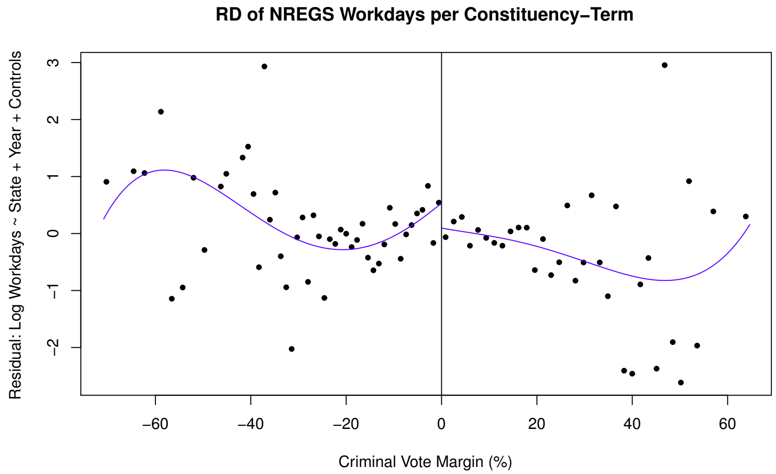
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

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 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.

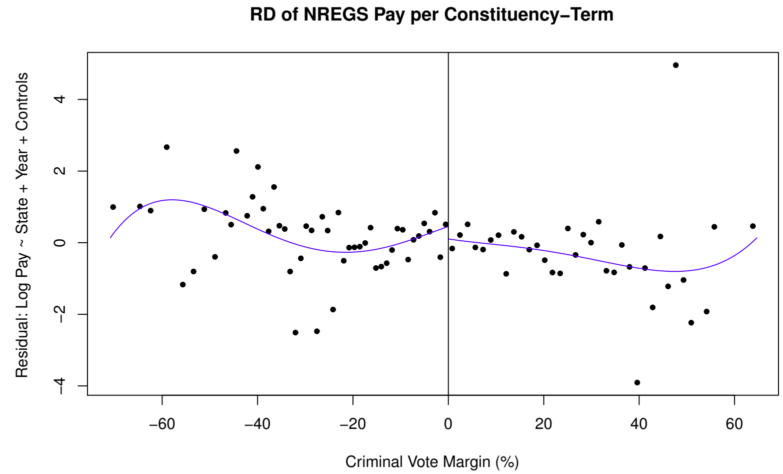
Table 5 makes this comparison explicit by successively adding controls and fixed effects for State and Election-Year to each outcome. The first column for each outcome is the baseline (same model as in table 4). The second column adds in controls (for now, just the number of votes cast per constituency 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 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 covariates in the appendix.

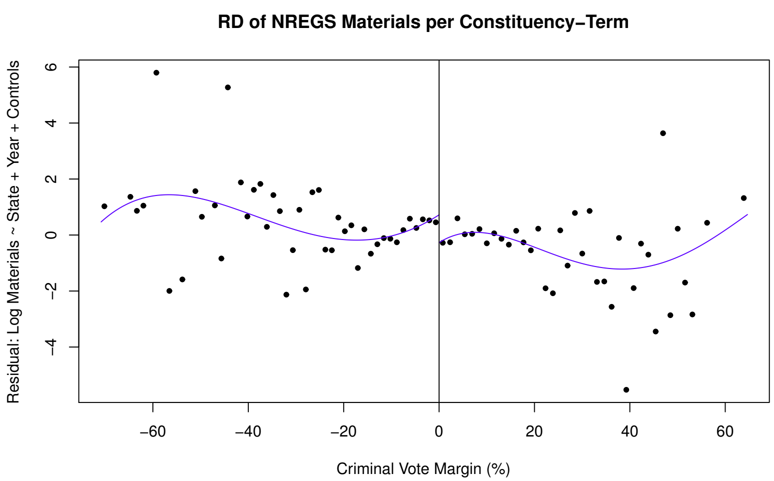
Figure 7: Main RD Plots - Controls and Fixed Effects



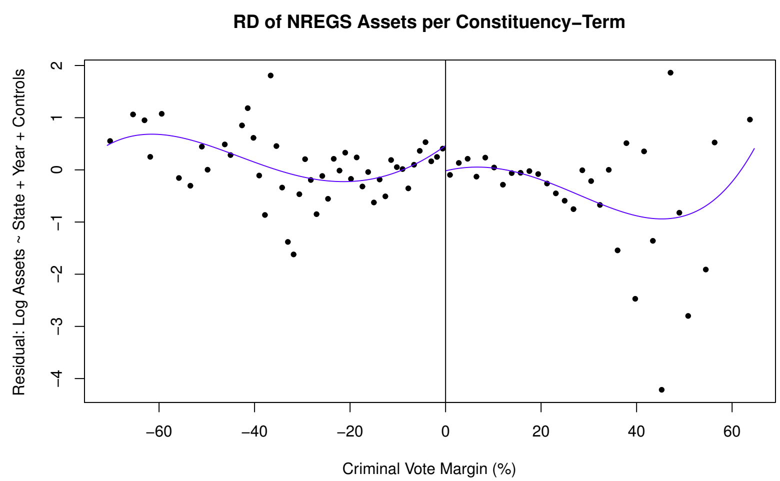
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Assets

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 4th order, global polynomial regression on either side of the cutpoint.

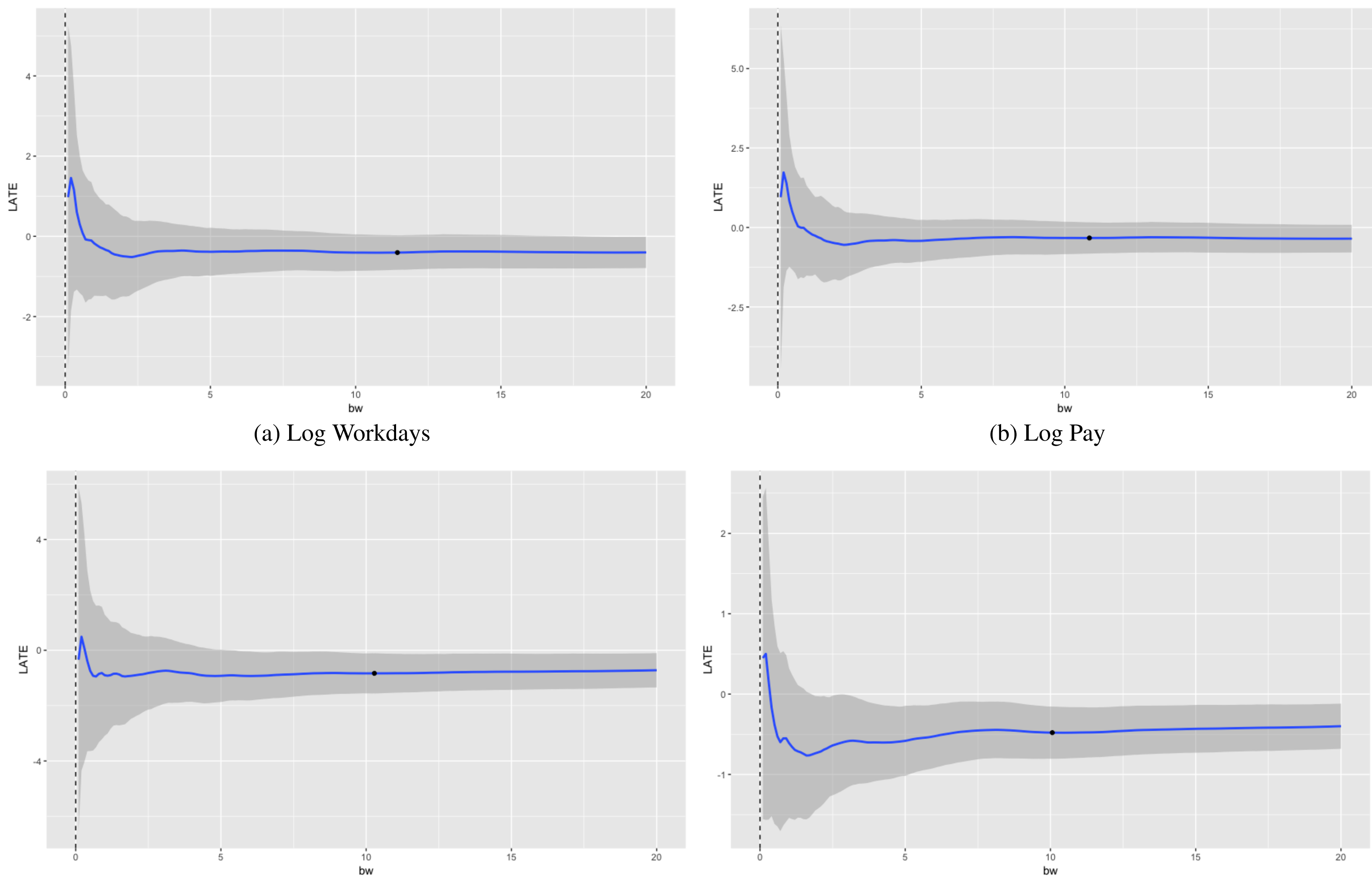
5.1 Sensitivity Analysis

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.[34] Figure 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 assets (8(d)) remains significant across bandwidth choices too.

Secondly, the RD literature recommends several different bandwidth selection methods. Table 9 in appendix B.1 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).

[34]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 8: Sensitivity Analysis - LATE for Varying Bandwidths



(a) Log Workdays

(b) Log Pay

(c) Log Materials

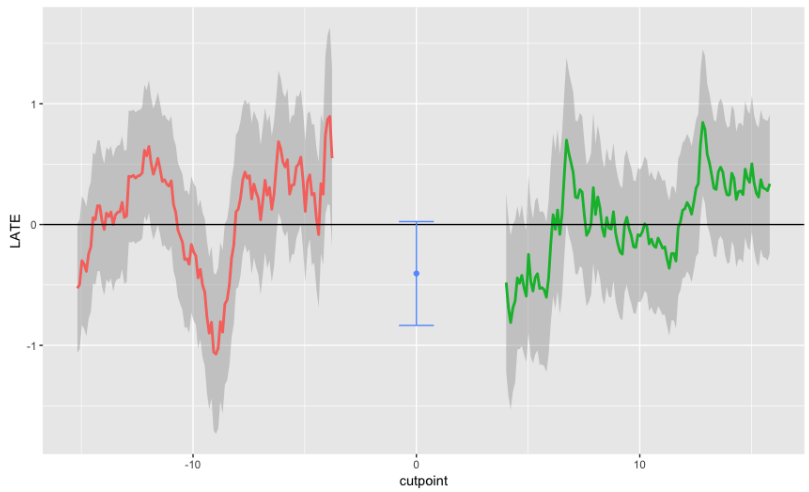
(d) Log Assets

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.

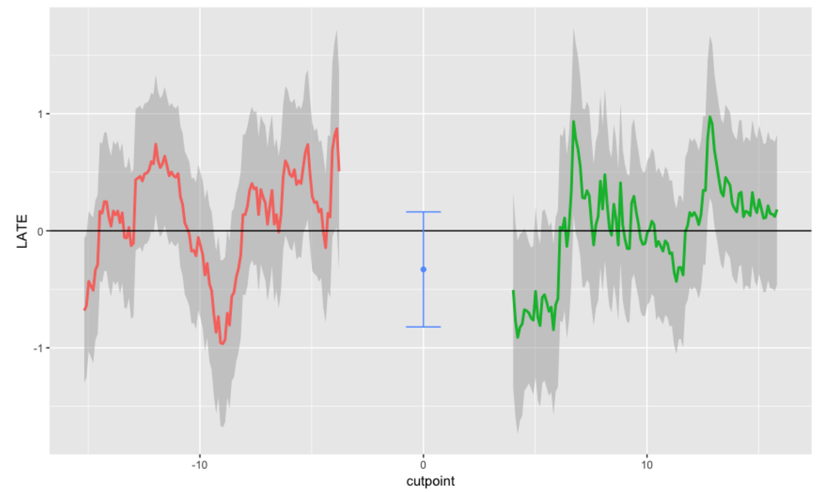
Next, I estimate treatment effects for a variety of functional forms. Gelman and Imbens (2014) recommend the use of local linear or quadratic polynomials instead of controlling higher order polynomials. Their results indicate that higher order polynomials can given large weight to observations far from the cut-point, are highly sensitive to the degree of the polynomial and produce 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 (Appendix B.2, tables 10-13). Encouragingly, the *Assets* outcome remains statistically significant across all polynomial choices though the estimate varies.

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 9). The estimates for the placebo cutpoints are not entirely stable. For example, for the *Assets* 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.

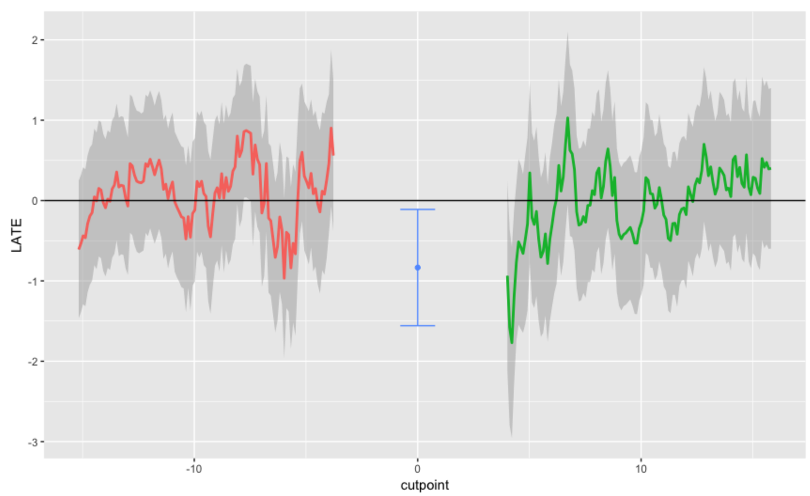
Figure 9: Placebo Tests - LATE for Varying Cutpoints- Baseline with Fixed Effects and RDRobust data driven BWS



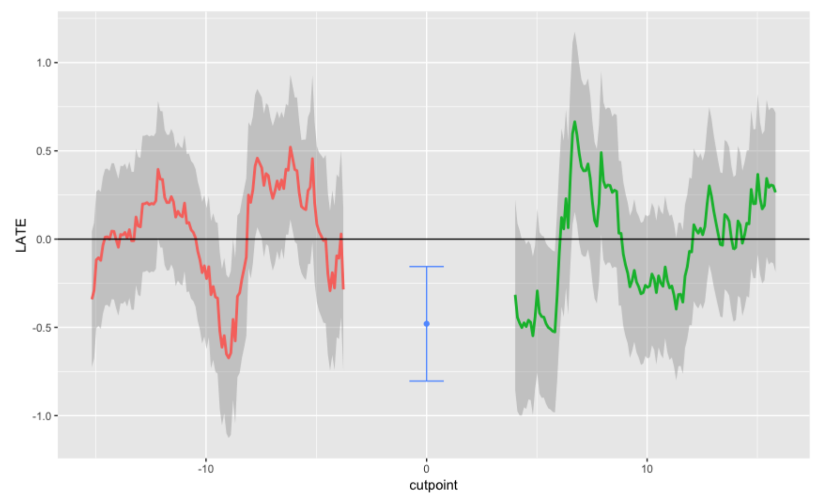
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Assets

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.

5.2 Serious Criminals

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.[35] 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 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 *Assets* remain roughly identical in the fixed effects specification, see Table 6).[36] *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

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

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

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."

Table 6: RD Estimates for Serious Charges

	Log Workdays	**Log Pay**	**Log Materials**	**Log Assets**
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.

5.3 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 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).[37] 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.[38] I estimate the discontinuity in wages per workdays for criminal and clean constituencies in Table 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 7 Column 1). Pay per workday (column 1) is imprecisely estimated and consistent with both large positive

[37]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.

[38]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.

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 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 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.[39] For example,

[39] 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.

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 assets. First, it is easier to hide the amount of corruption in soil based projects compared to more visually verifiable assets 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.[40] I leverage this information to create a typology 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 assets. I use the broad category type as these labels are standardized across all states.[41]

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.

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

[41]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 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.

6 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 assets 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.

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 future work, I investigate whether criminal politicians are more rooted in their community and capable of delivering personalized constituency service beyond access to state resources. Finally, the NREGS data measures only completed projects. Criminal politicians could be less capable of the local bureaucratic oversight needed to ensure project completion. However, when considering only serious charges, I find some evidence that accused MLAs cause a general reduction in NREGS access and benefit provision, beyond project completion.

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).[42] Criminal politicians are often thought of as constituent problem-solvers substituting for a dysfunctional state (Vaishnav 2017). However, in the case of NREGS, at least, accused politicians criminally underperform.

[42] In future work I test the targeting hypothesis using polling station data.

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A Controls

Table 8: 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)
2001 Census	Share of Agricultural Laborers
	Share of Marginal Workers
	Population
	Minority Share
	Education Index
	Medical Index
	Water Index
	Road Index
	Urbanization
	Irrigation Index
Dataset	**Candidate Characteristics**
Affidavits	Wealth (self reported Assets)
	Liabilities (self reported)
	Education
	Age
	Member of National Party
	Member of Congress Party
	Caste
	Incumbent

B Sensitivity Analysis for Models Including All Charges

B.1 Varying Bandwidth Selectors

Table 9: 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.

B.2 Varying Local Polynomial Order

Table 10: 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 11: 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 12: 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 13: Local Polynomials Varying Order - Non Parametric

	Log Assets					
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.

B.3 Varying Global/Parametric Polynomials

Table 14: AIC for Parametric Polynomials (Baseline Spec, NO controls NO FE)

Polynomial Order	Log Workdays	Log Pay	Log Materials	Log Assets
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

C Serious Charges

C.1 Serious Charges Balance Tests

Table 15: 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 10: Candidate characteristics Balance Tests for Serious Charges

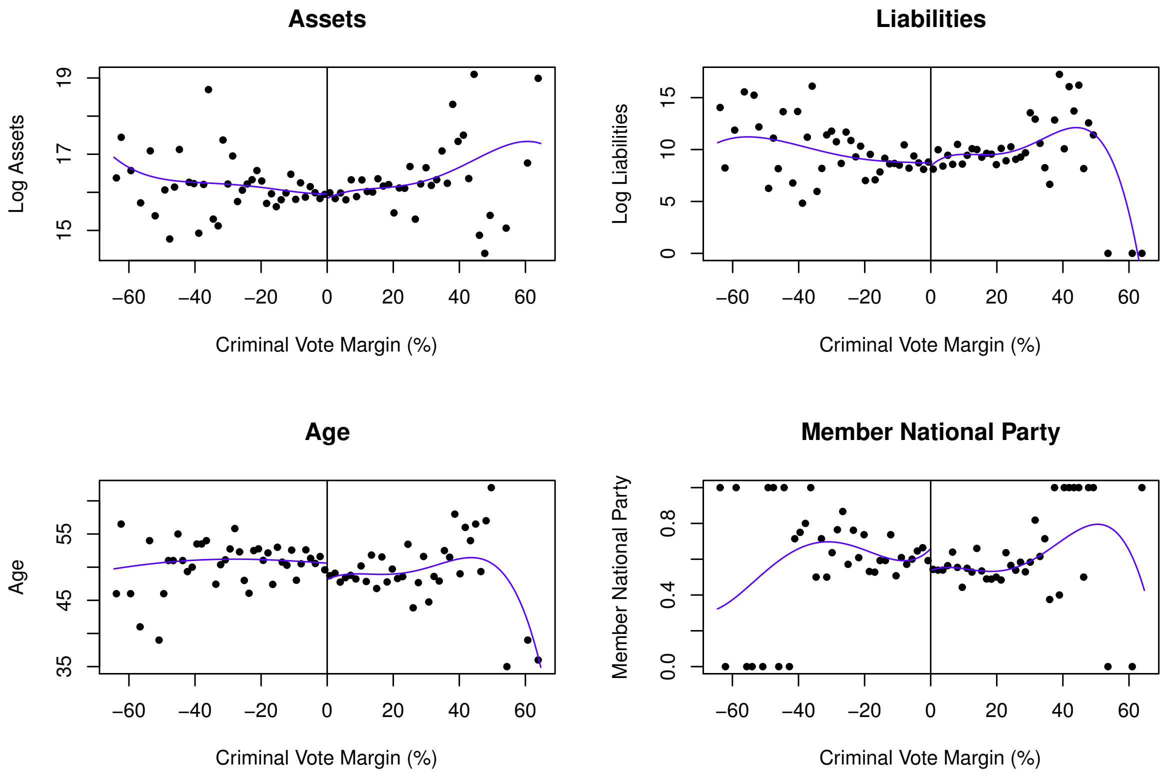


Figure 11: Constituency Balance Tests for Serious Charges

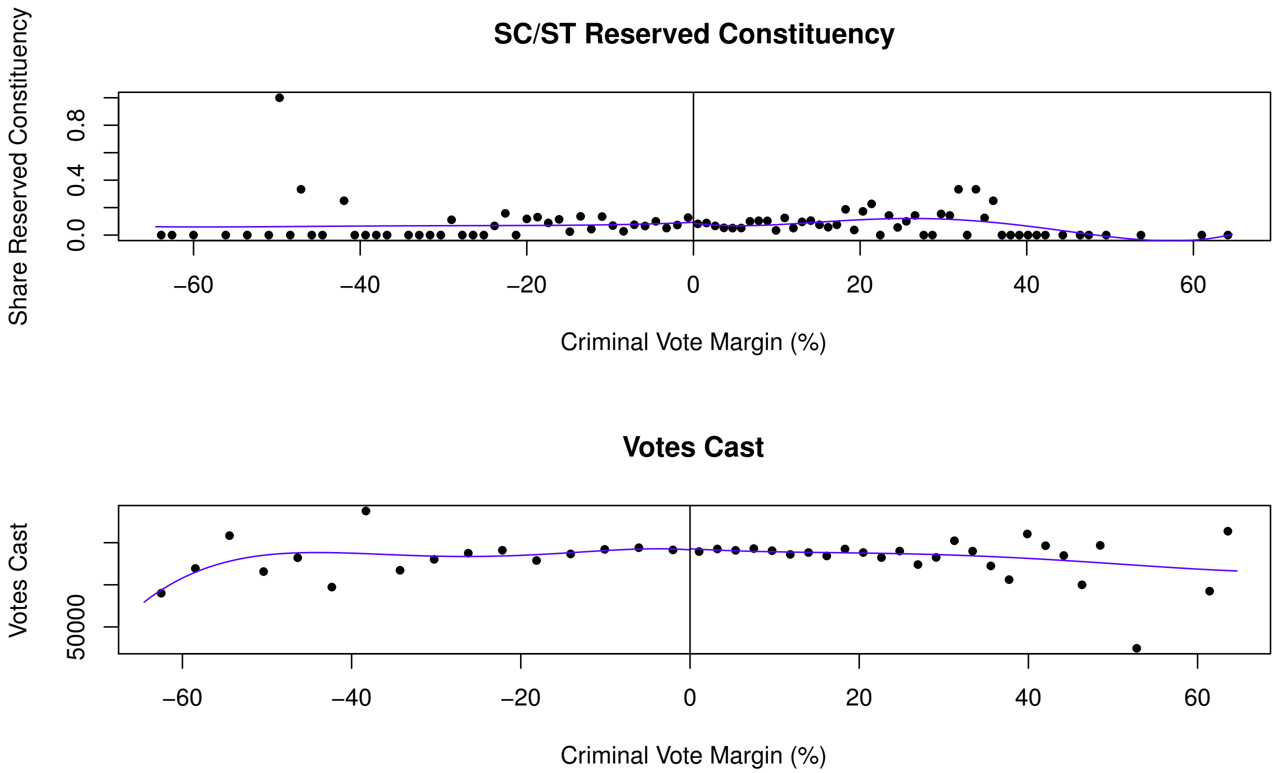


Table 16: Constituency Balance Tests for Serious Charges

	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

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

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

Table 17: Serious Charges- Local Polynomials Varying Order - Non Parametric

	Log Workdays	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	2216.00
Eff. Num. obs. Left	678.00	916.00	915.00	954.00	914.00	948.00
Eff. Num. obs. Right	702.00	1014.00	1012.00	1068.00	1010.00	1052.00
Eff. Num. obs. Left BC	876.00	1012.00	991.00	1012.00	962.00	990.00
Eff. Num. obs. Right BC	967.00	1128.00	1100.00	1130.00	1077.00	1100.00
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	6.00
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 18: 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. Left Bias Corr.	831.00	944.00	992.00	1012.00	958.00	1019.00
Eff. Num. obs. Right Bias Corr.	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 19: Serious Charges- Local Polynomials Varying Order - Non Parametric

	Log Materials	Log Materials	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	2212.00
Eff. Num. obs. Left	642.00	849.00	846.00	967.00	912.00	991.00
Eff. Num. obs. Right	655.00	935.00	928.00	1080.00	1021.00	1108.00
Eff. Num. obs. Left BC	832.00	956.00	925.00	1009.00	956.00	1015.00
Eff. Num. obs. Right BC	906.00	1071.00	1031.00	1133.00	1072.00	1145.00
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	6.00
Order Bias Corr. (q)	2.00	3.00	4.00	5.00	6.00	7.00

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

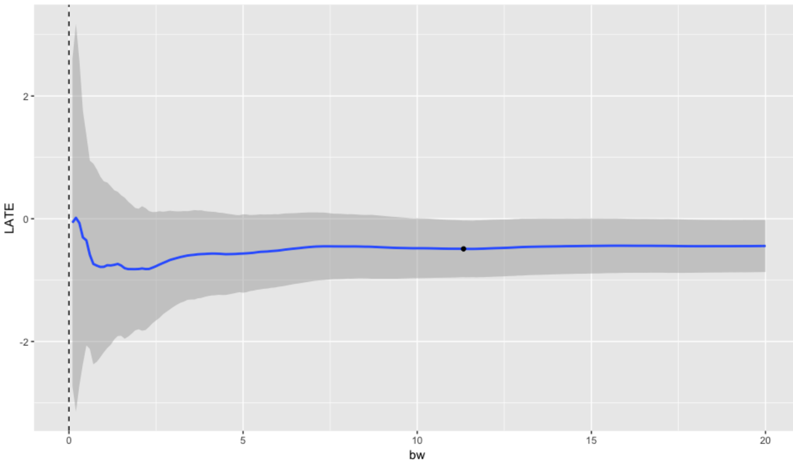
Table 20: Serious Charges- Local Polynomials Varying Order - Non Parametric

	Log Projects	Log Projects	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	2216.00
Eff. Num. obs. Left	596.00	777.00	833.00	920.00	916.00	983.00
Eff. Num. obs. Right	600.00	836.00	905.00	1023.00	1013.00	1093.00
Eff. Num. obs. Left BC	809.00	909.00	933.00	988.00	970.00	1020.00
Eff. Num. obs. Right BC	870.00	1004.00	1035.00	1096.00	1078.00	1146.00
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	6.00
Order Bias Corr. (q)	2.00	3.00	4.00	5.00	6.00	7.00

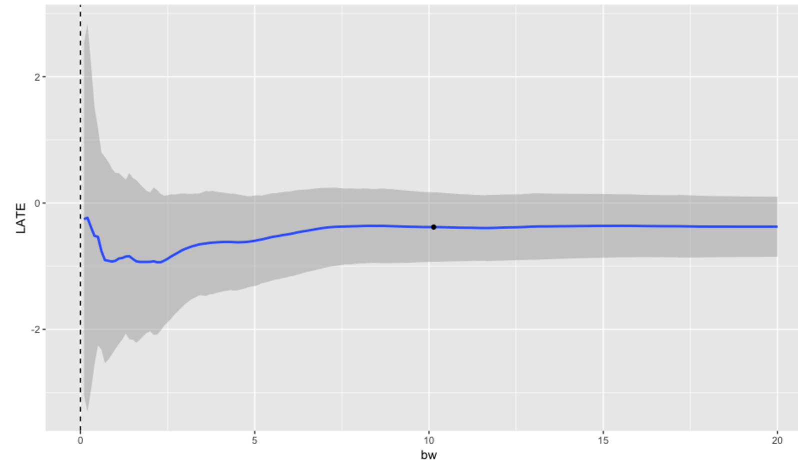
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

C.3 Serious Charges Bandwidth Sensitivity

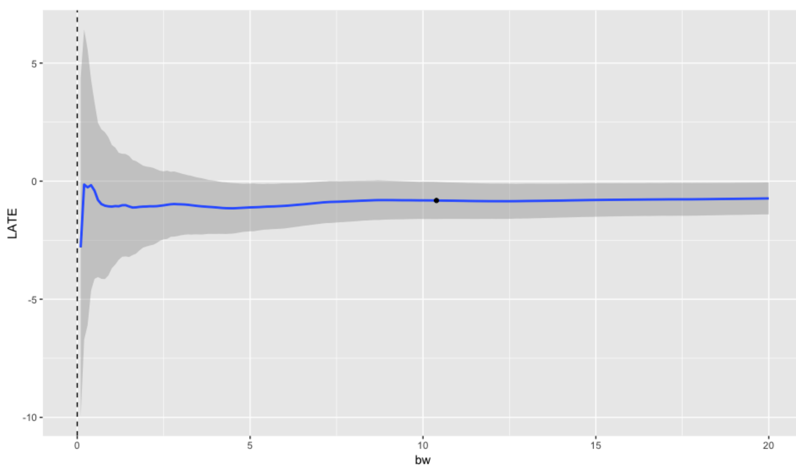
Figure 12: Serious Charges Sensitivity Analysis - LATE for Varying Bandwidths- Baseline with Fixed Effects and RDRobust data driven BWS



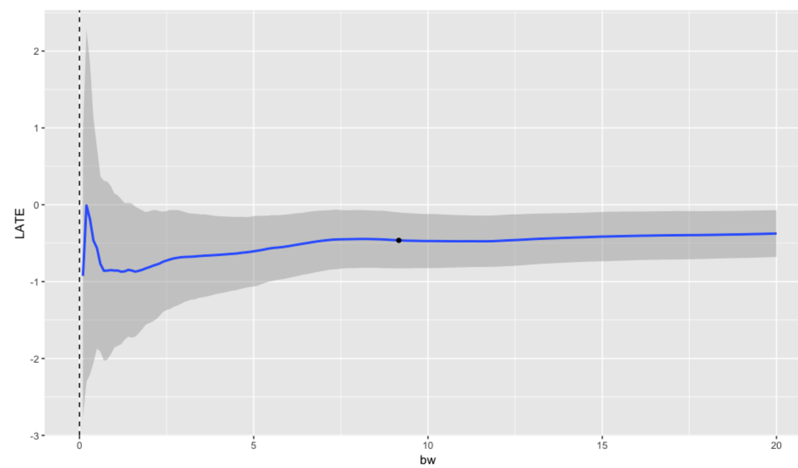
(a) Log Workdays



(b) Log Pay



(c) Log Materials

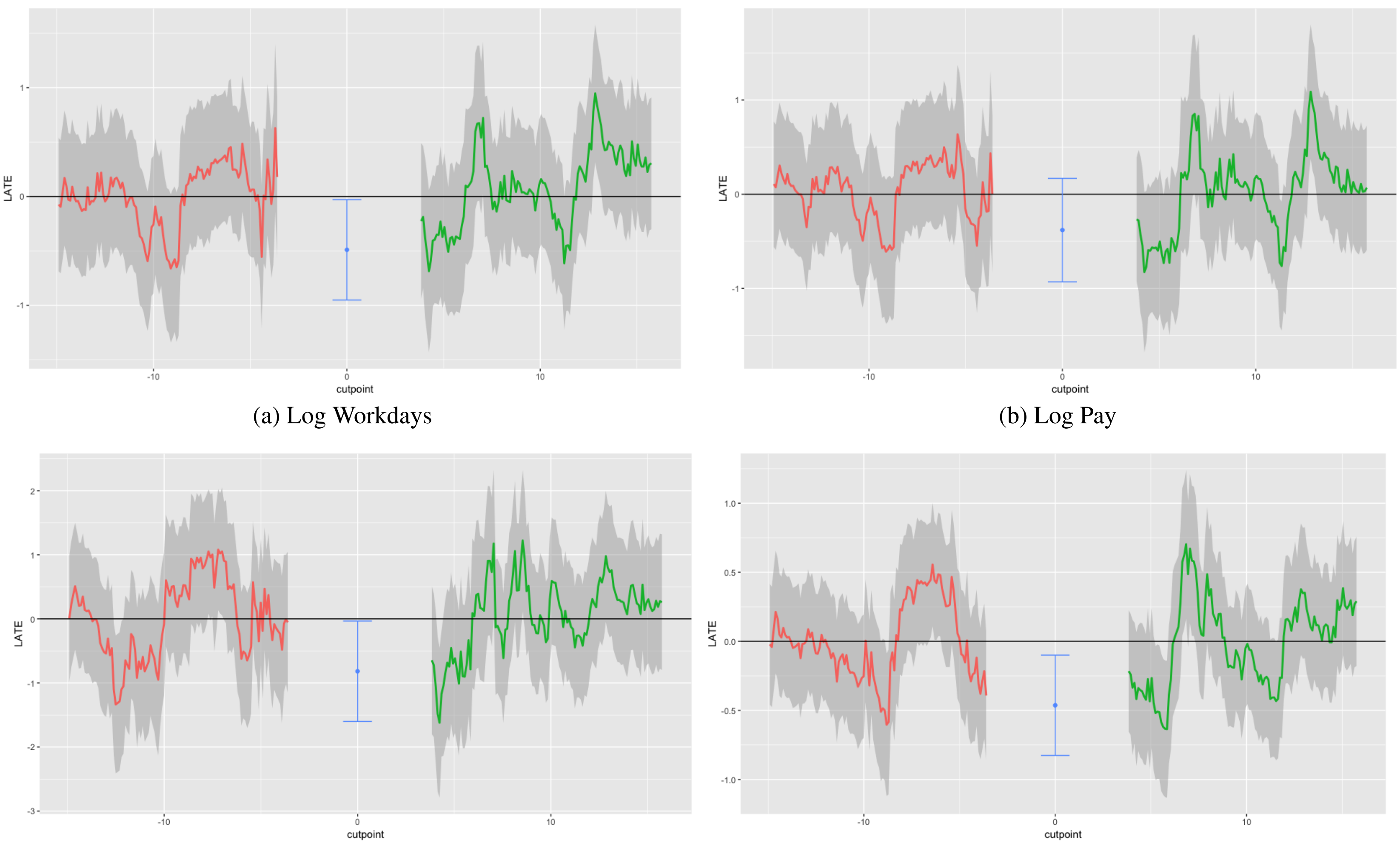


(d) Log Assets

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.

C.4 Serious Charges Placebo Tests

Figure 13: 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 Assets

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.

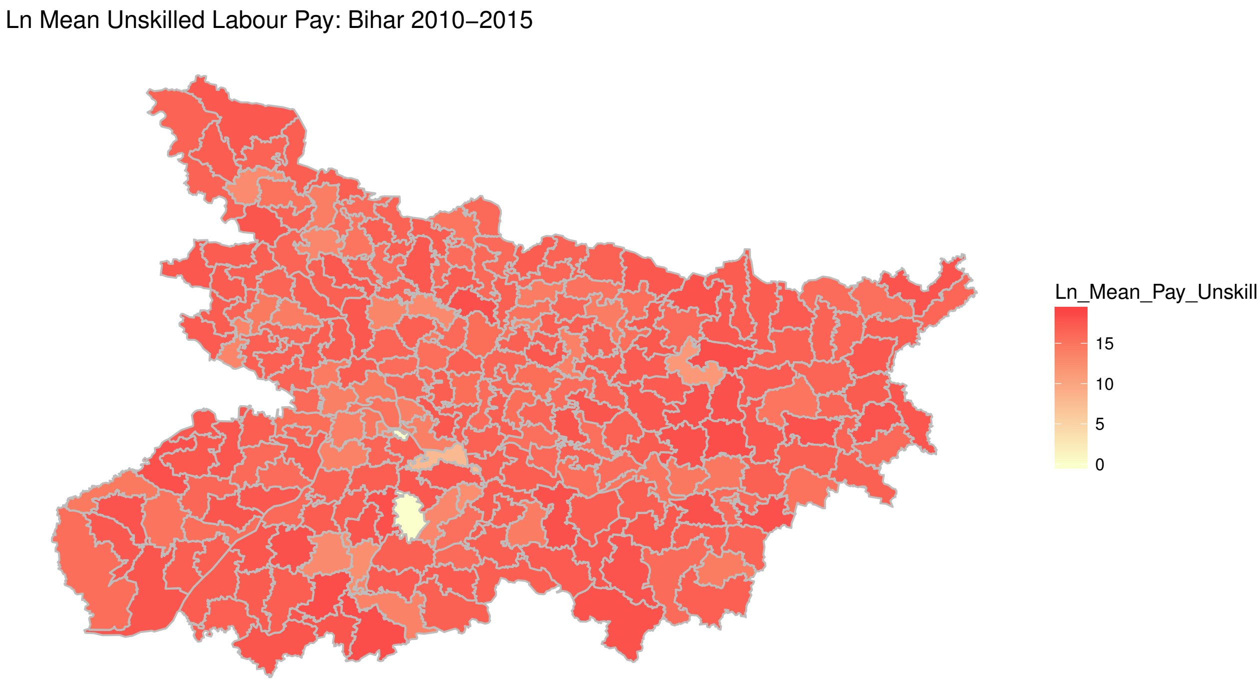
D State-Years in RD Sample

Table 21: 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

E Maps

Figure 14: Variation in Pay across Bihar Assembly Constituencies



F Unlogged Estimates

Table 22: RD Robust

	Workdays	Pay	Materials	Assets
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. Left Bias Corr.	1001.00	1012.00	1103.00	1080.00
Eff. Num. obs. Right Bias Corr.	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

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

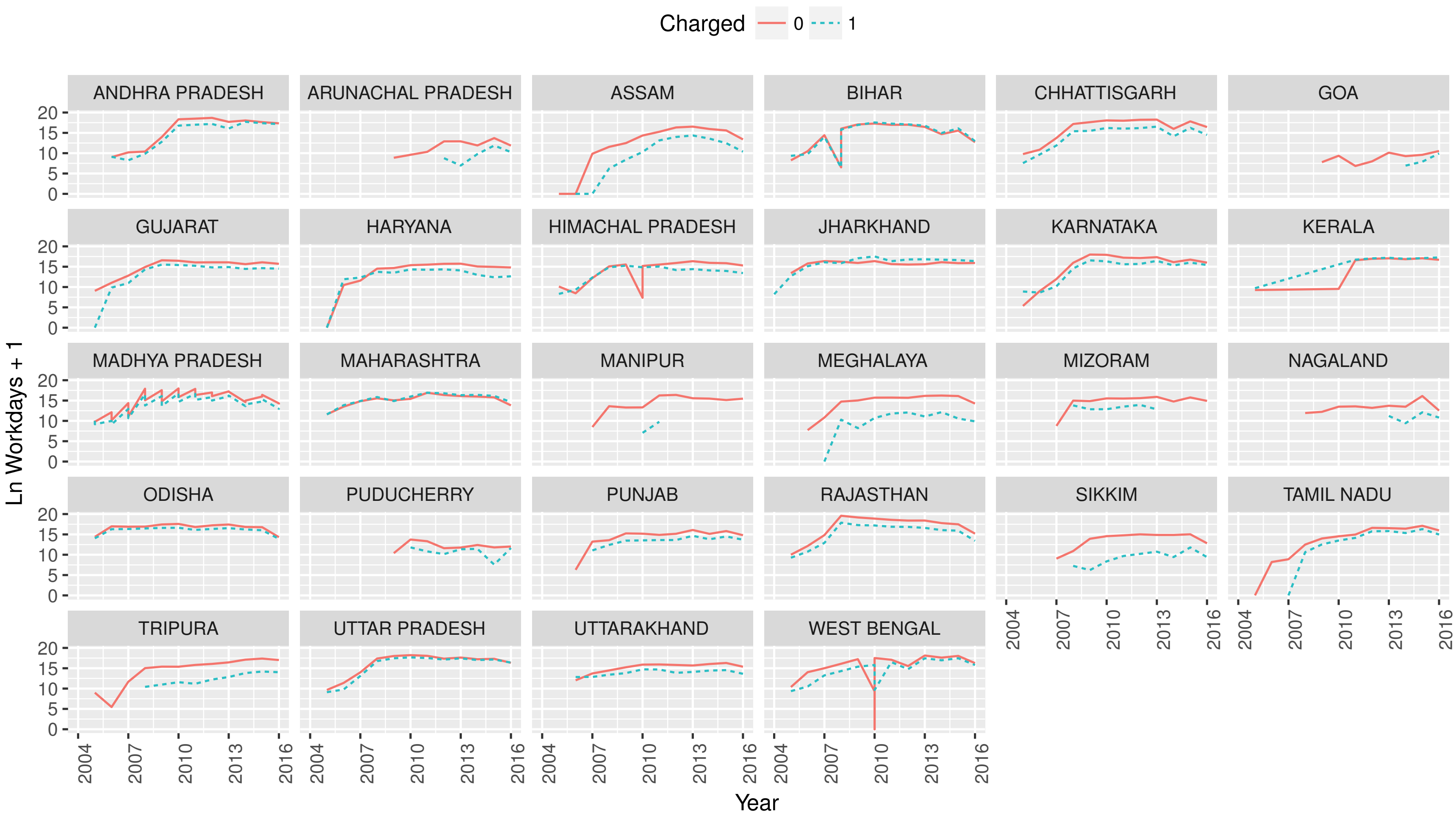
BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of 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.

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.[43] 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.

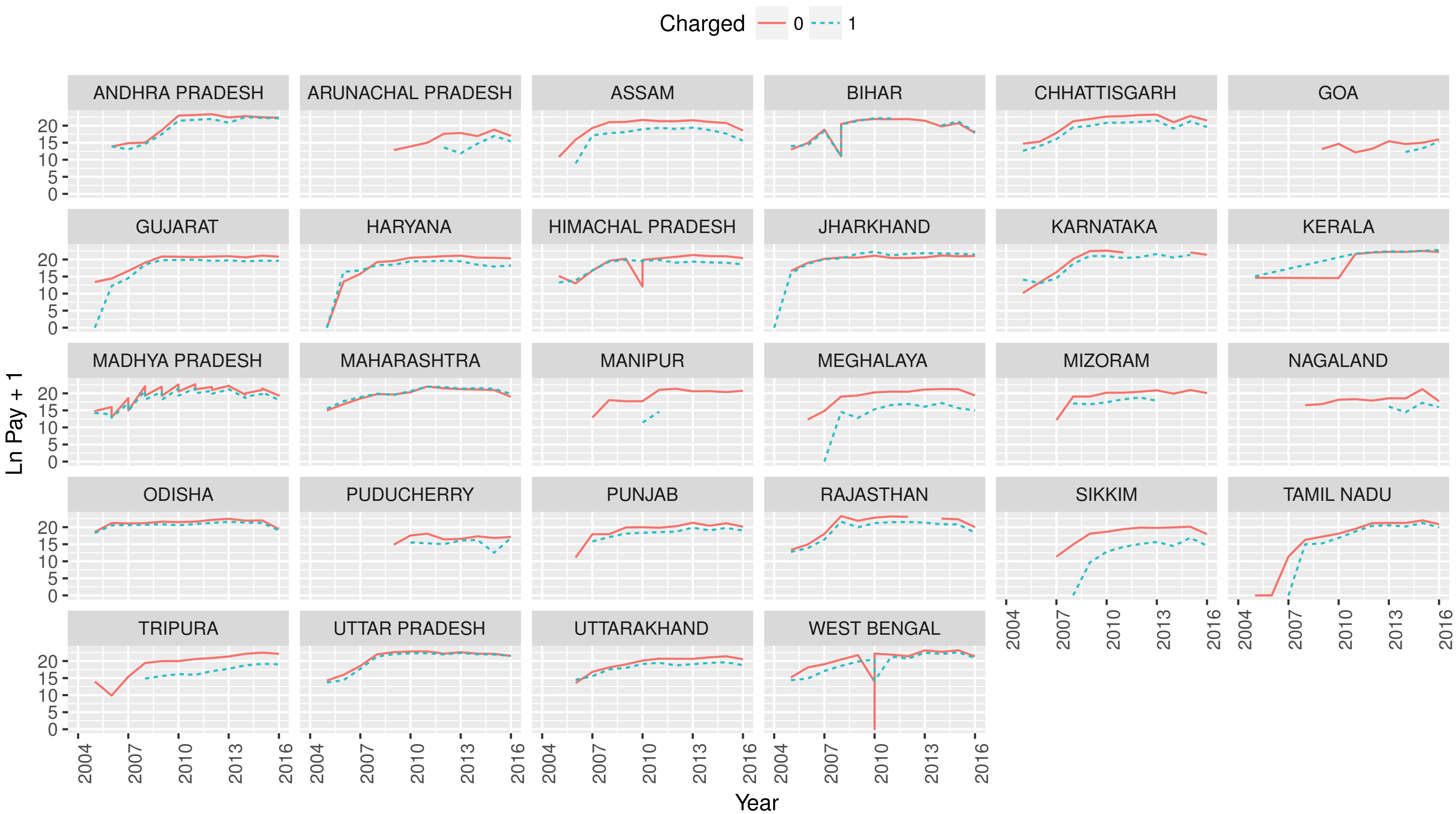
[43]This data is for the entire sample and is not restricted to mixed elections a la the RD sample. I also include all charges and do not restrict the definition of a charge to those of only a serious nature.

Figure 15: 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 16: 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 17: 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 18: Varation in Assets 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.

H Estimates from Various R Packages

Table 23: RD Package

	Log Workdays	Log Pay	Log Materials	Log Assets
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 24: RDD Tools

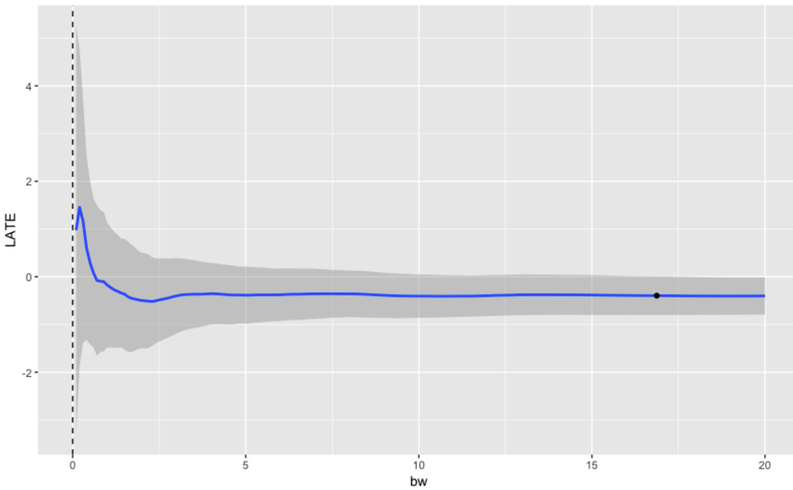
	Log Workdays	Log Pay	Log Materials	Log Assets
Estimate	-0.01	-0.03	-0.06	-0.01
	(0.17)	(0.18)	(0.29)	(0.13)
No. Obs	2679.00	2678.00	2670.00	2679.00
Order	1.00	1.00	1.00	1.00

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

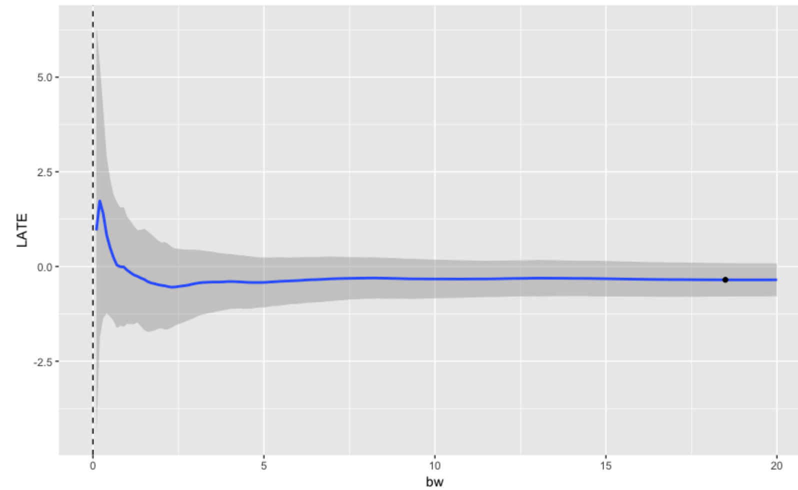
BW represents the bandwidth chosen by the CCT algorithm that minimizes Mean Squared Error. The number of observations represents those in the entire sample, while Effective number is the number of observations included inside the bandwidth. Local polynomials are estimated separately for each side of 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.

I Estimates from Varying Bandwith Sizes

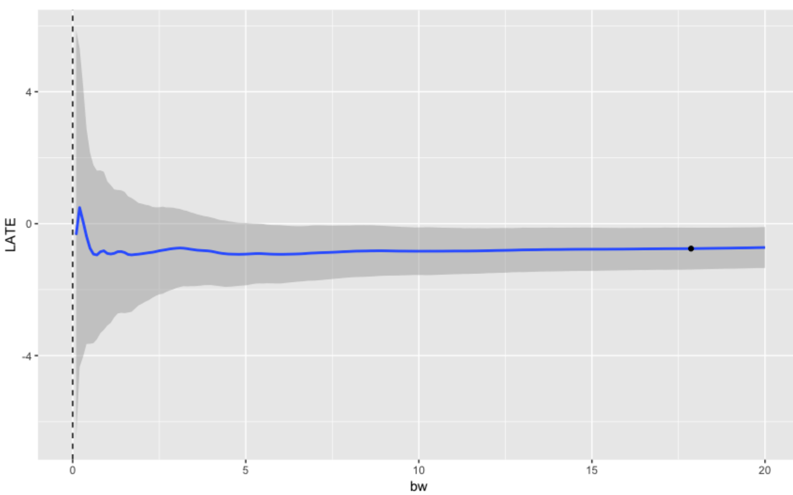
Figure 19: Sensitivity Analysis - LATE for Varying Bandwidths- Baseline with FE and rddtools



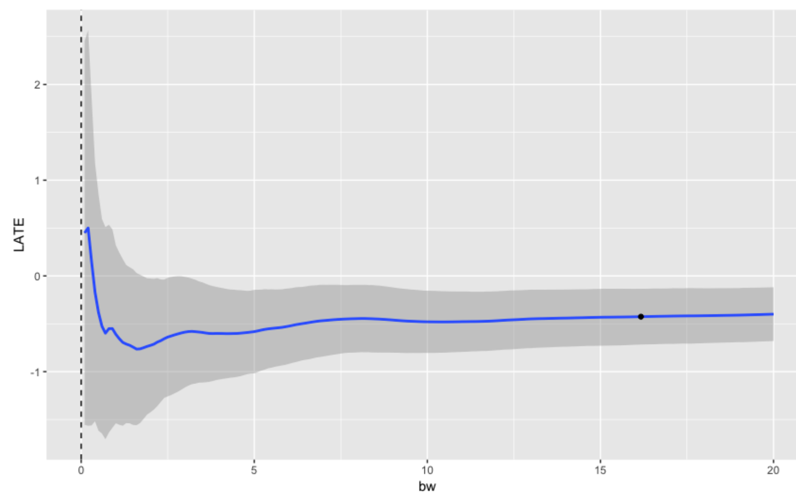
(a) Log Workdays



(b) Log Pay

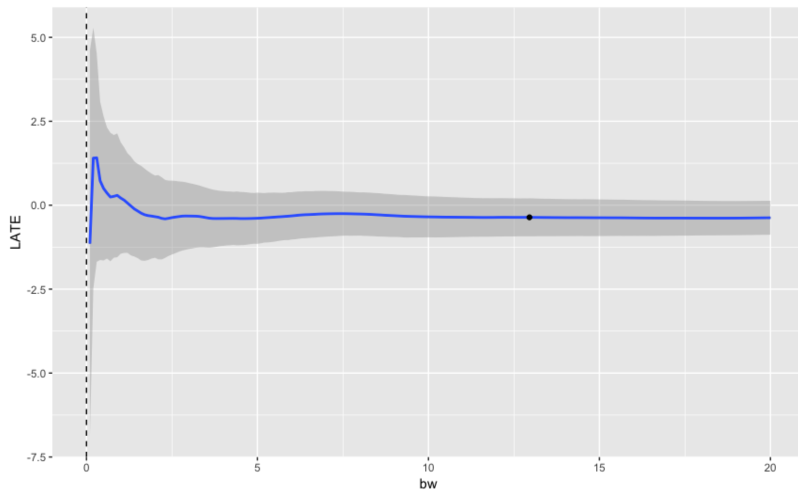


(c) Log Materials

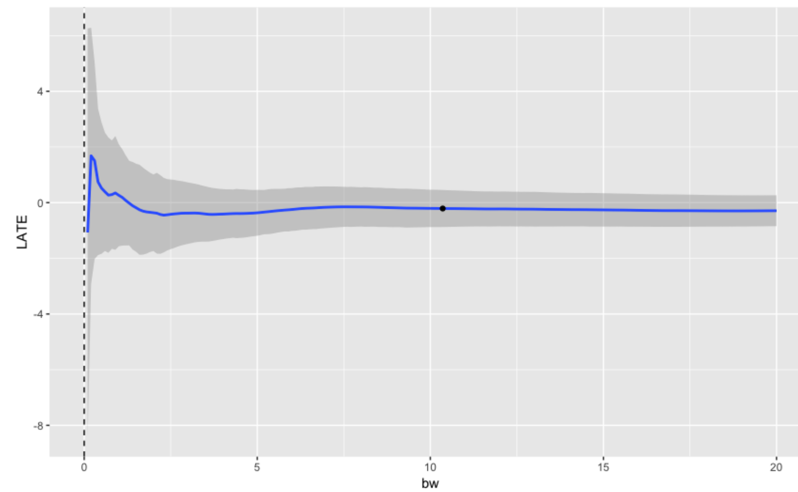


(d) Log Assets

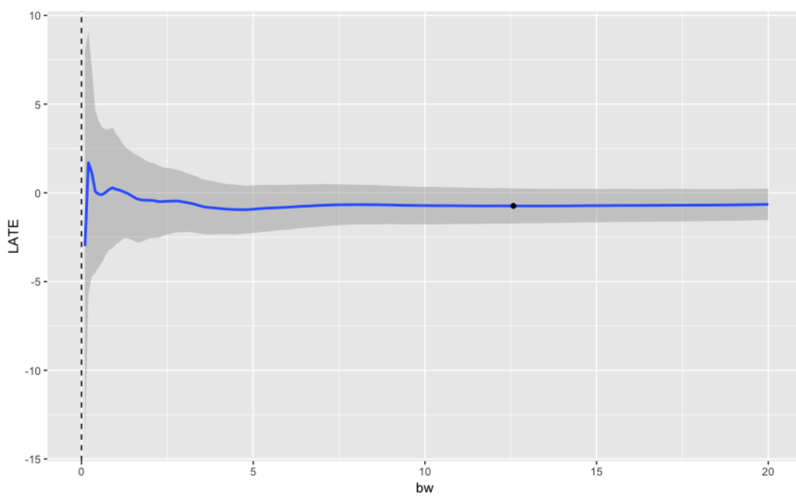
Figure 20: Sensitivity Analysis - LATE for Varying Bandwidths - Baseline with RDRobust data driven BWS



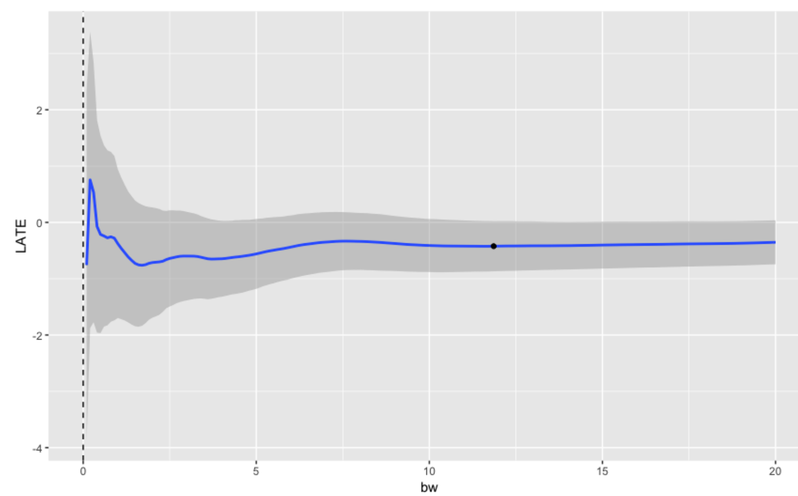
(a) Log Workdays



(b) Log Pay



(c) Log Materials



(d) Log Assets

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.

J Estimates from Varying Local Polynomials

Table 25: 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 26: 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

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

K Placebo Tests

Table 27: 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

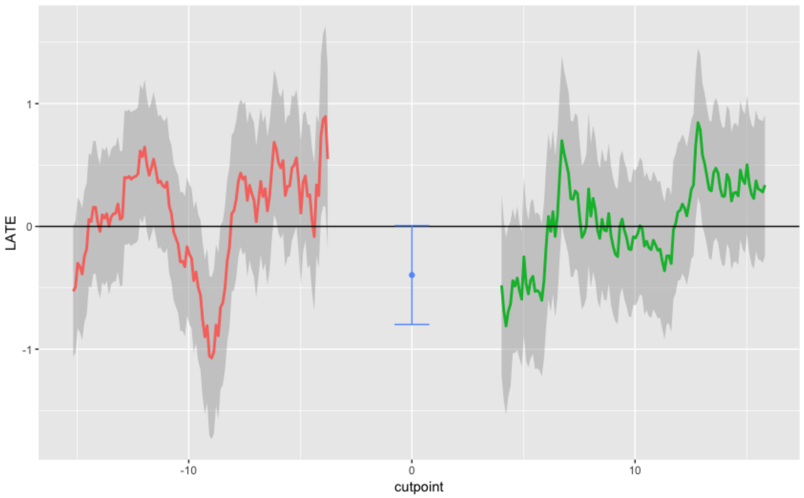
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Table 28: Local Polynomials Varying Order - Non Parametric

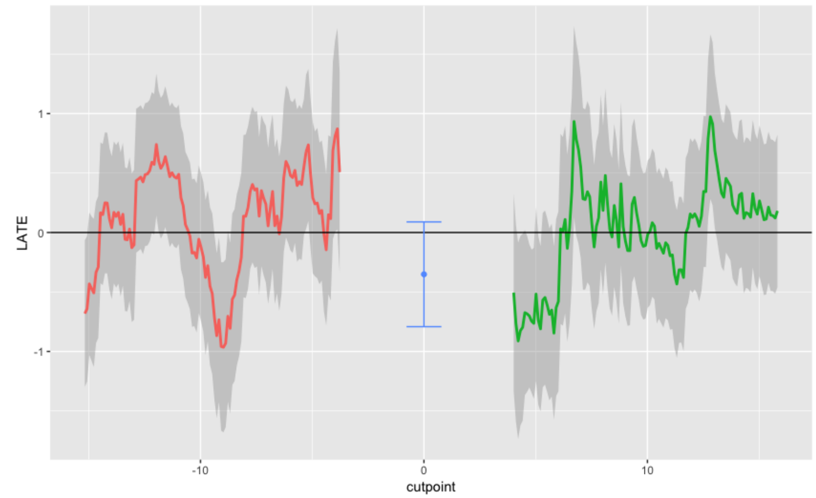
	Log Assets					
Polynomial Order =	1	2	3	4	5	6
Conventional	-0.42^{*}	-0.41	-0.39	-0.47	-0.63	-0.90^{*}
	(0.21)	(0.27)	(0.31)	(0.35)	(0.39)	(0.43)
Bias-Corrected	-0.48^{*}	-0.38	-0.41	-0.51	-0.66	-0.94^{*}
	(0.21)	(0.27)	(0.31)	(0.35)	(0.39)	(0.43)
Robust	-0.48^{*}	-0.38	-0.41	-0.51	-0.66	-0.94^{*}
	(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

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

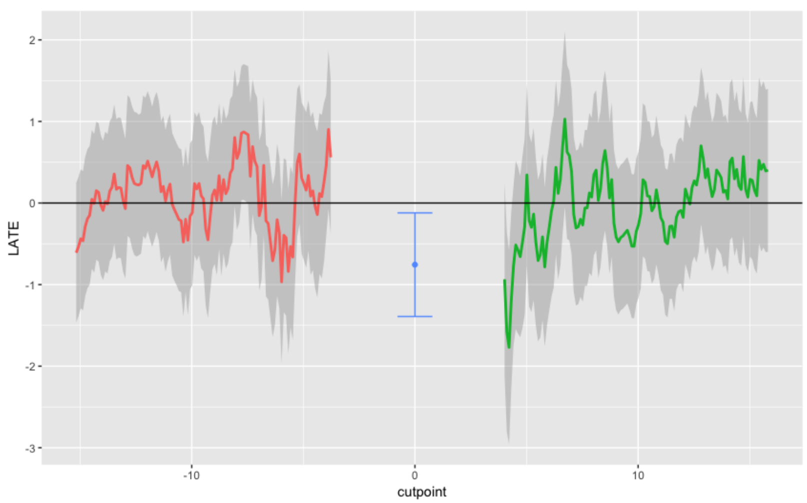
Figure 21: Placebo Tests - LATE for Varying Cutpoints- Baseline with FE and rddtools



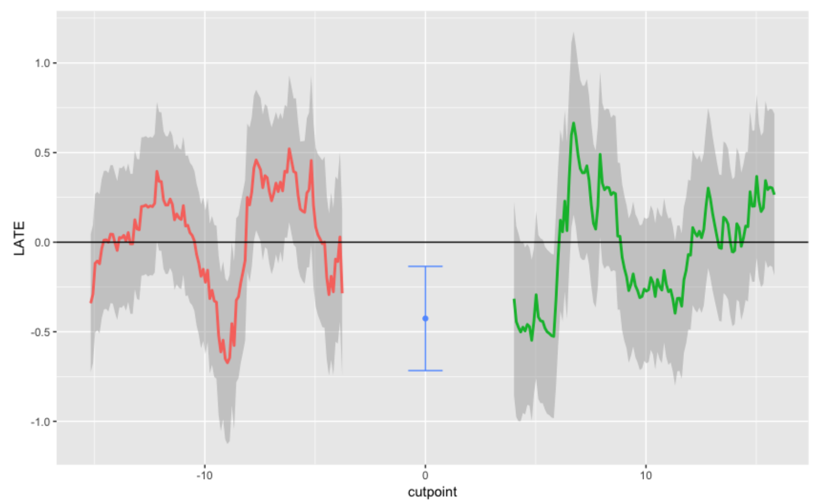
(a) Log Workdays



(b) Log Pay

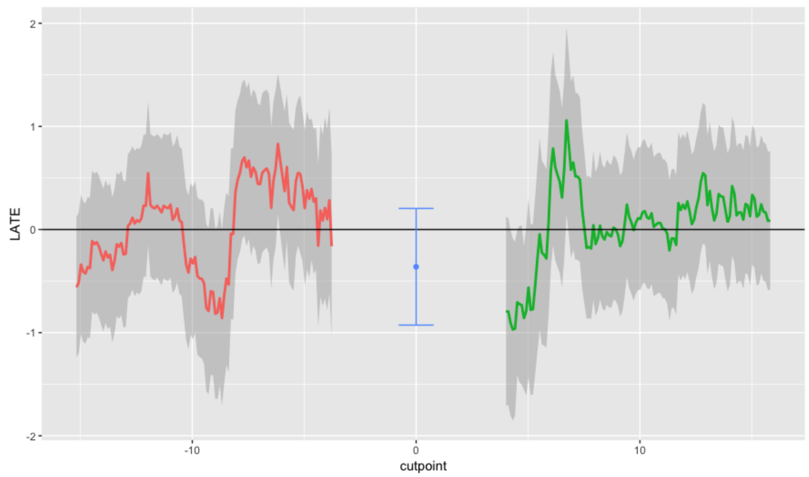


(c) Log Materials



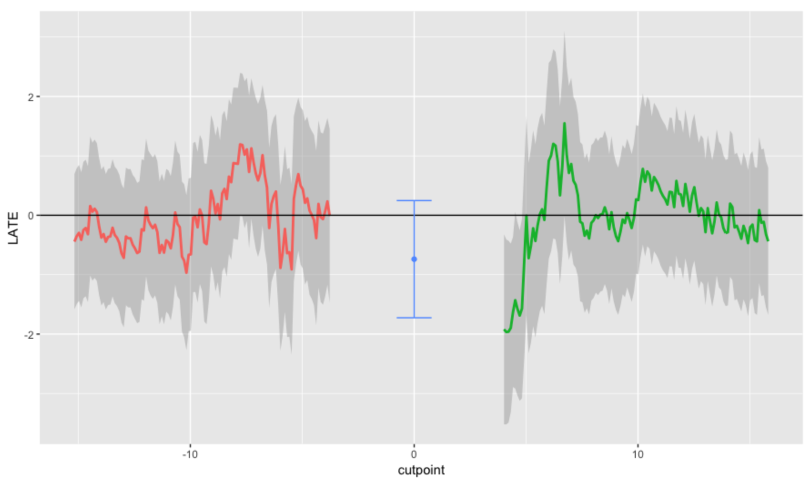
(d) Log Assets

Figure 22: Placebo Tests - LATE for Varying Cutpoints- Baseline with RDRobust data driven BWS

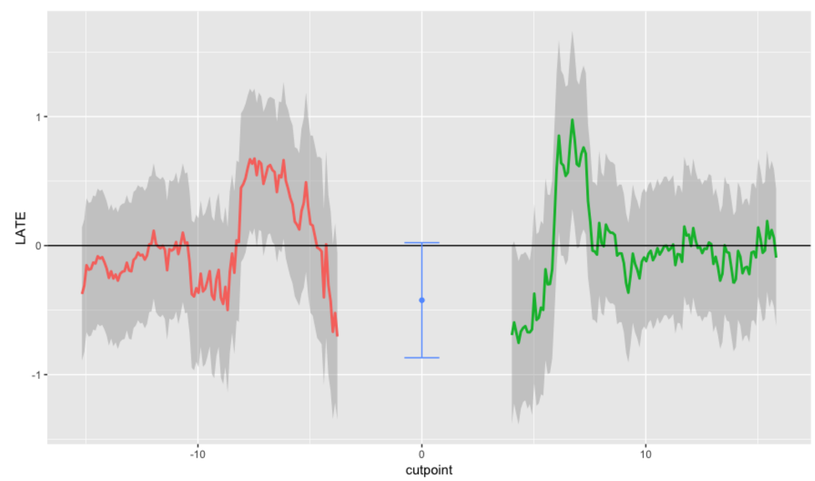


(a) Log Workdays

(b) Log Pay



(c) Log Materials



(d) Log Assets

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.

L Financial Years

Table 29: Financial Years: RD Robust

	Log Workdays	Log Pay	Log Materials	Log Assets
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. Left Bias Corr.	5412.00	5609.00	5867.00	5412.00
Eff. Num. obs. Right Bias Corr.	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 30: Financial Years - RD Robust

	Log Workdays			**Log Pay**			**Log Materials**			**Log Assets**		
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 31: Financial Years - Unlogged - RD Robust

	Workdays	Pay	Materials	Assets
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. Left Bias Corr.	5109	5227	5757	5303
Eff. Num. obs. Right Bias Corr.	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

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

M Wages and Employment per Project

While point estimates of NREGS provision are consistently negative across numerous specifications, only *Assets* 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 asset 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.[44]

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.[45] 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.

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

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

Table 32: 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.